NEUROPSYCHOLOGY AND COGNITION

VOLUME 6

The purpose of the Neuropsychology and Cognition series is to bring out volumes that promote understanding in topics relating brain and behavior. It is intended for use by both clinicians and research scientists in the fields of neuropsychology, cognitive psychology, psycholinguistics, speech and hearing, as well as education. Examples of topics to be covered in the series would relate to memory, language acquisition and breakdown, reading, attention, developing and aging brain. By addressing the theoretical, empirical, and applied aspects of brain-behavior relationships, this series will try to present the information in the fields of neuro-psychology and cognition in a coherent manner.

The titles published in this series are listed at the end of this volume.

WRITING SYSTEMS AND COGNITION

Perspectives from Psychology, Physiology, Linguistics, and Semiotics

Edited by

W. C. WATT
University of California, Irvine

![Kluwer logo]

KLUWER ACADEMIC PUBLISHERS
DORDRECHT / BOSTON / LONDON

Printed on acid-free paper

Printed in the Netherlands

TABLE OF CONTENTS

PART II

WRITING SYSTEMS

INTRODUCTION

The English spelling-system empowers those who master it to perform two pleasantly rewarding tasks: (1) conveying the English language, and (2) demonstrating command of an arcane and useless skill. It serves the first function well; it serves the second superbly, which accounts for its unbroken popularity with pedagogues whose livelihood depends on inculcating it and with those who in showing they have learned it can give evidence of belonging to a well-schooled leisure class able to spend the first fourth of their lives on vacation from productive labor.

Every writing-system offers both utilities, the linguistic and the social, but they do so in varying proportions. The social utility is in some measure fulfilled whenever and in whomever literacy is acquired, since for no one is the ability to write inborn, and—quite unlike the acquisition of spoken language—learning how to write requires time consciously if involuntarily devoted to its acquisition. The harder a language's writing-system is to gain control of, the greater is its social utility: Chinese is very high on this dimension; Spanish, whose 'phonemic' spelling borders on one-to-one, is very low. English falls somewhere in between. The linguistic utility of a given writing-system is partly independent of its social utility, but not wholly. Thus two systems that represent their spoken counterparts equally well will because of that fact offer a certain parity of access to the learner, who is after all already a speaker and—holding stylistic felicity to one side—seeks only to learn a means of representing what he knows; but on the other hand a system that represents speech very poorly, thus slighting its linguistic function, will for just that reason fail to exploit what speakers already know and so, being harder to acquire, bestow higher social status.

One could of course take a somewhat less cynical view of 'social utility' and suppose that a society might derive greater benefit from a populace that spent its early years learning useful things like mathematics or how to fix a leaky faucet instead of useless things like '<i> before <e> except after <c>'; and certainly, like many modern linguists, one could take a less cynical view of the degree to which the English spelling system serves its social purposes (as first defined above) at the expense of its linguistic ones. Thus many scholars have argued that the difficult English spelling system serves a 'higher' linguistic function in representing, besides the sound of spoken English, also important morphological and etymological information.[1] And so it does. For example the spelling of <hymn>, instead of <him>, which would be the straightforward way of writing the word's sound, provides in its silent final <n> the intelligence that when the derivational suffix <-al> is added the <n> will be sounded, correctly relating <hymn> and <hymnal>; and in its <y> it provides the further intelligence that <hymn> was borrowed from the Greek. Or at least it provides this information to those few who know both

89

words but who have never guessed them to be related and to those fewer
(except at Bryn Mawr) who know that words in which the vowel [I] is spelled
<y> are likely to have had a Greek provenance. Notoriously, these 'higher'
linguistic functions are served rather fitfully, since for example the silent <n>
of <limn> is never heard anywhere—there is no *<limnal>—and so it is
altogether useless and misleading; and many supposedly 'etymological' cues
are simply relics of past ignorance (for instance, <delight> is spelled as if
derived from Middle English or Anglo-Saxon, the <gh> representing, as in
<right>, a spoken [h] or the like (Sampson 1985: 205-206, 217); but in fact
<delight> derives via Old French from Latin <delectare>, so that the true
relation between <delight> and <delectation> is actually *obscured* by this
'etymological' spelling). Even putting aside the many instances in which
English spelling ingeniously belies morphological and etymological truths, we
could ask: Would even a perfect such system be worth the trouble of
acquiring it? Worth it to most readers? The answer is 'No.' Etymology is of
little interest to most, and properly so; and if it isn't inferable from *spoken*
English that 'hymn' and 'hymnal' are Siamese twins, their spellings' being
joined at the <n> is unlikely to provide useful knowledge. The inutility to
most readers of such spellings as <hymn, hymnal> is seen all the more clearly
on taking into consideration the fact that nothing essential to the pronunciation
of either word is conveyed by their being spelled so as to convey their
relationship: final [-mn] is impossible in English anyway, so that <-mn>
could *only* be pronounced as [-m], consistent with the general rule 'When two
final consonant-letters spell an impossible sound-sequence, pronounce only
the first one'[2]; and final unstressed <-mnal> could not be pronounced
otherwise than as [-mn'l]. The 'higher' linguistic information fitfully
conveyed by English spelling, at such cost in schooling, is of utility only to
the few readers concerned with such matters; and those poor wights could just
as well repair for it to the nearest good dictionary, where it can be found in
abundance.

Happily, however, what is mainly of social utility may neveretheless obey
principles reminiscent of those to which language itself adheres, and so merit
the interest of those who study language and related matters: linguists,
psychologists, semioticists. What counts in this regard is the degree to which
English (or other) spelling-conventions have been systematized, for to a
concomitant degree they may lend themselves to linguistic analysis and, for
that matter, may derive their organization by tapping the human linguistic
faculty or by mirroring in some measure the structure of language itself.
Significantly, in their 'mirroring' capacity the spelling conventions may
sometimes reflect linguistic structure even where they do not represent it, thus
providing a more abstract hence more intriguing 'mirroring.' As a case in

point, take the use in English (and in many other languages) of the conventions for the use of what I will call 'flanking marks': that is, of parentheses, quotation marks, and the like. The convention (roughly expressed) is that there must be an equal number of left- and right-flankers, so distributed that every left-flanker is closed by the next right-flanker not required to close an intervening left-flanker: (1), below, is correctly done; (2), incorrectly.

(1) <Richard Nixon, who was the 37th President (elected in 1968 over Hubert Humphrey [Vice President in the previous administration of Lyndon Johnson (1908-1973)]), resigned his office in 1974 and was succeeded by Spiro T. Agnew.>

(2) *<Spiro T. Agnew, the 39th Vice President (serving under the luckless Richard Nixon [1913-1974], acceded to the office of President when his predecessor died of the pox.>

Nothing in the spoken language corresponds directly to the succession of flanking marks in sentence (1)—there is no way of 'pronouncing' <)])>, for instance, that distinguishes <)])> from <)> or <)])])])]> or indeed from any other sort of pause—and yet (and this is the [in any case obvious] point) the conventions governing flanking marks are nevertheless strongly reminiscent of such syntactic conventions as those governing expressions like 'Neither on the one hand....nor on the other.' Thus, possibly, written conventions of this sort mimic features of the language even when they do not represent them; or at least they 'mirror' those features in such a way as to be amenable to linguistic analysis and (of especial immediate interest) shed light on extensions of linguistic structure into domains lying outside language proper.

In 'Some Graphotactic Constraints' James D. McCawley of the University of Chicago scrutinizes with relish some of the restrictions that govern how the elements of a given writing-system may cooccur. Some of these restrictions in English owe their origin to the peculiarities of the ordinary word-spelling system—for instance, to the fact that the verb 'to zinc' is spelled with a final <c> instead of <k>, requiring the <c> to 'harden' (stand for [k]) when <-ed> is added, which in turn requires that <c> become <ck>, which looks bizarre (*<zincked>), which in turn means that there *is* no way of spelling the past tense of 'to zinc.' Other restrictions, of perhaps still greater curiosity, are products of the English conventions governing the use of what I above called 'flanking marks' and other suprasegmentals. A case in point: the rules for the use of apostrophes and quotation marks, separately and in conjunction. It is certainly curious, here, that English typographic conventions specify that when a word in quotes terminates a sentence the period that marks the sentence-end, which function it performs in every other sort of sentence by

being the *last* graphic mark, *precedes* the quotation mark or marks, so that (3) is OK but (4) is not:

(3) <Asked if Saddam Hussein was the worst enemy the U.S. had ever had, President Bush said, 'Maybe not, but he was certainly our worst friend.'>

(4) *<Asked if his predecessor had erred in killing 200,000 Iraqis to put the Emir of Kuwait back on his throne, President Quayle said, 'Certainly not, to help any area of the USSR to gain its freedom from the godless atheists who run that place is a good and Christian act, by golly'.>

The conventions governing placement of close-quotation marks also specify that, like the period, the comma must precede them, though all other punctuation-marks, whether sentence-internal like <;> or sentence-terminal like <?>, must follow:

(5) <Wouldn't it be great if the Constitution had stipulated that American Presidents must take regnal numbers as kings do, so that 'Ronald I' would have been succeeded by 'George II'; and the latter would in turn have been succeeded by 'J. Danforth III I'?>

Is all that is involved here the desirability of saving the smaller punctuation-marks like <.> and <,> from being overlooked because outside the close-quote? Hardly. When <'> serves as an apostrophe instead of as a close-quote the conventions are reversed:

(6) <That house is the Evanses', I think (the one with the mange).>

(7) *<That car is the Evanses,' it's a neighborhood disgrace.>

And <.> follows sentence-internal <)>, as in (6). So if some canon of graphic esthetics is being observed its rules are a little capricious.

The orthography of <Evanses'> brings us to McCawley's next point. Curious indeed are the traditional conventions governing how to spell genitivized words, especially when they are also pluralized. Ordinarily these present no problem: in sound the plural nominative and genitive suffixes are identical, but are rather helpfully spelled with a trifling difference:

(8) <The cats ate the rats.> but:

(9) <The cats' celerity cut short the rats' asperity.>

A different picture emerges when what is genitivized already ends in <s> in the singular: proper names so ending, for instance, are (in the usual convention) genitivized in <'s> if of one syllable but in <'> if of two or more: <Ross's dissertation> but <McRoss' thesis>. But this difference is purely graphic and reflects no difference in pronunciation: <McRoss'> is pronounced exactly as if spelled <McRoss's>. The convention just noted for <cats, cats'> applies also to pluralized proper names ending in <s>:

(10) <The Evanses, poor old things, are childless but far from dogless.>

(11) <Last night somebody ran over the Evanses' little doggie and squashed him like a bug.>

And the conventions for pronouncing these proper-noun genitives are exactly the same as those governing the common-noun ones, so while <McRoss'> is pronounced as if spelled <McRoss's>, <the Evanses'> is *not* pronounced as if spelled <the Evanses's> (though see just below). It is perhaps partly owing to this skein of conventions that it is usual among the semi-literate to see <The Evans'> and <The White's> on mailboxes and—a still worse abomination—to hear 'the Evans' as the plural.

These are moderately familar facts, to which McCawley has added a couple of startling new dimensions. For one, he considers what happens when, instead of genitivizing a plural, one pluralizes a genitive. The popular restaurant <McDonald's>, if understood to be singular (as in 'We went to a McDonald's last night'), is in speech pluralized as if spelled <McDonald's's>, but in such cases is apparently spelled <McDonalds'>, precisely as if what were at issue was not the pluralization of the genitive <McDonald's> but the genitivization of the plural <McDonalds>. The result: something that instinctively, following rules that in fact do not apply, one pronounces 'McDonald's's,' precisely as desired. Thus this (entirely uncountenanced) extension of the English graphotactic rules has the result sought, showing that once those rules are utterly mastered, or utterly misunderstood, they are featly extrapolated from. He also considers what happens when one *genitivizes* a genitive, as in:

(12a) <Luisa likes Del Taco's tacos, but Andrew prefers McDonald's's.>
or possibly:
(12b) <Luisa likes Del Taco's tacos, but Andrew prefers McDonald's'.>
—which as he remarks (his n.7) is neither syntactically nor semantically [nor even phonologically] deviant; but graphically it's damn peculiar. (Thus spoken colloquial English allows one to pluralize or genitivize a genitive, but written English, even in the most colloquial register, balks at both <'s's> and <'s'>.) For another, he asks *why* the English genitive of proper nouns ending in <s> is so elaborately provided for, reminding us that, as in <Illinois' State House>, the 'Add only <'> to final <s>' rule for polysyllabic proper nouns applies even when the final <s> is silent. As a consequence any attempt to rationalize this convention on the grounds that it is meant to rule out 'unpronounceable' <s's> must contend not only with the fact that <Ross's> and <the princess's> disobey any such prohibition but also with the fact that <Illinois's> could seem unpronounceable only to someone so benighted as to pronounce <Illinois> with a final [z], so that any such rule boils at best down to this: 'The *cognoscenti* shall avoid the appearance of unpronounceable <s's>, even where such appear so only to the *ignoscenti*.'

As a further and even more provocative example, McCawley considers what happens when one genitivizes something in quotes, as in 'He likes "The New Yorker's" cartoons.' Here what goes between quotation marks is the genitive of the magazine's name—to avoid *<"The New Yorker"'s>, assuredly teratological—which runs directly against our sense of what is being quoted, the magazine's name. The semantics of any sentence containing such a form is *mandatorily* belied by its graphotactics. Implying, if we are to assume that the graphotactics do after all correctly convey the meaning somehow, that some graphic marks must be read in an order other than that in which they appear. We are accustomed to such conventions elsewhere, both in print—we read <$5> and <16> from right to left—and in speech—we understand 'All that glisters is not gold' as 'Not all that glisters is gold'—but cases where the usages of ordinary spelling involve such shenanigans have mostly escaped notice.[3] Continuing McCawley's argument, what are the rules determining how to genitivize something in italics: is it <He likes *Emma*'s apophthegms> or <He likes *Emma's* apophthegms>? The former. But what would have been the case in Latin, if they had italicized (no pun intended): <Sententias *Emma*e amat> or <Sententias *Emmae* amat>? Surely the latter, just as is presently the case, *mutatis mutandis,* in Russian (I am indebted to Christine A. Montgomery for this intelligence.) Oddly, it is the <'s> spelling of the English genitive that restricts italicization to the stem alone, as if <'> were a word-separator, in the disjunctive genitive pronouns like <*theirs*>, for instance, the genitivized word is italicized throughout. Now, it is true enough that the English genitive is a phrase-suffix rather than a mere word-suffix. This is shown in:

(13)   The Pope was reported as having winked at the man I saw yesterday's daughter.
—and in the fact that if a genitivized phrase ends in a genitivized word the vernacular result is like the genitivized genitive <McDonald's's> noted just above, though seemingly a little more acceptable, at least in::

(14)   This Ferrari isn't mine, it's a friend of mine's.
—though if the genitivized end-word ends in <s> or <'s> the result is cacophonous and in the second example as cacographous as ever:

(15)   ?That Lamborghini isn't his, it's a friend of his's.

(16)   ?That Maserati isn't Jack's, it's a friend of Jack's's.
—leading one to suppose that such locutions might be improved if they yielded to a rule of grammar 'Merge any double genitive into a syncretic single one,' replacing (15) with:

(17)   ?That Lamborghini isn't his, it's a friend of his.
But this sounds like an error, if only on the analogy of:

(18)   *This Ferrari isn't mine, it's a friend of mine.

—[in the sense of (14)], implying perhaps that, as in:

(19a) *This Bugatti isn't just his, it's both of us's.

(19b) *This Bugatti isn't just his, it's both of ours.

—one has in such instances overstepped the grammar (or encroached on the anti-grammar, or fallen between the two) and would do well to retreat to an adroit periphrase. Conclusion: in the vernacular a 'double genitive' is marginally acceptable when the first of them doesn't end in <s>, as in <mine>; otherwise the result, with or without syncretism, is foregrounded in speech and even more so on the page.

But these odd and rather wonderful facts are probably irrelevant in view of McCawley's observation (his n.5) that the Hungarian Dative marker is written *outside* its stem's end-quote, separated by a hyphen (as is possible only in such circumstances, unless the stem is a foreign import ending in a silent letter); and in view of the fact that the Romanian Genitive suffix (or genitivized postpositive article, more accurately) somewhat resembles English phrasal <'s> yet *is* italicized. That is, in a genitivized Romanian noun-phrase only the first word bears the genitive morpheme—indifferently 'bunului copil' or 'copilului bun' (= 'the good child's')—but it's <*bunului*>, not *<*bun* ului>.

(As an aside, if you accept the phrase <a <'>-less genitive> you accept as a correct graphic rendering something that when pronounced is impossible, since <'> could only be pronounced as 'apostrophe,' which cannot be directly preceded by the 'a' form of the indefinite article.)

It seems possible that the strange conventions governing the over-done avoidance of <s's> are somehow connected with the fact that the singular and plural genitives of e.g. 'cat'—<cat's> and <cats'>—sound exactly the same, as if on the (groundless) supposition that <cats'> owes its pronunciation to the spontaneous avoidance of the cacophonous <cats's>; but I have not been able to discover whether or not there is any justice to this conjecture. The other English graphotactic oddities to which McCawley invites our attention, however, cannot have this sort of explanation: for instance the avoidance of sentence-final <".> in favor of the semantically nonsensical <.">. Incidentally, if this avoidance serves the interests of some graphonomic esthetic, as was conjectured above as possible, we might add that seemingly esthetic principles do sometimes crop up in the spoken language; despite their having paraphrases in (20) and (21) respectively, (22) and (23) are generally avoided:

(20) Jim thinks Cher's pretty comely.

(21) The Duke died altogether gallantly.

(22) ?Jim thinks Cher's pretty pretty.

(23) ?The Duke died entirely gallantly.

—but the graphic/typographic conventions seem to be more swayed by such considerations than do the spoken ones. (Is the visible paid more heed?)

What McCawley's piece reminds us of most generally is the simple fact that, contrary to received opinion (in this as in all cases misguided), the written language is not just parasitic on the spoken, it has a life of its own. This does not in any way imply, as Jacques Derrida and his more credulous epigones would have us believe,[4] that the written language is somehow primary and the spoken, secondary; what it does imply is that the written language, in addition to or apart from slavishly representing the spoken language—obviously its primary function—has to a surprising extent its own and idiosyncratic rules, which have developed and are to some extent obeyed independently. They are therefore, of course, all the more worthy of linguistic inquiry.

Herbert E. Brekle of the University of Regensburg has provided us with a forceful attack on some aspects of W. C. Watt's treatment of how putative processes of historical change affect writing-systems. For a variety of reasons this is not the place to reply to his piece, except where clarification might aid the reader of the present volume.

First of all, then, Brekle seems to have based his adverse criticisms largely on a reading of one of Watt's conference papers (Watt 1983, given at a conference in Vienna in 1979) rather than on the longer articles in which some of the issues he finds slighted are in fact treated more fully and in far finer grain. Thus, for example, he finds fault with Watt for leaving unexplored the possibility that writing-systems are affected by forces other than his four 'evolutionary' tendencies, but this is not an inadvertent omission on Watt's part for, as that author has stated quite clearly (e.g., 1979*a*), the 'missing' influence is that of esthetics, as in the Greek 'geometrization' of <Γ> to <π>. That is, some of the historical forces that shape writing-systems are systematic or 'evolutionary,'[5] and if properly understood would therefore be predictable or at least retrodictable; while others, proceeding from conscious design principles, are as unpredictable as any other artistic tendency. It would be nonsense to aver that ordinary phonological change is on the same footing as W. C. Fields's partiality for palaver preponderating in p's, and it would be just as nonsensical to confound systematic and esthetic factors of any other sort. Conscious innovations are different from unconscious ones; for instance while the alphabet has shown a strong tendency to adopt a 'progressive' stance for its letters—they face in the direction of writing—when new sets of alphabet-like letters have been deliberatively innovated a disproportionate number of letters are gotten backwards (three cases in point: the new letters introduced by Claudius I; the 'runic' alphabets devised by J. R. R. Tolkien; and the Cherokee syllabary invented by Sequoyah).[6] The very historical

changes in the early alphabet that appear well explained as conforming to a tendency to 'homogenize' the letters (in this case so that they all faced rightwards), could not be explained at all if Sequoyah's half-backwards set of letters were taken as disproving any such tendency, which would be necessary if they were accorded the same standing as systems 'naturally' evolved over time.

Brekle also finds fault with Watt's four 'systematic' evolutionary forces, but here it is a little hard to make out the burden of his plaint. The four are: (1) *homogenization*, the tendency for elements of a system to become more alike; (2) *heterogenization*, the tendency to keep elements of a semiotic system different-enough to be distinguishable; (3) *facilitation*, the tendency to ease the elements' production; and finally (4) *inertia*, a purely conservative force tending to stabilize any system in whatever form it has been inherited. Clearly the first three forces, however independent their origin, may sometimes produce similar effects but at other times operate at cross-purposes; just as clearly the precise nature of their inter-relationships can be stated with confidence only after the historical evolution of many writing-systems has been analyzed in fine detail, a task that has only begun. Brekle's cavils over this issue seem only that. He wishes to extend *facilitation*, which Watt defines as easing letter-composition, to cover also changes resulting from the urge to ease letter-*recognition*, a plausible-enough proposal but one inadequately buttressed until it can be shown, as Brekle has not, that there are some evolutionary changes not otherwise explicable. (It is quite possible that users unconsciously subject letters to visual homogenization in part in the interests of making them more recognizable *as a set*, but I know of no demonstration that this is so, and even if it were so this would seem to imply a 'motive' underlying homogenization rather than a new aspect of facilitation as such.)

To take up one of Brekle's few specific objections, it is not at all the case that Watt has left the homogenizing tendency undefined nor that he has failed altogether to ground it, to the extent presently possible, in cognitive reality. That 'memory drift' often has an homogenizing or 'assimilative' effect was, surely, one of the more robust findings of the 'Gestaltist' work of the '20s and '30s;[7] what's more Watt has even proposed a specific mechanism whereby such an effect might operate in terms of his feature-specifications of the alphabetic letters. The mechanism, derived from contemporary practice in phonological theory, first supposes that generalizations are cognitively formed over a set of elements in such a way as to assign feature-values *en masse*, which values are therefore properly omitted (as redundant) for the individual letters; it then supposes that for any exception to such a generalization, whose values are therefore not omissible, those values must be stated individually. The mechanism then supposes that values for any given letter can be

forgotten.   It then supposes that someone who has forgotten such a value—say, the value determining the placement relative to its vertical stroke of a particular letter's augmentation—may nevertheless remember the relevant generalization—say, 'Augmentations go to the right'; and that such a person in such a situation can then apply that generalization in an attempt to recover the forgotten value. Now, if the letter at issue was among the majority of letters having their augmentation to the right, the placement of its augmentation would thus be correctly recovered; but otherwise it would be 'recovered' incorrectly and thereby reversed. In this way an exceptional left-facing letter such as <J> would after being 'recovered' end up facing rightwards—<ſ>—; while a right-facing letter such as <P>, after a lapse in every other way identical, would end up as <P>. Such an hypothesized mechanism makes no prejudgment about which sort of letter—regular or exceptional—is more likely to be partially forgotten; but it does prescribe that, once 'recovery' of a forgotten detail is dependent on invocation of the pertinent generalization, only the regular letter can be recovered correctly, since the effect on the exceptional one will be that it will be transformed into a letter conforming to the generalization, that is, that it will be regularized, thus (by definition) increasing the homogeneity of the set of letters as a whole.

If this is not a detailed hypothesis concerning the operation of 'homogenization,' it is hard to imagine what one would look like. What's more since homogenization is nothing other than the reduction of exceptionality it is hard to imagine how any other hypothesis could so closely fit analytic detail to (well-attested) phylogenetic and ontogenetic events. This is not to imply that Watt's hypothesis has been decisively upheld, or indeed that it cannot be sharpened: sharpened, for instance, so as to accommodate the fact that as a set homogenizes its exceptionalities must change their nature as well as their representation proportional to the whole and the degree to which they are exceptional relative to the generalizations that capture and define the set's regularities; but that there is further work to be done in this area can come as no news to anyone following its progress.

Philip A. Luelsdorff, who like Herbert Brekle hails from the University of Regensburg, is becoming known for his studies of the relative productivity of rules, especially those controlling how language is written. In his contribution as sole author to this volume, 'Developmental Morphographemics II,' he takes further his inquiry into how rules for writing language are acquired; and in the paper by Luelsdorff and Sergej V. Chesnokov of DA-Systems in Moscow those rules are characterized in terms of Chesnokov's 'Determinacy Analysis.' Together these two chapters reopen the issue of rule-productivity, an issue in which interest seems to vary cyclically, and relate that issue to language learning and linguistic 'evolution.' They also reintroduce, as the authors'

preferred means of inserting probability-of-alteration into the formal account of linguistic structure, the notion of Analogic Change. The engine that drives their account of analogical extensions, then, is the pressure of alternation in established paradigms, which pressure is roughly speaking the greater the larger the paradigm and the more 'familiar' its pertinent members. Pertinence in morphophonological change is roughly dependent on likeness of morphological function and phonological constituency; similarly, pertinence in morphographemic alternation and change is dependent on morphological function and graphonomic constituency. (To influence analogical extensions, forms must have similar functions and be sounded or spelled similarly.)

Both chapters are well-argued and require no further clarification here. The following paragraphs are meant mainly to put them in context and to raise the issue of what I will call 'default analogies.'

As generally used in linguistic dicussions an 'analogy' is a veiled implication of approximately this form: 'If two similar linguistic items "A_1" and "A_2" are concategorial (of the same grammatical category), and are respectively so related to two other concategorial items "B_1" and "B_2" as to designate "B_1" and "B_2" as derivates, then "B_2" shall resemble "A_2" in the same manner [via the same rule(s)] as "B_1" resembles "A_1." For example, if two phonologically-similar verbs ('ride' and 'dive' for instance) are to generate derived forms for their past tenses and if 'ride' derives 'rode' then 'dive' should derive 'dove.' (The foregoing is stated for simplicity's sake in terms of just two 'A' terms and the same number of corresponding 'B' terms, but is easily generalized.) So understood, an analogy underlies the extension of any paradigm. The analogy does not 'replace' ordinary generative rules, of course, it includes them, since otherwise *how* 'B_2' derives from 'B_1' would remain unspecified: we the readers would 'know' but the grammar, imagined as an automaton, would not. (For instance, the rule that derives 'rode' from 'ride' is not 'Preserve the number of phonemes but pick one and change it'; the new past tense of 'dive' is 'dove,' not 'dife'.)   What must be included in the analogy at issue is a morphophonological rule for changing [ay] to [o] in a context generalized from 'ride,' 'drive,' and so on. But once such a rule is included, the paradigmatic extension that innovates 'dove' is seen to be in fact an extension of the morphophonological rule; the role of the analogy is that of 'motivating' the extension of that rule by expressing the perception of similarity that provokes the extension. (The rule derives 'rode' from 'ride' and is then extended to derive 'dove' from 'dive'; the analogy captures the pertinent similarity between 'ride' and 'dive'—ideally in a way that puts e.g. 'arrive' beyond the pale of that similarity—and so subjects 'dive' to the rule. Notice, by the way, that 'arrove' can't be excluded on the grounds that English irregular verbs must be monosyllabic, as see 'beseech/besought.') In

a linguistic account that does not attempt to 'motivate' (provide a rationale for) the extensions of its rules, such analogies are superfluous.

Luelsdorff is interested in the 'motivation' of rules, hence, on his argument, in analogies.

It deserves mention at this point that there is one kind of derivation that in modern linguistic accounts is not ordinarily stated in terms of rules—though it certainly could be—and that one might therefore, for that factitious reason, take to be a case of pure analogy as such. These 'derivations' are the kind acquired by people learning a related foreign language rich in cognates. In learning French for instance most English-speaking students acquire the generalization 'English nouns ending in <ity> have as their French counterparts nouns ending in <ité>.' This is a good heuristic; it is certainly in the nature of an analogy (notice that the derivation of the French noun, for instance <audacité>, demands the contextual presence of the English one, <audacity>); and though it is easy to state this as a conventional rewrite rule ordinary linguistic practice offers nowhere to put it, for certainly it does not belong in the grammars of either English or French. It is assuredly tempting to think that the bilingual derivation of <audacité> on the analogy of <audacity> is not very different from the monolingual derivation of 'dove' on analogy of 'rode' or the like, and argues for 'pure analogy.' But this apparent case of 'pure analogy' is in fact pure illusion, one founded not on the absence of extensible rules but on the absence of a good place to put them. (Where they go is into a 'Grammar of English <—> French.')

One final point: in respect to producing the correct French counterpart, <audacity> is what teachers of French sometimes call a 'faux ami': it analogizes <audacité>, which is nonexistent in French; the correct form is <audace>. We refer again to 'faux amis' below.

Just above we illustrated so-called 'analogic change' with the hackneyed example: 'dive/dived/[have] dived' has changed in American English to 'dive/dove/dived or diven.' In like manner 'thrive/thrived/thrived' has changed in British English to 'thrive/throve/thriven.' Both extensions—the former abominated by the British, the latter found odious by Americans—are on seeming 'analogy' with paradigmatic alternations like 'drive/drove/driven' and 'ride/rode/ridden.' The main reason for believing that these extensions are due to analogy—to speakers' tacit inferences that 'dive must be to its Past as [the similar] drive is to drove'—is the notion that such changes appear to be highly restricted events heavily dependent on phonological similarity or even rhyme. 'Dived' became 'dove' but 'sneezed' didn't become *'snoze.' The argument phases naturally into the appealing observation that any account of English that failed to 'rationalize' 'dived—>dove' by making such a change less surprising than *'sneezed—>snoze' or (in spades) *'flew—>floze'

would be less than adequate. Granted: but where should such information reside? In the grammar itself, or in some adjunct? And how should such information be couched? Clearly part of what is involved is the relative frequency or rather familiarity of the 'drive/drove' part of the analogy—its base, shall we say— and how and where can such information best be lodged? Certainly no traditional generative grammar has an obvious place to enter such information as bears on such questions; morphophonemic paradigms state only what alternations are allowed, not how many verbs (in the case at hand) they apply to, still less how familiar or how salient those verbs might be. (George Lakoff devoted his dissertation to the problem of how to deal with 'irregularity' or 'exceptionality' in syntax [1965/1970, passim], but issues such as saliency of analogic models and probability of paradigmatic extension have mostly lain doggo.) Whence the appeal of Luelsdorff's claim that this kind of information is best embodied in a rather different kind of grammar, one that incorporates analogies. And indeed the obvious alternative, stating generative morphological rules in such a way as to specify their strength (number of clients), hence presumably their pressure to absorb similar forms thitherto lying beyond their reach, remains unexplored, to my knowledge. And what has just been said for morphophonemic alternations and extensions holds in spades for those of morphographemics, since (notoriously) the graphonomics of language is only nowadays being seriously examined at all.

A grammar should contain the seeds of its own destruction. The complete linguistic account of a language must incorporate what its speakers 'have in their heads' that enables them to learn, store, and use it; and since language can change only as a product of what speakers 'have in their heads' clearly the potential for 'evolutionary' change should be part of any adequate description. Such change need not be a direct product of the grammar as such—it might spring instead from a sense of the language that is better accommodated in some sort of associated device or 'adjunct'—but on the other hand if this sort of tacit knowledge is easily accommodated by the grammar itself the usual argument from parsimony would militate against adscititious 'adjuncts.' To which it can perhaps be added that notions that what can accommodated in the grammar is to be judged by what can be stated in some prevailing formalism, are nowadays less respectable than they were at one time. There is now a multiplicity of competing formalisms available; if adding 'analogic' to the armamentarium were efficacious in some way or other, and could be done parsimoniously, few would argue that it was to be discountenanced simply because it demanded an extension or even replacement of the favored current formalism. We cannot know more than this about 'analogy' until we have

examined its claims, which is where, at the present moment, Luelsdorff enters the picture.

There are changes that are a little harder to provide plausible accounts for than 'dived—>dove.' To take one example, the in-progress American change of 'sneak/sneaked/sneaked' to 'sneak/snuck/snuck' does not have any rhyming models to echo—the closest are 'dig/dug/dug' and 'stick/stuck/stuck'—and in fact it appears to sidestep the obvious model 'speak/spoke/spoken.' Why not the euphonious *'sneak/snoke/snoken'? Or, on the analogy of 'seek/sought/sought' and 'wreak/wrought/wrought,' the still more melodious *'sneak/snought/snought'? Or, going a little further afield, why not *'sneak/snake/sneaken' on the analogy of 'eat/ate/eaten'; or from 'sleep/slept/slept' why not *'sneak/snecked/snecked' or from 'see/saw/seen', why not *'sneak/snawk/sneaken'? To take up another but related point, why is it 'sneak' that gets remodeled in this fashion rather than some other common verb such as 'peek' (*'peek/pought/pought')?

Nor is 'snuck' alone; the process that substitutes a neologism in [∧] for the approved past tense is mildly productive. Thus one hears, in the vernacular or in jocular vernacularization, 'squuz' as the past tense of 'squeeze,' 'thunk' as the past tense of 'think,' and (quite commonly) 'brung' as the past tense of 'bring.' All of these are perhaps by 'analogy' from 'wring/wrung/wrung' and the like (and the rarer 'brang,' from 'ring/rang/rung' and the like): but why? And why are verb-stems (infinitives) containing [i]—as is true of all those just cited—unusually prone to neologizing a past in [∧]? Is it perhaps that these verbs have an unusually diverse hence confusing set of what I referred to earlier as 'faux amis'—'bring, wring, ring, see, speak, eat, sleep'—all assonating—as against the farraginous ablauts 'brought, wrung, rang, saw, spoke, ate, slept'—and that this plethora of possible models induces uncertainty and thus promotes creation of a new form in [∧] as a sort of default analogy? (A 'default analogy' might be likelier when a multiplicity of possible analogies is available—a number of common rhyming or at least assonating verb-stems, say, offering a bewildering variety of possible derivates.)

'Dove' is now 'correct' American English; 'snuck' is vernacular; 'brung' is said only by the ignorant (or rather, by the grammatically disadvantaged). 'Snuck' and 'brung' can both be used for humorous effect, as indicating a sort of antic rusticity, but the latter, as less accepted, is funnier than the former. Both 'brung' and 'snuck,' on the analogy of 'wring/wrung' and the like, are plausibly the product of a default analogy invoked to escape *faux amis.* But there are a couple of other vernacular coinages in [∧] that elude such easy characterizations: I mean 'slud' as the past of 'slide' and 'drug' as the past of 'drag,' neither of which seem obviously the product of any analogy at all,

conventional or 'default.' But whether or not this indicates that a popular 'default analogy' might in time produce a still more general 'default conjugation' is a question that must await another time.[8]

In sum, a 'grammar of analogies' capable of rationalizing extensible morphophonological alternation is not so simple as one might have hoped; and any expectation that 'analogical extension' might operate in this domain as it perhaps operates to extend sets like 'gleam, glitter, glow' and so on (Bloomfield 1895) becomes correspondingly guarded. Nor have we more than mentioned the problem of arranging for the specification of both 'sneak/sneaked/sneaked' and 'squeak/squeaked/squeaked' so as to predict or retrodict that 'sneak/snuck/snuck' is a likelier extension than 'squeak/squuck/squuck.'

W. C. Watt of the University of California, Irvine, presents in his chapter a revised version of a central part of his bimodal grammar of the English majuscules, and discusses this in terms of facility, homogeneity, and 'contingent universals.' His basic thrust is to show that the visual and motoric aspects of written language—respectively expressed in his 'phanemic' and 'kinemic' rules—are rather complexly interwoven. Thus, for instance, whether a given version of the alphabet is curvilinear (as for English) or rectilinear (as in the runic and various archaic versions) depends on the materials in use when the forms were determined: paper and many other materials are such as to tend to turn the angles < and > into curves, whereas wood and like surfaces are such as to tend to turn the curves (and) into angles. The choice between curvilinearity and rectilinearity—a choice that basically applies to each entire alphabet—is fundamentally a kinemic matter. This choice has visual or phanemic consequences, since < and (are visually distinguished; that alphabets tend strongly not to have both curvilinear and rectilinear variants of the same letter—not both <C> and <<> for example—must in the main be due not to any difficulty of distinguishing them visually but to the difficulty of executing two such forms, on whatever medium, so as to keep them distinct. Implication: in the generation of each alphabet the kinemic choice between curves and angles ought to precede the phanemic specification of the letters' visual aspects. (Each letter must be specified both as a phanemic entity and, to some extent independently, as a kinemic one. A curve such as) or an angle such as > is both kinemic [it must be drawn] and phanemic [it must be visible]; but some aspects of the alphabet are purely kinemic, others purely phanemic. For example the *direction* in which) is drawn—as standardly made, clockwise in <P>, counterclockwise in <O>—is purely a kinemic matter; while the fact that <C> is *symmetrical* on the horizontal axis is a purely phanemic one.) On the other hand, while phanemic curvilinearity is determined by kinemic considerations and so ought

logically to follow their resolution, at the same time the basic *raison d'être* of any writing system is to be read, that is, is visual; we execute certain strokes-on-the-page not for the exercise but to realize an <A>. So the determining characterization of any given letter, prior to the actuation of kinemic factors, is logically the visual or phanemic one. But if phanemic choices precede kinemic ones in some instances but follow them in others, the generation of each letter must proceed cyclically, accessing first the phanemic rules, then the kinemic, then returning to 'START' and accessing both sets of rules again. The need for such an organization, and its consequences, form the heart of Watt's paper.

The kinemic letter-characterizations generated by Watt's grammar are in intent precisely the 'abstract motor programs' now generally held to underlie the motor-commands that actuate the hands to execute the letters (Watt 1979*a*, 1980; van Galen and Teulings 1983: 10; Thomassen, Tibosch, and Maarse 1989:227). However, 'abstract motor programs' [AMP] considered as generated by a kinemic grammar are not quite identical to the same AMP considered as simply forming a set of twenty-six such. The difference is this: the grammar captures the relevant *generalizations* over the set of abstract motor programs, their homeogeneities, thus capturing what it is that makes them a set instead of a congeries. In addition, the 'natural extension set' of pseudo-letters is also generated, as a bonus, making the over-all description available for experimental test, as see Jameson's chapter discussed just below. What the grammar does is motivate the inclusion of certain programs (namely, the twenty-six and their natural extensions) rather than the many other programs imaginable: it has explanatory power. The reader resistant to the distinction in question might ask what it is that precludes a set of twenty-six AMP from consisting, say, of twelve programs drawing the letters <A> through <L> in the usual way; six programs drawing letters <M> through <R> from bottom to top; and letters <S> through <Z> drawn from the middle outwards. In fine, the abstract motor programs and the generative kinemic grammar form a natural union.

As one final note, a grammar such as Watt's in no way implies that someone wishing to retrieve a letter-shape or letter-program must do so by using the generative grammar as a production device, or by fishing through the grammar's products feature by feature, say, or stroke by stroke. This would be silly, in view of the well-founded conclusion that letters are retrieved from memory as wholes (Teulings, Thomassen, and van Galen 1983: 165). Nor does such a grammar imply that letters are *recognized* feature by feature or line-segment by line-segment. It implies only that *some* cognitive activities will actuate such a grammar. For example, the grammar seems to have captured the cognitive judgments that underlie subjects' rank-ordering of

letter-candidates by wellformedness criteria (as see Jameson's chapter); and the cognitive generalizations that explain certain beginners' errors; and, presumably partly as a consequence of the two cognitive activities first-mentioned, the cognitive errors that constitute, uncorrected, a prime cause of historical change.

Kimberly Jameson, also of the University of California, Irvine, shows how a particular generative grammar of the alphabet can be put to experimental test in order to estimate the extent to which the grammar corresponds to the cognitive model that users of that writing system must 'have in their heads.' Her basic grammar is a predecessor of the one that Watt writes of in the preceding chapter, but the two are quite similar, which comment holds also for the second version she uses for a second series of experiments. These experiments were designed to test whether or not Watt's grammars' tacit assignments of relative 'wellformedness' rankings to a variety of letter-like figures agree with the judgments of relative acceptability made by experimental subjects. To the extent that the two wellformedness rankings agree, Watt's grammars can be claimed to reflect cognitive reality. Her subjects fell into two classes: naive undergraduates, who 'know' the alphabet from having learned and used it; and 'experts,' who 'know' the alphabet both naively and from the vantage of being actively involved in designing new fonts and the like. Both sets of subjects made intuitive wellformedness judgments of the form, 'Of these two letter-like forms, the nth is the more acceptable as a new addition to the present alphabet'; both sets of these judgments accorded well with how wellformed the grammar found them, with the 'experts' performing somewhat better than the undergraduates. In sum, then, the alphabetic grammars she tested were shown to 'predict' the behavior of her subjects, hence on the usual reasoning to correlate with the cognitive grammars that people, in order to perform as they do, must in some sense have in their heads. She also discusses, quite enterprisingly, the extent to which the sum of individual cognitive grammars can be viewed as a common group-grammar; and she does this in respect to the importation into psychological work of some quite fascinating results from the field of cognitive anthropology (Jameson and Romney 1990).

It bears mention that, like many experimental designs, the one just sketched entailed some simplifying assumptions. (These do not affect the validity of her findings.) In particular, Jameson dealt in all cases with a single rank-ordering of her letter-like figures, whether those derived from the grammars or those obtained from analyzing her subjects' judgments; and there is good reason to believe that the well-known 'predilection for single orderings' (De Soto 1961) may have merged into one dimension a complex of judgments ultimately to be considered as involving several. For one thing,

Watt's grammars clearly separate two kinds of generative rules, the kinemic and the phanemic, the former specifying how letters are executed, the latter specifying how they look. Subjects using kinemic principles ('To what extent is this figure drawable in a manner resembling that in which the existent letters are drawn?') can be expected to judge the wellformedness of submitted letter-candidates in such a way as to derive a rank-ordering in some degree differing from the one implied by subjects using phanemic principles ('To what extent does this figure's *appearance* resemble that of the existent letters?'). Backwards figures, for instance—<Я, ⌐ > and kindred forms—are in my own experience sometimes accepted on a purely visual basis, then rejected when the subject has to draw them. They *look* letter-like, but they don't *feel* letter-like. For another thing, like any grammar, Watt's grammars of the alphabetic letters stigmatize new figures as 'less letter-like' on a variety of criteria: how many of the grammar's rules must be suspended to generate the figure; how 'high' in the generative path those rules are; and so on. Here again we may be presented with more than one dimension along which figures may fall away from full letterlikeness, hence be judged 'less letter-like.' And for a third thing, like any conventional generative grammar Watt's alphabetic grammars do not specify the comparative *generality* of their rules (how many letter-like figures are subject to them); and it seems plausible that the broader a rule's scope the more stigmatized is any figure that breaks it.

To retain our interest, any generative grammar must lay claim to having some bearing on cognitive reality, to representing in some fashion the device that people have in their heads. Assuredly, Jameson's demonstration that people behave *as if* their tacit 'knowledge' of the alphabet jibes with Watt's grammar will be pleasing to those who have given that grammar credence. On the other hand, as Jameson herself observes, much further work lies ahead before her findings can be regarded as decisive. Other experimental paradigms will have to be brought into play, since it is notorious that the findings of one experimental paradigm do not invariably agree with those of another, for the same object of inquiry. In testing phonological theories, for instance, it was once found that which of two theories more accurately predicted subject response depended on what task the subjects performed: if intrusion errors were criterial then Morris Halle's solution was better than Wayne Wickelgren's, but if judgments of inter-sound similarities were criterial it was Wickelgren's that was better (Singh 1976:115). It seems possible that people use different strategies, appealing to different sorts of alphabetic 'knowledge,' for different tasks; and predictable that people will use different strategies if modalities as different as the visual and the kinesthetic are involved (the latter is in fact well-demonstrated, as by Goodnow and by Millar [both 1971].) This holds true especially when one experimental paradigm

engages perceptual strategies and another engages cognitive strategies, as should be the case where the first presents letters under poor viewing conditions and records instances where subjects mistake one letter for another, and the second asks subjects to make conscious and to some extent deliberated judgments about letters' visual characteristics.   And indeed the available evidence confirms this view: the perceptual 'confusion matrices' of Townsend et al. (e.g., 1971) do not always assign the same 'letter-similarity' rankings to letter-pairs as does Watt's grammar, which is based on putative cognitive evidence, or the same as do Jameson's cognitive experiments. So far so good: perceptual and cognitive performances need not converge on a single account of any psychological domain. The difficulty is that it is difficult to be certain that perceptual strategies have not intruded into what is essentially a cognitive task, skewing the results.   Jameson has shown commendable caution in stating her conclusions, and one looks forward to her future work in the area.

As I mentioned above in roaming appreciatively among McCawley's points interrelating close-quotes and other punctuation-marks, sometimes considerations of 'graphonomic esthetics' seem to have a bearing on graphotactic and other graphonomic conventions, a remark of limited merit since, as perhaps with most esthetic issues, these considerations are hard to get a handle on.   Certainly, in any discussion of graphonomics and graphonomic evolution, esthetics is always knocking at the door. Edmund S. Meltzer, the well-known Egyptologist who as of his writing was at the Claremont Graduate School, in his contribution to this volume discusses the fact—simple on the surface, complex on any closer examination—that Egyptian Hieratic is by some found ugly; by others, pretty. He finds it pretty. Who could disagree, or fail to concur in the usual judgment that 'brushy' writing-systems like Arabic, Chinese, Japanese, or so-called 'Hebrew', are 'pretty' in a sense that our more telluric Greco-Roman system is not? And who could fail to retort that the Greco-Roman system has an esthetic of its own, one having to do with symmetry and architectural solidity instead of the 'expressionistic' impulses of the systems that owe their forms to the brush? It may seem odd, in a volume mostly devoted to cognitive and other psychological dimensions of writing, to raise such an issue: but isn't esthetics a cognitive issue too, in spades, even if one we cannot yet treat very exactly? In any event I have insisted on including one piece taking up the 'esthetic' dimension, despite the plain fact that at present we can only read it, perhaps intuitively agree with it, and wonder how to prove it.

The penultimate chapter in this section devoted to 'writing systems' as such is due to Denise Schmandt-Besserat of the University of Texas at Austin. At least half the excitement currently aroused by the subject of writing-systems in

the United States is owed to Schmandt-Besserat, who in a series of articles
has achieved the seemingly impossible: adopting and extending Amiet's thesis
concerning the origin of writing, she has made the subject of 'origins'
academically respectable and—still more astonishingly—popular; and she has
induced many students of writing-systems to take a renewed interest in the
seminal advancements due to the enigmatic people of Sumer. Here, she takes
up the subject of the early social implications of the availability of writing.
Such 'sociosemiotic' considerations as we might term them often bear on the
full understanding of such systems; but just as often we ignore them, or rather
put them to one side for the time being, because they seem relatively
intractable. For instance, it is clear that the development of such ancillary
writing-systems as the cattlebrands of the Western United States depend
critically on their use among cattlemen—how different from each other must
the various brands be, given the likelihood of their being found together in the
same corral? (Watt 1988)—but this is an issue as often postponed as raised.
([Watt 1988] is irrefragably a case in point.) How do the social requirements
of an advanced society such as that of Sumer condition the development of a
writing-system whose purpose is to sustain those requirements; how does the
availability of a means of making permanent records affect the society whose
records they are? Science and business, those seeming antitheses, depend
equally on writing, as do many forms of religion ('The People of the Book'),
but in ways that have scarcely begun to be examined seriously.
Schmandt-Besserat has begun to examine them seriously, and for that we owe
her a debt.

The last paper in this section, by Roger D. Woodard of the University of
Southern California, subjects two scripts, the Mycenaean Syllabary (or
'Linear B') and its sister-script the Cypriot Syllabary, to an extremely
penetrating analysis in an effort to describe how they operated so as to
represent the syllables of ancient (pre-classical) Greek. This is a brilliant
paper. As he points out, no syllabary could be considered well-suited to write
Greek syllables, since Greek is rather rich in consonant-clusters (<CC>) and
in closed syllables (<VC(C)>), precisely what a syllabary is poor at
representing. (English lends itself to representation by a syllabary just as
poorly or even more so, and for the same reasons.) How, then, did the
scribes who adopted these writing-systems for so unsympathetic a language
overcome this natural drawback; how clever were they? (Or, how fine-tuned
were their intuitions?) For Mycenaean, Woodard posits a 'hierarchy of
orthographic strength;' for any cluster, both consonants will be fully
represented if and only if the 'orthographic strength' of the first is at least

equal to that of the second; otherwise, partially represented; where the pertinent cline of orthographic strength is:

stop > fricative > nasal > glide > liquid.

Furthermore, the same cline is posited to underlie the Cypriot spelling strategy, which has this form: In any consonant-cluster if the first is at least equal to the second it will be written with the CV symbol whose V is identical to the V following the cluster; otherwise it will be written with the CV symbol whose V is identical to that V preceding the cluster. Woodard lists the few exceptions to his rule; but those apart it seems to be a quite robust generalization, and one of a peculiarly subtle character. Woodard then shows the relation between his cline of orthographic strength and the cline of sonority, postulated by Ferdinand de Saussure among others, which ranks consonants by increasing sonority; the two clines are identical except for interchanging the last two elements. He concludes by arguing that, in Mycenaean and Cypriot Greek, the sonority hierarchy in fact matches the orthographic hierarchy he posited to explain how the two syllabaries work. Thus the orthographic cline is, for those languages, the sonority cline. From this he infers that both syllabaries descend from a common source and that the existence of such a source is evidence that, at some time about the middle of the second millennium B.C., there lived a scribe/linguist who 'was capable of bringing significant analytic abilities to bear on the analysis of language.' Since it is now established that a similar genius organized the Ugaritic or Canaanite alphabet so as to reflect similar insights, at about the same time (Watt 1989a), it must now be a matter of lively curiosity whether the two ancient linguistic traditions—both preceding Pāṇini and the Sanskrit phonologists by about a millennium—were in any way related. But in any case Woodard's chapter throws the ancient Greek world into a rather different light, and offers independent evidence for the thesis that phonology was among the earliest of human sciences.

In all, the papers of this section raise a set of related points, most of them related directly or indirectly to cognitive issues: (1) What are the conventions governing writing-systems and their uses; (2) How did they get that way?; (3) What do they imply about the human cognitive capacity and its 'linguistic' components?; (4) How can we know how 'cognitive' a given model is?; (5) How 'common' a possession is the cognitive model of the alphabet?; (6) How 'esthetic' are our judgments and actions respecting graphonomic systems?; (7) To what extent is our society dependent on the passage via written records of the knowledge of one generation to the next; and (8) how old is linguistics and how likely is a clever scribe to be a clever linguist? Finally, at their most general level, these chapters throw new light on any adage to the effect that civilization and writing, and writing and linguistics, are mutually dependent.

NOTES

[1]The first coherent modern argument to this effect was due to Noam Chomsky and Morris Halle (1968); R. L. Venezky's book (1970) is another well-known example.

[2]This 'law' has many exceptions, however (<paradigm>; <coign>), unlike its counterpart for initial clusters ('pronounce only the second'—<pneumatic>; <chthonic>). Note that, as the last example shows, such rules were better stated in terms of orthographs, to include <ch>, <th>, and so on.

[3]The conventions of hieroglyphic Egyptian were such that the genitive, which in spoken Egyptian followed the word(s) denoting the thing possessed (as in French: 'le nez du roi'), was also so sequenced in written Egyptian *unless* the possessor in question was royal, in which case it preceded, requiring that the written word-order be read in reverse (Vernus 1990: 369).

[4]Derrida (1967, *passim*), but the reader interested in the matter would save time by repairing to John M. Ellis' blistering discussion (1989: 18-37).

Actually, all that Derrida seems to mean by claiming 'primacy' for writing is that writing (like any other means of recording speech) liberates a text's content from the likelihood of clarification by its author—especially when he's had the good grace to die—making it a sort of Autonomous Semantic Machine, hence open to rather 'free' (or 'readerly') interpretations, thus privileging its interpreters (Derrida not least). In short, the interpreter/critic is freed to find messages other than those meant, even messages where none were intended. It would therefore be quite unfair to characterize Deconstruction, as might otherwise be tempting, as consisting in the main of the rehash by second-rate minds of third-rate ideas in fourth-rate prose; the 'primacy of writing' is a *first*-rate idea. It's a somewhat self-defeating one, though, since it puts Deconstruction on a par with Astrology.

It bears noting that written French is sometimes *plus clair* than the spoken language: 'Tout chien obéit au flic' and 'Tout chien obéit aux flics,' for instance, are univocal as written, ambiguous as spoken. The humor of many French puns (e.g, <LHOOQ>) depends on this fact.

[5]I've argued elsewhere (Watt 1979*b*) that 'evolution" is not the fittest term for *cognitively*-conditioned changes, since (in a much profounder sense than is generally bandied about) these are Lamarckian or teleological: alphabets, for example, tend if free to change to become more homogeneous, to a point, a process similar to Lamarck's fancied tendency of biota to gravitate towards 'perfection.' There is nothing mysterious about this, it's just that the mental or rather neural stage on which cognitive systems 'evolve' affects that 'evolution' by cognitively structuring it, rather than, like the physical stage on which biological evolution is acted, merely culling out misfits (Watt 1989*b*). For those sensitive to etymology the better term for Lamarckian historical change would be not 'evolution' but 'advolution' (Watt 1979*b*). Nevertheless in what follows I will cater to popular prejudice in using the term 'evolution[ary]' to cover all cases of 'systematic change,' whether Darwinian (one damn thing after another) or Lamarckian (thus we head for the Absolute).

[6]The Roman Emperor Claudius I promoted the coined letters < ⅃ > for [w], <Ɔ> for [ps], and <⊢> for [u] (Diringer 1968: 421). These innovations rubbed Romans the wrong way, despite their exalted provenance, and died when Claudius I did (at the hands of his wife). Sequoyah suffered the destiny typically meted out to prophets, that of having his accomplishments often rejected while alive, accepted without question or [needed] revision once dead.

[7]For early treatments see Gibson (1929) and Fehrer (1935). Basing his conclusion on extensive experimentation, Gibson wrote: 'A reproduction of one figure is frequently changed in the direction of another figure if the two figures have been previously associated in consciousness (1929: 39).

Also (1929: 37): 'A figure which had been apprehended as similar to an object often, but not invariably, was reproduced more and more like the object as time went on.' Another Gestaltist term for 'homogenization' or 'assimilation' was 'levelling' (i.e., reduction of inter-element differences), which was sometimes contrasted with 'sharpening' (exaggeration of inter-element differences, i.e., what I have called 'heterogenization.') Writing more than two decades after Gibson, and as if in a different era, Bruner, Busiek, and Minturn said: 'Any given category of objects may be conceived to have...a "typical instance." When stimulus input is inadequate...the resulting percept will be assimilated toward the typical instance.... The same process may operate in memory: assimilating being a systematic change in the "memory image" in the direction of typicality. Any reduction in stimulus determination, through reduced stimulus adequacy or through an alteration of forces of organization in a memory trace, will bring about this effect' (1952: 155). How a visually-perceived object is described linguistically may also affect how it is misrecalled (Rock 1973: 119-121), but this factor would seem to play but a minor role in the misrecollection of one majuscule as another, and a still more minor role in the misrecollection that results in a letter's reversal.

The assimilation of visual forms toward their common denominator (or 'prototype') may be compared to the 'analogical' gravitation of some English irregular verbs toward each other or toward a putative 'default conjugation.'

8It also bears mention that a number of English irregular verbs have two canonical conjugations: for instance, next to 'smell/smelled/smelled' there is 'smell/smelt/smelt'; next to 'dream/dreampt/dreampt' there is 'dream/dreamed/dreamed'; and so on. The verbs rhyming with 'smell' seem to be in a special state of flux: for myself and most Americans 'smell' and 'spell' have regularized but 'sell' and 'tell' remain irregular; and, perhaps relatedly, 'kneel' has now regularized for many in the States (though not for me, nor 'deal' nor 'feel' for anyone literate). 'Show' appears for many speakers to have two participles, 'shown' when following the auxiliary directly but 'showed' when an adverb intervenes ('I had shown him the book'; 'I had already showed him the book'); where all educated Americans say 'ate' as the past of 'eat' and view 'et' as a rusticism, for educated Britons 'eat,' pronounced as if spelled <et>, is the only accepted past form; and of course the British say 'got' and 'forgot' where Americans say 'gotten' and 'forgotten' (and Partridge gives 'thunk' as Yorkshire dialect [1990]). The verb 'speed' is curious in that it is conjugated differently depending on whether or not it is followed by the particle 'up': 'speed/sped/sped' but 'speeded up/speeded up/speeded up.' There are a couple of cases where two homophonous stems lead to different derivations. One of these we have already noticed: 'wring/ring'; a far less common one is 'cleave,' which in the sense of 'divide' derives 'clove' and 'cleft'—this verb survives mainly in the forms 'cleaver' and, of course, 'cleavage'—but which in the (antithetical) sense of 'adhere [to]' derives 'clave' or 'cleaved' and then 'cleaved' again. Beeves are 'hung,' people are 'hanged.' There are at least two English verbs that are conjugated differently in their literal and their metaphorical meanings: 'Molly *wove* the tapestry' but 'Leopold *weaved* down the street' and 'The shroud is all *sewn* up' but 'The case is all *sewed* up.' Where the literal and metaphorical usages converge, either conjugation is acceptable: 'The strands of his story were all sewed/sewn up.' Other possible sources of confusion, though like 'cleave' of such low frequency as presumably not to matter, are those verbs with two distinct participial forms, the second—such as '[new-] mown', '[clean-] shaven,' 'cloven [hoof],' 'sunken,' for some speakers 'sawn,' 'proven,' 'carven'—serving as the adjective. I mention also that mistakenly using a [ʌ] participle for the past is rife among the ignorant: 'it stunk'; 'Honey, I Shrunk the Kids' (a movie-title from the degenerate 90's). In short, anyone giving way to a 'default conjugation' as a way of resolving an *embarras de richesses* is in a way simply showing common Zen savvy.

It must also be mentioned that analogy-by-rhyme has at least one extraordinary exemplar, namely the usual plural 'cantaloupe,' unique to my knowledge among produce (*'I bought two parsnip'), which surely is based on the generous rhyme with 'antelope,' which in turn is so pluralized in conformity to the general rule (from venery): 'The names of game animals have plurals identical to their singulars.' (Thus 'bear' is pluralized as 'bear' when hunted, 'bears' when baited.) From which it

seems reasonable to infer that the stronger the rhyming model the more powerful its analogizing power: if two trisyllabic words rhyme on ultima, penult, and antepenult, the analogic suggestion is almost irresistible, it seems, even if the words denote things as semantically distant as game-animals and produce. Or does it count that both are comestibles?

REFERENCES

Bloomfield, M., 1895. 'On assimilation and adaptation in congeneric classes of words.' *American Journal of Philology* 16.409-434.

Bruner, J.S., Busiek, R.D., and Minturn, A.L., 1962. 'Assimilation in the immediate reproduction of visually perceived figures.' *Journal of Experimental Psychology* 44.151-155.

Chomsky, N., and Halle, M., 1968. *The Sound Pattern of English*. New York: Harper and Row.

Derrida, J., 1967. *De la grammatologie*. Paris: Editions de Minuit.

De Soto, C. B., 1961. 'The predilection for single orderings.' *Journal of Abnormal and Social Psychology* 62.16-23.

Diringer, D., 1968. *The Alphabet: A Key to the History of Mankind*. New York: Funk and Wagnalls.

Ellis, J. M., 1989. *Against Deconstruction*. Princeton: Princeton University Press.

Fehrer, E., 1935. 'An investigation of the learning of visually perceived forms.' *American Journal of Psychology* 47. 187-221.

Goodnow, J. J., 1971. 'Eye and hand: Differential memory and its effect on matching.' *Neuropsychologica* 9.89-95.

Gibson, J. J., 1929. 'The reproduction of visually perceived forms.' *Journal of Experimental Psychology* 12.1-39.

Jameson, K., and Romney, A. K., 1990. 'Consensus on semiotic models of alphabetic systems.' *Journal of Quantitative Anthropology* 2.289-303.

Lakoff, G., 1965/1970. *On the Nature of Syntactic Irregularity* (Report #NSF-16 to the National Science Foundation). Cambridge: The Computation Laboratory of Harvard University. [Doctoral Dissertation, Indiana University, 1965]. Also as *Irregularity in Syntax,* New York: Holt, Rinehart, and Winston, 1970.

Millar, S., 1971. 'Visual and haptic cue utilization by preschool children: The recognition of visual and haptic stimuli presented separately and together.' *Journal of Experimental Child Psychology* 12. 88-94.

Partridge, E., 1990. *Partridge's Concise Dictionary of Slang and Unconventional English* (P. Beale, ed.). New York: Macmillan.

Rock, I., 1973. *Orientation and Form*. New York: Academic Press.

Sampson, G., 1985. *Writing Systems: A Linguistic Introduction*. Stanford: Stanford University Press.

Singh, S., 1976. *Distinctive Features: Theory and Validation*. Baltimore: University Park Press.

Teulings, H. L., Thomassen, A. J. W. M., and van Galen, G. P., 1983. 'Preparation of partly precued handwriting movements: The size of movement units in handwriting.' *In* (Thomassen, Keuss, and van Galen 1983).

Thomassen, A.J.W.M., Tibosch, H. J. C. M., and Maarse, F. J., 1989. 'The effect of context on stroke direction and stroke order in handwriting.' *In* Plamondon, R., Suen, C. Y., and Simner, M. L. (eds.), *Computer Recognition and Human Production of Handwriting*. Singapore: World Scientific Publishing.

Thomassen, A.J.W.M., Keuss, P.J.G., and van Galen, G.P., ed., 1983. *Motor Aspects of Handwriting*. Vol. 54 of *Acta Psychologica*.

Townsend, J. T., 1971. 'Theoretical analysis of an alphabetic confusion matrix.' *Perception & Psychophysics* 9.40-50.

van Galen, G. P., and Teulings, H.L., 1983. 'The independent monitoring of form and scale factors in handwriting.' *In* (Thomassen, Keuss, and van Galen 1983).

Venezky, R. L., 1970. *The Structure of English Orthography*.

Vernus, P., 1990. Response to J. Drouin. *In* (Sirat, Irigoin, and Poulle 1990).

Watt, W. C., 1979a. 'Iconic equilibrium.' *Semiotica* 28.31-62.

Watt, W. C., 1979b. 'Against evolution.' *Linguistics and Philosophy* 14.83-88.

Watt, W. C., 1980. 'What is the proper characterization of the alphabet? II: Composition.' *Ars Semeiotica* 3.3-46.

Watt, W. C., 1983. 'Mode, modality, and iconic evolution.' In T. Borbé, ed., *Semiotics Unfolding (Approaches to Semiotics*, 68). Amsterdam: Mouton.

Watt, W. C. , 1988. 'Cattlebrands,' Review of *The Manual of Brands and Marks*, by M. Wolfenstine. *Semiotica* 55.295-301.

Watt, W. C., 1989*a*. 'The Ras Shamra Matrix.' *Semiotica* 74.61-108.

Watt, W. C., 1989*b*. 'Semiotic evolution as a vindication of Lamarck.' *In* I. Rauch and G. F. Carr (eds.), *The Semiotic Bridge*. Berlin: Mouton de Gruyter.

SOME THOUGHTS ON A HISTORICO-GENETIC THEORY
OF THE LETTERSHAPES OF OUR ALPHABET

It is the aim of the following remarks to clarify at least some of the theoretical concepts and criteria that have been proposed by W.C. Watt in some of his recent articles. It is to be hoped that our discussion could perhaps result in paving the way towards an overall framework for a historico-genetic theory of the lettershapes of our alphabet. To my knowledge no such framework with a reasonable degree of explanatory power has yet been sent forth from the quarters of palaeographers and epigraphers. These scholars have restricted their endeavors mostly to taxonomic and descriptive statements on a wide variety of morphological variants of lettershapes and their chronological succession. I do not, however, intend to minimize the results hitherto obtained by representatives of these venerable, time-honored disciplines. To the contrary: in the second part of a work in progress (Brekle, in preparation) I will have to rely heavily on a great number of epigraphical and palaeographical works whose high descriptive quality commands my full respect (e.g. Mallon 1952, Jeffery 1961, Tjäder 1974, McCarter 1975).

In his eminently important article (Watt 1983a: 1543-50), Watt defines the historico-genetic process of the development of our lettershapes as a kind of "iconic evolution". His definition runs as follows:

. . . the systematic change over time of such iconic systems as the upper-case alphabet, which whenever it has been free to do so has evolved in a non-random way, along the two distinct planes of its realization, urged by four distinct and to some extent competing forces. (Watt 1983a: 1543)

Under closer scrutiny Watt's definition of "iconic evolution" may seem problematic from various aspects. Firstly, he presupposes the historico-morphological development of lettershapes to be a systematic process. This would mean that there should be an empirically well-founded and well-defined set of criteria and factors that would enable us to describe and explain morphological changes completely and consistently at any moment of the overall development of the lettershapes of our alphabet. The first difficulty arises with Watt's general operative restriction on the whole process: "... whenever it has been free to do so...". Either Watt leaves room for a dimension or a factor that is not covered by his four "forces" (see below), which would mean that his machinery of descriptive and explanatory concepts would be incomplete regarding the overall domain of iconic evolution, or Watt's four "forces" are meant to cover the domain completely (this seems to

129

be the most plausible reading). In the latter case, however, this general operative restriction would seem rather superfluous.

Let us now have a closer look at the four "forces" postulated by Watt in order to account theoretically for changes or non-changes of lettershapes at any given moment of their historical development. He distinguishes two major and two minor "forces":

1a. "homogenization, a force reminiscent of gravity in that it draws an alphabet's letters 'closer together', i.e. into greater similarity";

1b. "facilitation, a force which tends to reduce the effort to produce the letters;"

2a. "heterogenization, or 'antigravity', a force tending to keep letters from becoming so similar as to be indistinguishable;" and

2b. "inertia, a force acting as a sort of 'frein vital' on all processes of change, impeding them." (1543).

The first question a cognitivistically oriented scientist will ask is: where and how are these four "forces" anchored in empirical reality? It is unfortunate that Watt does not give sufficiently clear-cut answers. Instead one might get the impression that he hypothesizes four entities as explanantia without always supplying the exact conditions for the use of these concepts. In other words: how do these "forces" map on properties of sets of writing and reading acts of human beings; what are the conditions for the execution of these acts and the morphological structure of the products of the writing acts all of which should be captured within a cognitivistic-topological theory? In addition such a theory should also take care of economical, technical, and social factors which if only marginally may be relevant for the execution of writing acts and their products and their quasi evolutionary changes. Another important question that should be answered within the framework envisaged here is the following: to what extent are these quasi-evolutionary processes of an intentional and/or conventional nature? (in the sense of Lewis 1969). This last point, the role of conventions, is a particularly critical one for the status of Watt's "inertia," whose explication as a sort of "frein vital" reminds one unfavorably of similar hypostasizations (e.g. Bergsons's "élan vital"). Obviously Watt (1983a: 1546) wants this impeding "force" to be understood as a conservative tendency which leaves the morphology of single elements of an alphabetical system unchanged within a certain span of time. He justifies the introduction of this "force" by the necessity for a counterforce to "facilitation" and by the assumption that "heterogenization" alone will not suffice to control "homogenization" on the phanemic level. Now, all this looks as if Watt imagines something like a parallelogram of forces, a model serving to describe and explain temporary equilibria within an alphabetical system. In my opinion the theoretical fruitfulness of such a simple model depends to a large extent on

the possiblity of giving enough empirical substance to its vectors and to their interaction. On this again depends the degree of explanatory adequacy that can be ascribed to the theory connected with such a model. We could imagine as a sort of minimal interpretation of Watt's "inertia" a state of an alphabet in which none of the others "forces" would be operative. It is then perhaps not only a terminological question to speak of an "impeding force". According to my conviction there is no necessity for defining "inertia" in a purely negative way. Instead, the effects of conventions established at some given time, or of weaker imitation preferences on the shape of letters, should not be underestimated. If within a community of writers and readers conventions about the prototypically valid shape of letters have been established,[1] then the threshold for deviating from a morphological status quo is set relatively high. To this extent Watt's classification of "inertia" as a "minor force" may seem dubious.

There is at least one outstanding example of phanemic-kinemic continuity pervading practically the whole developmental history of our alphabet: the hasta + coda principle.[2] According to recent results in the domain of cognitive psychology[3] this principle can be seen at work in the phanemic grammar of an alphabet as a "good" perception-economical program; the same holds true for the kinemic grammar of lettershape production once we conceive of the initial/final downstroke of a lettershape as a kinemic hasta. It seems obvious that from a quasi-evolutionary viewpoint this hasta + coda principle can be interpreted as a very successful topological pattern; in spite of some ups and downs in its quantitative distribution in different developmental stages of our alphabet[4] this principle can be accepted as a topological constant over relevant subsets of our alphabet from its early beginnings up to the present day.

Whether this continuity together with different factors influencing it positively or negatively can be captured in a descriptively and theoretically satisfactory way within Watt's parallelogram of forces, is at present still an open question.

Further on Watt assumes as operative domains of his four "forces" the mode of execution of script (governed by a "kinemic cognitive grammar") and the mode of reception (governed by a "phanemic cognitive grammar"). Quite remarkable is Watt's statement that "the forces at issue operate independently on the two grammars just postulated" (1983a 1544 f.). A few lines further on, however, he corrects this rash statement by stating that "in finer grain the two forces [facilitation and homogenization] are not altogether independent after all", instead he considers the results of the operation of these two "forces" as "interlinked". Now it is basically correct to assume that normally a change in the kinemic program of a lettershape brings along with it a change in

its phanemic appearance, but that is not necessary, and this may not be true at all times. Put differently, optimizing the act of writing will not necessarily lead to homogenizing effects in the phanemic mode of a letter. The following examples may elucidate this. A closer look at the historical developments of some upper-case lettershapes <D, E, F, H> into their respective lower-case equivalents <d, e, f, h> will reveal that a heterogenization process must have taken place. This process must have been at least partly due to economizing factors in the kinemic mode, moreover we have to state that the lower-case equivalents of <E, F, H> show a marked degree of dissimilarity among themselves, whereas the upper-case letters appear to be quite homogeneous.

Surprisingly enough Watt claims that his "facilitation force is obviously restricted to the executive mode" (1983a:1545). According to my understanding of writing and reading processes this claim is false. In the course of the historical development of our alphabet we most certainly encounter lettershapes whose ease of readability varied considerably. This may depend, for example, on whether there was a hasta + coda structure present or not. If lettershapes changed from a point of time t_1 to t_2 in such a way that they show such a structure at t_2 but not at t_1, then under the premise that hasta + coda structures are easier to process in the receptive mode[5] we may speak of a case of facilitation in the domain of reading. However, this case need not also be a case of homogenization; this latter case would only occur if, within a given state of the development of lettershapes, we have a tendency that is favorable to the realization of the hasta + coda principle.

As regards his "facilitation force", Watt is of the opinion that this "force" is used consciously by writers. This would mean that writers would have produced more economical lettershapes with full consciousness, perhaps even intentionally. In some cases this may have been so, but normally the principle of least effort will have been at work below the level of consciousness. That this is so may be seen today: we jot down notes more or less hastily and the limits of facilitation (or the maximum of writing speed) will be controlled by the contextually supported readability of such notes by their producer (of course, for another reader the limits will have to be set much lower).

From this point we may now try to arrive at a better understanding of Watt's "heterogenization force". In my opinion Watt is right in saying that "heterogenization is the counterforce [to homogenization] that reading imposes on writing" (1983a: 1546), i.e. heterogenization is determined by sufficient discriminability or the minimal/optimal readability of single letters or letter combinations. In addition, other relevant factors influencing the degree of heterogenization such as context and graphotactic regularities/irregularities on the level of morphemes, words, word-forms, or even phrases will have to be taken into consideration.[6]

By way of example I quote here the Latin word form *minimi* (imagine this to be written or printed in the black-letter textura of the 14th/15th century). What we have here is nothing other than 11 vertical strokes with their respective on- and off-sets in sequence (the i-dot was not yet used consistently!). Although such extreme cases were certainly decodable with the aid of morphosyntactic and contextual hints, we are nevertheless forced to classify such phenomena negatively according to the criterion of easy readability. This is so because the effortless immediate access to such word-forms (or their basic forms) in the internal lexicon of the reader is not guaranteed. What we have here in addition is a graphotactically or syntagmatically conditioned case of maximal homogenization of lettershapes *in sequentia*. An example of a paradigmatical clash (= homogenization) of lettershapes can be seen in the near-collapse of <b> and <d> in pre-Carolingian times (for details see Mallon 1952). The majuscule form <B> developed into <ß> and this form - by way of a structural transformation of the original hasta + coda structure - developed into cursive <ð>. Thus the clash with the uncial form <ð> was programmed. The solution was to fall back on the earlier form.[7]

This brief discussion of two examples may serve as hints at a possible empirical justification of Watt's "heterogenization force". In contradistinction to his "facilitation force" the factors conducive to heterogenization of lettershapes must have been altered by writers/readers on the basis of their negative discrimination experiences more or less consciously (at least in some cases). Different problems arise with Watt's "homogenization force," which he qualifies as operating "mostly on the cognitive phanemic grammar, altering the remembered form of letters (or, equivalently, the rules for determining the forms of letters)" (1983a: 1545). I am inclined to agree with Watt that we are more willing to ascribe homogeneity to visually perceptible lettershapes on the basis of their topologically definable similarities than to recognize similarities in the kinemic programs of lettershapes (similarities or identities in the vectorial and sequential structure of their "strokes"). However, this should not blind us vis-à-vis the reality of kinemically conditioned homogenization, more precisely as regards its relevance to far-reaching changes in lettershapes. I mention here only the considerable effects of the kinemic hasta + coda program in the development of the lapidary variety of the Greco-Roman Capitals; it can be shown that the kinemic programs of the majority of lettershapes have been dictated by the hasta + coda principle.

Watt himself mentions this point when discussing the kinemic program of the letter A; this program agrees with the kinemic properties of the majority of majuscules in that these start with a downstroke (= kinemic hasta) (see Watt 1983a: 1547 f.).

Among the factors that may trigger such changes as lead to greater homogeneity between the elements of the alphabet we may mention the following: 1. more economical writing programs i.e. Watt's "facilitation force" which he, however, considers as being independent from the "homogenization force"[8]; 2. factors residing in conventionalization processes, or 3. in conscious decisions taken by writers. The two last-mentioned factors might be responsible for one of the most far-reaching changes in the history of our alphabet, namely the process of vertical-axial symmetrization of a considerable number of lettershapes during the transitional period from the Old Greek to the Classical Greek alphabet.[9] With the exception of archaic Pi (Γ) which got its symmetrical form ∏ only later, this major change was completed by ca. -400. In my opinion it is still an open question from which criteria we could derive the preferences of the Old Greeks for this marked tendency towards greater vertical-axial symmetry and thus towards a new type of homogeneity among their lettershapes. Falling back on aesthetic criteria with whose development an entity called Greek genius is sometimes credited, will not make things much easier.

In his highly interesting and partly rather speculative article "Canons of Alphabetic Change" (Watt 1988b) Watt tries to connect his "homogenization force" with Darwinian or Neo-Darwinian principles. Although he recognizes eventually that such an attempt is beset with a number of problems, he nevertheless states boldly that "the tendency to homogenize letters has a particular goal: homogeneity" (1988b: 136). Does it make sense to say that a tendency has a goal? Even if we try to rephrase this by saying: some writers show a tendency to homogenize their lettershapes and this is also their intention, their goal, the situation will not be much better, simply because of the existence of a number of historico-empirical pieces of evidence that point in another direction.

Could we not instead say that homogeneity may also be a consequence of economically optimized writing programs? These programs may themselves be induced by a set of appropriate neuroanatomic conditions. The results of such processes would then be checked by better or worse phanemic discriminability of the respective lettershapes. Furthermore, it is not clear which purpose homogeneity *per se* could serve.

I see still other problems in Watt's proposal to explain homogenization within a subset of lettershapes in a given historical or individual developmental situation by relying on processes like "memory drift" or "misrecollection". Going into some detail: commonly accepted results of epigraphical research are not consonant with Watt's assumption that "homogeneities appear markedly at around 750 B.C., the period of the Archaic Greek alphabets" (1983a: 1547).[10] Among themselves the numerous local Old Greek alphabets

do not show a high degree of homogeneity; the situation changed drastically after a decree (-403) in which the Ionian alphabet was declared the standard form. Another argument against Watt's "misrecollection hypothesis" can be seen in the existence of wax tablets with engraved prototype alphabets (already attested in the 7th/6th centuries).[11]

Furthermore I find it difficult to accept Watt's correlation between archaic epochs of script development and the learning behavior of children at the age 5 to 6. That there is homogenization of lettershapes at this age is well attested[12] but it seems clear that this is due to a widespread cognitive strategy in early childhood, namely (over)generalization. One example of this can be seen in the (over)generalization of the abstract kinemic program: hasta + right coda (notorious cases are the letter pairs <b-d> and <p-q>). The conditions for this kinemic program to be applied are very simple: whenever the child encounters letters with a basic hasta + coda structure then he/she will add the coda-elements on the right-hand side of the hasta. This implies that the hasta + coda principle must have become internalized at a rather early stage of the writing learning process. If that is so, then children will not even try to remember the correct lettershape. Similar ideas can be found in Watt 1983b.[13] What is decisive here is that we should be able to arrive at well-founded decisions on which parameters in which order should be used to define the degree of systematicity/homogeneity of an alphabet. These parameters will have to be valid for the analysis of historical processes and for the description of historical states of the morphological and functional structure of a given semiotic system and its elements.

Among these parameters that are globally relevant for an alphabetic system we will certainly find the direction of writing (in our days dextrograde) and—Watt does not mention this explicitly—the vertical structure of the elements of an alphabet as it manifests itself in the two-line schema of the classical upper-case letter forms. This schema can be considered as being of level one because any lettershape must have an upper and lower limit.[14] Our present-day lower-case alphabet works within a four-line schema; the vertical extension of lower-case letters varies between the middle field <e, a, ...>, the middle+lower field ("descenders": <p, q, y> ..) and the middle + upper field ("ascenders": <d, f, h>..). Letters with a maximal vertical extension occur only as variants of Italic or running scripts (e.g. <f> or the old long <ſ>). The description of the development from the level-one schema to the three-level schema arrived at in Carolingian times needs special attention.

The following topologically definable and cognitivistically motivated parameters are claimed to be descriptively essential and explanatorily useful categories; their domains are on the one hand the letter-internal morphology and on the other hand—in the sense of overall criteria for evaluations of degrees

of systematicity and homogeneity–the various developmental stages of whole alphabets.

1. SYMMETRY - ASYMMETRY

Contrary to Watt (1988b: 130), I am of the opinion that horizontal-axial symmetry of lettershapes cannot be taken to be a historically or cognitively relevant property. Vertical-axial symmetry, however, is certainly the type of symmetry that is of great importance, both from the viewpoint of description and of explanation.[15]   At least as important is vertical-axial asymmetry, both for a systematic description and explanation of the historical development of the majuscule series and still much more so for the minuscule series of our alphabet. As I have shown above, asymmetry manifests itself most clearly in the phanemic hasta + códa structure of lettershapes.

2. VECTORIALITY

Intuitively it has been recognized for a long time that many letters seem to "look" in one direction; i.e. in the direction of writing; this intuition corresponds again nicely with the hasta + coda principle. Among the topologically possible types of vertical-axial asymmetry (compare e.g. the structure of <N >, <S > and <Z >) we can identify one type as the one which we have called hasta + coda structure. Its historical development and varying quantitative distribution pervade both the phanemic and kinemic grammars of our alphabet, moreover it can be shown that the prominence of this structure can also be supported by deep-rooted cognitive mechanisms. This vectorial structure can be seen in the great majority of the elements of our alphabet (with both majuscules and the minuscules); the ideal case is such that a vertical line segment (hasta) has one or two additions (coda(e)) in the direction of writing (deviations from this rule can be explained historically in a principled way). The correlation between letter-internal vectoriality and the direction of writing can be explained on technical grounds: from the early beginnings of our alphabet onwards we find many lettershapes that show a more or less vertical initial or final downstroke (= kinemic hasta) which regularly appears as an initial phanemic hasta. This holds true for the Phoenician-Greek-Roman line of development, but not e.g. for the Hebrew Quadrata.

3. RECTILINEARITY AND CURVILINEARITY

In order to arrive at a satisfactory detailed description of the morphological internal structure of letters we have to provide for the distribution of these relatively concrete, immediately visible qualities across the more abstract symmetries and vectorialities of letterstructures. A special case is the property

of rectangularity, understood as a relation that determines the connection of minimal strokes with each other.[16] How such a distribution can be analyzed and represented in a principled way has been admirably demonstrated by Watt in his contributions since 1975 both for the kinemic and for the phanemic modality of the majuscules of our alphabet. Whether, however, Watt's kinemic and phanemic rules and representations can be given a cognitive foundation in every respect, remains for the time being an open question.

4. VERTICAL STRUCTURING OF LETTERSHAPES

As already indicated above, special attention should be given to a very important step in the history of our alphabet, namely the development of so-called minuscules (transition from a two-line schema to a four-line schema). To my knowledge we do not yet possess an empirically and theoretically fully satisfactory account of this sequence of stages from Roman cursives to various *scripturae rusticae* to uncial and semi-uncial scripts up to the Carolingian minuscules.

It was the aim of my deliberations - mostly on the basis of Watt's interesting and sometimes provocative work - to show the fruitfulness of a historico-cognitive approach and to offer some proposals for the direction of future research; if possible, such studies should be carried out in a closer cooperation with epigraphers and paleographers.

NOTES

1I mention here the important role of standard alphabets, e.g. the famous Marsiliana tablet and later specimen of the same type.

2Watt 1983b: 394 et passim uses the equivalent terms *vexillum* and *augmentation*.

3There will be an extended discussion of this principle in Brekle (in preparation).

4Compare e.g. the diminishing force of this principle in the transitional period from archaic Greek letter forms to classical forms; it gave way to a remarkably strong tendency to favor vertical-axial symmetry (cf. Brekle 1987).

5Cf. e.g. Sinz 1978: 156 and Harcum 1964; equally relevant in this context are several articles by Kolers [1969, 1975, 1980, and 1983].

6See the detailed investigations of context effects on the perception of lettershapes by McClelland and Rumelhart (1981).

7Note that the development from <D> via the uncial <∂> to the Carolingian standard form <∂> presents another case of structural transformation of the original hasta + coda structure; in fact we have here a complete reversal of the topological roles of the constituents of the lettershape.

8Watt 1983a: 1547: ". . . facilitation and homogenization, the two strongest forces, are independent of each other . . ."

9See Brekle 1987 for a detailed discussion.

10See e.g. (McCarter 1975) where we can see from the plates alone that the differences between Phoenician and Archaic Greek alphabets of the 8th century are rather slight; moreover McCarter dates the earliest Greek inscriptions only in the last quarter of the 8th century. For detailed discussions on the age of the Greek alphabet see Pfohl 1968 and Wachter 1987.

11The best known and most often discussed example is the wax tablet of Marsiliana d'Albegna, stemming from Greece and found in Etruria. See also Wachter 1987 for other prototype alphabets.

12For writing ontogenesis in general see (Thomassen and Teulings 1983).

13" . . . alle Urteile über relative Systemhaftigkeit sind im Kern Urteile über relative Homogenität". (1983b: 375).

14The early historical development of our alphabet shows clearly that in Phoenician and Archaic Greek times we cannot speak of a two-line schema in the strict sense: some of the letter-shapes differed in their vertical extensions (e.g. Delta and Omicron often appeared markedly smaller than other letter-shapes); a strict two-line schema can be seen only in Classical Greek and Roman times.

15See (Brekle 1987) where the relevance of this type of symmetry is demonstrated for the transitional period from the Phoenician and Archaic Greek alphabet to the Classical Greek alphabet.

16Watt (1983b:376) sees in the development of old Greek Gamma Γ or Γ → classical Γ a case of rectilinearization, in fact it is a case of rectangularization; in the case of ▽ → Δ we should speak of equiangularization.

REFERENCES

Brekle, H.E., 1987. "Konventionsbasierte Kriterien der Buchstabenstruktur am Beispiel der Entwicklung der kanaanäisch-phönizischen zur altgriechischen Schrift". *Kodikas/Code Ars Semeiotica* 10:229-246.

-----(in preparation). Die Antiqualinie von -1500 bis +1500.

Harcum, E.R., 1964. "Effects of Symmetry on the Perception of Tachistoscopic Patterns". *The American Journal of Psychology* 77/1: 600-606.

Jeffery, L.H., 1961. *The Local Scripts of Archaic Greece*. Oxford: Clarendon Press.

Kolers, P.A. and Perkins, D.N., 1969. "Orientation of letters and their speed of recognition". *Perception & Psychophysics*. Vol 5 (5).

-----1975. "Spatial and Ordinal Components of Form Perception and Literacy". *Cognitive Psychology* 7/1975: 228-265.

Kolers, P. A., Palef, S.R., Stelmach, L.B., 1980. "Graphemic analysis underlying literacy". *Memory and Cognition* 8(4): 322-328.

Kolers, P.A., 1983. "Polarization of reading performance'. In Coulmas, F. and Ehlich, K. (eds.): *Writing in Focus*. Berlin: Mouton: 371-391.

Mallon, J., 1952. *Paléographie romaine*. Madrid (= Scripturae. Monumenta et Studia III, Consejo superior de investigaciones cientificas, Instituto Antonio de Nebrija de Filologia).

McCarter, P.K. Jr., 1975. *The Antiquity of the Greek Alphabet and the Early Phoenician Scripts*. Missoula, Montana: Scholars Press (= Harvard Semitic Monographs, 9).

McClelland, J.L. and Rumelhart, D.E., 1981. "An Interactive Activation Model of Context Effects in Letter Perception: Part I. An Account of Basic Findings". *Psychological Review* 88: 375-407.

Pfohl, G. (ed.), 1968. *Das Alphabet. Entstehung und Entwicklung der griechischen Schrift*. Darmstadt: Wissenschaftliche Buchgesellschaft.

Sinz, R., 1978. *Gehirn und Gedächtnis. Funktion und Leistung des menschlichen Gehirns*. Stuttgart, New York: Fischer.

Thomassen, A. and Teulings, H.-L., 1983. "The Development of Handwriting". Martlew, M. (ed.): *The Psychology of Written Language*. New York: John Wiley & Sons: 179-213.

Tjäder, J.-O., 1974. "Der Ursprung der Unzialschrift". *Basler Zeitschrift für Geschichte und Altertumskunde* 74: 9-40.

Wachter, R., 1987. *Altlateinische Inschriften. Sprachliche und epigraphische Untersuchungen zu den Dokumenten bis etwa 150 v. Chr*. Bern, Frankfurt a. M., New York, Paris: Lang.

Watt, W.C., 1983a. "Mode, Modality, and Iconic Evolution'. *Semiotics Unfolding*. Proceedings of the Second Congress of the International Association for Semiotic Studies, Vienna, July 1979. Vol. III (ed. T. Borbé). Berlin, New York, Amsterdam: Mouton, 1543-1550.

-----1983b. "Grade der Systemhaftigkeit. Zur Homogenität der Alphabetschrift". *Zeitschrift für Semiotik* 5:371-399.

-----1988a. "What is the Proper Characterization of the Alphabet? IV: Union". *Semiotica* 70: 199-241.

-----1988b. "Canons of Alphabetic Change". *The Alphabet and the Brain* (eds. de Kerckhove and Lumsden). Berlin, Heidelberg, New York, London, Paris, Tokyo: Springer. 122-152.