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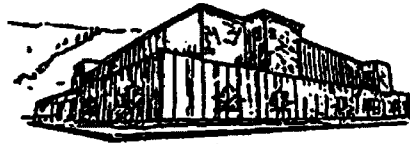
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**On the Origins of Linguistic Structure:  
Three Models of Regular and Irregular Past Tense Formation**

by

Benjamin J. Sienicki

B.A. University of Montana, 1998

presented in partial fulfillment of the requirements

for the degree of

Master of Arts

The University of Montana

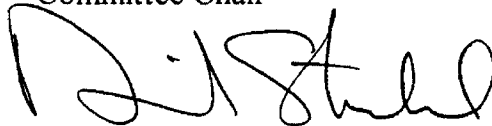
May 2001

*Approved by:*



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Committee Chair



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**On the Origins of Linguistic Structure:  
Three Models of Regular and Irregular Past Tense Formation**Committee Chair: Irene Appelbaum 

Bates and Goodman (1999) represent a “unified lexicalist” approach to grammar, arguing that both grammar and the lexicon are subserved by the same domain-general learning mechanism, and further, that grammar “emerges” from the lexicon. Plunkett and Marchman’s work on past tense formation (1991, 1993), improving upon the modeling techniques of Rumelhart and McClelland (1986), exemplifies a unified lexicalist approach. By abandoning the more traditional dual-mechanism approach (Chomsky & Halle, 1968; Chomsky, 1968, 1986, 1988; Pinker, 1994, 1999; Pinker & Prince 1988, 1994), unified lexicalists aim to provide a more plausible account of child language acquisition beyond the rote-learning phase. Pinker (1999), in the spirit of the Pinker & Prince (1988, 1994), repudiates the unified lexicalist approach, however, on grounds that single mechanisms model the acquisition of regular and irregular morphology inaccurately. Results of psychological studies such as the “wug” test (Gleason, 1958; Pinker, 1999) suggest that the transition to the system-building phase (Stage 2 in the u-shaped learning process) is largely underdetermined in the unified account. It is argued that unified lexicalists fail to (i) offer a coherent definition of “emergence” and (ii) adequately clarify how, or by what mechanism(s), grammar can properly be said to emerge from the lexicon. On the other hand, it is argued that Pinker fails to (i) provide a clear account of that which is “instinctual” about the dual mechanism when it comes to regular and irregular morphology, (ii) address the improvements made by connectionists on the single mechanism model, and (iii) explain how his higher-level psychological theory can be implemented at the lower neurological level (without appeal to a connectionist “abstract neurology”). In a more comprehensive approach to the emergence of regular and irregular past tense, one that operates on different levels of analysis (psychological versus neurological), both single- and dual-mechanism accounts hold indispensable pieces of the explanatory puzzle.

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## 1.0 The Issue:

A Martian scientist, landing on earth to perform a taxonomy of its species, would rightly conclude that there is something unique to the system of communication used by humans. The grammatical structure of human language renders it distinct from all other instances of animal communication. Only human language reveals such combinatorial, exponential, and recursive (Pinker, 1999: 1-19) power, or “generativity.” What causally determines the grammatical structure of human language is one of the most fundamental questions concerning linguistics of the 21st century.

Linguistics of the late 1950s, fueled by the revolutionary work of Noam Chomsky, was compelled to the view that a grammar module, operating according to innate grammatical principles, accounts for the structure of human language. On Chomsky’s view, the structure of language is essentially hard-wired, or built into the brain. Our biogenetic endowment ensures a domain-specific mechanism, a “mental organ,” which is dedicated to the acquisition of grammatical rules. The grammar module is a highly systematic--indeed, *productive*--cognitive mechanism. The lexicon, by contrast, is considered to be the repository of the arbitrary (e.g. Chomsky & Halle, 1968).

In the mid-1980s, an alternative to the classical nativist/empiricist dichotomy began to take shape, what is referred to as “emergentism.” On the emergentist view, the structure of language is largely self-organized (from both evolutionary and developmental standpoints), resulting from conservative genetic interaction with a structured world. Humans are biogenetically endowed with non-linguistic, general cognitive mechanisms, and the rate at which these mechanisms develop actually acts as a constraint on language



acquisition and systemization. Linguistic structure is thus a by-product of maturational constraints on the development of non-linguistic cognitive mechanisms, and lexical processes accomplished by a general cognitive mechanism which detects regularities in the input. Rules of grammar are generalized over these regularities, emerging as *distributed*, “virtual” representations, not as explicit unconscious representations (as it is often put) inherent in the brain’s architecture.

Allegiance to Chomsky’s approach is nevertheless still pervasive. It seems the dominant paradigm in linguistics today, although gradually shifting, retains some fundamental nativist assumptions (e.g. innate knowledge of language, domain-specificity, genetic determinism). Pinker (1999), for instance, holds that the structure of language is *primarily* generated by grammar, or by a “rule” module; further, this module operates according to (putatively) innate principles. Although Pinker dissents from Chomsky and Halle’s treatment of the irregulars (see “Rules All the Way Down” section), he concurs with them on one fundamental point: linguistic structure is most significantly determined by a language-specific cognitive organ, the microcircuitry of which has been “hard-wired,” or prespecified in the genome (though not necessarily *directly* prespecified, see “Unified Lexicalist,” “Convergent Model,” and “Discussion” sections). Our biogenetic endowment ensures a complete modular separation between lexical and grammatical processes (i.e. between “words” and “rules”). The question concerning nativists and emergentists alike is whether the structure of language is determined by a domain-*specific* module, dedicated to the acquisition of grammatical rules, or by domain-*general* cognitive mechanisms.

## 1.1 The Models:

Chomsky and Halle (1968) treat irregular and regular morphology as a highly lawful, rule-bound enterprise, in which most regular and irregular changes boil down to a small handful of “austere” grammatical rules. For example, the past tense changes of nearly 165 irregular verbs are handled by only three phonological rules (Chomsky and Halle, 1968; also Pinker 1999: 92-4). Chomsky and Halle thus present a “rules all the way down” dual-mechanism model. One mechanism, a robust grammar module including at least one sub-module for phonological processes, generates infinite structure according to explicit unconscious rules, while the other mechanism, the lexicon, is finite, variable, and more or less discontinuous. (See figure 1.)

Pinker (1999), though, departs from Chomsky and Halle’s view that irregular verbs are handled by explicit unconscious rules. Pinker presents an alternative model of irregular past tense formation, in which irregulars are mediated by a parallel distributed processor, or a pattern associator memory. The pattern associator produces rule-*like* behavior (stem-stem and change-change structures), but only according to local processes of association, not according to explicit unconscious rules. Pinker thus presents a dual-mechanism model, in which irregulars are handled by a pattern associator and regulars by a “rule” module (a symbol processor). Pinker posits an innately constrained, hard-wired “blocking mechanism” to account for much of the success children have with past tense formation toward the end of system-building phase and throughout the fine-tuning phase (Stage 3 in the U-shaped learning pattern). (See figure 2.)

Bates and Goodman (1999), Plunkett and Marchman (1993), and Rumelhart and

McClelland (1986) all contribute evidence for language's emergence. Elements from the work of each of these scholars represents a third approach to modeling regular and irregular past tense formation--the emergent approach. Bates and Goodman (1999), for instance, argue that grammar "emerges" from the developmental and mechanical processes of a single, general cognitive device (modeled by Rumelhart and McClelland and Plunkett and Marchman for past tense formation). Bates, Goodman, Rumelhart, McClelland, Plunkett, and Marchman all entertain some version of the single mechanism model, in which linguistic structure is an emergent by-product of maturational constraints and the inherent processes of associative memory (i.e. pattern association). Emergentism is informed by state-of-the-art research being done in human genetic mapping (the Human Genome Project), evolutionary biology, developmental psychology, and computer science. (See figure 3.)

## 2.0 Unified Lexicalism:

From their title, "On the Emergence of Grammar from the Lexicon," Bates and Goodman (1999) make it clear that their theory of how children come to perform complex grammatical operations departs from traditional empiricist (Aristotle-->Locke-->Skinner) and nativist (Plato-->Descartes-->Chomsky) views. Bates and Goodman attempt to place the nature-nurture debate on a novel playing field, preserving elements of both extremist views, and departing from other problematic claims. But how exactly should we characterize this departure? On the empiricist view, children *learn* how to use language with general learning mechanisms and the help of explicit training (also "on analogy," via

“generalization,” or even by way of “operant conditioning” in behaviorist terms), rather than knowledge of language somehow being encoded in the genome. Empiricists, then, believe that grammar is learned bottom-up, through inductive learning accomplished by mechanisms similar to (if not the same as) ones we use to learn how to tie our shoes, mentally rotate images, succeed at Magic Eye, direct our attention, store information, and generalize over past experience. Nativists, by contrast, emphasize top-down acquisition of knowledge of language; that is, to a considerable extent KOL is thought to be present at birth, children having been biogenetically endowed (e.g. by natural selection or physical law) with a domain-*specific* (i.e. for grammar proper) mechanism (or “mental organ”) that operates according to built-in principles (Chomsky, 1986, 1995).

Bates and Goodman’s notion of emergentism, specifically with regard to the emergence of grammar from the lexicon, departs from traditional empiricism because it entails some version of the biological endowment argument (e.g. chronotopic innateness; see Elman, 1999), albeit a toned-down version. Emergentists admit that our biological endowment of *non*-linguistic mechanisms used in language computations constrains language acquisition. “Learning plays a central role but does so within biological constraints” (Bates & Goodman, 1999: 31). It is not the case that we bring a “blank slate” to the task of language acquisition; rather, acquisition occurs in a complex synergy between biogenetic constraints (on the development of domain-general mechanisms) and environmental constraints like the structure of the input. Grammatical abilities are thought to be spawned *indirectly* from genetic specification. Bates and Goodman depart from the nativist tradition, however, arguing that grammar is mediated by domain-*general* learning

mechanisms; in fact, grammar emerges out of the lexicon, which itself is a product of general cognition. With these departures from both empiricism and nativism, emergentism denotes a “genuine third alternative” (31). Bates and Goodman leave the “toning-down of the innateness claim” project to other researchers (Elman, 1996, 1999), and primarily take issue with the nativist claim of domain-specificity.

My view is that domain-specificity is an untenable claim at the *implementational* (neural) level. In light of current findings in genetics and neuroscience, it is implausible that our biogenetic endowment would specify precisely that class of stimuli which neural “language” mechanisms can serve. Even if humans are limited to the use of domain-general cognitive mechanisms throughout early ontogenesis (1-4 years of age), it is not necessary to view general cognition as an unconstrained monolith--entirely open-ended--with no sub-mechanisms or sub-layers<sup>1</sup>. The proposal is that we can have a “modular” theory (in the strict sense of multi-layered processing), yet simultaneously retain the claim to domain-generality (and thus, *emergence*). The single/dual mechanism characterization of past tense models does not map directly onto the domain-generality/domain-specificity distinction. Modularity effects detected as early as age 3 (e.g. in past tense formation, see Kim et al., 1994) might, in fact, be signs that two different *general* cognitive sub-layers, or mechanisms, are beginning to dissociate, each subserving a more specialized general cognitive function (e.g. rapid versus attended categorization of stimuli). By no means, though, do we have to rely upon *direct* genetic constraints to ensure these effects. Even Bates and Goodman admit that modularization is a normal aspect of ontogenesis (1999:

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<sup>1</sup>Newport et. al (1999), for example, find evidence for an innate statistical learning mechanism, but this is a

64). Nevertheless, modularity effects are the *outcome* of development, not its cause.

Bates and Goodman propose a “unified lexicalist” (37) approach to grammar in the spirit of Goldberg (1999). The unified lexicalist approach to grammar’s emergence stands in direct opposition to nativist approaches that posit domain-specificity.

Most nativists concede that the lexicon is finite, varies markedly over languages, and must be learned (at least in part) through brute-force [bottom-up] inductive procedures that are also used for other forms of learning, linguistic and non-linguistic...[But] Because core grammar is universal, functionally opaque, and infinitively generative, the domain-general procedures that are used to acquire words cannot (it is argued) work for the acquisition and processing of grammar. (Bates and Goodman, 37)

If grammar cannot be learned bottom-up, then grammatical and lexical development must unfold on independent developmental pathways (Chomsky, 1986; Pinker, 1994, 1999; Ouhalla, 1999). Both nativists and Bates and Goodman would agree that *lexical* learning utilizes general cognitive mechanisms. The heart of the debate is whether grammar (putatively domain-specific) shares an *interface* with the lexicon (i.e. in the sense of two separate modules sharing information via a mediating mechanism), or, on the other hand, whether it *emerges* from the lexicon (i.e. is subserved by the same domain-general mechanism as the lexicon). If grammar and the lexicon turn out to be separate modules, we have no grounds for claiming that grammar “emerges” from general learning mechanisms. Contrarily, if general learning mechanisms are all children bring to the task of language acquisition, we must re-evaluate the fundamentals of generative linguistics in general. We could no longer hold the view that grammar is determined by “knowledge of

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component of *general* cognition.

language” (Chomsky, 1986), present in our *brains*; instead, grammar would be determined by “knowledge of linguists,” and their descriptive vocabulary.

In certain atypical populations (e.g. brain damaged patients with localized lesions), nativists look for a double dissociation between grammatical and lexical proficiency as evidence that *domain-specific* structures in the brain subserve these two different types of operations, or in any case, that the same mechanism does not subserve both. For example, if grammatical proficiency after brain trauma remains stable while lexical proficiency or general learning plummets, this is an indication that grammar and the lexicon are mediated by *separate* mechanisms. If, in a different population, grammatical proficiency after brain trauma plummets while general learning remains intact, this is even further evidence that we have two separate mechanisms at work. Thus, nativists would clearly *not* agree with Bates and Goodman that the lexicon and grammar are “unified” in a strong sense<sup>2</sup>. Bates and Goodman provide evidence that the lexicon and grammar are inextricably tied throughout childhood (1-3 years old) in normal populations, citing a strong correlation between *grammatical* scores on parental reports/proficiency tests and *lexical* tallies, so strong that an assessment of lexical proficiency is the best available predictor of later grammatical proficiency. Never do grammatical skills outstrip lexical skills, even into the “very heart of grammatical development” (46), between age 3 and 3.5, when normally developing children can produce most of the elementary syntactic structures of their language (passives, relative clauses, etc.). In abnormal populations (late/early talkers,

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<sup>2</sup>Current generative linguistics (e.g. Chomsky’s *minimalist* program, 1995) focuses on how Universal Grammar (UG) accesses the lexicon, so clearly there is thought to be some exchange of information at an *interface*, but this exchange *only* occurs at the interface (otherwise the mechanisms are autonomous).

children with brain lesions, Williams syndrome, Down syndrome, Specific Language Impairment), apparent dissociations, or even double dissociations, are either non-existent within the 1-3 age bracket, or result from *non*-linguistic deficits.

The only two cases of clear dissociation, that of DNS and SLI, can be traced to acoustic deficits, which themselves (coupled with other non-linguistic impairments) account for differences in vocabulary size and grammatical complexity. DNS individuals, for example, exhibit significant impairment of auditory short-term memory compared to WMS individuals, though they score significantly better than WMS patients on visual short-term memory tasks (Wang & Bellugi, 1994). The DNS sample's auditory deficit, in conjunction with other general cognitive deficits (low IQ, 40-60), account for the fact that DNS vocabulary and grammar skills dissociate, while WMS scores reveal no such dissociation.

Under these circumstances, it is perhaps not surprising the DNS individuals are selectively impaired in the ability to detect, store, and retrieve those aspects of their linguistic input that are lowest in phonological salience (as Leonard, Bortolini, Caselli, McGregor, & Sabbadini, 1992, reported for children with SLI) and lowest in visual imagery (as Goodglass & Menn, 1985, reported for adults with Broca's aphasia). (Bates & Goodman, 1999: 62)

To date, no population--normal or abnormal--exhibits a dissociation of grammatical and lexical proficiency that can be traced to impairment of a domain-specific grammar module, as Pinker (1991) suggests for DNS individuals.

Bates and Goodman are careful to draw a distinction between *domain-specificity* and *localization* (65). A certain function can be "localized" (i.e. mediated routinely by the same region in the brain) and simultaneously attend to *other* classes of stimuli (e.g. non-



linguistic stimuli). Bates and Goodman concede that localization effects might lead to modularity later in life, but not during the formative years of child language acquisition (64). The flaw in Bates and Goodman's argument is that from a correlation between test scores in their data (MLU, CDI, etc.), they infer that "the acquisition and neural representation of grammar and the lexicon are accomplished by domain-general mechanisms" (39). Few nativists, neither Pinker nor Chomsky, for example, nor emergentists demanding an account of the *mechanisms* whereby grammar "emerges," would accept this leap from correlation to causation.

Bates and Goodman's evidence (a strong lexicon/grammar correlation across normal populations and a lack of dissociation in atypical populations) can actually be accommodated by the nativist approach. As Bates and Goodman themselves recognize, "correlation is not [common] cause" (43). Even if we grant that lexical and grammatical proficiency are reliable predictors of each other cross-linguistically, we can only say that a dissociation "seem[s] to require" a "separate neural system for grammar" (67), and conversely, that a strong correlation is evidence only that the lexicon and grammar *seem to be* mediated by the same domain-general mechanism. The same evidence could be seen in an entirely different light. From the nativist's perspective, all the evidence shows is that the development of two independent proficiencies (lexical and grammatical) is highly correlated. This would make perfect sense for the nativist because the lexicon and grammar are thought to share an interface with one another which propels language acquisition into and beyond the first word combination stage (18-20 months). Syntax must have some way of accessing semantics, especially after 18-20 months. Bates and

Goodman's faulty assumption is that nativist accounts *necessarily* presuppose a "hard" dissociation. Bates and Goodman might be mistaking the operation of a syntax-semantics interface, which would reveal a strong lexicon/grammar correlation, as the operation of syntax proper. Even a generative nativist (not that these two characterizations are necessarily coextensive) would recognize a lawful correlation between grammar and the lexicon at the syntax-semantics interface (e.g. Chomsky, 1968, 1981, 1986, 1988, 1995).

Bates and Goodman claim that grammar "emerges" from the lexicon, but without violating their own principle, they cannot provide an adequate account as to what this means, or *how* (by what mechanism) emergence occurs, so their use of the term rings with "spookiness," or at least underdeveloped hypotheses. As such, emergence in Bates and Goodman's sense is only a thumb nail sketch of a "genuine third alternative." Bates and Goodman provide a number of examples of emergent outcomes, representing the specific senses of non-predictability, self-organization, and so on, but they never explain what the "emergence of grammar from the lexicon" means, aside from the reticent suggestion that lexical and grammatical processing "seem to be" mediated by the same domain-general learning mechanism. This problem of vagueness is not only terminological, indicative of an inadequate or incoherent definition of "emergence"; even worse, it is *substantive*, indicative of an inadequate account of the *mechanisms* whereby grammar emerges. Part of my project, then, is to formulate a definition of grammar's emergence in terms of non-predictability, self-organization, and non-additivity, thereby rendering the implicit connections in the analogies (honeycomb, giraffe's neck, ram's horn, and bubbles) explicit. My role is to de-mystify Bates and Goodman's use of the term "emergence," by providing

a *mechanistic* account of the emergence of grammar (in specific, an account of the grammatical structures involved with past tense).

Nevertheless, without more definitive evidence, whether lexical and grammatical development occur on the same or on different pathways remains unanswerable. The evidence that Bates and Goodman offers might be regarded as dubious, moreover, for a number of methodological reasons (see “Problems” section). Another part of my project is to make clear that the possibility of Bates and Goodman’s proposal (domain-generality) being generalized to explain cross-linguistic data is largely an open-ended empirical question. Either we can dig in the trenches looking for evidence for a *structured* lexicon (e.g. for prototypical light Vs and the grammatical features they encode; Goldberg, 1999) or for strong enough constraints on grammar’s emergence (Elman, 1999), or we can take the view that we need to look at *specific* grammatical operations and determine what kind of neural mechanisms these operations require (e.g. serial vs. parallel processors; Pinker, 1999).

It is possible that grammar and the lexicon are, in fact, modularized, though not altogether informationally encapsulated, and implemented in very different types of neural architecture. The “almost lawful” correlation could in fact be a result of the independent but simultaneous development of two interfaced modules, especially in light of the fact that *both* lexical and grammatical operations must be performed for the production (/comprehension) of phrases and early sentences (i.e. beyond 18 months). When we look at specific grammatical operations (e.g. regular and irregular past tense formation; Pinker, 1999), “words” and “rules” appear to be implemented in very different neural processors.

It is an unnecessary move, though, to take the further step and claim that grammar is *representationally* innate (Elman, 1996, 1999), but I think that most nativists would avoid this obvious blunder.

Nowhere in the nativist literature (Chomsky, 1956, 1986, 1988; Pinker, 1989, 1994, 1999) is a claim made for representational innateness to the extent Elman (1999) proposes (i.e. to the extent that KOL is *directly* encoded in the genome, that the genome prespecifies all relevant synaptic connections for UG's implementation). One of the criticisms brought to bear on Pinker (this paper) focuses on his failure to clarify *how* certain genes ensure the wiring scheme that implements KOL. Vagueness is ultimately Pinker's downfall, though in this case, it at least prevents him from being mis-categorized.

The representationally innate position, as sketched by Elman, is a straw-man position, and when we throw this option out as a possible explanation of what nativists have in mind, we are left with a toned-down version of innateness, just a shade stronger than the emergentists'. Still, a shade's difference could mean *all* the difference in this debate. Chomsky (1986, 1988) and Pinker (1994) suggest that certain genes code for domain-*specific* linguistic (non-lexical) mechanisms; whereas Bates/Goodman and Elman (1999) suggest that certain genes code for domain-*general* mechanisms which get used throughout development for linguistic processing (and perhaps other non-linguistic processing), and that the maturational schedule of these mechanisms actually acts as a constraint on child language learning.

## 2.1 "Emergence" for Bates and Goodman:

Bates and Goodman provide many examples of emergence (32-35), but the “bubble” analogy is their first. The spherical shape of bubbles is accomplished not because something *in the bubbles* necessitates this structure, but because it is the “only possible solution to achieving maximum volume with minimum surface area” (32). The mathematic law governing volume/surface area relations limits the architectural possibilities to a single outcome. So long as soap and water come together (and basic environmental conditions are met), this brute fact about the world stands in the way of bubbles taking on alternative shapes (e.g. triangles or diamonds), and thus constrains them into adopting the spherical design solution. This is an example of *self-organizing* design, in the sense that the structure of bubbles is not prespecified in any of the properties of soap and water. Bubbles find themselves in a spherical shape time and time again because the world provides no other possible solution. The soap and water blindly follow local constraints (i.e. mathematical facts about the world) and the outcome of sphericalness emerges.

Insofar as language is concerned, grammar “emerges” for Bates and Goodman from interactions between lexical processes (/development) and a structured world--in this case, the structure of the input. Bates and Goodman align themselves with Bates and MacWhinney (1989), espousing the view that grammar (/logic) emerges because the possible design solutions to the problem of mapping a rich set of meanings onto a limited speech channel are constrained by limits of memory, perception, and motor planning (see p. 33; also Elman, 1999). These limitations (memory, perception, motor planning) are, of course, governed by innate maturational constraints (our “internal clock” during

ontogenesis); other innate constraints (e.g. unit, local, and global level architectural constraints) on *non*-linguistic organs also play a role in simplifying the mapping problem. This is precisely where Bates and Goodman's view departs from traditional empiricism (*indirect* prespecification), because the solution to the mapping problem is not derived from learning alone. On the emergentist view, though, grammar is not genetic "all the way down" (i.e. not directly, or representationally), but emerges from *conservative* genetic interaction (genes coding for non-linguistic mechanisms like memory according to a chronotopically innate schedules) with a structured environment. It is not entirely a fact about the genes that kids possess knowledge of language, but a fact about the way the world hangs together, and how this "hanging together" constrains certain outcomes. On the emergentist view, then, grammar is to a large extent *self-organizing*.

To summarize: emergentists emphasize the way seemingly domain-specific structures are generated as *by-products* of processes occurring in other domains, processes which are essentially myopic to the overall plan or output. Linguistic structure is *not* causally tied to a genetically prespecified, domain-specific language organ, but to (i) conservative genetic interaction (e.g. "timing constraints" on the maturational schedule of memory which elicit the "less is more" effect, see Newport, 1990 & Elman, 1999) governing the development of mechanisms within the domain of *general* cognition, (ii) the inherent processes of associative memory (see "Single Mechanism" section) and other general cognitive mechanisms (e.g. see Newport, 1999), and (iii) structural regularities in the input (environmental).

Bates and Goodman also refer to emergence as non-predictability: "outcomes can

arise for reasons that are not obvious or predictable from any of the individual inputs into the problem [e.g. soap, water, etc.]” (32), but they would probably agree (to avoid occult forces) that once you *do* have a mechanistic account of all the relevant constraints which govern, for example, soap-environment, soap-water, water-environment interactions, you *can* predict how the lower level needs to be “fixed” in order to produce the higher-level phenomenon (i.e. spherical bubbles). Emergence as non-predictability, for Bates and Goodman, is thus an *epistemological* form of emergence, characterizing what we presently do not know about configuring the lower-level properties of the lexicon and meeting basic environmental conditions to compel grammar’s emergence (i.e. to account mechanistically for grammar’s emergence in terms of maturational constraints; Newport, 1990 & Elman, 1999, as well as other general cognitive constraints, Gupta & Dell, 1999, Newport et al., 1999, and so on). The chore, which Bates and Goodman fail to adequately do in this article, is to explicate the lower level mechanisms that facilitate grammar’s emergence. We should be careful not to let any “pinches of magic” into our account and assume that we could *never* uncover the relevant constraints governing grammar (or bubble formation). In fact, we are well on our way to uncovering the relevant constraints when it comes to grammar, and considerable progress has already been made.

Bates and Goodman add to their running definition of emergence that the spherical form of bubbles is “not explained by the soap, the water, or the little boy who blows the bubble,” which indicates a sense of non-additivity, that is, that the whole (the spherical shape) is not just the sum of its parts (soap, water, etc.). The outcome in emergent

processes is not prespecified in any individual property or input to the problem. All inputs, plus constraints *in the world*, account for the bubble's emergence. So for language (specifically past tense), the relevant constraints are as follows: the architecture and developmental rate of lexicon, which is itself constrained by the architecture and developmental rate of whatever general learning mechanisms subserve it (see also the "less is more" hypothesis; Newport, 1990); the structure of the input (e.g. ratio of regular to irregular past tense forms), and the regularities occurring therewith; social factors (Snow, 1999), for example, the fact that kids are innately constrained to attract and engage in social interaction in general; innate *non*-linguistic mechanisms used for linguistic tasks (Newport et al., 1999); and language itself, co-evolving with our brains, "shifting" due to evolutionary pressures to make it assimilate more readily into the brains of our children (Deacon, 1997). A complex synergy involving all factors is what "fixes," or forces the outcome of child language acquisition. Unlike non-predictability, non-additivity is not an epistemological claim about what we currently do not know about fixing the lower-level properties, but a claim about how the outcome (grammatical structure) is generated. Grammar, as non-additive, is not a *direct* result of a grammar module's inherent architecture and developmental rate, but an indirect *by-product* of all these forces doing their own local jobs (each being more or less "dumb" to the other's job or to some plan of design).

Extending the analogy, grammatical representations are not defined by a prespecified "blueprint" of neural microcircuitry mediating UG (again, this claim is largely left for other researchers to haggle over, e.g. Elman 1999), nor can grammatical



processing be traced to a domain-*specific* module (this is the central issue for Bates and Goodman). Even in cases of SLI, where specifically *linguistic* operations are affected, there is no need to invoke the independent development hypothesis (that language-specific and general learning mechanisms develop on separate pathways). It is equally (if not more) feasible to consider *non-linguistic* factors in SLI.

Children with SLI score significantly below age-matched controls on at least some non-linguistic measures, including mental imagery and mental rotation (Johnson 1994), symbolic play (Thal & Katich 1996), and shifting attention (Townsend, Wulfeck, Nichols, & Koch 1995). Tallal and her associates proposed that specific vulnerability of morphology is a by-product of a subtle deficit in the ability to perceive rapid temporal sequences of auditory stimuli. (B&G, 63)

From the developmental perspective, a perceptual, acoustic, or general cognitive deficiency (or all three) early on could lead to large-scale changes in the outcome (i.e. to SLI). What would traditionally be called *performance* constraints affect SLI patients' *competence*, or at least hamper the grammar module from reaching whatever threshold of input it requires to "turn on" its innate mechanism (in other words, for kids to hone in on the appropriate rules for the appropriate language).

As a *by-product* of interactions among three non-linguistic factors, SLI is itself a sort of "emergent," self-organized outcome. There is no need to posit a single, damaged gene (or set of genes) which codes for the precise wiring of the inflectional system, and what follows, that SLI is thus a *direct* genetic outcome. The genes involved in the disease are not dedicated to prespecifying an inflectional "blueprint," and probably serve either directly or indirectly in a multitude of different functions, many of which are *general* cognitive functions (see "Discussion" section--multifunctional genes). Genetic impairment

in SLI does not *directly* shut down the production of a morphology network in the brain. It does shut down, or at least impair, various *general* cognitive mechanisms (the acoustic system, attention directing or attention sustaining systems, mechanisms involved in mental imaging, etc.), and this, in turn, shuts down the production of a mature inflectional system. If the acoustic system is impaired, SLI patients would find it more difficult to detect the affixal components of words, which are generally the least perceptually salient components<sup>3</sup>.

If we entertain the possibility that grammar and the lexicon might be mediated by the same domain-general mechanism, then we can further entertain the possibility that SLI results from damage to that mechanism. This would explain not only the linguistic impairments SLI patients suffer, but also their *non*-linguistic impairments, which nativists cannot easily account for (nativists generally look for a dissociation between grammatical and general cognitive abilities). The problem with interpreting SLI as an emergent outcome is that we must assume that Bates and Goodman are correct about domain-generalness in the first place.

My difficulty with Bates and Goodman's examples of emergence is that they do not match up exactly with the emergence of *grammar*. Take, for instance, the bubbles analogy. When soap and water combine, there is only *one* contributing factor to the outcome which guarantees "maximum volume with minimum surface area," namely that

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<sup>3</sup>Stress in inflected English words rarely falls on the inflectional suffix. In other languages where stress falls on the suffix more often (e.g. Spanish preterite), it may *prima facie* seem easier to overcome this deficit, but upon closer examination, this option is improbable. Languages that stress inflectional affixes are usually highly inflectional languages, so the likelihood of error increases as the possibility for acoustic deficiency decreases.

mathematical law which governs volume/surface area relations. But clearly this is too direct a constraint in terms *grammar's* emergence. Grammar is the result of a whole collocation of domain-general processes, but it is clearly *not* such a direct and overwhelming “forced move.” There are an infinite number of possible hypotheses that children could form about which rules are applicable to their particular language, and what the rules are to start with (Chomsky, 1986, 1988). Not just *one* factor contributes to a child’s ability to overcome this induction problem. It is a much more subtle affair, a “conspiracy” of mechanisms in Elman’s terms (1999).

The bee analogy (32) portrays a much *less* direct process by which honeycomb structures emerge.

When a bee returns to the hive after collecting pollen, she deposits a drop of wax-coated honey. Each of these honey balls is round and approximately the same size. As these balls get packed together, they take on the familiar hexagonal shape that we see in the honeycomb. There is no gene in the bee that codes for hexagonality in the honeycomb, nor is there any overt communication regarding the shaping of the cells of the honeycomb. Rather, this form is an emergent consequence of the application of packing rules to a collection of honey balls of roughly uniform size. (MacWhinney, 1999)

The regular structure of the honeycomb arises from the interaction of forces that wax balls exert on each other when compressed. The honeycomb can be *described* by a rule, but the mechanism which *produces* it does not contain any statement of this rule [my italics]. (Rumelhart & McClelland, 1988)

Lacking both a genetically prespecified “blueprint” for honeycombs and a way to communicate a common design with one another, bees simply follow local constraints--sniffing out chemicals, secreting substances--and the structure emerges time and time again. The local constraints governing the interaction of wax deposits--“packing rules,” laws of physics, laws of molecular chemistry, physiological mechanics, behavioral

regularities in other domains--interact in the packing process, and honeycombs emerge. There is clearly some sense in which this behavior has *evolved*, but we need not appeal solely to natural selection (and further, *direct* genetic encoding) to explicate the mechanisms that account for it. *Self-organizational* mechanisms do just fine; in fact, because honeycomb construction occurs in the *absence* of direct genetic encoding, a genetic determinist's account of the honeycomb phenomenon would be flat wrong.

Nevertheless, Bates and Goodman fall short of specifying, for the case of grammar, what exactly these local constraints are, or *by what mechanisms* grammar emerges from the lexicon. Bates and Goodman appeal to statistical regularities in childrens' vocabularies--"critical mass" effects resulting from vocabulary size, threshold levels of lexical proficiency before grammar can kick in, etc.--but a sheer bulk of vocabulary items in the lexicon cannot, by itself, ensure the emergence of grammar. Vocabulary size may be *one* of the contributing factors to grammar's emergence, but it is far too weak a constraint on its own. In theory, one could learn an enormous number of vocabulary items in a given language, yet fail to know how to string them together into an acceptable sentence.

Moreover, Bates and Goodman's inference from these data (vocab/grammar correlations) is itself unwarranted. *Because lexical performance is lawfully correlated with grammatical performance throughout childhood (correlation), grammar must be subserved by the same domain-general mechanism as the lexicon (causation)*. There is no further step in Bates and Goodman's project to show us how *particular* grammatical operations are implemented by lexical mechanisms, and they *have* to be--at least

throughout early stages in development--because we apparently only have *one* mechanism (unless, of course, we entertain alternative explanations). For past tense, then, the list of questions remains: if a critical mass of vocabulary items needs to be met in order to trigger grammatical processes, what ratio of regular to irregular verbs must there be to ensure a standard acquisition rate? To what extent are irregularization or regularization errors a factor of this ratio (environment), or a factor of innate architecture (this latter part of the question pertains even to general learning mechanisms)? And the list goes on for past tense and for other structures. Although researchers have filled in many of the gaps, Bates and Goodman's immediate claims (domain-general, a lack of dissociation) should really stand or fall by their *own* data. When it comes to their own data, however, their claims go grossly under-supported. Not only is the logic of their argument flawed (correlation=>causation), but more problematically, they fail to clarify the lexical mechanisms whereby particular grammatical structures emerge. Thus, their use of the term "emergence" requires further clarification, appeal to outside sources of data, or redefinition altogether.

## 2.2 Problems with Bates and Goodman's Data:

One problem with Bates and Goodman's data is that it is *performance* data, and Chomsky, for example, would likely just throw this out as an invalid and unreliable indicator of grammatical *competence*. Children possess tacit knowledge of language, which should be tested as well ("teased out") to determine precisely when specific grammatical structures are acquired, and which linguistic principles are being applied.

Without an account of *knowledge of language* (competence), we have no grounds for linking grammar (what would this be?) to the lexicon. The problem is unavoidable for Bates and Goodman, whose data is primarily based on parental reports which document *performance*, unless we abandon the competence-performance distinction altogether (as it turns out, some view this as the most sensible alternative<sup>4</sup>).

Another problem is that the data for grammatical proficiency at various stages in the sample's development are based on MLU (mean length of utterance) scores. A standard complaint in applied linguistics is that MLU scores are cross-culturally unreliable. Comparing the scores of an English toddler with those of a Spanish speaker is inequitable because the English toddler can get a score of 1-word-in-length for "go," but it is unclear whether the Spanish toddler should get a 1 or 2-word-in-length score for "va" ("go," or "you go," also "he goes," "it goes," or "she goes"). In highly inflected languages, this problem can mean a margin of error of 2-4 words (e.g. in Spanish, "*dámelo*" can be interpreted as 5 words, "you give it to me"). This is an astronomical number on the developmental scale, translating into a difference of over 20 months<sup>5</sup>! Bates and Goodman anticipate this problem, and offer corroborating data from Italian, a language more richly inflected than English. Their hypotheses are confirmed by Caselli and Casadio (1995) in the Italian data (presumably having controlled for the MLU problem), but still this means that we only have two cases, that of English and Italian, upon which to base our general theory of grammar (and of how and whence grammar emerges).

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<sup>4</sup>For example, Givon (1999) characterizes the distinction as a "radical sanitization of the facts of natural language use," a "logical sleight of hand" (83-4).

<sup>5</sup>Wittgenstein addresses this problem in a different context at the beginning of the *PI* (p. 9e).

The third problem is the potential “apples and oranges” discrepancy in drawing a developmental link between grammar and the lexicon (40). For example, there is a remarkably strong correlation between the development of the big toe and the development of grammatical abilities, “up to the ceiling,” so to speak, but this does *not* imply that functions of the big toe and grammatical operations are mediated by the same mechanism. Nevertheless, does the “apples and oranges” criticism have anything to say about what we are interested in, grammar and the *lexicon*? The lexicon, unlike the big toe, is at least a device that serves a *linguistic* function, so the correlation does seem to be a more reasonable one. *Both* are imperative for language (beyond an 8 month babbling stage) to even get off the ground. It is questionable, though, from Bates and Goodman’s data alone, that grammar *must* emerge from the lexicon (without clearly defining what this means), and further, that both *must be* subserved by non-linguistic mechanisms (this latter inference is the most controversial).

Nativists offer striking evidence to suggest that linguistic and non-linguistic development are, in fact, modularized. Smith and Tsimpli (1991) report on a 29 year old mentally handicapped individual with a non-verbal IQ averaging between 60 and 70, who enjoys native proficiency in English and remarkable proficiency in a number of other languages (Ouhalla 1999: 4). Curtiss (1981) and Yamada (1990) report additional cases in which linguistic proficiency is negatively correlated with general cognitive skills. With evidence such as this, the nativist is able to take the same data (Bates and Goodman’s) and interpret it as the development of an *interface* between grammar and the lexicon. This could be seen as a problem for Bates and Goodman because they propose that

grammatical and lexical operations are mediated by the *same* mechanism throughout childhood.

Even *connectionist* networks modeling aspects of lexical and grammatical development frequently find themselves committed to modularity. The connectionist models of past tense (Rumelhart & McClelland, 1988; Plunkett & Marchman, 1991, 1993; Daugherty & Seidenberg, 1994, 1999), for example, are all committed to at least a phonology module, which decodes the input signal into a readable form for the network, and encodes the network's output into the projected English pronunciation for each given past tense form. Multi-layered processing of this sort operates according to principles similar to those governing serial processing, one layer's function depending upon another's in a serial, unidirectional manner. Some modelers build in further complexity to their networks by adding a "context" or "hidden" layer to serve more specialized jobs than the network as the whole (i.e. the formation of "internal representations," see footnote 11). Most connectionists would nevertheless happily agree to modularity in strict connectionist terms (i.e. multi-layered processing). It is the very *nature* of connectionist processing that is of central concern, not hair-splitting over the definition of "modularity." It is the realization that there is a necessity for such models to demonstrate how mechanically and mathematically higher-level "lawful" psychological behaviors can be implemented at the lower neural level.

We must be careful, nonetheless, to keep the issues cleanly apart. Bates and Goodman call for reassessment of a number of different nativist assumptions: (i) innateness, (ii) modularity, or information encapsulation, that is, that grammatical and



lexical processes are kept distinct except at their interface (see Fodor, *Modularity of the Mind*, 1983), and (iii) domain-specificity, or encapsulation of one specific domain of processing (“grammar” set off from the “lexicon,” “syntax” from “semantics,” and so on), in other words, one mechanism (grammar) serving one class of stimuli and only one, and the lexicon following suit. A function can be localized (i.e. routinely subserved by mechanisms in a predictable neural region), and simultaneously serve other classes of stimuli. Agrammatic patients with damage to Broca’s area, for example, have trouble processing and detecting suffixes, less salient word endings, and inflections (Pinker, 1999: 248); so we can safely say that before trauma this function (inflection) was localized (after all, damage to the region leads to direct and specific impairment). But Broca’s area has also been found to serve *other* classes of stimuli (e.g. in nonverbal motor planning, see B&G, 1999: 65). Thus, localization is not equivalent to domain-specificity. Nonetheless, a function *cannot* be domain-specific without being modularized, which is precisely why Bates and Goodman appear to conflate the two notions in their article. It is argued (see “Discussion” section) that the domain-specificity/modularity relationship need not hold the other way around (modularity=>domain-specificity), so long as we re-focus our definition of modularity in terms of multi-layered processing. Still, the more standard notion of modularity (a la Fodor) should be applied to the dual-route theories of past tense presented by Chomsky & Halle (1968) and Pinker (1999). In each theory, the two notions (Fodorian modularity/domain-specificity) indeed go hand and hand.

### 3.0 A Single Mechanism for Past Tense--Simulations of Emergent Processes:

### 3.1 Implementations--

All connectionist models of human behavior follow from the basic assumption that the behaviors they mimic are functions of associative memory. Any instance of “knowledge of language,” or better, any “generalization about language,” emerges in connectionist networks in a more or less bottom-up fashion. Thus, connectionist models stand in direct opposition to nativist models, as knowledge of language is in no sense built into the networks, the networks in no sense “knowing” the rules which merely describe their behavior. For this very reason, connectionist models exemplify some basic emergentist principles. Processing in one domain (general associative memory), in combination with other non-linguistic constraints (“maturational” constraints on memory), beget what appears to be lawful, rule-governed behavior in an entirely different domain (grammar)<sup>6</sup>.

From the operation of “dumb” local mechanisms (nodes, connections) comprising associative memory, what might be described as “grammatical knowledge” emerges, even though *no* rules of grammar are actually programmed into the network. The network’s behavior can be *characterized* as lawful, but no prespecified rules (specific to linguistic systems) govern its function. Associative memory is, of course, governed by rules, but these rules determine elements of general cognition, and are not characteristic of the linguistic system *per se* (e.g. Hume’s laws of *contiguity*--if A appears with B, associate them--and *resemblance*--if A looks like B, let B share A’s associations, Pinker 1999: 104).

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<sup>6</sup>See Elman (1999) for techniques on how to model maturational constraints in connectionist nets. Also, note that “emergence” is being used here in the strict sense of “developmental emergence,” as defined by MacWhinney (1999: xi). Elsewhere I allude to biological (evolutionary) emergence, but these issues should be

Linguistic knowledge, or generalization, is a function of the domain-general processes of associative memory, not a function of a domain-specific grammar module. Whatever structure or systematicity emerges in the network ultimately finds its source in association. Thus, connectionism is essentially a “memory all the way down” model of language and human behavior. From a structured training set and local associative processes, complex behaviors emerge in an altogether different domain (such as language). If we accept connectionist networks as valid homologues to certain key aspects of human brains (at least at *some* level of analysis), we begin to recognize them as tangible models of how associative mechanisms can overcome the apparent “induction problem” (Chomsky, 1986, 1988).

### *3.2 The Model's Job--*

Given a problem, connectionist networks generalize over patterns in memory to derive a solution. Any similarities between input and stored data affect the structure and clustering of features recorded in memory. Similar features are clustered together; so when making predictions about novel inputs, connectionist nets search their databases for similarities between features of the input and features of previous (stored) inputs. If similarities are detected, the network makes a prediction that the novel input must be computed similarly to the analogous one(s) stored in memory. New items are therefore “learned” on analogy, but only after the network has been adequately trained on a certain

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kept separate.

number of stem/past tense pairs.

For past tense formation, this would mean that (at least) the phonetic features of irregulars with similar stems and similar change patterns would form patterns in associative memory. A certain “training set” would be introduced to the network to provide it with a database to work with. In this case, the training set would consist of stems and fully inflected forms, and the resulting database would consist of features of stems and fully inflected forms, overlapping where similar. Training replicates *input* and *learning*, a certain amount of which is required to “switch on” the inflectional system (to this end, even generativists concede the importance of input). To model the transition from stage 1 to stage 2, for example, it has to be assumed that a certain number of correct irregular and regular forms (and types) have made it into the lexicon. And to model stage 3, we would need an even greater number.

Generativists might object that training is “cheating” in a sense, because it flies in the face of the “poverty of stimulus” argument (Chomsky, 1986; 1988). It is arguable, however, that criticisms of this nature misinterpret what really happens during training. During training, the teacher signal only flags *that* some discrepancy has been found, not *how to fix it*. It just indicates that *something* has gone wrong, but not exactly *what* has gone wrong. There is no teacher to say, “That’s not how we say it; we say ‘walked.’” About the only thing a teacher would be saying in this case is “Oops.” If *walk* gets inputted and comes out *woked* (god forbid), the network adjusts the connections to the units which misfired (as well as the threshold value for those units). It “hunts and pecks” until it stumbles across the right answer, but it never *gives* the right answer. In fact, it is

“news to itself” that it has the right answer. Training is thus a more or less *bottom-up* process.

If the network has been trained on *walk-walked*, and when subsequently fed /wak/, outputs /wakId/ instead of /wakt/, it compares the incorrect output with the correct form in memory, and the network’s weights change according to its “learning rule” (which conveys a “dumb” mechanical operation, like “add .2,” or “subtract .8”--an *algorithm*). Likewise, if a child entering stage 2, whose lexicon contains an instance of *walk-walked* (memorized “rote” in Stage 1), produces /wakId/ (say, due to performance constraints), the child’s associative memory would most likely detect that /wakId/ contains features not shared by the dominant pattern, and would cluster it separately (as a “working hypothesis”). The more and more *walk-walked* is used correctly in the future, and the more and more regulars are added to the stem-stem and change-change pattern, the less and less robust the “working hypothesis” pattern becomes; thus, *walk-wooked* becomes an increasingly less likely candidate for use in past tense computation.

When given a novel stem, the network in a sense “blends” features of the stored past tense forms of similar sounding (and meaning) verbs. For example, given the novel verb “spling,” the network would (i) represent it as a certain distinct set of features (phonetic, semantic, both, or others), (ii) cluster this set of features with similar ones in memory (the “lexicon”), and either (iii) superimpose the /e/ sound onto /spl\_n/ to produce “splang” on analogy to *spring*-->*sprang* (or to other members of the *ing-ang-ung* family), or (iv) generalize the most probable change in featural sets (add /d/--if /d/ is indeed the most common overlapping feature of past tenses in memory at the point when “spling” is

inputted); (iv) occurs in the case that other change patterns (i.e. irregular ones) are not strong enough to attract the novel verb into their families. Clearly, then, one of the chief concerns for modelers of English past tense is what amount, type, and distribution of featural sets in memory (e.g. the ratio of regular to irregular verbs) acts as a constraint on past tense learning.

#### 4.0 The Rumelhart and McClelland Model:

In the 1980s, Rumelhart and McClelland (hereafter R&M) and the PDP research group sent a shock wave throughout the scientific community with their claim that highly lawful human behavior can be implemented in a system that follows no explicit rules. The successes of connectionist models challenge nativists to reconsider their biases toward language acquisition, or at least to reconsider *specifically* what behaviors are a function of associative memory (general cognition) as opposed to a function of grammar. R&M's success at modeling past tense has challenged more traditional rule-driven models of the inflectional system to reassess whether "rules of grammar" characterize the inherent construction and mechanistic procedure of the system, or whether they are mere epiphenomena of some other domain of processes (the tools of descriptive linguists).

If R&M are correct that regular and irregular past tense *can* be successfully learned by their network, then the model is at least beneficial to show that there *may be* a more viable alternative model to rule-based, modular theories; this could *at least* lead to revisions of more traditional symbolic models--*revisionist* connectionism (e.g. Pinker's revision of the lexicon in the dual-route account). If R&M are further correct that past

tense *is* learned by a sort of domain-general pattern associator in human brains, then we can either (i) eliminate the more traditional symbolic theories (this would be the most radical move--*eliminative* connectionism--in which case “rules of grammar” would be *inexact* descriptions of both higher and lower level processes), or (ii) explore connectionist networks as models of how higher-level symbolic processes are *implemented* at the lower level, in which case “rules of grammar” would be *exact* descriptions at the higher level but only *approximate* descriptions at the lower level (“*implementational*” connectionsm). Establishing that past tense *is* subserved by domain-general mechanisms is a project left for other researchers, outside of R&M’s immediate scope. Thus, they can only establish that PDPs *can* learn past tense, and that this *may be* the right model to go with.

R&M model past tense formation in an attempt to show that past tense can be learned without explicit unconscious rules (i.e. with absolutely no KOL built in). To do this successfully, R&M’s network needs to accurately model the three-stage u-shaped learning curve, which has now become standard in the study of past tense (Brown, 1973; Ervin, 1964; Kuczaj, 1977). In stage 1, kids use a small handful of verbs in the past tense. These are usually the highest frequency verbs (“light verbs,” *do-did, go-went, put-put, make-made, give-gave*, and others like *eat-ate, walk-walked, play-played*), the majority of which are irregulars. Kids in stage 1 use the correct forms most of the time, and make few mistakes, as their lexicon has not yet been cluttered by the surge of novel forms.

The transition from stage 1 to stage 2 is where the controversy gets thick, as it has elsewhere been taken as evidence for implicit KOL (Pinker&Prince, 1988, 1994; Pinker,

1999). By stage 2, kids have built upon their limited repertoires of verbs from stage 1, and more and more regulars have seeped into their lexicons (the ratio of regulars to irregulars is spreading). In stage 2, kids begin to generalize the regular rule to most novel forms, and *overgeneralize* it to irregular forms, even to ones they have routinely gotten correct in stage 1. Pinker (1999) sees the transition to stage 2 as indicative of separate mechanisms for words and rules. If a child says “walked” for the past tense of ‘walk,’ she may have just *memorized* the past form, but if she predicts the past tense of the novel verb ‘pick’ is “picked,” there is evidence that she implicitly “knows” the regular rule. Further, if Pinker can show that regularization of novel stems has little or nothing to do with the frequency and distribution of the input, we have evidence that the onset of the rule mechanism’s operation (at beginning of Stage 2) is motivated by factors outside the domain of lexical learning. Nevertheless, if R&M can demonstrate that their model not only replicates the u-shaped learning curve, but also overgeneralizes the regular rule to novel stems in stage 2, then we have a viable alternative to rule-based, dual-mechanism theories.

In stage 3, regularization of novel stems continues, but kids have regained their ability to produce the correct irregular past tenses. Clusters of exceptions begin forming in memory, e.g. the *ing-ang-ung* family, and the inflectional system stabilizes. Once clusters of exceptions form, the tendency to make *irregularization* errors increases, as patterns in memory attract candidate stems to their change class based on stem-stem similarities. Thus, in stage 2, the novel verb “spling” would most probably be regularized, although in stage 3, when the *ing-ang-ung* change family has gained strength, it may resist



the dominant regular pattern to be *irregularized* (especially where there is only one featural difference, e.g. between “spring” [+lateral] and “spring” [-lateral]). In stage 3, the number of regulars in the lexicon outweighs the number of irregulars, so the irregular pattern has to be incredibly robust for irregularization to occur.

#### 4.1 Patterns--

R&M designed a connectionist network which decodes phonologically represented input sequences (verb stems, past tense forms) into distinct sets of features. During training, the model is fed the phonological representation of both the stem of a verb and its past tense (e.g. /ækt/, “act,” and /æktId/, “acted”). Each node codes for groups of 3 phonemes (Wickelphones), so /ækt/ would be decoded as {#æk, ækt, kt#}, and the 3 nodes coding for {#æk}, {ækt}, and {kt#} (respectively) would fire upon input. The Wickelphone encoding solution allows R&M to represent words in as “distributed” representations, without forfeiting their distinctness. If each node coded for a single phoneme (rather than 3), every last bit of positional information would be lost upon input (e.g. the difference between “tip” and “pit” would be lost because the same nodes would fire for each, see Pinker, 1999: 111-3). A model that encodes words as featural sets without segmenting them in some fashion would not be able to represent words with overlapping featural sets (e.g. *tip-pit*, *slit-silt*, etc.). Even “slit” and “silt” remain distinct under such a model: /sIIt/-->{#sl, sII, IIt, It#} while /sIIIt/-->{#sI, sII, IIt, It#}, each with no two Wickelphones in common.

The trouble with the Wickelphone solution is that there are too many of them, and they are too specific. Assuming that we distinguish 35 different phonemes, the number of Wickelphones would be  $35^3$ , or 42,875, not even counting the Wickelphones containing word boundaries. And, if we postulate one input unit and one output unit in our model for each Wickelphone, we require a rather large connection matrix ( $4.3 \times 10^4$ ) to represent all their possible connections.

To avoid the problem of hyper-complexity, R&M boiled Wickelphones down into Wickelfeatures, each Wickelphone defined by a distinct set of Wickelfeatures. Each node, then, codes for a specific set of 3 features (one for each phoneme in each Wickelphone). For example, a node that encodes [[+silibant], [+lateral], [+high]] (respectively) would fire for the Wickelphone {sII} (from “slit”), and other nodes would fire for {sII} according to the same criterion (e.g. a node encoding [[-voice], [+voice], [+front]] would also fire). When all is said and done, nodes encoding the features [+vocalic], [+high], [+front], [-long] would all fire for the /I/ in “slit” (ditto for the rest of *slit*’s phonemes and their relevant features), rendering the representation distinct from all others (e.g. distinct from the representation for “slot”).

Phonetic features offer a significantly more compact way of representing the elements that make up words. Thirty-five phonemes can be represented using only 16 features. Each phoneme in a given Wickelphone is assigned a value (0 or 1) for each of the 16 features. This means that the preceding and following “context” phonemes (e.g. /p/ & /t/ in the Wickelphone /pIt/ from {#pI, pIt, It#}) are also assigned featural values. Context information is recorded by memory in order to formulate stem-stem and change-change patterns. Due to its features, /pIt/ might fall into the *hit-hit* pattern, and later, into the regular “add /-Id/ [+cons, +dental]\_” pattern based on stem-stem and especially stem-

final similarities, /pIt/ ending in a dental consonant. Without context phonemes, generalization would be a highly inefficient mode of learning, as there would be little information in terms of stem-stem similarities upon which to *base* generalization. Since the difference between “silt” and “slit” would be lost, for example, the network would never be able to predict any difference between their past tense forms (using “silt” as a V here), even though “to silt” always goes *silted*, yet “to slit” can either go *slitted* or *slit*.

In R&M’s network, words are *distributed* representations; that is, words are represented by a distinct pattern of activation over the input units, each unit encoding a particular set of features (i.e. whatever input units “fire,” or get fed a “1,” represent a given word). No single unit codes for a single word, and the same unit gets re-used in many different representations (e.g. units coding for [+vocalic, +high, +front] might fire for any word with an /I/ or an /i/). *Predicted* past tense forms are represented by whatever pattern of activation occurs over the output units. Just as neurons perform only simple mechanical operations (get excited, fire ballistically, etc.), so too do nodes (units). Since each output unit has a threshold value, not only does the learning rule adjust the weights if an error is detected, it also slightly lowers or raises the threshold value for the output nodes. During training, modelers note the *output* pattern of activation for each word that gets run through, stems and past tense forms alike, so that each input has a predictable output; this way modelers have the ability to know when an error occurs (i.e. when the expected pattern is different from the actual one).

Whenever there is a discrepancy between the target output (stored in memory after being learned) and the actual response, in other words, whenever a certain set of nodes

representing the target output are supposed to fire but “miss,” the learning rule kicks in to mechanically adjust the network’s weights (it also adjusts the threshold values of the deviant output units). The “weights” in R&M’s network are numeric values which fill the variables of its learning algorithm (e.g. ‘ $X - .2/output = “1”$  & target = “0,”’ or ‘ $Y + .8/o=0$  &  $t=1$ ’ [simplified for clarity]). These values represent the strength of connectivity between input and output nodes. In neural terms, we would be talking about how dedicated a neural synapse (axon-dendrite interface) is to a given stimulus (e.g. in localized functions, the neural pathway gets routinely entrenched by the same stimulus--a highly connected system). According to one of its uses, the computational “neural” network, or connectionist network, is essentially a mathematical, algorithmic metaphor for lower level processes--for “neural information sharing.” Connectionism thus provides an “abstract neurology” (as it is often put, e.g. Fodor & Pylyshyn, 1988) for implementing higher-level theories of behavior. Connectionist networks are mechanisms designed to receive some bit of numerically encoded information (for our purposes, verb stems) and compute an output value based on computations across weights.

The value “1” represents a node firing (a certain bit of information being inputted), so if a node fires, we have “1” times the value of the weight, say, .2, which equals .2 (our output). If the threshold value of a connected output unit is above .2 (say, it’s 1), the unit doesn’t fire. If the unit that didn’t fire causes a mis-match between the actual output and the target vector, the learning rule kicks in. The learning rule adjusts the weights (e.g. ‘ $Y + .8/o=0$  &  $t=1$ ,’ so ‘ $.2 + .8=1$ ,’ the node fires), as well as the threshold value (“1” in our example, so we’re guaranteed a “hit”). All things being equal, an input that causes an

error can be re-fed to the network, and the weights and threshold values will eventually stabilize. Stable states of the network are signs of robust patterns in memory. If a novel input has features that are strongly associated, modelers can predict (approximately) what weight settings and threshold values they need to activate the target (based on patterns of activation and connectivity for computing similar inputs). The behavior of stable systems can rightly be characterized as lawful. “Rules,” as descriptions of lawful behavior, are also distributed, in that they capture a certain pattern of connectivity between input and output nodes (i.e. whatever pattern of weight settings and threshold values is required to fire only those output nodes representing the correct form for a particular class).

Words are also “distributed” in a different (but related) sense, insofar as they are stored as sets of features overlapping with other sets where similar features are shared. As such, words are bits and pieces of “shared” phonological information (features).

Outputted past tense forms for novel verbs are essentially “blends” of features from the input sequence with features of past tense forms of similar stems in memory. Because words are nothing but sets of features in R&M’s network, memory is not taxed by needless information. Memory encodes only the *minimal* amount of information needed to distinguish words and compute their past tenses.

The “-ed rule,” or better, the “-ed generalization,” is *implemented* in whatever pattern of activation and connectivity elicits a particular regular past tense. In a sense, then, each past tense form gets its own mini-rule. The more robust a pattern in memory gets, the more stable the pattern of connectivity; that is, the “setting” of weights used to compute verbs with similar features gets more and more regular (but rarely is it *identical*).

The “average” weight settings for each verb in a particular class would approximate the “general rule” for that class, but “rule” here would merely be a descriptive term (rules are not explicitly programmed into the network, or *prescriptions* for behavior). The *most* stable pattern of connectivity, reflective of the most robust pattern in memory, is what could be called the “most regular rule.”

No input-output connections are hard-wired (weights preset) to compute regular verbs (nor could we point to anything we could rightly call a “word” in any single unit). A word inputted over and over again sparks a somewhat novel pattern of connectivity each time it is processed. As training increases, words *more or less* take the same pattern, so really we can at best make *probabilistic* predictions about the network’s next state. The sense of “implemented rule” gets extremely muddled in connectionist systems. Mini-rules have no variables, containing only those values that have been inputted into a *particular* computation (only those values of a particular cluster of “words”); or at best, it contains an *average* of these values. At the level of nodes and connections, nothing falling under the rubric “rule of grammar” is prespecified. No node follows the explicit program “add - ed.” Regardless, the structure *emerges*. From a linguist’s perspective, it is a consequence of highly lawful linguistic behavior (rule application), but the only rules at work at the lower level are general associative rules.

Innate KOL could be modeled by hard-wiring, or prespecifying, certain weights and threshold values. But this would undercut the very goal of connectionism, which is to model KOL without built-in explicit rules. Words and rules in connectionist networks are thus like *virtual* entities. Bits and pieces of featural segments generated by the network to

constitute a predicted past tense form are analyzed into coherent words upon output. Words are therefore both “causal” and “analytic” emergents (see Appelbaum, 2000). Outputs for novel stems are structures (distinct sets of features) causally generated from domain-general processes of association, yet they reflect the operation of a *linguistic* rule. Moreover, the only representations that are generated for words are *distributed*, which leads us to the second sense in which words are emergent. Distributed representations--generated by “blending” features--are subsequently *analyzed* as strings of phonemes, that is, as coherent “wholes” (at least we *hear* them as such). The decoder mechanism built into R&M’s network would handle the task of “analysis.” For reasons of tedium, R&M sent only a subset of featural sets (outputs) through the decoder; in fact, *they* were the ones who interpreted the rest of the output patterns (Pinker & Prince, 1988; also see R&M, pp. 269-271). Their model is primarily concerned with causally *generating* structures. As a model of the way *we* compute past tense, however, we would have to appeal to some level of analysis at which words appear to be coherent “wholes.”

Because “rules” in R&M’s network are generalizations over patterns in memory, or stable states of the network, regularity is essentially *subregularity*. We can *describe* the network’s subregularities with talk of “rules,” but the rule remains unspecified. When we get down to the level of nodes and connections, all we have are local, “dumb” mechanical operations in an entirely different domain. In a sense, then, rules are analytic emergents as well. They characterize the average distribution of connectivity required to compute members of a robust class, or the weight settings that naturally fall out of patterns in memory. From a psychological perspective, though, children seem to *apply* the rule

which characterizes these subregularities--“add -ed”--especially in stage 3, so R&M’s network arguably shows us only *one* level of the process (i.e. the lower level).

Without explicitly programmed grammatical rules, R&M’s model succeeded at modeling the u-shaped learning curve for past tense formation. The 460 verbs it was trained on were then run through the model, stems only, and it predicted all 460 of the correct past tense forms. More importantly, when novel irregular stems were run through it, the model achieved an 85% success rate, and a 90% success rate with novel regulars (261). (See tables 1 and 2.) Not only does the model demonstrate the presence of stable patterns in memory--so much so that the patterns attract novel verbs to their class, potentially causing *irregularization* errors (mimicking late stage 2 and stage 3)--it also demonstrates *productivity* when it comes to regular past tense. If irregular patterns aren’t strong enough and novel stems not similar enough, the network fits the novel verb into the most robust pattern of all--regularity<sup>7</sup>. Generalization to the regular pattern occurs with *every eligible stem*, the hallmark of a productive inflectional system.

#### 4.2 Problems--

We should not be immediately convinced that we need to abandon rule-based, dual-route approaches altogether. Pinker & Prince (1988, 1994) and Pinker (1999) bring a number of criticisms to bear on connectionist models of past tense such as R&M’s.

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<sup>7</sup>Of course, there are three sub-classes of regularity (/ -d/, / -Id/, and / -t/), but decision between them can be made on similarity of the end “trigger” feature. We have / -Id/ for stems ending [+dental, +cons], / -t/ for stems ending [-voice, +cons], and / -d/ for stems ending [+voice, +cons or +vowel] (see also R&M, p. 247).



Pinker's main contention (1999) is that connectionist networks do fine at modeling *irregular* past tense, but poorly at modeling *regular*, due to problems inherent in the architecture of unconstrained connectionist systems in general. Pinker argues that connectionist theorists have misinterpreted inconsistencies between modeling data and child acquisition data to be indicative of shortcomings in modeling techniques (which has fueled connectionist research for the past two decades). Pinker submits that inconsistencies of this nature should in fact tell us that single mechanism architecture is *itself* insufficient to handle the child acquisition data.

What connectionists gain with *non-modular* architecture (a single mechanism alternative), they lose in accuracy. The lack of a morphology module in the R&M model, which only maps phonological representations of the input onto phonological representations in the output, causes considerable disanalogy with human language processing. No information as to "structure of lexical entry," "head of word," "root of word," "morphological category--N, V, Adjective," "morphological structure," "word = stem + affix," nor any lexical-semantic information, gets inputted into R&M's inflectional system. But kids and adults alike are sensitive to exactly this sort of information, to the *structure* of words, not just to the *sounds* words, when processing regular and irregular inflections. Thus, the accuracy of the model stands to be questioned.

In a series of studies across a multitude of populations--college students, children ranging from 3-10 years of age--Kim et. al (1994) provide evidence that children's inflectional decisions have more to do with morphosyntactic concerns than with phonology alone. Two of the experiments, one on 3-5 year olds and another on 6-8 year

olds, aimed at teasing out children's tacit morphological knowledge of "root," "head," "noun," "verb," and "lexical entry." The children in each study were given forced verb choices like the following:

1.) This is a fly. Can you say 'This is a fly?' I'm going to fly this board.  
(*Put flies all over the board*)  
I just \_\_\_\_ .

2a) This airplane is going to fly. Can you say 'This airplane is going to fly?' This airplane is about to fly through the air.  
(*Have the airplane fly about*)  
The airplane just \_\_\_\_ .

2b.) Mickey likes to drive really fast. Look, Mickey is going to fly down the road. Can you say 'Mickey is going to fly down the road?'  
(*Have Mickey drive fast down the road*)  
Mickey just \_\_\_\_\_ .

Children 6-8 years of age generalized the regular rule to questions like #1 (denominals) 66.7% of the time, and children 3-5 64.1% of the time. The 6-8 year olds regularized questions like #2a (verb roots) only 11.1% of the time, whereas 3-5 year olds regularized #2b (semantically extended verb roots) 46.6% of the time. Children 6-8 irregularized questions like #1 17.6% of the time, and 3-5 year olds 5.6%. 6-8 year olds irregularized questions like #2a 87.0% of the time, and 3-5 year olds 22.6% (for #2b).

So why don't children cue on phonological similarity (as R&M's network would) and irregularize #1 more than they do (only ~35% of the time)? There must be something else that their inflectional system is attuned to. There are three options--morphology, semantics, or both. MacWhinney & Leinbach (1991), with their updated model of past tense, encode basic semantic (as well as phonetic) features into the input units, and are thus able to surmount problems that cripple older models (e.g. homonyms with different

past tenses). Still, it is not certain that semantic information acts as *input* to the inflectional system. If it did, the network would predict that verbs with similar meanings take similar past tenses, which, for obvious reasons, is an unreliable and unnecessary indicator of past tense form (e.g. *hit*, *slap*, and *strike* are semantically related but all take different past tenses--*hit*, *slapped*, and *struck*). Semantically related families of words do not consistently undergo the same past tense change. The fact that children produce different past tenses for *hit*, *slap*, and *strike* may reflect the need to index a specific *lexical item* to decipher past tense (e.g. a specific stored past tense form, along with its lexical features, e.g. N, V, or Adj.).

Lakoff (1987) would explain the fact that kids don't irregularize #1 more often by appealing to the semantic principle of "central sense," that is, if a polysemous verb--*fly/fly(out)*--has an irregular form, its central sense (the one kids more likely associate with the verb--'fly' as in "birds fly") will be irregular. The tendency, then, is to predict that if the central sense is irregular, any non-central sense falls under the regular rule. "When a verb is given an extended or metaphorical meaning, the new sense is felt to be dissimilar to the original, and this inhibits the speaker from using the original's irregular form" (Pinker, 1999: 151). Pinker nevertheless provides a number of counterexamples which indicate that semantic extension *per se* has no bearing on a word's past tense. For example, novel words made by adding prefixes to irregulars almost always take the *irregular*, even though the "sense" of the word may radically change. Instances are *overeate-overate* from *eat-ate*, *overshot*, *preshrank*, *remade*, *outsold*, *undid*, and the list goes on. This effect occurs with idioms as well, like "*flew* off the handle," not "*flied* off the handle."

If it's not semantic information that children are cuing on when they choose *flied* for #1, what is it? Pinker (1999) argues that children exploit morphological cues in the input to overcome the mapping problem with novel senses. What compels children to choose *flied* in Kim's study (~65% of the time) is implicit knowledge of "root," "head," "lexical entry," and "morphological category." Since the root of *flied* is the noun 'fly' (as in "the buzzing fly"), not the verb, the VP *flied* must be headless, or "exocentric" (Kim et al., 1994, 181). What would its head be? We've already established that it is a noun, not a verb, so it must be 'fly' (N). But it *can't* be 'fly' (N), because then the properties of 'fly' (N) would percolate up to the phrasal level, and *flied (to fly)* would have to be a noun phrase! Because *flied* is headless, there is no possibility of the irregular form heading this phrase, percolating up its own properties, so no irregular properties ever get inputted into the inflectional system, only properties of the noun 'fly.' Kids build *flied* by accessing 'fly' (N) from memory, and inputting it into their inflectional systems. They couldn't possibly be building "flied" from 'fly' (V), because 'fly' (V) is stored with its irregular past tense form (*flew*), which would inevitably trigger the blocking mechanism, *yet the regular rule for "flied" remains unblocked.*

The inability of R&M's network to reliably predict the regularity of *fly* in "fly the board" is arguably a major setback for single mechanism theories. Whichever theory accounts for the most data is the theory that should be advanced. Single-mechanism theories of past tense lack the appropriate lexical and morphological tools to account for the fact that kids say *flied* in "Mickey flied the board" (Put flies on the board). MacWhinney and Leinbach (1991), it is argued, fail to make much headway. There is

little evidence that children are predicting *flied* based on semantics alone. In fact, semantics is an *unreliable* guide into the mapping problem, as counterexample after counterexample leads kids astray. To be able to reliably predict *flied* for #1, modelers would have to build morphology layers into their networks, but once this is done, we have a full-fledged modular system (albeit “modules,” or layers, of PDPs). A morphology layer would *partially* satisfy Pinker, but not completely. Something appears to be wrong with the mechanism itself.

R&M’s model stores the past tenses of *regulars* in memory; it *memorizes* regular past tenses, even though there is no need to because regularization occurs by statistical default. According to Pinker (1999), regular past tense forms are stored in the lexicon only in root form (untagged). The two components of the linguistic system--grammar (with its sub-components) and the lexicon--are inputted with an eligible stem in parallel. The lexicon searches its database for a match, for the same or similar root clustered with its irregular past tense form. If it finds a match, the rule is blocked by an innate blocking mechanism. If not, the rule fires as a default. (See figure 4.) Stored past tense forms for regulars would be superfluous. All we need is a *stem* to fill the default rule’s variable, and this can be derived from roots stored in the lexicon (via the application of “lexical rules”). Thus, Pinker paints a much less structured, more economical portrait of memory than the connectionists. No “regular pattern” exists in Pinker’s lexicon, as each case of regularity is subsumed under the rule.

The *flew-flied* ambiguity in the Kim et. al. (1994) study is part of a larger problem for R&M’s network. Pinker & Prince (1988, 1994) point out that the network cannot tell

the difference between homonyms (break-broke, brake-braked, ring-rang, wring-wrung; lie-lied, lie-lay), insofar as the network uses *sound* (and sound alone) to compute past tense. Children, on the other hand, especially in stage 3, attune themselves to the *structure* of words during linguistic processing, that is, to the fact that a word can take a stem plus an affix, that words have roots, and that words, like phrases, have heads. If words were stored in a more coherent sense in the network (i.e. with semantic and/or morphological representations), the homonym problem would be solved. Like kids, the network would simply note that “braked,” for example, is exocentric, so all the properties of the irregular verb form would be blocked. Because “braked” is headless, there is no pathway up which these irregular properties could percolate. Without “lexical entries” to encode such morphological and semantic information--bridging the gap between sound and meaning--R&M’s network cannot accurately model the acquisition of past tense.

A handful of R&M’s predicted outputs for novel verbs, moreover, turn out to be ugly, mangled blends of features (tour-->toueder, mail-->membled). Whenever novel regular stems are fed to R&M’s network which contain features peripheral (or orthogonal) to other regular patterns in memory, it blends whatever scraps of overlapping information are applicable to form the novel past tense. But this is not true when it comes to people, who, when encountering a novel stem with little to *no* similarity to acceptable words of their language, routinely generalize the regular pattern (e.g. in the “ploamph” test; Prasada & Pinker, 1993). Prasada & Pinker (1993) gave both humans and a replica of R&M’s network a past tense formation task for novel stems (e.g. “plip,” “glinth,” “smaig,” “ploamph,” “smeerg”). Humans generalize the “-ed” suffix to all novel verbs (without

similar sounding irregulars) *regardless* of similarity to other regular stems, in fact, *regardless* of whether the novel stem is an acceptable English word. For example, “plip” (a phonotactically acceptable word in English) was turned into “plipped” by humans at nearly the same rate that “ploamph” (an phonotactically unacceptable in English) was turned into “ploamphed.” The network, on the other hand, came up with bizarre blends not found in the human data, like *brilth-->prevailed*, or even worse, *smeej-->leefloag*, *ploanth-->bro*, *smeeb-->imin*. Moreover, it was not even able to produce the past tense of “ploamph” except on 10% of the trials (Pinker, 1999: 143).

*Irregulars*, of course, should be susceptible to the *mail-membled* error because they are stored in the lexicon. Regulars, on the other hand, should not be affected by such association because they are *not* stored in the lexicon (but “mail” *is* regular, so this would be an obvious problem for R&M). Nevertheless, before we can claim that *mail-membled* is a problem for R&M, we have to assume Pinker is right about regular storage (or the lack thereof) in the first place. In R&M’s defense, it is not clear that children are immune from such error (although adults probably are). Pinker and Pasada’s tests were based primarily on *adult* performance, but when we look at past tense elicitations done on *kids* (e.g. Kim et al., 1994), jumbled answers *are* occasionally given. In fact, Kim et al. reserve a column in their data for “uncodable” responses, which, for 3-5 year olds in Experiment #2, occur nearly 10% of the time (191). *Membled* would be one such “uncodable” response (though there would have to be a limit to stretching this, e.g. *yield-->rilt*, see “Convergent Model” section).

At this point, only one of the problems mentioned really undermines R&M’s

project--the homonym problem (e.g. *ring*-->*rang*, but *wring*-->*wrung*, *fly*-->*flew*, but *fly*-->*flied* when derived from a noun). A related problem is that Wickelphonology cannot even represent the words of some languages. The meanings associated with full reduplications in reduplicating languages would be lost altogether, bringing into question the cross-cultural accuracy of R&M's model. *Algal*, in Australian Oykangand, means 'more or less straight' whereas *algalgal* means 'perfectly straight' (Sommer, 1980; Pinker & Prince, 1988), but these words are indistinguishable according to Wickelfeatures alone. *Algal* gets treated by the network as {#al, alg, lga, gal, al#}, being further decomposed into triplets of Wickelfeatures, and *algalgal* gets treated as {#al, alg, lga, gal, alg, lga, gal, al#}, also being further decomposed. But there is no Wickelfeature that will be present in the one set for *algal* while not in other for *algalgal* (the same nodes would fire for each). Thus, there is no principled way for the network to detect it is dealing with two different words with two different meanings, unless we build a MacWhinney and Leinbach (1991) type widget into the model.

Finally, we come to the issue of "jiggery-pokery" (Pinker's term)--the criticism that R&M's results are contrived, or "forced," due to their manipulation of training data and input units. For one, R&M purposefully "blurred" the Wickelfeature representation (the input) to enhance generalization (R&M, 1988: 238-9). "This is accomplished by turning on, in addition to the 16 primary Wickelfeatures, a randomly selected subset of the similar Wickelfeatures, specifically, those having the same value for the central feature and one of the two context phonemes" (238). The result of turning on Wickelfeatures superfluous to the task of representing the input is that "each word will activate a larger



set of Wickelfeatures, allowing what is learned about one sequence of phonemes to generalize more readily to other similar but not identical sequences” (238). By sparking Wickelfeatures of other members of a given class (pattern), the network gets a head start at figuring out which weight setting is required to compute the right answer. The superfluous Wickelfeatures are like “lead blockers.” They cause the network’s weight settings to be in a state partially equivalent to the one needed to compute members of a particular pattern (of which the novel verb is a candidate). Without “blurring,” the network runs the risk of becoming entrenched in idiosyncratic input features, in which case generalization would slow to a creeping halt.

Secondly, R&M structured their input to facilitate u-shaped learning, but this is arguably an instance of “jiggery-pokery” as well. In the first stage of training, the network was fed 10 stem-past tense pairs, 2 of which were regular (20%). In stage 2, though, R&M changed the regular-irregular ratio, and regulars constituted 80% of the input. But R&M’s training set is in many ways orthogonal to the input kids actually get exposed to (Pinker & Prince, 1988, 1994; Pinker, 1999). The ratio of regulars to irregulars in the *actual* data is no more than 50% throughout stage 2 (Brown, 1973; Slobin, 1971; Pinker & Prince, 1988), a figure which would seriously impair the network’s ability to stumble across the regular pattern<sup>8</sup>. After all, it is a probabilistic machine, and the strength of patterns in memory (i.e. of “types”) is relative to the input frequency of “token” members of the relevant class. In a 50-50 situation (50% regular, 50% irregular), we would most

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<sup>8</sup>This ratio is contradicted by Marchman & Bates (1994: 353-4), whose figure reads 55% regulars around age 2;3. Nevertheless, this is insufficient for R&M’s simple recurrent network (SRN), though success can be met with such an input distribution in newer models with hidden layers.

likely wind up with close to 50-50 results (i.e. the regular “rule” would potentially forfeit its regularity). Moreover, R&M abruptly increased the size of the training set (into phase 2 of training) from 10 verbs (cycled 10 times) to 420 verbs (cycled 190 times), but this too yields considerable divergence from the child acquisition data (Dromi, 1987; Bates et al., 1992; Marchman & Bates, 1994), which speaks more of *incremental* learning. Both cases of “jiggery-pokery” addressed by Pinker (1999) appear to indicate that the model is fundamentally inadequate.

#### 4.3 Implications--

If we reject MacWhinney and Leinbach’s model, what remains is a model riddled with holes. R&M’s model has no idea of “word structure.” Nevertheless, kids and adults alike are sensitive to more than just the *sounds* of words. Kids and adults generalize the regular rule to “denominal” (derived from a N) and “exocentric” (headless) verbs, for example, indicating that sound alone falls short of adequate input for the inflectional system (Kim et al., 1994; Pinker & Prince, 1988, 1994; Pinker 1999). Denominal verbs like *to fly out (to center field)*, *to ring (the bottle)*, *to spit (the pig)*, *to high-stick (the goalie)* are routinely regularized by kids and adults (Kim et al., 1994; Pinker & Prince, 1988, 1994; Pinker, 1999). Few irregularization errors occur with denominal verbs, despite the fact that many of these verbs are phonologically identical to irregulars (*fly-flew*, *ring-rang*, *spit-spat*, *stick-stuck*). This is evidence that sound alone is an inadequate predictor of past tense.

What blocks the irregular rule in cases of denominal verbs is the fact that they are

linked to the noun form, but not to their verb forms. The verb form of “fly” is *not* the head of “to fly out” (in fact, it is a headless, or “exocentric” construction); rather, it is derived from the NP “fly ball,” or more simply, from “fly” (as in “a pop fly”). Again, if “fly” (as in “fly ball”) were the head, whose properties in the standard theory percolate up to the phrasal level, then “to fly out” would have to be a noun phrase. Thus, “to fly out” must be a headless VP. The fact that people are sensitive to these sorts of morphosyntactic constraints--seldom producing “flew out” (to center field) even though “flew out” (the window) allures them into the y-->ew change pattern--demonstrates that people are, albeit unconsciously, applying notions like “head,” “root,” “lexical entry,” etc.

R&M’s model handles *irregulars* with good results; even Pinker sees the lexicon as a sort of pattern associator, inherently skilled at detecting, storing, and generating patterns of features, generalizing over these patterns when novel stems are encountered. R&M’s model is also good at explaining how *irregularization* errors are made (e.g. *squeeze-->squoze*), as the stem-stem and change-change patterns grow stronger and stronger into stage 3. It is argued, however, that R&M’s model fails to adequately explain how (i) *regulars* are stored and processed, (ii) novel verbs are *regularized*, (iii) *regularization* errors occur, and (iv) *regulars* appear to be immune to frequency effects (the connectionist model predicts that the more a stem gets processed, regardless of its irregular/regular status, the quicker the discernment task will be).

R&M’s inadequate account of regular inflection necessitates (at least) a reassessment of unified lexicalist claims to non-modularity, and perhaps even a reassessment of the modeling potential of connectionist networks in general. Because the

behavior of R&M's network is conditioned by frequency effects and regular-irregular ratios in the input, *yet regular-irregular ratios can be radically different across individuals and cultures* (Pinker, 1999: 234-5; also McCarthy & Prince, 1990; Omar, 1973), R&M's model might offer little more than an anecdotal account of past tense formation. This, of course, is a major blow to connectionist accounts, as they aim to account for the *general* constraints during language acquisition, constraints which *determine* the acquisition of rule-like behavior across individuals.

### 5.0 The "Bottleneck" Model:

Plunkett and Marchman (1993) provide an alternative phonology-only model, improving upon techniques used by R&M. By surveying the field of updated models (Plunkett & Marchman, 1991, 1993; MacWhinney & Leinbach, 1991; Daugherty & Seidenberg, 1994), we can be clear about which problems result from the nature of unconstrained connectionist systems, and which result from inadequate modeling techniques. If we can demonstrate that the problems can be ironed out in newer networks, then much of the criticism focused on the *nature* of these models will dissipate. Only then can we be sure that we have a viable alternative to dual-route accounts.

Plunkett & Marchman (hereafter P&M) distinguish between macro and micro u-shaped learning. In the traditional account of u-shaped learning (Cazden, 1968; Ervin & Miller, 1963), which R&M duplicate, the transition to stage 2 is marked by indiscriminate overgeneralization of the regular suffix. P&M point out, though, that kids *selectively* overgeneralize only to a particular subset of irregulars (even ones produced correctly in

stage 1), either on the basis of stem-stem similarities with other regulars (e.g. *take-->taked*), a *lack* of sufficient stem-stem similarities with other *irregular* patterns (e.g. *catch-->caught*), or strictly on a “best-guess” basis for stems without any telling likenesses (e.g. novel stems, *ploamph-->ploamphed*). Studies of naturalistic past tense usage (Marcus et al., 1992) and psychological elicitation tests (Marchman, 1988; Marchman & Plunkett, 1991) put the “regular rule imperialism” hypothesis to rest. These studies indicate that more of a *micro* effect occurs in stage 2 (i.e. *discriminate* overgeneralization).

Micro u-shaped learning of past tense has been successfully implemented in previous models (e.g. P&M, 1991), and this has been accomplished *without any discontinuities in the training data*, that is, no abrupt changes in vocabulary size and no skewed regular-irregular ratio. The results of this work suggest that the onset of overgeneralization errors in stage 2 follows not from manipulation of the training set (as in R&M’s model), but from *competition* between different change patterns in memory. The fact that kids overgeneralize the “-ed” rule in stage 2 does not stem from dual-mechanism architecture, in which the lexicon *fails to retrieve* the irregular and, in turn, *fails to block* the rule; rather, overgeneralization stems from the fact that kids have a *single* mechanism, and that different patterns in memory are competing for the use of its connections. P&M’s model can thus be labeled the “bottleneck model” (see figure 5).

P&M’s project is to analyze the effects that *incremental* learning (Elman, 1991, 1999) can have on the network’s performance. In the wake of their 1991 work, P&M fed their network a plausible training set, only this time around, instead of feeding stems one

by one at a more or less constant rate, they increased the numbers of tokens (and thereby *types* as well) *incrementally*. R&M's training set went from 10 verbs to 420 verbs going into stage 2. P&M's model, on the other hand, goes from 20 to 80 to 100 to 140 to 200 to 260, on up to 500. Nor do P&M "blur" the input to enhance generalization. Thus, P&M resist the temptation to "force" the sorts of effects linked to the transition to stage 2. Instead, they allow the *network* to determine its own course of action.

The network does assume the micro u-shaped curve in the course of its learning, and ultimately succeeds at mapping novel verbs correctly over 90% of the time. *Qualitative* changes in the network's organization (stability of weight and threshold settings), and its very ability to model the micro u-shape, follow from incremental *quantitative* and *structural* changes in the verb vocabulary (changes in number of tokens and distribution of types). Grammatical structure thus *emerges* in P&M's network. The domain-general processes of associative memory, in conjunction with a structured world (*incremental* input), elicit *qualitative* changes in an entirely different domain (inflection, linguistics).

### 5.1 Problems--

P&M skirt much of the difficulty arising from Wickelphonology. In their model, an artificial language is used to represent English verbs<sup>9</sup>. "Each verb in the dictionary consists of a consonant-vowel-consonant (CVC) string, a CCV string or a VCC string.

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<sup>9</sup>This might seem dubious, but for one, the artificial language follows English phonotactic constraints, and two, P&M's results have been replicated in a number of other networks with units that encode actual English words (e.g. MacWhinney & Leinbach, 1991; Daugherty & Seidenberg, 1994).

Each string is phonologically well-formed, even though it may not correspond to an actual English word” (30). Each stem is further set equal to a particular change class. Only those stems with the appropriate phonetic feature(s) are admitted into a particular change class, paralleling constraints on actual English stems. For example, if a stem ends in a [+cons, +dental] segment, it can either belong to the irregular “identity” (no change) class, e.g. *hit-hit*, or to the regular /-Id/ suffix class, e.g. *pit-pitted*. P&M then set this CVC stem (ending in a dental consonant) equal to one of the two applicable classes, and are therefore able to detect whether an error occurs in the output. Because P&M make the added modification that *each* phoneme be encoded featurally (rather than clusters of three as in R&M’s model), their network decomposes words much less arbitrarily--indeed, much more *realistically*--than R&M’s. P&M’s network appears to organize itself much like Pinker (1999) suggests the lexicon organizes itself (i.e. according to “family resemblance,” see Wittgenstein’s *PI*). Distributed representations of words overlap where similar features are shared (e.g. “spring” and “sing” excite many of same nodes).

One of the few criticisms that P&M fall prey to is the homonym problem, but this may not be a genuine concern. MacWhinney & Leinbach address the homonym problem by encoding semantic features of the input. The counterargument is that semantics is an unreliable predictor of past tense (Pinker 1999; Kim et al., 1994). But is it as reliable as it needs to be? Let us assume that semantic extension gives us the right answer 50% of the time, on par on Pinker’s prediction of chance. Now if kids are aware that the root of *flied* in “fly the board” is the noun ‘fly,’ doesn’t this imply that they know *flied* means something different from *flew*? Semantics may not be everything, but at least it accounts

for *some* of the input. “Direct access” research (Cotrell & Plunkett, 1991), for instance, demonstrates that people occasionally compute past tense *directly* from meaning, with *no* recourse to the present tense form in memory. Could we at least, then, get the critics to agree on 60% odds, just slightly above chance?

Let us assume that our arguments have thus far been convincing, and that it is perfectly feasible that semantic extension could help us at least break chance odds. At this point, we turn to the child acquisition data (Kim et al., 1994) to see whether our 60% prediction is accurate. How often do kids regularize a verb with a non-central sense? Only 64.1% of the time (3-5 year olds, Experiment 2, p. 191), quite close to our 60% prediction. Odds like these hardly make a social science, and this is true in *either* direction. The merit of semantics as a solution to the homonym (and reduplication) problem is largely underdetermined in *both* accounts. It is dubious, therefore, that such a problem is indicative of anything more than inadequate modeling techniques.

There is one criticism, however, which P&M are clearly subject to. P&M set three different output units equal to the 3 different regular suffixes (/ -Id/, / -t/, & / -d/). If a novel stem does not fit into one of the patterns in memory, it gets assigned to the most probabilistic “sure bet”; after a certain degree of training, this would of course be the regular pattern (at least in English!). *Which* regular suffix unit fires depends, of course, on the stem-final feature (e.g. [+cons, +dental]-->/ -Id/). Thus, the model appears to distinguish (albeit from *our* perspective) between stems and affixes, some crude form of morphology emerging from the network’s behavior. It also at least *appears* to be applying rules. Both “morphology” and “rules” emerge as by-products of the network’s



subregularities, which themselves result from PDP association. The inclusion of suffix units, however, is arguably a *manipulation* of connectionist architecture. It “forces” affinity to the child regularization data. Regular endings are essentially “patched in,” and regularization of a given stem frequently occurs because memory cannot fit it into an irregular pattern.

The procedure goes as follows: when fed a novel stem, the network consults its database for family resemblance to other irregular stem-past tense pairs as well as to regulars (with regulars, the deciding factor is frequently their word-final feature); if an irregular “closest match” is found, the novel stem falls into the relevant irregular change class; if not, or if matched with a *regular* class based on similarities, the stem goes through more or less unaltered, that is, until it is finally outputted, in which case the appropriate regular suffix unit fires (i.e. no “blending” of features is necessary to manufacture most regular past tenses). Note that this is a similar process described by Pinker (1999). In Pinker’s account, if an irregular “closest match” is found in memory, it *blocks* the rule; otherwise the rule fires as a default. Thus, P&M are able to model the default status of the regular rule within the confines of a “single” mechanism, *but at what cost?*

P&M “patched in” these regular suffixes, and further, the distinction between stem and affix. They set out to provide an alternative to dual-mechanism approaches, yet ultimately commit themselves to dual architecture. If a separate group of units handle the most crucial aspect of the regularization task (i.e. suffixation), and these units are not directly involved in any correct irregular computation, then we can reasonably conclude that we have two separate mechanisms. The regular unit “patch” is essentially a sub-

mechanism of the larger neural net. Because P&M “hard-wire” regular suffixes into their network’s output units, the regular suffixation change is frequently not even *generated* by the the mechanism that handles the irregulars (e.g. for “ploamph,” which has no overlapping features with patterns in memory). In fact, regularization often depends on the *failure* of the network to find sufficient associations. Pinker (1999: 145) characterizes all such attempts as “shameful,” and he might have a case here. P&M’s modeling difficulties indicate the greater difficulty of accurately modeling regular formation within the confines of a single mechanism. P&M make no progress toward a coherent account of how regular past tenses are consistently *computed* (i.e. *generated*) by the same associative net that computes irregulars. In the “ploamph” example, it is the *lack* of successful association that causes the appropriate regular unit to fire upon output. Thus, P&M make no progress in explicating the lexical *associative* mechanisms out of which regular inflectional grammar can be said to “emerge.”

#### 6.0 The “Rules All the Way Down” Dual-Mechanism Model:

The seeds of Pinker’s approach, which stands in opposition to single-mechanism approaches, lie in more traditional dual-mechanism (“dual-route”) accounts. Chomsky and Halle (1968) subsume the lexicon under syntax, but clarify that lexical processes differ characteristically from syntactic processes. “The syntactic component of grammar contains a lexicon which lists lexical items and their inherent properties, in particular, those phonological properties that are not determined by general rule” (44). The lexicon is characterized as a list of formatives contained in a sub-module of syntax (e.g. *boy, dog,*

and the associated concepts defining each<sup>10</sup>), but only the idiosyncratic properties of these formatives are encoded. Any regularities in the lexicon are “extracted” by the grammar module, and subsumed under a particular set of rules.

Lexicon construction proceeds according to “lexical rules” which specify the way formatives are to be encoded. For example, lexical rules establish that *walk* (/wak/) takes the form “[v[ walk] past]v” in the lexicon--subject to the regular past tense rule with a non-exceptional “past” diacritic--as opposed to *take* (/tek/), which takes the form “[v[ t\*k]]v,” specifying irregular formation, or that the formative #tek# falls under some particular vowel change rule in the phonology module. Really, though, *walk* does not require a “past” tag at all. The very fact that it is *not* tagged for irregularity (as *take* is) is sufficient for the system to determine that it falls under the regular “default” rule. In fact, no regular past tenses can be found in Chomsky and Halle’s lexicon at all, only “untensed” root forms, inertly waiting higher-level rules to build them up into full-fledged morphosyntactic units (words, phrases, etc.).

Well-formedness rules specify the most minimal amount of structure required in the lexicon to keep irregulars from being regularized, and to specify the appropriateness of the regular rule elsewhere. Outside of lexical rules governing well-formedness (e.g. the necessary inclusion of the irregular past diacritic “\*” and the unnecessary inclusion of diacritics for regulars), the lexicon *per se* accounts for little of the structure of language.

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*Boy* and *dog* would actually be represented in the lexicon in symbolic form, encoding the relevant phonetic feature matrixes for each (see p. 9).

It primarily consists of unrelated slots which get filled by representations for words. The actual vowel change that turns /tek/ into /tuk/, for example, falls under phonological rather than lexical rules, so the lexicon alone establishes only *what* vowel change needs to occur, not *how* it occurs. Phonology takes the lexical representation as its input, and assigns to it a phonetic description according to universal, and hence (putatively) innate principles of phonology.

In the *take* example, “readjustment rules” (10-11), presumably at the interface of the lexicon and phonology, tweak the lexical representation “[v[tek] past]v” into “[v[ t\*k]]v,” flagging phonology to change the /e/ to an /u/. Once the *take-took* change rule is acquired, that is, as error rates fall in the third stage of u-shaped learning, the “[v[tek] past]v” lexical entry gets overwritten by “[v[ t\*k]]v,” providing more definitive information for the phonology module, and thus promoting more accurate and efficient processing of irregular past tense. Note, though, that Chomsky and Halle must assume that a critical mass of past experience with the irregular *took* must be reached before the generalization can be made that it gets the lexical diacritic “\*.” In part, then, the rules for storing and forming irregulars *do* require learning, rather than being strictly “acquired,” and such learning must draw from general learning mechanisms (pattern associators) which make generalizations over data in memory.

The conclusion that acquiring the past tense *took* involves general learning mechanisms might seem controversial to critics of generative phonology, but Chomsky and Halle make it clear that their theory accommodates this fact. The grammar module in its entirety is linked to short and long-term memory (10), otherwise known as a 2-stage

memory. Short-term memory operates in real-time, so it stores the input sequence for grammar mechanisms to access in the production of surface structures. Syntactic and phonological surface-structure representations of input sequences depend on short-term memory, whereas syntactic, phonological, lexical, and especially semantic d-structure representations draw from long-term databases. What makes the grammar module so efficient in its productive, rule-driven task is the fact that it need not be cluttered by idiosyncratic information (the lexicon handles this) nor by any information other than the rules themselves (general memory handles this). In the grammar module, rules “rule.”

Insofar as the lexicon provides only minimal information about the phonological forms that a given stem can partake in (e.g. “telegraph,” “telegraphic,” and “telegraphy”), individual entries are stripped of unnecessary morphological and phonological baggage. In Chomsky & Halle’s lexicon (1968), the phonetic variations of ‘telegraph,’ as in “telegraph,” “telegraphic,” and “telegraphy,” do not get separate entries. Only one entry (+tele+græf+) gets listed (encoded in a featural matrix), and the alternate phonetic representations /télɪgræf/, /tɛlɛgræf/, and /təlɛgrɪf/ are generated by morphological and phonological rules in separate modules. For example, assigning appropriate stress contours to the alternatives is not the business of the lexicon, but that of phonology (11-12).

With grammar being subserved by syntax and other highly rule-governed sub-modules, and the lexicon containing only minimal information, the source of linguistic structure on Chomsky and Halle’s view is clearly grammar proper. Chomsky and Halle present a fully modularized, serial symbol processor account of language processing *par*

*excellence*. Syntax, along with its sub-modules--phonology, morphology, and the lexicon--generate multi-level structural descriptions of input sequences (morphological & lexical at the word level, syntactic at the phrasal & sentence level, and phonological at both), by assigning to the sequences both a surface structure and a deep-structure. These descriptions--represented by multi-level phrase-structure trees--are then fed to a semantics module and assigned a semantic representation. Thus, speaker-to-hearer language processing arrives at the end of its cycle, from the input, sound, to its output, meaning (see figure 6). Because the rules for generating structural descriptions of input sequences exploit variables (i.e. *any* eligible stem falls under the regular phonological change rule), in other words, because grammatical processes are *productive*, an infinite number of possible sound-meaning correspondences can be generated by Chomsky and Halle's system.

The acquisition of specific grammatical operations like past tense can be explicated, according to Chomsky and Halle, by pinpointing those productive rules which govern regulars and irregulars. On this view, both regulars and irregulars are subject to phonological rules. Most every change-change pattern in the lexicon (e.g. the o-->e change pattern in *flow-flew*, *blow-blew*, *grow-grew*) can be collapsed into a small handful of austere rules which specify particular feature changes to a given irregular stem. Take for example the *rise-rose* and *take-took* changes.

If we take the present tense form as the underlying form, we must assign the lexical representations /rīz/, /tæk/, respectively, which give [rāyz], [tēyk] in the usual way. To derive the past tense forms, we first apply a rule shifting backness and rounding, which is widely applicable to irregular verbs and other irregular forms... This gives [rūz], [tōk]. Diphthongization and Vowel Shift give [rōwz], [tōwk]. Finally, reapplication of the Vowel Shift rule gives the forms

[rōwz], [tūwk]. (202)

Only two rules, one being applied recursively, are needed to account for the *rise-rose* and *take-took* change. In this case, the diacritic “\*” in the lexical entries “[v[ r\*z]]v” and “[v[t\*k]]v” not only signifies which phonological laws the stem falls under in past tense formation, but further that the stem requires two cycles of the Vowel Shift rule.

### 6.1 Problems--

*Cling-chung, tell-told, bind-bound, break-broke* changes all follow an instance of the Vowel Shift rule [-back]-->[+back, +round], as well as *run-ran* and *hold-held* ([+back]-->[-back, -round]). *Eat-ate, choose-chose, sing-sang, sit-sat* changes all fall under the Lowering Ablaut rule ([+high]-->[-high]), *bear-bore* follows a Backing Ablaut rule (mid front vowel --> mid back vowel), and *flee-fled, shoot-shot* fall under the Shortening Ablaut rule (long vowel --> short). In total, nearly 165 irregular verbs are generated by only 3 phonological rules (Pinker, 1999: 92). The economy of the “rules all the way down” model is striking, but it comes at a price.

Chomsky and Halle discount the even more traditional view that *all* irregulars are learned bottom-up (rote). Chomsky and Halle see verbs sitting on a continuum from completely regular on one extreme (*walk-walked*) to completely irregular on the other (*go-went, be-was*). Irregulars formed by suppletion (*go-went*) are memorized outright by general cognitive mechanisms, then stored in the lexicon as “[v[went] past]v”; fully regular verbs are generated by general rule, regular past tense forms not being stored in the lexicon; and verbs “less” irregular than *go-went* are generated by more specific rules (as

with the *ing-ang-ung* change-change family), being stored in the lexicon with the appropriate irregular change tag (e.g. “\*”).

Chomsky and Halle’s characterization of the non-suppleted irregulars (in other words, *most* irregulars) is nevertheless doubly problematic. First, the Vowel Shift rule, and its sub-rules, Lowering, Backing, and Shortening Ablaut, all follow from a troubled assumption, that “ontogeny recapitulates phylogeny” (Pinker, 1999) in the minds of modern day speakers. Take for example the *flee-fled* change. Chomsky and Halle handle this example with the Shortening Ablaut rule, which specifies where “[v[fl\*]]v” occurs in the lexicon, make the change [+long]-->[-long]. Enter the troubled assumption. The *ee* sound in “flee” is *not* just a drawn out, longer version of the *e* sound in “fled.” These are *qualitatively different* vowels, /i/ being high and tense (plus a /y/ sound usually follows, creating a diphthong--/fliyɔ/), and /ɛ/ being mid and lax. “Long” vowel and “short” vowel “have been misnomers in English at least since the Great Vowel Shift in the fifteenth century, when people scrambled the pronunciation of vowels” (Pinker, 1999: 95). As people “scrambled” vocalic pronunciations more and more over time, the less and less applicable became rules for manipulating specific qualities of vowels.

Chomsky and Halle argue that d-structures must be stored in our lexicons-- otherwise we run into the problem of *which* s-structure to store (see “telegraph” example). The Vowel Shift rule applies because the d-structure for a stem like *flee* in modern brains is more or less *identical* to the d-structure in Chaucer’s brain! “According to the Chomsky-Halle theory, the mental representations of words in different centuries over the past millennium, and in all modern dialects, are the same; English has changed primarily by



adding phonological rules” (Pinker, 1999: 98). Because we have d-structures stored in our lexicons, vocalic pronunciations from before the Great Vowel Shift are preserved, so the Vowel Shift rule still applies as it did 600 years ago. But unless Chomsky and Halle can produce convincing evidence that kids in modern times are routinely exposed to the Middle English pronunciations of irregulars (which is altogether unfounded), Chomsky and Halle must claim that the construction of d-structure is informed by innate knowledge of vowel qualities from before the Great Vowel Shift (which is equally untenable-- evolution at an unprecedented rate).

The second problem with Chomsky and Halle’s model is that it cannot account for the irregular data. Irregulars are inundated with stem-stem and change-change similarities, or patterns: *blow-blew, grow-grew, know-knew, throw-threw; bind-bound, find-found, grind-ground, wind-wound; drink-drank, shrink-shrank, sink-sank, stink-stank; bear-bore, swear-swore, tear-tore, wear-wore*. Moreover, these patterns seem to be *productive* in some sense (at least semi-productive). *Dive* found itself resisting its regularity in the early part of this century, and ultimately fell into the i-->o change pattern on analogy with *drive-drove*; now the proper past tenses are “dived” (unusual in the U.S.) and “dove” (the usual version). *Caught, cost, flung, knelt, quit, slung, stuck, and strung* were all lured by irregular patterns within the last few centuries (Jespersen, 1942). Diachronic shifts such as these suggest that the lexicon is more like a pattern associator than a list of unrelated slots.

Kids and adults alike inevitably make *irregularization* errors (*squeeze-squoze* on analogy with *freeze-froze, bite-bote, bring-brang, trick-truck*, see Xu & Pinker, 1992; also

Pinker, 1999). Given nonsense stems with stem-stem similarities to genuine irregulars like *spling*, people are easily seduced by the irregular *ing-ang-ung* family and produce *splang* as a past tense (Bybee and Moder, 1983)<sup>11</sup>. Evidently, then, the lexicon is not just an inert, idiosyncratic “list” of items, but a mechanism hungry for patterns, one that literally *generates* these patterns by detecting regularities in the input. The lexicon must thus account for more of the structure in past tense formation than traditionally thought. Lexical structure of this nature cannot be “distilled out” in the form of rules. “No rule can cleanly pick out the i-->u verbs, which is why Chomsky, Halle, [and Mohanan] didn’t bother looking for conditions that triggered each rule but resorted to listing the verbs individually” (Pinker, 1999: 102-3). Following the advice of Bybee & Slobin (1982), Pinker therefore settles on *family resemblance* categories for irregular clusters, as rigid rules are not flexible enough to accommodate irregular changes (e.g. *sling-->slung* though *spring-->sprang*).

Chomsky and Halle’s model predicts not a drop of systematicity for lexical processes (i.e. that lexical association of a stem with patterns in memory might “seduce” that stem into a particular change class). Generalization of a particular change rule is not handled by the lexicon in Chomsky and Halle’s account, but by the grammar module. Nevertheless, it is dubious that novel irregulars are generated by a general rule. A general rule of this nature (part of competence proper) would be too rigid for irregular change

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<sup>11</sup>Experiments like Bybee & Moder’s suggest that these errors are not just performance errors, but a result of competence, as they are “teased out” of the participants. Chomsky would clearly include the KOL kids bring to these experiments in the domain of competence, but would qualify that *splang* is generated by rule (at least in the 1968 literature). Only exceptionless rules fit into the domain of competence proper (see also Givon, 1999).

class discriminations (errors of commission would occur, e.g. “bring” would be incorrectly admitted to the *ing-ang* family). If Chomsky and Halle included *general* conditions designating when and only when a specific change should occur for a particular irregular cluster, they could rightly say that *generalization* is what causes novel irregular structures. But because they list these conditions for each verb individually, they forfeit the model’s ability to generalize to novel irregulars (i.e. to “stereotype”). Thus, contrary to Chomsky and Halle’s account, irregulars are best described as being learned bottom-up (on analogy--by association--or by rote memorization, see Pinker, 1999; also Aronoff, 1975), due to the fact that rigidly designated rules cannot account for irregular family membership.

Moreover, it is not even clear that the lexicon stores word-rule associations *at all* when it comes to irregular verbs. “Bybee & Slobin (1982) point out that speech errors occurring when irregular past tenses are elicited are virtually always existing but incorrect English words (e.g. *rise-raise*), never novel rule products (e.g. *rise-rewse*)” (Pinker & Prince, 1988: 333). These results demonstrate the lexicon’s prowess at storing word-word associations (hence “raise” for the past tense of “rise”), but demonstrate no such ability when it comes to word-rule associations. Chomsky and Halle can thus account for the regulars, and for *regularization* errors, that is, until kids have enough experience under their belts, the lexical entry for “take” remains “[v[ take] past]v” (no diacritic), so it falls under the general rule; but they fall short of an adequate account of irregular verbs and *irregularization* errors. Chomsky and Halle cannot adequately account for (i) the *way* in which irregulars are formed (due to the first problem with d-structure), (ii) stem-stem and

change-change patterns in the lexicon (due to the second problem, their characterization of the lexicon), and (iii) *irregularization* errors (due again to the second problem).

### 7.0 A Convergent Perspective:

The only way to get a clear grasp on how the lexicon and grammar interact and are implemented in the brain is to study *specific* grammatical operations, paying close attention to the sorts of processing each seems to demand. Rumelhart and McClelland (1986) and Plunkett and Marchman (1991, 1993) claim that irregular and regular past tense verbs can each be handled by the same mechanism, that is, by the same neural processor (a pattern associator memory, or PDP). On the other side of the debate, Pinker (1999) argues that the lexicon and grammar (and at a finer grain of detail, “words and rules”) are mediated by *different* sorts of neural processors, concentrating specifically on the inflectional system (morphology module). Most of Pinker’s data specifically targets the English past tense and nominative plural systems, but studies have been replicated in a variety of other languages (e.g. Arabic and German; Pinker, 1999: 211-239).

On the surface, English irregular verbs appear to be a grab-bag bunch whose rules for past tense formation are fossils from before the Great Vowel Shift (“strong” verbs), or whose rules have been corrupted over time (e.g. *burned*-->*burnt*; *dived*-->*dove*). But when we look closer, there is a significant amount of structure in the lexicon which has traditionally been neglected. Rules for forming the past tense of strong verbs before the Great Vowel Shift appear as “artifacts,” or patterns in the lexicons of modern day speakers resulting from stem-stem and change-change similarity between strong verbs that

have held strong, as it were, to the test of time. Before the Great Vowel Shift, English speakers produced the past tense of strong verbs by rule (ablaut), top-down, whereas during and after the Shift, the phonological rules for past tense formation became increasingly more inapplicable, so past tense forms for these verbs needed to be learned more and more bottom-up (i.e. memorized rote or via association, not generated by rule). Most strong verb past tense forms find their way into the modern day speakers' lexicons in a more or less bottom-up fashion.

When it comes to irregular past tense verbs, the degree of structure they exhibit paints a less traditional portrait of the lexicon. Stem-stem and change-change similarities between irregulars form basic structures (or patterns) in the lexicon, and these patterns seem to have some rule-like qualities, as in the *dived/dove* example. For many modern day speakers (whose dialect seldom puts "dived" to use), the production of the past tense "dived" is blocked because "dove" has been memorized and is clustered in the lexicon with other verb forms of similar patterns. For speakers of dialects including both uses, "dove" is either generated by phonological rule (which is unlikely, see previous section) or on analogy with verbs in the i-->o change pattern. The latter phenomenon falls out quite naturally from the model, so long as we consider the lexicon as a type of pattern associator.

Because efficient pattern recognition is a characteristic shortcoming of rule-driven serial symbol processors (Bechtel, 1988), it is an unlikely candidate for English irregular verbs. "It has not been easy to develop rule-based processing systems with good pattern recognition abilities, in part because such recognition requires the machine to be able to

deal with an enormous number of contextual clues, whose relevance may differ from context to context” (Bechtel, 1988: 262). Because the categorization of a novel irregular stem depends on recognition of an appropriate change pattern (based on contextual cues), a serial symbol processor would not be an efficient mechanism for irregulars. A rule-based serial symbol processor operates according to necessary and sufficient conditions for category membership, what Givon (1999) calls “Platonic categories.” A serial symbol processor would thus have considerable trouble handling the fuzzy categorical status of irregular “families,” or clusters. Thus, departing from Chomsky and Halle’s (1968) view that irregular formation boils down to a few stripped-down, austere rules, Pinker suggests that it is handled by a type of pattern associator memory, or parallel distributed processor.

When we look at *regular* past tense formation, on the other hand, it appears to be a highly rule-governed process, and less constrained connectionist networks model it inaccurately. Egedi and Sproat (1991) beefed up (not “beft up”) the connectionist model of past tense, ridding it of problematic Wickelphonology (as do Plunkett&Marchman, 1993; MacWhinney & Leinbach, 1991; and Daugherty and Seidenberg, 1994). They added a hidden-layer for more complex computational abilities (“internal representations”<sup>12</sup>), and a training set based on acceptable ratios and numbers of regular and irregular verbs. Unlike P&M, Egedi & Sproat did not hard-wire, or “patch in,” the regular suffix, so the same nodes and connections mediated both regular and irregular

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<sup>12</sup>A hidden layer of units enables networks to construct internal representations, usually using different groupings of features than input representations. Because it provides additional and slightly different featural representations, the network has more raw data to work with (i.e. to associate). Thus, with a hidden layer, the likelihood increases that if there are regularities to be exploited, they will be detected and recorded by the network. Units in the hidden layer are connected only to input and/or output units, and thus receive no external

inflections. The network generalized change patterns to irregular stems as effectively as humans, but failed miserably at the regulars, leading the researchers (and Pinker, 1999) to conclude that different sorts of processors are required for regulars and irregulars. Egedi and Sproat's network (i) assigned to regular stems the "zero" suffix when a change should have been made, (ii) confused regular input stems with other regulars (*train-->trailed*, *speak-->smoked*, *glow-->glanced*, *conflict-->conflated*), and (iii) about 25% of the time, produced unpredictable, almost unimaginable errors like *wink-->wok*, *yield-->rilt*, *satisfy-->sedderded*, and *quiver-->quess* (145). The network simply misrepresents the way regular verbs are computed. Regular inflections result from *suffixation*, not from "blending" features associatively.

Pinker (1999) interprets the network's inadequacy with respect to regular verbs to be indicative that the connectionist model itself is incomplete. In the symbol processing account, the regular rule, as a default, exploits *variables* to predict the proper past tense. *Any* stem is eligible for the rule once it gets inputted into the rule box (even the stem 'input,' whose root form takes the irregular), which is precisely why the past tense of "take," for example, occasionally comes out "tooked" in child speech error data (Brown, 1973). The "rule box" is more or less unaffected by *which* stem it gets fed--precisely why the input "took" does not block the rule in producing "tooked." The regular rule does not discriminate; it fires ballistically (all or nothing). Because symbol processors exploit variables in just such a fashion, and because rules in symbol processors designate ballistic,

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stimuli.

serial operations (X-->Y/ \_), rules appear to be implemented in very different neural hardware than words. Regulars are thus generated top-down (generated by rule), from a symbol processor that takes eligible stems as input and serially copies them (with the appropriate phonological alterations), then adds “-ed” (/ -Id/, / -t/, or / -d/). Irregulars, on the other hand, are handled by a pattern associator, learned and generated from memory in a more or less bottom-up fashion. What makes Pinker’s model so efficient is that his lexicon is uncluttered by regular past tense forms, saving much needed time when the database is searched for a match.

The procedure for formulating past tense in Pinker’s account goes as follows: a verb stem is inputted in parallel to the two main components of the inflectional system (the lexicon and grammar); the lexicon, a sort of associative memory, scans its database for similar stems which fall into a given change pattern; if a “closest match” is found, the stem gets generalized to that class, and the output of the rule mechanism, a serial symbol processor, is blocked--an irregular past tense form is outputted; if *no* similarities are found during the search, the regular rule fires. Thus, past tense forms for regulars need not be stored. Indeed, it is precisely the *absence* of regular stem forms in memory that triggers the application of the default rule. (See figure 4.)

Because generative rules of grammar are too rigid (exceptionless) for the irregulars, yet a system devoid of rules (or further constraints) cannot adequately account for the regulars, a hybrid position between Chomsky & Halle’s and Rumelhart & McClelland’s seems to be in store when it comes to past tense formation. Regular past tense formation is best described as being mediated by a *serial* processor, which uses top-



down information (representations, “word=stem+affix” templates, “blocking” mechanisms etc.<sup>13</sup>); whereas irregular past tense formation is best described as being mediated by a parallel distributed processor, using primarily bottom-up processes, with no explicit rules or pre-set weights, although connections can be altered, or strengthened, when a verb is adopted into a particular pattern. In other words, irregulars are memorized rote, and regulars are generated by rule. At the psychological level, a hybrid view makes sense because it does not entail what Pinker calls “jiggery-pokery,” or “fudging,” in trying to model child past tense learning accurately.

### 7.1 Problems--

Pinker’s model predicts frequency effects for the irregulars, but not for regulars. Subjects in one experiment, for example, produced infrequent irregulars much more slowly than frequent ones, but the same result failed to hold for regulars (Pinker, 1999: 129). Since regular past tenses are not stored, there would be no delay in applying the rule due to tenuous memory traces. Daugherty and Seidenberg (1994), however, demonstrate that frequency actually can affect a certain class of regular verbs, which they label the “regular but inconsistent” class. Verbs like *bake-baked* that have close irregular neighbors in phonological space (e.g. *take-took*) are, in fact, sensitive to frequency. Pinker (1999)

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<sup>13</sup>These need not be *representationally* innate to the extent Elman (1999) suggests. Take, for example, the blocking mechanism, which can be explained with recourse to local and global architectural constraints (less direct), or with recourse to functional pressures on developing brains for both rapid and attended processing (for rules and words, respectively).

stipulates that *bake-baked* is “attracted” by stem-stem similarities into the /e-->/u/ change class, and thereby stored in the lexicon. Doublet past tenses must be stored as well, otherwise it would be impossible to produce both forms; for example, the past tense form *dreamt* would always block the production of *dreamed*. So now we have all doublets (*slit/slitted*, *dreamed/dreamt*) plus all “regular but inconsistent” past tense forms in the lexicon. The problem becomes clear when we sit down and list all the words that this would include.

*Slit/slitted, rid/ridded, dreamed/dreamt, dived/dove, braked (broke), baked (took), blinked (drank), pitted (hit), flitted (hit), shit/shat/shitted, lied (lay), squeezed (froze), sneezed (froze), faked (broke), ached (broke), caked (broke), showed (grew), bowed (grew), mowed (grew), towed (grew), flowed (grew), dinged (brang), pinged (brang), winged (brang), flaked (took), bared (bore), cared (bore), shared (bore), paired (bore), pared (bore), fared (bore), pined (found), wined (found), dined (found), minded (found),* and the list goes on, must all be stored. Furthermore, any time a novel verb is encountered that is phonologically similar to an irregular stem-stem similarity class, it too is stored. This paints a much different picture of the lexicon, one shot through with exceptional regular past tenses. The sheer bulk of intermediary cases indicates that our “regular” classification cannot be as rigid as Pinker makes it out to be. Yet rigid classification of regulars is precisely what a serial symbol processor demands for efficient computation (i.e. “Platonic categories”). Daugherty and Seidenberg (hereafter D&S) see this as an indication that we need to abandon the “rules and exceptions” approach to grammar, exemplified by Pinker (1999) for the regular past tense. They instead adopt a “continuum

of regularity” view. Unlike Chomsky & Halle’s “continuum of regularity,” however, D&S’s is not rule-governed in the traditional sense. More of a “prototypical” rule seems to be in order for the regulars, rather than an exceptionless rule of grammar (see Givón, 1999; also Rosch, 1975). The *most* regular set of regulars (those without similar-sounding irregulars) would be clustered closest to the mean (e.g. *talk-talked*), and the “regular but inconsistent” would define the next circle out (e.g. *bake-baked*), and so on until we get to the *least* frequent “regular but inconsistent” verb, the one that takes the longest to produce.

According to D&S, Pinker’s solution to the “regular but inconsistent” problem is *ad hoc*. “Note that there is no independent basis for assuming that the ‘associative net’ will necessarily exhibit this property [the attraction of “regular but inconsistent”]; it is merely stipulated as a means for handling some novel behavioral facts” (D&S, 1994: 382). In the dual-route theory, there is no basis for distinguishing “pure” regulars from “regular but inconsistent” types. The efficacy of Pinker’s blocking mechanism (and lexical look-up) appears to be in jeopardy. Pinker cannot explain how *squoze* is frequently produced without admitting that his model suffers inherent mechanical shortcomings (i.e. false-alarm blocking). Moreover, he cannot account for frequency effects on “regular but inconsistent” without claiming that all such past tenses (and doublets) are stored in the lexicon--a move which seems to fly in the face of his own lexical storage criterion (i.e. *irregularity*).

In connectionist models, however, the “regular but inconsistent” phenomenon falls out naturally because the same weights are being used to compute both irregulars and

regulars. Because Pinker cannot account for such a large class of regular verbs (without recourse to *ad hoc* stipulation), it is questionable that connectionist accounts are parasitic of symbolic accounts, as he claims elsewhere on similar grounds (Pinker & Prince, 1988). In point of fact, it might be the case that the relation holds the other way around. At least part of Pinker's project needs to be devoted to explicating the lower-level mechanisms that ground his higher-level theory of language cognition (see "Discussion" section).

Another problem Pinker inevitably faces is that of vagueness. Throughout his writings (Pinker&Prince, 1988, 1994; Pinker, 1989, 1994, 1999), Pinker suggests that a variety of past tense phenomena are consequences of innate architecture. He alludes to many such innate mechanisms--"word=stem+affix" templates, templates specifying lexical categories (N, V, Adj.), and the blocking mechanism--but his account of *how these are innate* (i.e. *type* of innateness) is lacking (see Elman, 1999), nor does he explain how these so-called "templates" are *implemented*. Elman himself appears to have trouble pinning Pinker down--though he eventually categorizes him as a representational nativist (1999: 3). Even where Pinker refers to "representations" (e.g. "word=stem+affix" templates, blocking principles), he fails to explain *how* they got there. He does appeal to natural selection (e.g. Pinker, 1994), but natural selection *per se* is blind to distinctions between direct and indirect genetic encoding, only ensuring *that* a given behavior gets replicated, not specifically *how* or *to what extent* genes code for it. The quote Elman cites as evidence that Pinker is a representationalist (from Pinker, 1994: 93, 97) contains vague talk of "instinct," a "certain wiring of microcircuitry," but it does not clarify *to what extent* genes have a role in ensuring this "wiring." Pinker is working at such a high level of

description, it is unclear whether he has a specific neurological framework in mind; in turn, he fails to provide a coherent neural-level interpretation of his theory. Pinker sufficiently understates the lower-level implications of his theory to ward off any stereotypes, but this is an obvious problem for those outside the field of psychology, who want an empirically *grounded* account of innate constraints they can “sink their teeth into.”

Pinker’s account of past tense is beneficial, though, in exposing “tricks” used by modelers, making explicit the requirements for an adequate model of past tense. It does seem that kids and adults alike cue on more than just sound when determining past tense. Further, there is marked evidence that children cue on the *structure* of words, not only on sound (and semantics). Moreover, *most* regulars do appear to be immune to frequency effects, which might mean they follow a rule with *variables*, the hallmark of a symbol processor; that is, because *any* stem is acceptable, the rule does not hesitate, even for the most infrequently used verbs. Connectionist accounts have thus far failed to explicate various higher level phenomena (e.g. the fact that kids say “fied” in Kim’s test as often as they do). At the psychological level, a description of how children come to form past tense *in connectionist terms* is inadequate. Children simply do not appear to process regular past tense the way Rumelhart and McClelland’s model does.

(See figure 7 for a summary of the phenomena to be explained for past tense and how each model handles the explanation.)

## 8.0 Discussion:

Although Pinker, Prince, Kim, Egedi and Sproat all work to establish the need for

a modular system at the functional (psychological, cognitive) level, we do not have to rule out the possibility that at the *neural* (or implementational) level, the processor used for regulars might in fact be a PDP. Pinker himself fails to adequately explain how his higher-level theory can be applied at the lower level, yet connectionism accomplishes just such a task (i.e. *mediation*; see Smolensky, 1986). Akin to P&M's "patching in" of regular units, we would have to include a highly constrained PDP, but it is indeed possible to have "modules" of PDPs (i.e. multi-layered processing). PDP modeling is by no means synonymous with, or committed to, single-mechanism hypotheses (nor is it committed to entirely open-ended, unconstrained computation). For our purposes, one module, a less constrained PDP, would handle the irregulars, which would be stored both in stem and past tense form. A sub-module, a highly constrained PDP, would be inputted with the relevant verb-stem in parallel, and would be dedicated to processing regulars. If the less constrained PDP cannot find an irregular match, the highly constrained PDP's prediction shines through; if it *can* find a match, the constrained PDP's output portals are blocked (à la Pinker's blocking mechanism<sup>14</sup>). The regular suffix would thus be assigned as a default, so no regular past tense forms would need to be stored. We could also build semantic and morphological units (or layers) into the model.

Such a model would account for the lack of frequency effects on most regulars, yet reserve the possibility that frequency might affect "regular but inconsistent" verbs. The

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<sup>14</sup>Actually, this is not exactly "Pinker's" blocking mechanism. His version comprises part of a domain-specific system, but the system described here is inherently domain-general (although it may *specialize* in language-related computations). Functional pressures for both rapid and attended processing in general (see Givon, 1999) could easily account for modularization effects that occur in one's lifetime (see B&G, p. 64). Or it might be the case that the blocking mechanism is in fact innate; still, we can account for this fact with recourse to *less*

theory predicts a critical window of time during which an appropriate irregular pattern can be located, after which the rule inevitably gets applied (e.g. for “ploamph,” after a certain time spent unsuccessfully searching for an irregular match, the system’s default would apply). Because regular but inconsistencies share phonologically similar featural sets with irregulars (stem-stem), it would take longer for the lexicon to determine whether there is sufficient featural overlap to warrant blocking the rule. The lexicon, a type of *pattern associator*, “attracts” regular but inconsistencies into irregular patterns. Our options are as follows: one, either stipulate this feature of our model (i.e. the attraction and subsequent delay with regular but inconsistencies due to lexical association), and further, accept the inevitability of dual architecture in accounting for the data; or two, be completely ignorant of the facts of past tense inflection--that we attune ourselves to the *structure* of words as well as to sound, that regulars are flat botched by Egedi & Sproat’s network, that regulars would be mental “baggage” if stored like irregulars, that most regulars are immune to frequency effects, and so on.

Pinker can get away with stipulation for regular but inconsistencies because he views the lexicon as a type of associative net, inherently *generating* patterns, attracting candidate stems to applicable classes. “The words-and-rules theory predicts only that people don’t *depend* on stored [regular] past-tense forms, not that they are *incapable* of storing them” (Pinker, 1999: 137). If a regular past tense stem has too much featural overlap, it must be stored, otherwise it would cause the inflectional system considerable “noise” each time it

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direct genetic inscription (e.g. local and/or global architectural constraints, see Elman, 1999).

gets inputted. The promptness of the rule's firing, moreover, is contingent upon the promptness of the lexicon's decision procedure. During and after stage 3, verbs like *bake*, for example, would cause less delay than rarer verbs like *pare*, due to the fact that the memory trace to *bake*'s featural representation would be much stronger. The more frequently a regular but inconsistent verb is processed, the less delay the lexicon has in fitting it into an appropriate pattern. Regulars that are *not* inconsistent would be computed without excess delay (i.e. just the amount of time it takes for the irregular processor to realize there is no match). Conversely, then, another prediction is that the more overlapping features a regular but inconsistent stem has with irregular patterns in memory, the longer it will take to compute its past tense (up to a certain "critical" point, when the default operation fires). These predictions, based on a positive correlation between frequency and processing speed, have been confirmed in experiments on "parallel-race" theory (Baayen & Schreuder, 1995; Baayen, Dijkstra, and Schreuder, 1997).

There is further evidence for a dual-mechanism from a neurolinguistic perspective. Rhee, Ullman, and Pinker (1999) used MEG (Magnetoencephalographic) technology to chart the neural regions that "light up" when past tense is formed in non-impaired brains. About a quarter of a second after the initial stimulus (an English verb stem), left temporal and parietal regions light up for both regulars and irregulars alike. About a tenth of a second later, left frontal regions light up, *but only when the stimulus is a regular stem*. These results are compatible with the dual-mechanism account. For both regular and irregular stems, the lexicon is scanned for a match (the process shown by the first recorded



pattern of neural activity); if a match is found, the rule is blocked (explaining the lack of dual neural activity for irregulars); if not, the regular rule fires in a separate mechanism (the process shown by the second recorded pattern of neural activity). These data lend support to Pinker's claim that "rules" (for regular suffixation) and "words" (for irregular formation) are subserved by *separate* mechanisms implemented in different specialization regions of the brain.

In effect, we would be forfeiting the single mechanism hypothesis, adopting a modularized system (i.e. multi-layered), but we would not necessarily be forfeiting the claim to domain-generality. The reason why it is exceedingly difficult for neuroscientists to pinpoint domain-specific neural language mechanisms is due to a larger issue of *plasticity*. Because the brain is plastic, individuals suffering brain damage to one region of cortex can lose a specific ability, yet after some time, recover the function utilizing neural mechanisms in an entirely different cortical region<sup>15</sup>. After hemispherectomy, for example, split-brain patients can potentially recover language abilities in their *right* hemisphere (Gazzaniga, 1983), even though for most people so-called "language sites" are located in the *left* hemisphere. Neurological studies have also revealed that left hemisphere regions traditionally thought to control language processing also subserve other *non-linguistic* tasks, like the planning of motor sequences (see Kimura, 1976) and "analytic" processing (see Bradshaw & Nettleton, 1981).

Merzenich et al. (1983) mapped cortical sites to specific hand movements and

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<sup>15</sup>This is potentially an example of both *multiple realizability* (MR) and *context dependence* (CD). One unitary higher-level function can be implemented in widely multifarious neurological sites and states (MR). Moreover, neural sites and states may take on additional functions previously un-mappable to them (CD).

sensations in owl monkeys; after damage to these sites, previously “silent” clusters of neurons assumed the responsibility of controlling the monkey’s hand. In a series of experiments, O’Leary et al. (1989, 1993) transplanted fetal plugs of cortical tissue from one specialization region into an altogether different region, and surprisingly, the transplanted neural tissue assumed the appropriate neurophysiological configuration for each novel context. As Sejnowski puts it, “the relatively uniform structure of cerebral cortex suggests that it is capable of applying a general-purpose style of computation to many processing domains...” (1986: 372)<sup>16</sup>. The logic of the brain is not as restrictive as the logic of linguists, who, for ease of study, divide linguistic functions cleanly into distinct domains (Deacon makes a similar argument, 1997). Even if cortical regions *specialize* in one domain of processing, we cannot make the additional claim that these regions are *domain-specific* (i.e. innately constrained to mediate one and only one class of stimuli). At this point in our science, we are not even certain to what extent *localization* is a function of genes or environment (Caplan, 1987: 456), let alone domain-specificity.

Nevertheless, Pinker indicates that domain-specificity and serial symbol architecture are characteristic of the *brain*, not just the mind (Pinker, 1994; Pinker, 1999: 241-68).

So our ability to tie the steps of language processing to circuits in the brain is still rudimentary. For now we must settle for something simpler: clues that regular and irregular words depend on different sets of brain systems...and clues that irregulars depend more on the system for word memory and regulars more on the

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<sup>16</sup>At an even finer grain of detail, neurons themselves are able to convey a variety of different contents (multifunctionality). Hebb (1949) pioneered this idea, but refer also to Edelman (1989: 50), “a single group [of neurons] can participate in more than one kind of signaling function.”

system for rules. (1999: 243)

How are we to interpret such a “system for rules?” If there is a system in the *brain* whose operations depend on explicit unconscious rules (as argued 1999: 1-239 for cognition), we have evidence that Pinker’s higher-level theory of regular formation is an *implemented* theory (or at least it presents itself as one). But if his higher level theory of cognition is an *implemented* theory, then “innate KOL,” “domain-specificity,” “explicit unconscious rules,” and “serial symbol processing” can all be taken as descriptions of the *brain*. At the *neural* level, however, these descriptions would be entirely inaccurate.

The neurological evidence suggests that rigid domain-specificity cannot be imposed upon neural mechanisms subserving language. If, as Pinker projects (1994, Ch. 10), 98% of all language related disorders result from impairment of some site along the sylvian fissure in the left hemisphere, clearly language is *localized* across most individuals; we can even go so far as to say that perisylvian regions *specialize* in language-related computations, that is, that these regions *primarily* subserve language but also (or otherwise) serve other classes of stimuli. But it is quite another thing for language to be *domain-specific*, implemented in neural mechanisms that can *only* serve linguistic stimuli, and the evidence clearly does not stack in this direction. Inflectional abilities are impaired in agrammatic aphasics with damage near Broca’s area, for example, yet the same patients can exhibit impairment to systems involved in a number of *non*-linguistic tasks (Erhard, Kato, Strick, & Ugurbil, 1996). SLI patients also suffer setbacks on a number of different *general* cognitive tasks, not only on tasks specific to language (Johnson, 1994; Thal & Katich, 1996; Townsend, Wulfeck, Nichols, & Koch, 1995). Thus, the neural mechanisms

subserving language must at least partly be involved in various general cognitive functions (assuming there is no undiagnosed damage elsewhere).

In the new theory, a particular sub-set of general cognitive mechanisms would be recruited for symbolic-style computations (based on hundreds of thousands of years of computing highly regular patterns if we want to go this far, or simply based on modularization effects that occur in one's lifetime, see B&G, p. 64). Modularity effects of this nature would satisfy the cognitive need for both rapid (highly constrained) and attended (less constrained) processing (again, see Givon, 1999). This would be "implementational connectionism" (see Fodor & Pylyshyn, 1988; Pinker & Prince, 1988), invalidating the claim to "eliminative connectionism," but it would nevertheless emphasize the fact that PDPs are our best available models of how higher-level symbolic operations are implemented at the lower level (i.e. in *local* processes). On such a view, regular past tense structures could rightly be said to *emerge* (as by-products of domain-general processes), though this particular process of emergence would be much more highly constrained, as in Bates and Goodman's bubble analogy.

The beauty of the "implemented symbolic processing" account of past tense lies in the fact that a system of PDP modules of this nature mirrors the beauty of our biological endowment itself. For the past five decades, nativists have been approaching the problem incorrectly. Instead of pigeon-holing each seemingly unlearnable behavior or trait into a genetic substrate (e.g. genes that code for a "word=stem+affix" template, *language* genes, etc.), we need to approach the problem much more *interactively*. Human genomes have been forever evolving *in conjunction with structure already in the world*, as a result, our

genes specify only the minimal information required to ensure a given behavior. “The modest number of human genes means that we must look elsewhere for the mechanisms that generate the complexities inherent in human development” (Venter et al., *Science*, 16 Feb., 2001, p. 1346). The beauty of our boigenetic endowment is that it is subtle (i.e. “modest”). Genetic determinism, the claim that the most formative constraint on ontogenesis is the genome, in other words, that complex higher-level behaviors are predetermined, or “hard-wired” in the genome, is an untenable claim in light of modern findings in genetics (Venter et al., p. 1348).

Results of the Human Genome Project speak more to the emergentist account of indirect, *conservative* genetic interaction presented by Bates & Goodman and Elman (1999)--a complex synergy of gene-gene, gene-environment, gene-organism, and environment-organism interactions throughout ontogenesis. Entering the Human Genome Project, researchers originally predicted that they would find about 100,000 different genes, our complete biological endowment. Much to their surprise, they found about 30,000, significantly fewer than the number of genes that make up a grain of rice<sup>17</sup>. Each gene is dedicated to a multitude of functions (also refer to Greenspan, 1995), not just to a single domain, and genes get re-used in multiple gene-gene interactions (frequently dubbed “epistasis”). Nor do genes specify maximal structure (i.e. direct gene-behavior correspondences) when the need to is non-existent, that is, where our biological endowment of non-linguistic mechanisms and other complex interactions adequately

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<sup>17</sup>Refer to these addresses: [www.nhgri.nih.gov](http://www.nhgri.nih.gov) (Human Genome Project site) and [www.celera.com](http://www.celera.com) (Corporate Genome Project site). Also refer to *Science*, February 16, 2001 and *Nature*, February 15, 2001. For a layperson’s summary of related issues, refer to the *Missoulian*, February 12, 2001, “Human genetic map

handle the behavioral phenomena.

For language, we are getting increasingly closer to the view that maximal structure (e.g. *language* genes, *UG* genes) and domain-specific architecture are unnecessary in many domains of processing. As the list of general cognitive mechanisms used for language expands (e.g. MacWhinney et al., 1999), the need for *direct* innate architecture diminishes. *Some* innate architecture is required to overcome the induction problem, but whether or not this is domain-specific architecture remains to be seen. To date, there is no conclusive genetic or neurological evidence that the human genome codes for rigid domain-specificity (or “modularity” in the standard Fodorian sense) when it comes to “words” and “rules.” Specialization regions are the *outcome*, not the cause, of ontogenesis. Linguistic research needs to be methodical in discovering whether our ever expanding collocation of general cognitive mechanisms is sufficient to overcome the induction problem in particular domains. Only then can we be sure what belongs in grammar proper, or if there is anything there at all.

An alternative research program would be dedicated to the discovery of *how* associative memory, and its sub-layers, need to be constrained in order to overcome the induction problem, conceding that *some* innate constraints on general learning need to be posited, though much more indirect ones (see Elman, 1999). The empirical task for the future is to determine in which domains of inquiry less constrained connectionist networks are applicable (e.g. irregulars but not regulars in past tense morphology), as opposed those

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smaller than expected,” by Robert S. Boyd.

in which highly constrained PDPs are applicable (e.g. regulars but not irregulars). To adequately account for the origins of linguistic structure, we cannot stop at the conclusion that different domains seem to require different mechanisms. We must go further to explain how these mechanisms are biologically implemented at the lower level, where “linguistic intelligence” is implemented in domain-general architecture and *local* processes. Nature is thrifty. If a linguistic function can be accomplished by domain-general mechanisms, there is no need to waste precious resources in order to create a domain-specific one. A constraint free, open-ended memory runs flat into the induction problem, but an associative mechanism constrained by its own innate architecture and schedule of development--and by structure in the world--can clearly overcome this problem (especially if we build further constraints and sub-modules into the model to handle rigid rule-bound behavior *in general*).

#### 9.0 A Comprehensive Approach to Language’s Emergence:

*At different levels of analysis*, both Bates/Goodman and Pinker provide accurate descriptions of the way children come to perform complex grammatical operations. To provide an adequate and well-supported description of the rule-bound behavior underlying English regular past tense formation at the *psychological* level, we must appeal to some form of serial symbol processing, and abandon less constrained connectionist networks as inaccurate models. At the implementational level, though, there is no need to abandon connectionist descriptions. Rule-based learning has been successfully implemented in connectionist networks for the past two decades (Rumelhart and McClelland, 1986;

Rumelhart, Smolensky, McClelland & Hinton, 1986; Touretsky, 1986; Elman, 1999).

Rule-based operations (i.e. grammar) might very well *seem* like they are implemented in a serial symbol processor from a higher-level perspective, but at the neural (or “abstract neurological”) level actually be implemented in a connectionist network. Rumelhart and McClelland have made it clear that “their own program is one addressed to the microstructures of cognition, and this seems to allow for the possibility that more traditional cognitive models might characterize the macrostructure” (Bechtel 265).

It becomes important, then, to develop some way of relating the more abstract, cognitive-level theory to the underlying neurophysiology. More fundamentally, this relation is central to conceptions of the relation between mind and brain. It is therefore of considerable importance to have an explicit theoretical framework for conceptualizing the exact nature of this relation. (R&M, 1988: 329)

Evidently, then, much of the criticism Pinker levels at Rumelhart and McClelland is misdirected, as *both* of their accounts are necessary for a comprehensive description of past tense learning, one that includes descriptions at the micro- (implementational, neurological) *and* macro- (functional, psychological) levels. From a macro-level perspective, we have evidence of innately constrained domain-specific machinery at work, and built-in KOL undergirding acquisition. From a micro-level perspective, on the other hand, all we have are “dumb” statistical and mechanical procedures at work, and non-linguistic machinery used in linguistic (and perhaps other) domains.

Symbol processing is implemented in connectionist architecture, not the other way around, as Pinker & Prince argue (1988). If it were the other way around, our account would provide no adequate explanation as to how symbol processing is implemented at the lower level; but such an explanation is imperative (otherwise our higher-level theory



might correspond to nothing *physical* in the world). This is clearly not a *reductionist* account of the phenomena (though token physicalist). The integrity of *each* level of description needs to be preserved in a more comprehensive account. In the case of past tense formation, a certain quality of description is lost when we descend to the descriptive vocabulary of the lower level, from “knowledge of lexical root,” “knowledge of head,” “knowledge of lexical entry” to rudimentary cellular mechanics. We could not, in theory, build our psychological description solely from a neurological account of the phenomena. At some level of analysis, the seams of these two theories do not neatly overlap (at least isomorphically, or type physically; for further discussion on the larger issue of non-reducibility of psychology to neurology, see Fodor, 1974). By resisting the temptation to *eliminate* the higher-level description with one from the lower level, we have *both* theories with which to build a unified, comprehensive account. Thus, we should be careful not to collapse the two levels into one, as they are each necessary (though insufficient) in our comprehensive view. Although from a lower-level perspective, higher-level phenomena (e.g. rule application) may be mere *by-products* of lower-level processes, the lower level *per se* gives us an impoverished account.

If descriptions solely at the lower level run the risk of mis-characterizing higher-level grammatical operations, then likewise descriptions solely at the higher level might miss the possibility that rigid rule-bound behavior can, in fact, be implemented in highly constrained connectionist networks. Neural nets whose weights are pre-set, whose learning rule is strong enough, etc., exhibit highly constrained, rule-like, but nevertheless *emergent* behavior (in the sense that grammar is mediated by a mechanism that might

otherwise subserve non-linguistic classes of stimuli, or by one that might subserve *both* classes of stimuli). Pinker does not entertain this possibility because he draws his conclusions from observation of mostly higher level phenomena (e.g. the “wug test,” and other such *psychological tests*).

Insofar as the “emergence of grammar from the lexicon” is concerned, when we look at specific grammatical operations at the *neural* level, this might simply mean is that grammar is mediated by a *similar* processor (similar to the one that mediates the lexicon), though one much more rigidly constrained. But clearly this is neither the strength nor variety of “emergence” that Bates and Goodman have in mind; for they want to say grammar is mediated by the *same* processor as the lexicon (i.e. the same set of nodes and connections). In this regard, the dual-mechanism account is essentially correct that there are separate mechanisms for words and rules; it is not necessarily correct, however, that at the neural level, one mechanism is a serial symbol processor, and the other a PDP. At the neural level, a serial symbol processor dedicated to language and only language is highly unlikely<sup>18</sup>. A PDP type processor is far more reasonable because it follows principles of general cognition, putting *plausible* neurophysiological mechanisms to work (i.e. more flexible categorization of stimuli and domain-general architecture). It is unclear what biological mechanisms (or “hardware”) would even be available for a lower-level explanation of past tense in classical serial symbol processing terms, especially in light of

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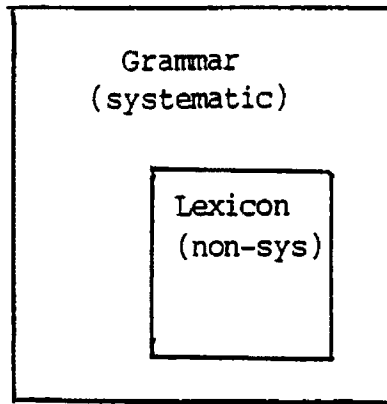
<sup>18</sup>For a detailed summary of why serial symbol processors are unlikely at the *implementational* level, refer to Fodor & Pylyshyn, 1988: 335-7. Three of the reasons included therewith are the “100 step constraint” (=>SSPs too slow for brain-style computation), acute sensitivity to damage and noise (=>SSPs suffer catastrophic damage, whereas brains usually undergo reorganization), and lastly, rigidity (=>rules governing SSPs lack adequate flexibility).

the domain-specificity claim. Thus, Pinker's description of past tense formation at the psychological level is fairly exact--for both regulars and irregulars; at the neural level, by contrast, he offers a fairly exact description of irregular formation, but only an *approximate* description of regular formation.

At the neural level, the general *type* of mechanism that Bates and Goodman propose for grammar is perfectly permissible (but note that this is not such a radical claim now). At the psychological level, however, appealing to less constrained versions of this same mechanism leads to inadequate descriptions of particular grammatical phenomena. The question of *which* grammatical operations prove to be more rigidly constrained (and *how*), as opposed to those which prove to be motivated by less constrained associative devices, is essentially an open-ended empirical matter, a puzzle for linguistics of the 21st century.

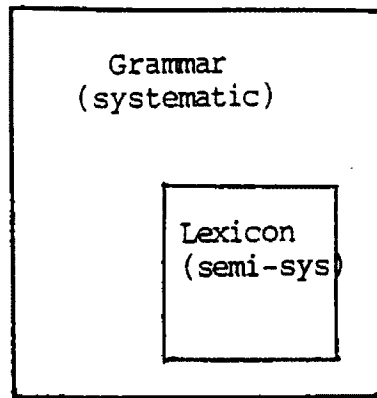
**Figure 1:**

Chomsky & Halle (1968)



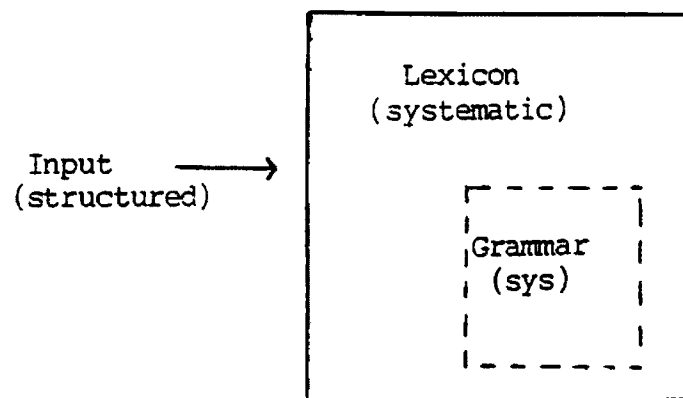
**Figure 2:**

Pinker (1999)



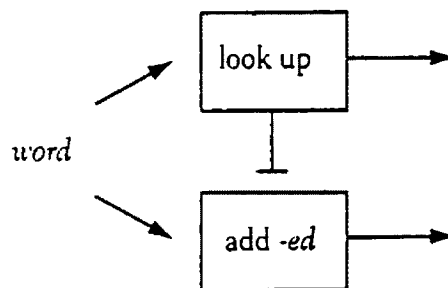
**Figure 3:**

Bates & Goodman (1999)



**Figure 4:**

(from Pinker, 1999: 130)



**Figure 5:**

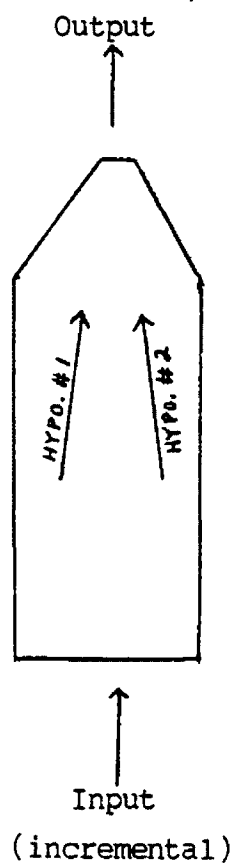
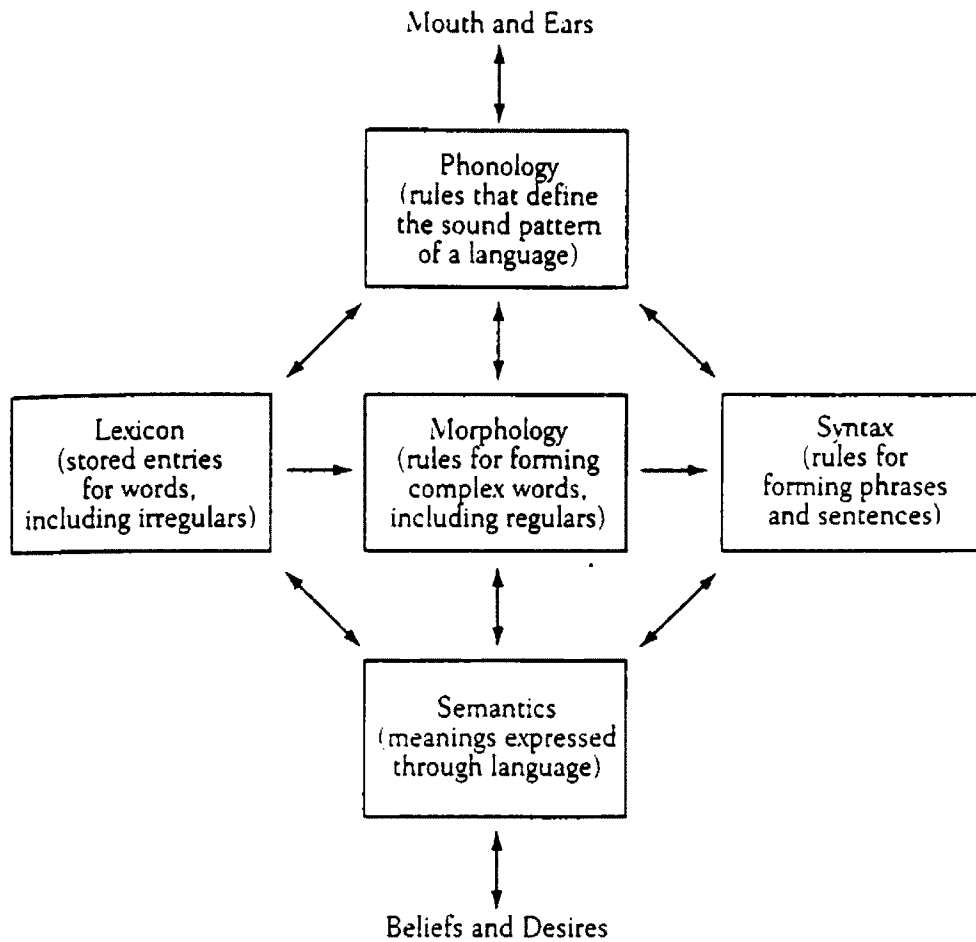


Figure 6:

(from Pinker, 1999: 23)<sup>19</sup>



<sup>19</sup>The only difference between Pinker's sketch of the linguistic system and Chomsky & Halle's (1968) is the inclusion of irregular past tense forms in the lexicon. Chomsky & Halle would only include minimal structure in the lexicon for irregulars, so only the root form plus an appropriate change "tag" would be stored.

**Figure 7:**

*Which Mechanism Accomplishes What--*

	IRREGULARS	REGULARS	GENERALIZATION
C/H (1968):	G	G	G
R/M (1988):	L	L	L
P/M (1993):	L	L/?*	L/?*
D/S (1994):	L	L	L
P (1999):	L	G	L

\*The “?” symbol means there is some discrepancy. P/M “patch in” regular suffixes to the output units, so the same weights that compute irregulars do not necessarily *generate* regular past tenses; this can happen in the *absence* of a found irregular “closest match.” Also, generalization of the “-ed” suffix to novel stems may fall out of the same process--the *absence* of a “closest match.”

*Facts to be accounted for--*

(adequate accounts get a “\$” symbol, otherwise “X”)

	REG. ERRORS (selective)	IRR. ERRORS (s.)	REG. BUT INCONS.
C/H:	X	X	X
R/M:	X	X	X
P/M:	X	\$	\$
D/S:	X	\$	\$
P:	\$	\$	X

C/H and R/M can only account for macro u-shaped learning, but not micro. P/M & D/S cannot account for regularization errors because candidate verbs may have little phonological similarity to patterns in memory; but this is really the only basis for generalizing the “-ed” suffix, unless we rely on the regular pattern being the most robust pattern in memory (which it is *not* cross-culturally). Pinker can deal with this problem because he distinguishes between regularity based on frequency and *psychological* regularity, the later of which acts as the default rule. Nevertheless, Pinker cannot account for the regular but inconsistent class (see “A Convergent Model” section).

(from R&M, 1986: 263-4)

**Table 1:**

THE MODEL'S RESPONSES TO UNFAMILIAR  
LOW-FREQUENCY IRREGULAR VERBS

Verb Type	Presented Word	Phonetic Input	Phonetic Response	English Rendition	Response Strength
I	bid	/bid/	/bidi/	(bid)	0.55
	thrust	/tr <sup>h</sup> st/	/tr <sup>h</sup> st <sup>h</sup> d/	(thrusted)	0.57
II	bend	/bend/	/bend <sup>h</sup> d/	(bended)	0.28
	lend	/lend/	/lend <sup>h</sup> d/	(lended)	0.70
III	creep	/kreɪp/	/krept/	(creeped)	0.51
	weep	/weɪp/	/wept/	(weaped)	0.34
			/wept/	(wept)	0.33
IV	catch	/kætʃ/	/kækt/	(catched)	0.67
V	breed	/breɪd/	/breɪd <sup>h</sup> d/	(breeded)	0.48
	grind	/graɪnd/	/graɪnd/	(grind)	0.44
	wind	/waɪnd/	/waɪnd/	(wind)	0.37
VI	cling	/klaɪn/	/klaɪnd/	(clinged)	0.28
			/klaɪn/	(clung)	0.23
	dig	/dɪg/	/dɪgd/	(digged)	0.22
VII	tear	/teɪr/	/stɪkt/	(sticked)	0.53
			/terd/	(teared)	0.90



**Table 2:**

SYSTEM RESPONSES TO UNFAMILIAR LOW-FREQUENCY REGULAR VERBS

Verb Type	Presented Word	Phonetic Input	Phonetic Response	English Rendition	Response Rendition
End in /d	guard	/gʌrd/	/gʌrd/	(guard)	0.29
			/gʌrdˈd/	(guarded)	0.26
	kid	/kɪd/	/kɪd/	(kid)	0.39
			/kɪdˈd/	(kidded)	0.24
	mute	/maɪ/	/maɪˈd/	(muted)	0.43
		/mɑdˈd/	(maded)	0.23	
squat		/skwˈt/	/skwˈtˈd/	(squated)	0.27
			/skwˈt/	(squat)	0.22
			/skwˈkt/	(squawked)	0.21
End in unvoiced consonant	carp	/kɑrp/	/kɑrpt/	(carped)	0.28
			/kɑrptˈd/	(carpted)	0.21
	drip	/drɪp/	/drɪptˈd/	(dripped)	0.28
			/drɪpt/	(dripped)	0.22
	map	/mæp/	/mæptˈd/	(mapped)	0.24
			/mæpt/	(mapped)	0.22
	shape	/ʃæp/	/ʃæpt/	(shaped)	0.43
			/ʃɪpt/	(shipped)	0.27
	sip	/sɪp/	/sɪpt/	(sipped)	0.42
			/sept/	(sepped)	0.28
slip	/slɪp/	/slept/	(slept)	0.40	
smoke	/smOk/	/smOkˈtˈd/	(smokted)	0.29	
		/smOk/	(smoke)	0.22	
snap	/snæp/	/snæptˈd/	(snapped)	0.40	
step	/step/	/steptˈd/	(stepped)	0.59	
type	/tɪp/	/tɪptˈd/	(typed)	0.33	
End in voiced consonant or vowel	brown	/brwn/	/brwnd/	(browned)	0.46
			/brˈnd/	(browned)	0.39
	hug	/hʰg/	/hˈg/	(hug)	0.59
	mail	/maˈl/	/maˈld/	(mailed)	0.38
			/membˈld/	(mabled)	0.23
tour		/tʊr/	/tʊrdˈr/	(toureder)	0.31
			/tʊrd/	(toured)	0.25

**English Irregular & Regular Past Tense**  
(from Bybee & Slobin, 1982)

Irregular:

**I. No Change:** beat, hit, cut

**II. Stem-final /d/-->/t/:** send/sent, build/built

**III. Internal Vowel Change (IVC) + stem-final /d/ or /t/:** feel/felt, lose/lost, say/said, tell/told

**IV. IVC, delete stem-final consonant (C), + stem-final /d/ or /t/:** bring/brought, catch/caught

**V. IVC where stem-final C [+dental]:** bite/bit, find/found, ride/rode

**VIa. VC /i/-->/a/:** sing/sang, drink/drank

**VIb. IVC /i/ or /a/ --> /ʌ/:** sting/stung, hang/hung

**VII. All other IVCs:** give/gave, break/broke

**VIII. VC where stem-final diphthong:** blow/blew, fly/flew

Regular:

**I. Add /-ɪd/ where stem-final C [+dental]:** start/started

**II. Add /-t/ where stem-final C [-voice]:** look/looked

**III. Add /-d/ where stem-final V, or C [+voice]:** move/moved

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