14 From Orthography to Phonology: An Attempt at an Old Interpretation

K. E. Patterson and J. Morton

INTRODUCTION

Deep dyslexic patients, when confronted with a printed non-word to pronounce, will generally choose to make no response. It is not that they cannot say anything (though of course they cannot say the "correct" thing), and if pressed they often will say something (a visually similar real word); but by preference their response will be "sorry, pass!"

The logogen model, when confronted with the question of how readers assign pronunciation to unfamiliar words or non-words, has generally chosen to make no response. It is not that the model could not say anything, and if pressed it did mumble something ("grapheme–phoneme conversion"); but by preference its response was "sorry, pass."

This disinclination to deal with a particular dimension of reading skill is not necessarily a failing. Every model, after all, has a differential emphasis or focus on components within its domain. The logogen model's primary focus (as regards reading) was on recognition of familiar written words by fluent readers; and precious little unambiguous evidence suggested that the procedures used to assemble a pronunciation for a non-word might be significantly implicated in skilled word recognition. If one were to criticise the logogen model for its hand-waving on the topic of non-word pronunciation, one might as well criticise it for ignoring the question of how visual input logogens are established in the process of reading acquisition. These are simply not questions to which the model specifically addressed itself.

But times moves on. Events of the past few years—both theoretical trends and empirical results—could be said to press any model of word recognition to say something about procedures for assembling pronunciation from print. This chapter represents the first stage of our reluctant compliance with such a demand, reluctant because satisfactory modelling must be based on satisfactory data and we are not convinced that this latter condition obtains at present.

There are many questions or facets in this general topic, but we shall limit ourselves to two: (1) What procedures are involved in the pronunciation of a written non-word like *pove*? (2) Are the same procedures involved (either obligatorily or optionally) in the pronunciation of a familiar written word like *love*? Note that we restrict our questions (and shall accordingly restrict the evidence to be considered) to the issue of pronunciation of words and non-words, including data on both the actual pronunciation given and the latency of the response. Like Henderson (Chapter 17 in this book), and for some of the same reasons, we do not deal here with data from the other major paradigm used in this area, namely lexical decision.

The "standard" hypothesis (e.g. Coltheart, 1978) in response to the first question is that, quite separate or at least separable from the routine whereby a person can "look up" or "address" the pronunciation of a known word, there is a *non-lexical* routine for assembling the pronunciation of any letter string, familiar or otherwise. By non-lexical, we mean that no reference would be made, at the time of assembling the phonological code, to correspondences between orthography and phonology in *specific known* words. To make this quite explicit (and perhaps to reveal straightaway the difficulty confronting such a hypothesis): suppose a person pronounces the non-word *pove*→/pʌv/ (to rhyme with *love*), as indeed occasionally happens (Glushko, 1979; Kay, 1982). The strong hypothesis of a non-lexical phonological routine must deny that such an event demonstrates consultation of the lexically stored pronunciation for *love*. All pronunciations must be capable of being generated by a set of subword-sized correspondences (however complex, context-sensitive, and/or vulnerable to influence by recent experience), without taking advice from lexical advocates.

Note that, in our view, a hypothesis of separable routines (any separable routines; in this case lexical and non-lexical phonology) does not require that the routines operate with complete independence in the normal system. Various aspects of this issue have been discussed by Henderson (1982), also briefly by Patterson (1981). From the point of view of a researcher interested only in the normal operation of cognitive processes, it might seem that, if two routines are not independent, then the question of their separability is without consequence or interest. We would, however, argue against this view. Even if two routines are not independent, variables may act on them independently; in this case, the interaction between these variables would be unintelligible if the two routines were treated as one. From a neuropsychological point of view, of course, the distinction is vital for other reasons. If two routines are separable, then even if they do not normally operate in complete isolation, it will be possible for only one of them to be impaired or lost as a result of neurological insult.

If a model includes a non-lexical routine for assembling phonology, then of course its procedures must ultimately be specified. In particular, it must be determined whether these procedures operate only on a single level such as translation of individual graphemes to phonemes (as in Coltheart, 1978) or on various levels including higher-order units (as in Parkin, 1983, 1984 or Shallice, Warrington, & McCarthy, 1983). The question of the *existence* of a non-lexical routine must, however, be kept separate from the specification of its procedures if it does exist. The two issues tend to be discussed together; as a result, data that may speak only to one of the issues are vulnerable to interpretation with regard to the other. In particular, if there is evidence that a system based solely on grapheme-to-phoneme translation will not suffice, this does not automatically invalidate the hypothesis of a separable non-lexical system.

The major alternative to a non-lexical routine for assembling phonology is, of course, the theory that pronunciations are assigned by analogy with and by specific reference to known lexical items (Baker & Smith, 1976; Baron, 1977; Glushko, 1979; Henderson, 1982; Kay & Marcel, 1981; Marcel, 1980). According to analogy theory, there are no abstract rules by which phonology can be assigned to orthographic segments (of whatever size); there are only orthographic and phonological representations of known words. As both kinds of representations are segmentable, phonology can be assembled for any novel combination of letters by: (1) finding words that contain the appropriate orthographic segments; (2) obtaining the phonology corresponding to those segments; and (3) cobbling together a pronunciation. Precisely the same procedure operates for a letter string that happens to be a real word; but in addition to information about pronunciation of its segments in *other* words, a real word of course also has its *own* whole-string orthographic and phonological specifications.

A non-lexical rule-governed routine and a lexical analogy-based routine do not, presumably, exhaust the theoretical possibilities, but these two are the major bids on the table at present. Needless to say, however, deciding between these alternatives (indeed, at some levels, even discriminating between them) is far from straightforward. As we have already indicated, the most serious current obstacle is not so much the rudimentary state of the theories as the inadequacy of the available data upon which the theories are to be based and evaluated. This complaint is not (or at least nor primarily) directed against the researchers who have collected these data. The degree of complexity of the system and the range of relevant variables are only gradually dawning on us all. Experimenters who have previously failed to consider all of this complexity are therefore scarcely to be condemned. Their *data*, on the other hand, if derived from designs now shown to be inadequate, do not escape this fate. Lest all of this sound arrogant, we hasten to add that even a full awareness of the crucial variables to be manipulated and controlled may not solve the problem. We echo the apt but depressing assessment of Cutler's (1981) title: "Making up materials is a confounded nuisance, or: Will we be able to run any psycholinguistic experiments at all in 1990?"

The confoundings in the existing data base seem sufficiently worrying to warrant description. Accordingly, before selecting a subset of data with which to tweak both non-lexical and lexical theories of assembled phonology, we shall digress to identify some of these problems.

In the 1970s, the only major variables that seemed germane to research on assembling phonology from print were the distinction between words and non-words and, for words, the distinction between regular and irregular (or "exceptional") spelling-to-sound correspondences. There was also the suggestion that a subject's strategy (for example, the extent to which he or she might use or rely on assembled phonology in processing words) could be altered by experimental manipulations like instructions (Stanovich & Bauer, 1978) or the nature of the non-words in a lexical decision task (Davelaar, Coltheart, Besner, & Jonasson, 1978). That, however, was all. In the mid-1980s, we are sadder but wiser, having discovered the following pertinent things:

1. Regularity of spelling-to-sound correspondence appears to be (or to demonstrate its influence as) a continuum rather than a dichotomy (Parkin, 1982; Shallice, Warrington, & McCarthy, 1983). Effects averaged over irregular words spanning the range of this continuum may therefore be misleading.
2. Regularity of spelling-to-sound correspondence is a separate dimension from regularity of spelling pattern: the words *pint* and *yacht* both have an exceptional relationship between orthography and pronunciation, but *pint* has a "normal" or regular spelling pattern whereas *yacht* is orthographically weird.[1] Orthographic irregularity exerts an influence on pronunciation latency over and above that of spelling-to-sound irregularity (Seidenberg, Waters, Barnes, & Tanenhaus, 1984). Experiments where the set of spelling-to-sound irregular words includes orthographically weird words (i.e. most experiments) may therefore be misleading.
3. Word frequency is a potent variable and, most crucially, appears to interact in its influence on pronunciation latency with other relevant

[1]As one of our patients remarked when asked to read the word *yacht*, "I know it must be 'yacht' because that is the only word where you get those three letters together," pointing to *cht*.

dimensions such as regularity of orthography or of spelling-to-sound correspondence. Indeed, Seidenberg et al. (1983) conclude that high-frequency words are insensitive to most or all of the other dimensions. Experiments averaging across a range of frequencies within a particular word class, even if frequency was appropriately matched between word classes (again, this will be the majority of experiments), may have to be discounted or reanalysed.

4. As is by now well known, pronunciation may be influenced not only by the word (or non-word's) own spelling-to-sound pattern but by the extent to which other similarly spelled words agree or conflict in pronunciation (Glushko, 1979). This "consistency" effect may be well known, but it is now suffering the same fate (indeed, perhaps an even more distressing version of it) as the regularity effect has done. In Glushko's analysis, consistency was a dichotomy: words and non-words were either consistent (e.g. *wane* or *fane*: all words ending in *ane* are pronounced /eɪn/), or inconsistent (e.g. *save* or *tave*: the word *have* is an unfriendly neighbour). It now seems that all sorts of patterns and degrees of consistency may influence performance (Henderson, Chapter 17, in this book; Kay, 1982; Parkin, 1983). We return to this point later in our chapter. For the moment, suffice it to say that each of the patterns listed in Table 14.1 probably requires separate status in an experimental analysis. Every experiment known to us combines at least several of these patterns into one supposedly homogeneous condition. A further complicating factor is that degree or pattern of consistency may affect words and non-words differently (Parkin, 1983).
5. Finally, the response to a particular word or non-word (its phonology or latency or both) may be influenced by a whole host of variables concerning the nature of other items in the list. This is all very well in (the minority of) studies designed specifically to examine such influences (e.g. the priming studies of Kay & Marcel, 1981, or of Rosson, 1983). In (the majority of) studies that seek only to assess performance on individual items, however, inter-item effects constitute confoundings that are often overlooked. Some of these are relatively simple effects (examples *a* and *b* below), whereas others involve complex interactions (example *c*).
   a. Performance on words (or non-words) in homogeneous lists of words (or non-words) differs from performance on comparable items in mixed lists of words and non-words (Andrews, 1982; Glushko, 1979).
   b. Significant consistency effects like that shown by Glushko may depend upon the (earlier) presence in the list of a conflicting neighbour (Seidenberg et al., 1983). Many experiments repeat spelling patterns with alternative pronunciations (e.g. *cost* and

TABLE 14.1
Fifteen Kinds of Letter String

Word Type	*Example*	*Characteristics*
Consistent	*gaze*	All words receive this same *regular* pronunciation of the body.
Consensus	*lint*	All words with one exception receive this same *regular* pronunciation of the body.
Heretic	*pint*	This word is the *irregularly* pronounced exception to the consensus.
Gang	*look*	All words with one exception receive this same *irregular* pronunciation of the body.
Hero	*spook*	This word is the *regularly* pronounced exception to the gang.
Gang without a hero	*cold*	All words receive this same *irregular* pronunciation of the body.
Ambiguous: conformist	*cove*	This is the regular pronunciation; there are many irregular exemplars for this body.
Ambiguous: independent	*love*	This is the (or a) irregular pronunciation; there are many regular exemplars for this body.
Hermit	*yacht*	No other word has this body.
Non-word Type	***Example***	***Characteristics***
Consistent	*taze*	Refer to word types above.
Consensus/heretic	*rint*	
Gang/hero	*pook*	
Gang without a hero	*vold*	
Ambiguous	*pove*	
Hermit	*nacht*	

Table 14.1 offers labels for various types of words and non-words reflecting both (1) the agreement (or otherwise) of pronunciation across the set of monosyllabic words sharing the same "body" (vowel plus terminal consonant), and (2) the regularity (or otherwise) of pronunciation with reference to GPC rules. Three of our labels are borrowed from other writers: "consistent" from Glushko (1979), and both "heretic" and "hermit" from Henderson (1982). The remainder, we think, are our own.

post) within one list (Andrews, 1982; Glushko, 1979; Parkin, 1983) and may therefore require caution in interpretation or (once again) reanalysis.

c. A significant effect of word frequency on pronunciation latency for *regular* words apparently obtains only when the list also contains words with an *irregular* spelling-to-sound correspondence (Norris, personal communication). Theoretical interpretation of this observation may have interesting implications. For the moment, this is just another example of the way in which effects are fickle, making generalisation across experiments a nightmare.

There is, in summary, a shortage of sound data available to anyone trying to model the process of assembling phonology from print; it is, however, a shortage rather than a famine, and we do choose to try. As implied by our title, this attempt will take the form of a modified "standard" model, where assembled phonology is produced by a non-lexical system. In the *unmodified* standard model (e.g. as proposed by Coltheart, 1978), the crucial aspects of the system are that it is non-lexical and rule-governed with rules defined only at the level of correspondences between graphemes and phonemes. The modifications that we propose are motivated by the following sets of observations, all of which appear to conflict with the unmodified version:

1. "Inconsistent" non-words are sometimes read aloud with an irregular pronunciation (e.g. *heaf*→/hef/ rather than /hif/). Averaging over a variety of non-word spelling patterns, Glushko (1979) obtained 9% of irregular pronunciations in his experiment 2 (non-words only) and 18% in experiment 1 (mixed words and non-words). Subsequent work establishes that the specific spelling pattern (as in Table 14.1) may substantially alter the obtained percentage of irregular pronunciations (Kay, 1982).

2. Pronunciation latencies may be significantly longer for inconsistent non-words like *heaf* than for consistent ones like *hean*. In Glushko's experiment 2, the mean difference between these two types was 22 msec. Again, subsequent data suggest that a finer-grained analysis of non-word spelling pattern will be required. Parkin (1983), for example, found augmented latencies only for spelling patterns that correspond to several different common pronunciations in words (labelled "ambiguous" in Table 14.1).

3. Pronunciation latencies may be longer for regular inconsistent words like *leaf* than for regular consistent words like *lean* (Andrews, 1982; Glushko, 1979). In Glushko's experiment 3 (words only), the mean difference between these two types was 17 msec. Once again, subsequent research indicates that this effect may require rethinking, given (1) its possible interpretation in terms of bias from other items in the list (Seidenberg et al., 1983); (2) its probable modification in terms of word frequency (Seidenberg et al., 1983); and (3) its uncertain reliability (Parkin, 1983, 1984).

4. Pronunciation of an inconsistent pseudoword can be significantly shifted towards irregularity (e.g. *yead*→/jed/ rather than /jid/) by prior presentation of appropriate irregular words. This was shown by Kay and Marcel (1981), whose experiment included a variety of conditions; for current purposes, the following contrast will suffice: following *head* (the irregular bias condition), *yead*→/jed/ on 39% of occasions; following *shed* (the pronunciation control condition), *yead*→/jed/ on only 10% of occasions.

5. Finally, it appears that the pronunciation assigned to an inconsistent non-word can be shifted towards irregularity by prior presentation of a word

semantically related to a word which would produce the Kay and Marcel bias effect. Rosson (1983) demonstrated that after *sofa, louch*→/lautʃ/ 89% of the time, whereas after *feel, louch*→/lautʃ/ only on 75% of occasions. As far as we can see, Rosson never actually says that the 14% fewer "regular" pronunciations of *louch* preceded by *feel* were /lʌtʃ/ (rhyming with *touch*); but one presumes that they were.

These five sets of observations constitute the data (at least, the most convincing data that we know of) that cause headaches for a standard, non-lexical GPC model. We now try to describe a *modified* standard model to see how it fares in accounting for these awkward observations. Subsequently, we briefly discuss (our understanding of) the procedures of an analogy model for assembling phonology from print. Finally, to share the headaches out equitably, we offer one or two observations awkward for such analogy models.

A MODIFIED STANDARD MODEL

In this model there are effectively three routines for word pronunciation. Two of these are lexical and one is non-lexical. The lexical routines both require that the visual input logogen system is used. In the first, the output from this logogen system accesses the semantics of the item, from which the lexical phonology can be addressed; in the second, the visual input logogen addresses the lexical phonology directly (see Morton & Patterson, 1980).

The central procedure of the non-lexical routine can be described as a set of mapping rules from orthographic strings to phonological strings. Accordingly we label this the orthography-to-phonology correspondence (OPC) system. There are two major senses in which the OPC system differs from and is more complex than Coltheart's (1978) grapheme-to-phoneme correspondence (GPC) system. First, the OPC system deals with two different sizes of orthographic unit: *graphemes* (i.e. the letter or letter combinations that correspond to single phonemes) and bodies (the vowel-plus-terminal-consonant segments of monosyllables that remain when the initial consonants or consonant clusters are removed).[2] Second, although we shall assume for the moment that mappings at the grapheme level are simple one-to-one transla-

[1]We have nothing to say at the moment concerning the pronunciation of polysyllabic words. We predict that complex morphophonological processes will exert an influence on the final outcome for polysyllables, and that there will therefore be no simple extension from the account for monosyllables. Data from MacCabe (1984) on the pronunciation of a set of consistent irregular non-words (which, in our terminology, included exemplars from both the gang-without-hero type and the hermit type) indicate that pronunciations corresponding to the irregular word analogy are four to five times more likely for disyllabic than for monosyllabic non-words.

tions, the mapping rules for bodies are more complex and will sometimes require one-to-several translations.

We acknowledge, but have little to say about, the problems entailed by a system operating concurrently with units of more than one size. These problems largely concern the mechanisms whereby, for a given letter string, the "solutions" for the different-length segments are either combined or selected amongst to produce a single response. For some sophisticated theorising on such mechanisms, we commend to you the chapter by Shallice and McCarthy in this volume (Chapter 15).

We have, then, an OPC system forming the centre of the non-lexical routine for assembling a pronunciation of a letter string. It is the centre in that it operates after orthographic parsing and prior to phonological assembly. It has two subsystems, a Coltheartian set of one-to-one mapping rules at the grapheme level and a more complicated set of mapping rules at the body level. To accomplish our goal of accounting for the five awkward sets of data introduced earlier, we shall have to provide considerably more detail regarding the operation of the OPC system and its interaction with the lexical system, and this we shall do almost immediately. It is perhaps worth noting at this stage, however, that the first two of the five observations are already predicted simply by the addition of a body system with one-to-several mapping rules: (1) a non-word with an ambiguous body (e.g. *pove*) may be given an "irregular" pronunciation (/pʌv/) because the OPC mapping rules for this body include *ove*→/ʌv/ as well as *ove*→/oʊv/; (2) whether *pove* is pronounced /poʊv/ or /pʌv/, its latency may be slower than pronunciation of a consistent non-word (like *pote*) because of a time penalty incurred in the choice between alternative pronunciations.

Interaction Between Lexical and Non-lexical Routines

For the pronunciation of a word, there will be two major routines, lexical and non-lexical (the full model of course includes two different lexical routines, but we assume that the fluent adult reader would normally pronounce words utilising direct, that is, non-semantically mediated, input–output connections). As we have already seen, Glushko's (1979) and Andrews' (1982) data indicate that regular but inconsistent words like *rove* take a little longer on average to pronounce than consistent ones (*rote*), though the effect is scarcely robust (Parkin, 1983). We assume that this effect is due to differences in the way that the two kinds of words are processed by the OPC system, because (all other things, e.g. word frequency, being equal) the lexical routine should be equally willing to serve *rote* and *rove*.[3] If pronunciation of a *word* is

[3]Note that this is a "strong" version of the visual input lexicon, in which the characteristics of an individual logogen are supposed not to be affected by similarly spelled words (Glushko's "orthographic neighbourhood"), at least under conditions that preclude visual confusions.

affected by the nature of its processing in the non-lexical routine, this suggests that the two routines are interacting. Two different kinds of interaction could be involved, which we label *conflict* and *interference*.

By the *conflict* model, there is a decision process that receives the pronunciations achieved by the two routines. When both pronunciations have been received, they are compared. If they are identical, then that pronunciation is produced. If they are different, then some decision is made which, usually, results in the lexical version being produced. The delay brought about by this extra decision in the case of inconsistent words would account for the latency data. Taken at face value, this model strikes us as silly: if the candidate pronunciations are marked as to their origin, then the lexical candidate might just as well be produced as soon as it arrives. (Note, however, that a model of this sort is considered, and judged not to be silly, by Henderson in this volume.) One rational modification might be that the candidates are not marked as to origin but that in the case of conflict, the two alternatives are checked to see if either corresponds to an entry in the phonological lexicon. This idea yields the following prediction: if the non-lexical routine offers an incorrect word (for example, regularisation of the word *gauge* will give /gɔ:dʒ/, which corresponds phonologically to the word *gorge*), then the checking procedure will fail, giving rise either to a major increase in errors (if the process is self-terminating) or to an increase in latency occasioned by the need to carry out more elaborate checks.

We shall opt instead for an *interference* model, in which the phonological codes produced by the two routines both go to a system whose function is to transform the code into a form suitable for production. For the moment, we shall identify this system with the response buffer of Morton's logogen model (e.g. Morton, 1969, 1979; Morton & Patterson, 1980). When the response buffer receives a code from either source, the transformation, which takes time, begins. If nothing else is received before the transformation is complete, then the transformed code is pronounced. If another code is received, it is compared with the first (which may or may not cost time in the transformation). If they agree, then the transformation process continues. If they disagree, then (1) there will be a time penalty due to the interruption and (2) a decision between the alternatives will require a lexical check.

In this modified standard model, the mean time for the lexical routine is faster than that for the non-lexical routine but the distributions have considerable overlap. Indeed, as Henderson (1982) emphasises, the data on pronunciation latencies for most words and non-words indicate such overlap, though the overlap may be negligible in the case of high-frequency words (Seidenberg et al., 1984). The third observation for which we seek an account—the small latency disadvantage suffered by regular inconsistent words (e.g. *rove*) relative to consistent words (*rote*)—can now be seen to arise in the following way. For either word, the lexical code will most often reach the response buffer and indeed begin to be pronounced before any code is received from the non-lexical routine; on some proportion of occasions, however, the code from the OPC system will arrive at the response buffer before processing of the lexical code is complete. Comparison of the two codes for *rote* will always yield a match because (as we shall see shortly) a correctly functioning OPC system can only produce the phonological code /roʊt/ for this string. With *rove*, however, the "body" subsystem of the OPC will sometimes send the code /rʌv/. On those occasions when the non-lexical routine both produces the code /rʌv/ and manages to get it to the response buffer before the lexical code has been pronounced, then whereas the correct pronunciation of *rove* will generally be ensured by a lexical check, the interruption and the lexical check will delay production of the response /roʊv/. Such occasions might be expected to be relatively infrequent; but then it is only a small (if indeed reliable; Parkin, 1983, 1984) average latency difference between *rote* and *rove* that we need to explain.

Regular words like *rove* are very rarely mispronounced, but errors on irregular words (typically regularisations such as *pint*→/pɪnt/) are considerably more common (e.g. 7% regularisation errors in Glushko's (1979) experiment 3 and almost 10% such errors for lower-frequency exception words in Seidenberg et al.'s (1983) experiment 3). In the modified standard model, such errors obviously occur on occasions when the non-lexical routine operates more quickly than the lexical routine; and these occasions, also obviously, derive from the lower end of the latency distribution of the non-lexical routine. Thus, the latency for *pint*→/pɪnt/ (rhyming with *mint*) should be substantially less, on average, than the latency for *nint*→/nɪnt/. We know of no proper data on this *specific* contrast, but some of our own recent data (Evett, Patterson, & Morton, 1985) provide a pertinent observation. One of our experiments, an almost exact repeat of Glushko's (1979) experiment 3, yielded a total of 16 regularisation errors (like *pint*→/pɪnt/), which had a mean latency of 522 msec. The equivalent mean latency for correct pronunciations of regular consistent words (e.g. *pink*) was 515 msec. We did not ask our subjects to pronounce non-words; but both Parkin (1983, using the same subjects in both conditions) and Glushko (1979, using different groups of subjects but from the same population) have shown an 80 msec advantage for consistent words as compared with consistent non-words. This suggests that our subjects' mean latency for pronouncing *nint* would have been at least 595 msec. In other words, there is tentative support for the prediction of the modified standard model that the error response *pint*→/pɪnt/ (measured at 522 msec) ought to be significantly faster than the "correct" response *nint*→/nɪnt/ (estimated at 595 msec). To the best of our understanding (but we postpone this issue until later), an analogy theory of pronunciation would not predict this effect.

The modified standard model as described thus far appears to deal with the first three of the five tasks set for it. The fourth task, an account of Kay and Marcel's (1981) pronunciation bias effect, is more Herculean, and demands a fuller description of how the non-lexical routine operates. A qualitative account is presented here; a quantitative account will be available in Morton, Patterson, Nimmo-Smith, Kay, and Evett (1985).

The OPC System: The "Body" Subsystem

At least four, and possibly five types of bodies require differentiation, and these correspond to the first five body types listed for *non-words* in Table 14.1. For *consistent* bodies like *aze*, the body subsystem offers a single mapping, *aze*→/eɪz/. For *consensus/heretic* bodies like *int*, the overwhelming consensus among words indicates /ɪnt/ (e.g. *mint*, *lint*, *hint*, etc.), with the single heretic *pint*. We suggest, along with Parkin (1983), that the OPC system basically treats such letter strings as if they were characterised by *consistency* rather than mere *consensus*. That is, the body subsystem offers a single mapping, *int*→/ɪnt/. Were it to include the alternative pronunciation, *int*→/aɪnt/, we would expect to observe two consequences. First, in "isolation" (that is, uninfluenced by recent exposure to the heretic word), non-words with consensus/heretic bodies such as *rint* or *tave* ought sometimes to be given the heretic pronunciation /raɪnt/ or /tæv/. Data from Kay (1982), however, indicate that under unbiased conditions, 99% of the pronunciations given to such non-words are regular, consensus pronunciations. Second, pronunciation latencies for *rint* and *tave* should be slowed (by the necessity of choosing between alternatives) relative to latencies for consistent non-words. Data from Parkin (1983), however, show no hint of such a latency difference.

Bodies of the *gang* type are, in a sense, the flip side of the coin from consensus and consistent types. A *gang* with a *hero* (e.g. *ook* where every relevant monosyllabic word except *spook*, the hero, is part of the gang: *book, cook, look, hook*, etc.) matches the *consensus/heretic* type precisely in terms of ratio (that is, many: one); but here of course the majority corresponds to the *irregular* pronunciation. A gang without a hero (e.g. *old* where *every* relevant word belongs to the gang: *cold, hold, bold, fold*, etc.) matches the *consistent* type (i.e. no exceptions); but once again the gang pronunciation is irregular rather than regular with respect to GPC rules. Given this parallelism, and because it is our contention that representations in the body subsystem are established on the basis of experience with words (indeed, how could it be otherwise?), then our assumption about *gang* bodies should be that only the majority mapping is represented; thus, from the body subsystem, *ook*→/ʊk/ and *old*→/oʊld/. We do, for the present, make this assumption; and we therefore interpret observations (which we discuss presently) of very frequent pronunciations of *pook*→/puk/ as demonstrating the ascendancy of the GPC subsystem over the body subsystem (which we also discuss presently). Whether gang bodies (especially those with heroes) have only one mapping listed or also include the "regular" pronunciation is, however, an empirical question, not yet resolved but surely resolvable, and so we reserve the right to modify our assumption on the basis of future, better data. After all, although the body subsystem can only learn from words, it may be eclectic in its formative years and be prepared to learn from words with similar but not identical bodies.

Ambiguous bodies are the only type for which it seems clear, at this stage, that at least two OPC mappings must be represented. Thus *ove* will have both /oʊv/ and /ʌv/ listed (and perhaps /uv/ as well, as in *move* and *prove*); *eaf* will have both /if/ and /ɛf/. The evidence forcing this assumption is precisely the evidence that was lacking to suggest OPC mappings for *heretic* pronunciations. First, Parkin (1983) has shown significant slowing of RTs to pronounce ambiguous non-words. Second, Kay (1982) has shown a significant proportion (around .15–.20) of irregular pronunciation assignments (like *pove*→/pʌv/) to ambiguous non-words.[4] In isolation (that is, unbiased by recent pronunciation experience with a word or non-word sharing the same body), we suggest that the body subsystem selects among alternative pronunciations for an ambiguous body at random but incurs a time penalty for this selection relative to bodies with a single candidate pronunciation.

Interaction Between the Two Subsystems of the OPC

We have already noted our current assumption that the GPC subsystem contains only one-to-one mappings (see Temple, Chapter 11 in this book, for a different assumption). Thus the grapheme *ea* will have the single translation /i/; it is only by virtue of the body subsystem that *eaf* can be non-lexically translated as /ɛf/. We expect to be able to account for all non-lexical pronunciation assignments, at least in the absence of factors to prime or bias pronunciation, by normal operation of the two subsystems of the OPC.[5] In the present model, the GPC routine will provide "regular" pronunciations for all letter strings; the body routine will provide: (1) regular pronunciations for both consistent and consensus/heretic bodies; (2) irregular pronunciations for gang bodies; and (3) an equivalent number of each alternative pronunciation for ambiguous bodies. Thus the phonology offered by the two subsystems will match for (1), conflict for (2) and sometimes match,

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[4]Kay's data do, however, indicate that some ambiguous bodies (e.g. *oll* and *eath*) give rise to *no* exception pronunciations in unbiased non-word reading. This point is discussed further in Morton, et al. (1985).

[5]We acknowledge that at least one (and probably more than one) aspect is missing here, namely some context-sensitive rules like *c* followed by *e*→/s/. At the moment we have not specified how such rules are handled, though we anticipate that it will be by the GPC subsystem.

sometimes conflict for (3). We now need to specify a decision rule between the subsystems. We note (and once again refer to Shallice and McCarthy, Chapter 15) that information from the two levels could in principle be combined; but we shall instead assume for the moment that decisions between the two subsystems of the OPC, like those between the lexical and non-lexical routines, are made on an *or*, not an *and* basis. We have as yet no proposal (and no pertinent data) regarding the relative times required for GPC-level and body-level solutions. Our suggestion for a decision rule is based entirely on an attempt to model the actual proportions of pronunciation assignments to various non-word types obtained by Kay (1982).

Kay's data give us three relevant values for r, that is, the average proportion of unbiased *regular* pronunciations: (1) as already indicated, $r = .99$ for non-words with consensus/heretic bodies like *yint*; (2) for non-words with ambiguous bodies that *in words* have more regular than irregular exemplars, $r = .87$; (3) for non-words with bodies that in words are represented by a majority of *irregular* exemplars, $r = .77$. In Kay's data, this third set includes both gang-with-hero bodies, and ambiguous bodies where the irregular pronunciation in words is the more common.

An assumption that the OPC code produced is controlled by the GPC subsystem on 70% of occasions provides a first-approximation fit for these data: (1) for consensus/heretic non-words, both GPC and body subsystems yield the regular pronunciation, so for these we predict $r = 1.0$ (Kay, 1982, obtained $r = .99$); (2) for ambiguous non-words, the GPC subsystem yields a regular pronunciation on all of its 70% of occasions; random sampling between the regular and irregular pronunciations in the remaining 30% of occasions controlled by the body subsystem means that, overall, predicted $r = .85$ (Kay obtained $r = .87$); (3) for the *ambiguous* non-words in this subset, once again predicted $r = .85$; for the gang non-words (where we have suggested that the body subsystem offers only the irregular pronunciation), predicted $r = .70$. If half of Kay's subset (3) were of each type, then overall predicted $r = .775$ (Kay obtained $r = .77$).

We are now, at long last, ready to attempt an explanation of awkward result number four—Kay and Marcel's (1981) priming or biasing effect. In their experiment, a non-word like *yead* received 39% of irregular pronunciations (/jed/) in the "irregular bias" condition (that is, when it followed the word *head*) but only 10% of irregular pronunciations in the "pronunciation control" condition (i.e. following the word *shed*). The first thing to note is that although Kay and Marcel (1981) label their pronunciation control condition "Rhyming word, different orthography (p. 402)," in fact a minority of the items in this condition *did* rhyme with the irregular bias word (as in *shed* for *head*). The majority of the pronunciation control items only shared vowel phonology with the irregular word (e.g. *wood* for *pull*, *does* for *love*, *aisle* for *pint*, etc.; see Kay & Marcel, 1981, p. 411). Because a proper control condition seemed to us to require rhymes, we (Evett, Patterson, & Morton, 1985) have performed a Kay and Marcel bias experiment with this modification. We replicated their clear and significant bias attributable to shared orthography (after *head*, *yead*→/jed/ on 44% of trials in our experiment). Our more adequate pronunciation control condition did, however, yield a larger value for the bias effect attributable to shared phonology: after *shed*, *yead*→/jed/ on 24% of occasions in our experiment (only 10% in Kay & Marcel), compared with a baseline of about 4%.

Although our replication of Kay and Marcel reduces the *size* of the orthographic bias effect (from about .30 to .20), it does not substantively alter the problem. Why should recent lexical experience affect the operation of a separate non-lexical OPC routine? Such a result of course fits neatly with the idea that all pronunciations, whether addressed directly or assembled by analogy, are obtained from lexical representations.

As Kay and Marcel themselves note, there is a way of explaining their bias result within a framework maintaining separate lexical and non-lexical routines: the lexical event engenders a temporary shift in the probability with which alternative non-lexical pronunciations are selected. We have proposed that, in the absence of a biasing word, the OPC body subsystem selects at random between /jid/ and /jɛd/ as pronunciations for *yead*, producing a body-r = .50 (overall r will of course be much higher because of the GPC subsystem). If *head* has just been pronounced, however, this random selection might abruptly shift to favour selection of the /ɛd/ alternative for any *ead* string, and then gradually drift back to its usual lack of preference. For this to work, it would be required that:

1. The stimulus word (*head*) activates the OPC orthographic body *ead*.
2. The phonological response (/hɛd/) activates the OPC phonological code /ɛd/.
3. The OPC system operates in such a way that concurrent activation of the two end elements of a mapping temporarily increments the future likelihood of following the pathway between them. Note that we cannot attribute the facilitation to recent *use* of the appropriate pathway because when *head* is presented, the OPC body subsystem will use the mapping to /hid/ on about 50% of occasions and the OPC system will produce /hid/ on about 85% of occasions.

Kay and Marcel (1981) provide three arguments against the preceding account of their result. The first is that it is a complicated process. We agree, but do not consider this a damning assessment; pronunciation by analogy is also a complex procedure. The second is that "the functional independence of the two processes is almost lost (p. 407)." This might seem a more worrying accusation, but it is not a major snag if one distinguishes, as we do, between separability and independence. Kay and Marcel's results probably rule out the idea of *independent* lexical and non-lexical routines, but not the notion of separable routines. As an example of this, Campbell (1983) has demonstrated an effect similar to Kay and Marcel's biasing result for *spelling* of non-words. When normal subjects have just heard the word "goat," for example, there is a significant shift in the probability with which they will spell the dictated non-word /foʊt/ as *foat* rather than *fote*. Campbell also performed this test on a surface dyslexic patient; his spelling of non-words was adequate (though not error-free), but showed no biasing effect. We interpret this contrast as reflecting the existence of a non-lexical spelling routine, which in the normal system can be influenced by lexical events but whose separability from a lexical spelling system is demonstrated by the neurological patient.[6] Likewise, we might expect certain surface dyslexic patients not to show Kay and Marcel's priming effect in non-word pronunciation. This test does not appear to have been done; however, Shallice, Warrington, and McCarthy (1983) report that their patient H.T.R. showed no significant effect, in pronouncing non-words, of the consistency with which the non-word's orthographic pattern is pronounced in words.

Kay and Marcel's third argument against the dual-routine explanation of priming hinges on the type/token distinction. According to Kay (1982, cited in Kay & Marcel, 1981), it is the number of types rather than the number of of tokens that determines the baseline probability of a particular pronunciation alternative. If this probability shifted with every occurrence of an alternative, then tokens (or frequency) could be expected to provide the dominant influence. To take a specific example, the frequency of *some* (1617 per million; Kucera & Francis, 1967) and that of *come* (630 per million) are both so high as probably to outweigh the combined frequencies of all regularly pronounced *ome* words (e.g. *home, dome, tome*). One's constant encounters with *some* and *come* might therefore be expected, over time, to yield a *permanent* predilection for the /ʌm/ reading of *ome* non-words; but of course the much more likely prounciation is /oʊm/. Kay and Marcel's interpretation (of the preference for *ome*→oʊm) is, of course, that there are more word tokens with this correspondence (which will therefore weigh heavily in the analogical process) than there are words with the alternative mapping *ome*→/ʌm/. We would argue, however, that whereas Kay (1982) may have convincingly demonstrated that the potent variable is *not* tokens, this does not automatically demonstrate that it *is* types. Indeed, Kay's own data on unbiased non-word pronunciations suggest an effect, but a rather

[6] It seems only fair to note that Campbell (1983) offered a somewhat different interpretation of her data. Emphasising the patient's *errors* in non-word spelling (rather than, as we have done, his many correct responses in this task), Campbell concluded that there is no fully competent non-lexical routine for spelling.

surprisingly small one, of the type dimension. Moving from bodies with a majority of regular pronunciations in words to those with a majority of irregular pronunciations only shifted the value of *r* down by about .10 (from .87 to .77) in Kay's data (an effect that was given a simple account in terms of the OPC system). Even a non-word like *jook*, with an overwhelming gang of *cook, book, look*, etc., was pronounced in a regular way /dʒuk/ by 80% of Kay's subjects. We suggest, therefore, that Kay and Marcel have overemphasised both the power of tokens and the power of the type/token distinction to subvert a dual-routine interpretation of their bias data.

In fact, in the modified standard model, neither types nor tokens are crucial. We need only consider two factors: (1) the effect attributable to shared phonology (i.e. the pronunciation control condition), which produces a bias of about 20% and operates across all body types; (2) the effect attributable to shared orthography, which operates only for ambiguous bodies. The simplest assumption is that, for a brief time after an ambiguous word has been read, the phonological alternative corresponding to that biasing word will always be selected by the body subsystem (instead of its unbiased procedure of selecting at random). After an irregular bias word, ambiguous bodies would then behave like gang bodies, with 30% irregular codes being produced by the OPC system. With a 20% *further* bias attributable to the effect of shared phonology, we are into the range of Kay and Marcel biasing effects without recourse to types, tokens, or the lexicon. The only additional assumption which we have made is that representation of a correspondence in the body subsystem is all-or-none rather than cumulative. This may engender a problem in explaining the mechanism of *acquisition* of body representations; but we hope that theories of reading acquisition will eventually offer solutions to such problems.

In summary, though we acknowledge that lexical biasing of non-lexical pronunciations necessitates a more complex and more interactive model than we might have wished, we do not find anything here which makes the modified standard model unworkable or untenable.

Finally, we face up to what is either the most difficult or the simplest (depending on the solution) observation of all for the modified standard model: Rosson's (1983) demonstration that *r* (the proportion of regular pronunciations) for an ambiguous non-word can be decreased by a word semantically related to a word that would produce the Kay and Marcel orthographic bias. For example, we know from Kay and Marcel that *louch* preceded by *touch* has a lower value of *r* than either *louch* in isolation or *louch* preceded by a pronunciation control word like *hutch*. Rosson (1983) has now shown that *louch* preceded by *feel* has a lower value of *r* than *louch* preceded by *sofa*.

This is a fish from a very different kettle. Trying to explain Kay and Marcel's (1981) bias result in terms of the operation of a non-lexical routine may have given us pause; trying to explain Rosson's (1983) bias result in these terms stops us altogether. The simple but naughty solution is to claim that, until some or all of the following questions are answered, we need not worry ourselves unduly about an account:

1. Rosson's result was certainly statistically reliable and thus should be replicable; but is it? The question may sound mean, but this is not exactly a psychological area noted for replicable phenomena.

2. What are the association values between Rosson's presented bias words and the mediating primed words (i.e. between *feel* and *touch*, *sofa* and *couch*, etc.)? Because these pairs are the only examples from the list given in the article,[7] we cannot answer this question. If the mediating primed words were highly predictable associates of the bias words, then the effect might be attributable to the induction of an association strategy on the subject's part, even though the critical items comprised only 24 pairs randomly interspersed among 82 fillers. Kay and Marcel (1981) went to some lengths to rule out an explanation of *their* bias result in terms of strategy, by demonstrating that the number of biased pronunciations did not increase as the experiment progressed, which one might have expected if the subjects gradually noticed the critical pairs. We are reassured by Kay and Marcel's analysis. None the less, first, it may be that such awareness (and therefore development of the strategy) occurs quickly rather than slowly; and, second, Rosson has provided no comparable reassurance for her result.

3. Rosson's data provide a measure of r for *louch* following either *sofa* (.89) or *feel* (.75), but no baseline measure. A 14% swing in r may seem large; but if average unbiased r for these ambiguous non-words is in fact around .82, then it is only a 7% shift (in each direction) that needs to be explained. The fact that Kay and Marcel (1981) found a major asymmetry, that is, a significantly larger shift from baseline towards irregularity than towards regularity, might make this seem implausible. Recall, however, that Kay and Marcel's critical non-word set contained a number of consensus/heretic bodies like *ave*, *aid*, *aste*, *atch*, etc. For these, according to both our model and Kay's (1982) data, unbiased r is essentially 1.0 and there is thus no room for an effect of a *regular* biasing word. Rosson's (1983) non-word set may have been more homogeneously of the ambiguous body type (again, we cannot tell from the article), permitting more equal (and smaller) effects in the two directions.

4. Finally, it may be critical to measure the duration of Rosson's effect. There are scraps of evidence to suggest that the bias produced by shared orthography, while temporary, may extend over a number of minutes and/or

[7]The practice (which is on the increase but obviously not yet universal) of publishing word lists in an appendix to the article seems a very desirable one.

intervening items. First, Kay (1982) found no reduction in the Kay and Marcel bias effect when either one or two items separated the members of a critical pair like *head* and *yead*. Second, and perhaps more dramatically, Seidenberg et al. (1983) have reinterpreted Glushko's (1979) finding of slowed pronunciation for regular inconsistent words (like *cost*) as dependent on the prior occurrence in the list of an irregularly pronounced word with the same body (e.g. *post*). In Seidenberg et al.'s experiment to demonstrate this influence, "prior occurrence" might mean something like 30 items intervening between *post* and *cost*. This effect of shared orthography on pronunciation (shown by Kay & Marcel, 1981; Seidenberg et al., 1983, etc.), which in the modified standard model represents an influence of a lexical event on the operation of the non-lexical routine, thus appears to have a moderate life-span. Rosson's bias result, if reliable, must be attributable to a very different set of processes and influences, and we predict that one indication of this difference may be a substantially reduced temporal durability relative to the bias from shared orthography.

With all of the foregoing queries unanswered, we still ought to give an indication of how our model might deal with Rosson's (1983) finding. This will require some further assumptions about the operation of the visual input logogen system with non-word stimuli. The simplest would involve the logogen for *touch* being affected by the stimulus *louch*, together with feedback from the cognitive system following prior presentation of *feel*. In the response buffer, the resulting lexical phonological code /tʌtʃ/ would then have to be segmented into /t-/ and /-ʌtʃ/, with the latter combining with /l-/ from the GPC subsystem of the OPC. A more detailed account awaits more detailed data.

Having tweaked the standard model of assembled pronunciation with the data most awkward for it, and having done our best (which is in some cases better than others) to modify the standard model such that it can accommodate these awkward observations, we feel that we have earned the right to subject analogy theory to some of the same treatment (the tweaking part, that is; the modifying we shall obviously leave to others).

ANALOGY THEORY

In order to see what unsolved problems might be lurking in the shadows for analogy theory yet to accommodate, we need to review, at least superficially, how the theory works. Its best specified version is to be found in Marcel (1980). One major question is whether links between orthographic and phonological representations exist only at the word (or morpheme) level, or whether these links also exist from individual letters and letter combinations to their phonological counterparts. At times, Marcel's account suggests the former, and thus seems to warrant its description as a purely lexical system. For example, "Each of the addresses in the visual input lexicon has at least two pointers. One of these leads to a semantic description. . . . The other is to an entry . . . in the output lexicon or speech vocabulary (p. 243)." The implication might be that whole-morpheme orthographic codes access whole-morpheme phonological codes, and all additional necessary operations (to account for non-word reading, specification of orthographic neighbourhood on the basis of similar segments, etc.) are handled by the fact that both types of code are *segmentable*.

In our view, the segmentability of these representations is not the issue and indeed is relatively uncontroversial. If the system truly managed with segmentable word or morpheme representations, we would not challenge the analogy theorists' claim to have an entirely lexical system. Our assessment, however, is that the model contains not just segmentable representations but already *segmented* ones. When discussing assignment of pronunciation to the non-word *kwib*, Marcel (1980) says: ". . . each letter, as it appears in words, will be accessed as a segment in the input lexicon. The most frequent pronunciation of each segment or letter (in the equivalent position in a word) will then be retrieved (p. 247)." In the limit, this system operates *like* a grapheme-to-phoneme converter, but "by recourse to lexical knowledge (p. 247)." It thus appears that a lexical entry in this model effectively includes all possible pairings between an orthographic string (of length from 1 to N letters) and its phonological equivalent that are derivable from the word in question. For a simple word like *mat*, we guess that the entry might look like this:

$$
\begin{array}{lll}
m_1\, a_2\, t_3 & \rightarrow & /m_1\, æ_2\, t_3/ \\
m_1\, a_2 & \rightarrow & /m_1\, æ_2\ / \\
a_2\, t_3 & \rightarrow & /\ æ_2\, t_3/ \\
m_1 & \rightarrow & /m_1\ / \\
a_2 & \rightarrow & /\ æ_2\ / \\
t_3 & \rightarrow & /\ t_3/
\end{array}
$$

We thus interpret Marcel as postulating mappings from orthographic segments to phonological segments at submorphemic levels, indeed, at individual grapheme and phoneme levels. This is *our* interpretation, but we do not see an alternative one. Without such mappings, given the two strings *have* and /hæv/, it is by no means clear how one could determine which orthographic segment yields which phonological segment—unless one were to use the kind of rule-based information that is explicitly prohibited from an analogy model. Despite the fact that these multi-level mappings are to be found within lexical entries, their existence leads us to think that the distinction between an analogy theory and our modified standard model is not best captured by the question of whether there is "non-lexical" information concerning pronunciation. Rather, the issue seems to be that information in the analogy model resides only in specific instances whereas, according to our model, the system "knows" about abstractions from the instances.

At least two issues appear to us to require specification in an analogy framework. The first concerns procedures for matching analogous segments. Terminal segments are supposed (by Glushko, 1979; Marcel, 1980) to form the primary source of analogy words. In a non-word like *kwib*, Marcel (1980) acknowledged that *k* and *w* will not constitute a single segment because they do not do so in any lexical entry. The question then arises, will the *ib* in *kwib* activate the entry for, for example, *bib*? With a non-word like *phave*, it is possible that the initial *ph* will be specially bracketed, thus preserving the indexing to give $(ph)_1\ a_2\ v_3\ e_4$. (This would also obviate the need to have some special means of blocking access to the large segment *have*.) In general, however, the procedures for creating the appropriate segments needed to access appropriate analogy words remain as yet underspecified. The second issue that requires attention is the mechanism by which larger (and, in the case of words, whole-string) segments over-ride smaller segments. The postulate that all and only conflicting subsegments are over-ruled, which Marcel used to account for the difference in latencies between consistent and inconsistent words, requires complex processes of comparison. Furthermore, the general over-riding principle reawakens a disquiet similar to that which we expressed in discussing a conflict version of the modified standard model. If the whole-string specification is marked as having some special quality (lexicality, or economical accounting for the input string, or whatever), then why would the system not just accept that specification and output its corresponding phonology? Why attend to the contents of smaller segments, only to militate reliably against them?

Having noted the major issues that seem to us to be underspecified in analogy theory, we turn now to two sets of observations awkward for such a theory, both of which have been mentioned in passing earlier in our chapter.

First, there are certain non-words for which analogy theory must predict a preponderance of "irregular" pronunciations. Examples given by Kay (1982) are *jook* and *nind*. Against the "regular" pronunciation of each (/dʒuk/ and /nɪnd/ respectively), there are what Henderson (1982) calls a maximum of lexical neighbours "... united in a hostile orthodoxy (p. 159)" and what we call simply "the gang." For *jook*, virtually every similar-ending word is pronounced like *book* (*book, cook, hook, look, rook, took*); only the hero *spook* accords with the "regular" pronunciation. For *nind*, virtually every similar-ending word is pronounced like *find* (*bind, find, hind, kind, mind, rind*); only *wind* (and even then only one of *its* versions) is a hero (or half-hero). Are these overwhelming analogies apparent in the assignment of pronunciations to non-words like *jook* and *nind*? We offer two sets of observations. The first comes from Kay (1982): out of 25 pronunciations, *jook*→/dʒuk/ (the "regular" pronunciation) 20 times (.80) and *nind*→/nɪnd/ 18 times (.72).

We also collected some data of our own, using the similar non-words *pook* and *lind*. We have often wondered if experiments that ask subjects to pronounce long lists of non-words, giving elaborate instructions about the task, might not encourage the development of unusual strategies. In our mini-experiment, therefore, we simply asked 60 people to pronounce each of the two items (printed on index cards, in lowercase letters), giving no previous explanation, and no warning that the items would be non-words. The subjects included a wide cross-section of people: secretaries, cleaners, students, psychologists, housewives, technicians. The results were as follows: 55/60 (.93) people produced a regular pronunciation of *pook* /puk/; only 4/60 pronounced it /pʊk/ to rhyme with the overwhelmingly common analogy of *book* and *cook*. For *lind*, all 60 people gave the regular pronunciation /lɪnd/; no one said /laɪnd/ as in *find*.

The results of our mini-experiment appear to conflict with the notion of pronunciation by lexical analogy. Of course one can (*post hoc*) find plenty of analogies to support the regular pronunciations obtained (e.g. *pool* for *pook*, *lint* for *lind*); but if analogy theory is forced to adopt such an open-door admission policy, it would seem to become rather unwieldy. Furthermore, in at least Glushko's version of the theory, choice of analogy is explicitly based on *terminal* orthographic segments. (Otherwise how could Glushko, 1979, define a pseudoword like *hean* as "regular"? It is regular by analogy with *lean* but irregular by analogy with *head*.) Yet it seems that our results *do* force analogy theory to the assumption of analogies based on other than terminal segments and indeed to the unpalatable assumption that more remote and/or smaller segment analogy words can exceed the influence of close analogies. As it seems implausible that the single hero word *spook* could dominate the 11-member gang of *book*, *cook*, *look*, etc., it would surely have to be the influence of words like *pool*, *moon*, and *food* that produces the high *r* for a non-word like *jook*. Most analogy theorists of our acquaintance (e.g. Henderson, 1982) have a proper respect for the palate, and we cannot imagine that they would prefer distant *food* to a nearby *cook*.

Second, as noted earlier, both Glushko (1979) and Parkin (1983) have data showing an average latency difference of 80 msec between pronunciation of consistent words and consistent non-words. In the modified standard model, this difference arises simply from the greater mean time of the non-lexical routine than of the lexical routine. In an analogy model, the slower time for non-words presumably reflects the time needed, in the absence of a whole-string specification, to consult the entire orthographic neighbourhood, obtain the appropriate segments of phonology from these various sources, and assemble a pronunciation. The problem for such an account, we suggest, is our recent observation (Evett, Patterson, & Morton, 1985) that the average latency for a regularisation error like *pint*→/pɪnt/ is scarcely longer than the latency for correctly pronouncing a consistent word and therefore, by implication, is substantially *shorter* than the latency for correctly pronouncing a consistent non-word. For reasons given earlier, our model predicts this result. In an analogy model, errors like *pint*→/pɪnt/ must derive from a failure of the lexical over-riding operation. When this fails, the procedure for producing a pronunciation must be the same as that for pronouncing a non-word, namely assembly of phonology from the neighbours.

Thus, it seems to us that an analogy theorist must predict equal latencies for *pint*→/pɪnt/ and *nint*→/nɪnt/. We suppose that he or she might argue (along similar lines to our argument for lexical and non-lexical distributions) that whole-string addressing and assembly from neighbours are procedures with overlapping time distributions, with the former producing a faster solution on the great majority of but not all of the occasions. Apart from the fact that this would involve abandoning the principle of lexical over-riding, it seems implausible to us. Majority rule (which is how the analogical process is supposed to work) requires one to wait until all of the votes have come in; how could this procedure ever be quicker than direct addressing of the single correct lexical candidate?

CONCLUSION

We have shown how a very simple expansion of the standard dual-routine model can account for a set of observations that seemed, on first glance, to controvert such a model. Our orthography-to-phonology correspondence system maintains an old grapheme-to-phoneme subsystem and postulates a new "body" subsystem. The addition of the latter allows us to account for (indeed, to predict) variable pronunciation *assignments* to ambiguous non-words and augmented pronunciation latencies for both ambiguous words and non-words. Though Kay and Marcel's (1981) observation (biasing of non-word pronunciation by orthographically similar words) has forced us to complicate our model by permitting lexical events to alter the operation of the non-lexical routine, the actual *performance* of our model probably predicts these bias effects more accurately than an analogy theory would do. The detailed, quantified performance of the model in terms of values of *r* for both unbiased and biased pronunciations of non-words will shortly be available in Morton, Patterson, Nimmo-Smith, Kay, and Evett (1985).

We look forward eagerly to reading an account of analogy theory drawn in sufficient detail to enable proper qualitative and quantitative comparisons between the two sorts of models. In closing, however, we reiterate the warning note on which we began, to ourselves and all others who would attempt an evaluation of these theories. Differences between the various body "types" and even between exemplars within one "type" are very large indeed; averaging over exemplars that behave differently can only obscure the issues. This is a depressing conclusion in its implications for the difficulty of constructing appropriate control items and for the necessity, given the inevitably small N for item type, of having a large N for subjects. It is, however, clear that even the atypically restrained generalisation across items in which we have indulged will turn out to be excessive.

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