send the eyes to. A recent review confirms that the frequency and the predictability of the words influence the likelihood of word skipping (Brysbaert & Vitu 1998). However, it also indicates that these effects are rather rare, and that when they occur they are smaller than the effects of both word length and launch site. As words get shorter, and the eyes are launched from closer to the beginning of the words, skipping rate increases drastically up to about 90% in the most extreme conditions. Undoubtedly, shorter and closer words are more likely to be identified in parafoveal vision, but this is not what causes them to be skipped more often. Indeed, a comparison between text reading and the scanning of meaningless material (or z-letter strings) reveals that string length and launch site influence string-skipping in the same manner as word length and launch site influence word skipping (Vitu et al. 1995). Such great regularities in the pattern of eye movements – present even when no cognitive processing is involved – strongly suggest that the decision of which word to fixate next is not cognitively determined, but rather, is primarily a function of low-level visuomotor processes.

As a final objection, I question the need to distinguish between two separate *Where* mechanisms, one that selects the next saccade target word and another sending the eyes at about the required location. Given that word length and launch site similarly influence both skipping rate and initial landing sites in words, a single mechanism may determine where the eyes move next. Furthermore, since visuomotor influences strongly deviate the eyes from the center of words, there may be no need to posit that they aim for the center of preselected words. Our assumption is that forward eye movements in reading are not directed to specific target locations, as the eyes would be naturally pulled by peripheral visual information in a center-of-gravity-type manner (see Vitu 1991a). Ongoing word identification processes would only intervene occasionally to modulate the oculomotor scanning pattern, favoring either the lengthening of planned saccades (see Lavigne et al. 2000), or the execution of regressive saccades to specific word locations (see Vitu & McConkie 1998; Vitu et al. 2000).

Authors' Response

Eye movements in reading: Models and data

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Abstract: The issues the commentators have raised and which we address, include: the debate over how attention is allocated during reading; our distinction between early and late stages of lexical processing; our assumptions about saccadic programming; the determinants of skipping and refixations; and the role that higher-level linguistic processing may play in influencing eye movements during reading. In addition, we provide a discussion of model development and principles for evaluating and comparing models. Although we acknowledge that E-Z Reader is incomplete, we maintain that it provides a good framework for systematically trying to understand how the cognitive, perceptual, and motor systems influence the eyes during reading.

R1. Introduction

Before we delve into specific issues and controversies about the issues raised by the commentaries, it might be of some value to try to characterize what we had hoped to accomplish with our model and thus indicate what we feel is the proper context in which it is best discussed and evaluated. Perhaps a good place to start is with a quote from E. B. Huey in the introduction to his classic book on the psychology of reading:

> And so to completely analyze what we do when we read would almost be the acme of a psychologist's achievements, for it would be to describe very many of the most intricate workings of the human mind, as well as to unravel the tangled story of the most remarkable specific performance that civilization has learned in all its history. (Huey 1908)

We hope no one misunderstood our modeling effort as pretending to be an attempt "to completely analyze what we do when we read." We indeed agree with Huey that to completely analyze what we do when we read would be a monumental achievement well beyond our efforts – and certainly well beyond what our field has learned, to date, about reading, the control of eye movements, and language processing.

Instead, we had hoped to develop a model that was a reasonable "zero-order approximation" of how cognitive processes control the eyes in reading. Our first goal was to come up with a model that could explain how ongoing cognitive processes *could* control eye movements during reading, subject to the constraints that it do so in real time and be plausible in terms of what is known about the underlying cognitive processes and the control of eye movements. We think we succeeded at this goal, and that this was a far from trivial task.

Our second goal was to make the model as simple as possible, so that it would be reasonably transparent with respect to how and why it worked. If we succeeded at this goal, then when the model failed to account for a phenomenon properly, it would be clear why the model did not work and, therefore, whether it was merely too simplistic and needed to be further elaborated, or whether it was fundamentally flawed. Put differently, if a model is simple enough, it can be a useful heuristic device, but when it gets too complex, it risks merely being a data-fitting algorithm. Put a third way, if the model is simple enough, it has the chance of generating both testable and interesting predictions about reading (i.e., predictions that can be falsified and that test basic assumptions of how the model explains the reading process).

Consistent with these goals, we tried to delimit our problem carefully so that we would have some chance of success. Most notably, we did not try to explain interword regressions (i.e., regressions to words that had been either previously read or skipped over). The reason for this is that there is ample evidence that many of these regressions are a result of various "higher-level" processing failures (e.g., attributable to general comprehension problems or temporary misparsing of sentences). Another reason is that modeling these kinds of regressions in any detail is several orders of magnitude more difficult than what we have attempted, both because we have a far from adequate cognitive theory of the processes that fail, and because it is quite unclear – given such a failure – exactly what should be targeted by an eye movement responding to this failure. We also did not pretend to have a serious or detailed model of word identification, both because how words are encoded is still a contentious issue that we hoped to bypass, and because a model of word identification (even without

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worrying about eye movements) that attempted to explain all the phenomena of word encoding that have been studied to date would have required an effort at least as large as our present modeling effort. Instead, we merely inserted a functional account of word encoding into our model that had reasonable processing characteristics.

In the prior paragraph, we have noted that a major goal of our model was to explain how cognitive processes can control eye movements in normal silent reading. Some of the commentaries, however, appear to take us to task for this, their essential claim being that cognitive processes have minimal control over eye movements in reading. This criticism is interestingly in contrast with several other commentaries that argue our model is quite limited, in that there are phenomena in reading indicating cognitive control over eye movements that the model does not account for (and that the commentators are skeptical can be accounted for by a model such as ours).

We will fill in the details below, but we remain convinced that our model is on the right track, in that it gives a good account for much of the forward progress of the eyes through the text and that this progression is importantly guided by cognitive control. Nonetheless, as we have indicated above and in the target article, our model is definitely incomplete, and we are trying to elaborate on the details to develop a more comprehensive model (e.g., we are currently trying to account for how subword units like morphemes can influence eye movements).

We wish to close this section with two disclosures. The first is that there was a conceptual oversight in the E-Z Reader 7 model presented in the target article: We assumed that the early visual processing stage does not have to be reinitiated when a previewed word is subsequently fixated. This assumption is clearly wrong, as it goes against the large amount of evidence that the integration of information across saccades in reading is *not* at the "visual" level (Rayner 1998). It also causes the model to predict far too much preview benefit, a fact that alerted us to the problem and that was made apparent to us by the excellent analysis in the **Findlay & White** commentary. A subsequent simulation in which the initial stage of processing had to be repeated on every fixation led to fits that were virtually as good as the original simulation; and furthermore, reasonable estimates of preview benefit emerged from this simulation. The second point is that our description of the model led some commentators to believe that we thought that the targeting of saccades was, *in principle,* completely controlled by word boundary information (i.e., the location of spaces between words). In keeping with our attempts to be parsimonious, we did think that such an assumption was not unreasonable in explaining the phenomena discussed in the target article. However, in our first attempts to model the processing of Finnish compound words (Pollatsek et al. 2003), it soon became apparent that more "cognitive" control was needed to explain the process of targeting refixations on these long words. In the next few sections of this reply, we will address what we see as the central issues that were raised by the commentaries.

R2. Parallel versus serial allocation of attention

Our assumption that attention is allocated serially should, like virtually all the assumptions in our model, be taken as a working hypothesis. However, it's an assumption we are reluctant to part with unless very strong evidence forces us to give it up. A major reason for wanting to hold onto it is that giving up this assumption would make the model significantly less transparent – and therefore much less useful as a heuristic device. Our general working assumption is that most people studying reading are interested in the cognitive processes underlying this activity. Many of these researchers, however, do not use the method of recording eye movements of people who are reading text, but instead rely on relatively artificial single-word paradigms (such as the lexical decision task) or more contrived reading paradigms (such as having the reader advance the text by pushing buttons). One of the reasons researchers resort to such techniques is economic: Doing good eye-movement research is expensive. However, it seems clear that another reason people shy away from engaging in eye-movement research is that they are afraid that the relationship between the cognitive processes they wish to study and the eye-movement record is hopelessly complex. As a result, a major motivation for developing the E-Z Reader model was to create a tool that would help demystify eye-movement measures and provide a reasonable rationale for their interpretation in the study of how readers comprehend written language.

Clearly, one aspect of the eye-movement record that our model is building on is the reasonably clear relation between the variables that indicate cognitive processing of language (e.g., the frequency of a word) and the fixation time on that word. (We discuss this relationship in detail in sect. R3, on lexical processing.) However, it seems equally clear that one can't merely assume that the time spent on a word is a direct measure of the time to process that word. First, the onset of the first fixation on a word can't coincide with the start of processing that word because – among other things – there is clear evidence that the processing of many words begins before they are fixated (Rayner 1998). Second, unless one assumes that eye movements are programmed instantaneously, the end of a fixation can't coincide with the termination of processing of that word. However, our assumption that words are processed serially allows one to draw the inference that the gaze duration on a word can be inferred to be the duration of a stage of word processing (the one that triggers the eye movement off the word). Therefore, our model does provide a good rationale for taking gaze durations on words seriously, and making reasonably simple inferences from them.

This inference rests quite heavily on the assumption that attention moves serially. Can we guarantee that this assumption is true? Clearly, we cannot. However, there is a reasonable amount of evidence in the attention literature (see, e.g., Sieroff et al. 1988) to suggest that attention is importantly object-based, and that – at least in certain cases – it moves in a serial fashion from object to object. Moreover, much of the evidence against this serial assumption in reading (see sect. R4, on parafoveal-on-foveal effects) seems quite questionable. We therefore see no reason to change this admittedly strong assumption about attention.

We should also point out that, from a modeling standpoint, it would be easier to adopt the view of attention espoused in the SWIFT model (Engbert et al. 2002): In this model, attention is a gradient that is allocated in parallel to several words, so that, instead of positing two stages of word identification (as in E-Z Reader), the model has many stages of word processing. In essence, an assumption that

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words are processed in parallel allows many more degrees of freedom in fitting the data.

We would like to make one more point about our serial-processing assumption. **Kennedy** takes us to task for our suggestion that this assumption is a good one because readers "need to keep word order straight." He then goes on to argue that written language, unlike spoken language, "can be inspected at will," so that text can be reprocessed. (This is presumably necessary to correct any initial misencodings of word order, among other things.) We certainly did not intend to argue that it was absolutely necessary for readers to correctly encode word order on their first pass to have any chance of understanding what was said in the text. However, we find Kennedy's argument a bit puzzling. Surely, he would agree that it benefits the reader quite a bit to get the word order correct on the first pass through the text, and that, if words are processed serially, this then helps readers encode words in their correct order. (Of course, even if the original encoding of order was correct, as it would be in a serial-encoding model, there could still be later memory errors.) We think an interesting question that Kennedy's comment raises, however, is whether there might be significant differences between languages in this regard. That is, word order is particularly important in conveying information in English because there is little in the way of case marking; so perhaps one might find that readers do attempt to process more than one word at a time in heavily case-marked languages, such as Russian.

R3. A more detailed model of lexical processing

As **Huestegge, Grainger & Radach** (**Heustegge et al.**) indicate, there are two general strategies that one might use to try to understand the relationship between word identification and eye movements during reading. The first is to modify an existing model of word identification so that it includes those assumptions necessary to explain how this process affects (and is affected by) the movement of the eyes through text. The obvious advantage of this approach is that it can potentially explain phenomena that have been observed in both single-word tasks (e.g., frequency effects in lexical decision) and natural reading. The drawback of this approach is that the resulting models may become so complex that they are difficult to evaluate; as Huestegge et al. point out, the model that has come closest to realizing this approach (the Glenmore model) has not yet been evaluated on a sentence corpus.

The second modeling strategy (which, as we noted previously, is the one that we have adopted) is to develop a minimal set of assumptions about word identification and eye-movement control, and then use these assumptions as a framework for examining both cognitive processes and eye-movement control in reading. The main virtue of this minimalist approach is that the inner "workings" of this type of model are transparent enough to serve as heuristics for thinking about the reading process and simple enough to be tested on large sentence corpora. The major drawback of this approach is that the models of lexical processing and eye-movement control are likely to be oversimplified and/or incomplete.

We agree with many of the commentators that our explanation of word identification is not fully specified. For example, **Andrews** and **Perea & Carreiras** correctly note that our model cannot handle orthographic neighborhood effects. Similarly, we agree with **Hyönä & Bertram** that our attempts to simulate the complex effects of morphology (Pollatsek et al. 2003) are preliminary and cannot account for the full range of effects that have been observed (e.g., Bertram & Hyönä 2003). Further, we agree with **Beauvillain & Pouget**, **Liversedge & White**, and **Underwood** that the inclusion of a preattentive visual processing stage in E-Z Reader does not (at present) provide an explanation for how the characteristics of a word's orthography affect where it is fixated (however, Underwood does suggest how our model might address this issue).

Clearly, our assumption that the speed of (both stages of) word processing can be completely accounted for by the written frequency of a word in the lexicon and its predictability from the prior text was intended to be a convenient initial step in attempting to relate cognitive processes in reading to the movement of the eyes through text. That is, when we proposed the model, we were fully aware that this assumption was an oversimplification as, among other things, we were involved in several of the studies that indicated that word parts, as well as word wholes, influence the time a word is fixated in text. However, we think the way to proceed is to consider each of these phenomena carefully and then to think about the minimal assumptions necessary to explain each of them. The other approach, which often involves using two or more processing modules operating in parallel (e.g., Reilly & Radach's [2003] Glenore model), may successfully predict the effects, but often not in a manner that suggests new or interesting phenomena, or in a manner that can be "unpacked" so as to reveal the assumptions that are responsible for making the predictions.

Several of the commentaries were more specific and argued that our assumption that words are identified in two stages is both vague and unjustified (**Huestegge et al.**; **Sereno, O'Donnell & Sereno** [**Sereno et al.**]; and **Shillcock, McDonald & Monaghan** [**Shillcock et al.**]). That is, a core principle in E-Z Reader is that the processing necessary to identify printed words is done in two stages: L_1 and L_2. In our earlier papers (Reichle et al. 1998; 1999), we described L_1 as a "familiarity check" that indicated full identification of a word was imminent, and L_2 as the "completion of lexical access," or the point at which a word's meaning was available to the language system for further processing. This distinction is important, because in our model, the completion of L_1 causes the oculomotor system to begin programming a saccade, whereas the completion of L_2 causes attention to shift to the next word. **Andrews** raises the question of whether this change in labels reflects a deeper conceptual change. Our answer is that there are at least two ways the distinction between L_1 and L_2 can be conceptualized, and that our goal in introducing these new labels was to make our model agnostic with respect to these different conceptualizations.

In earlier papers (Reichle et al. 1998; Rayner et al. 1998c), we referred to the global-activation memory models (e.g., MINERVA 2; Hintzman 1984) and pointed out that, according to these models, cues such as a word's orthographic pattern can be used to gain access to additional information in memory in two ways. The first is by matching the cue to the collective contents of memory. This process provides a rapid index of the cue's overall familiarity and serves as a basis for recognition decisions (Hintzman 1987; 1988). It is also consistent with our early conceptual-

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ization of the familiarity check. The second way of accessing information in memory is via a slower pattern-completion process that provides a basis for recall (Hintzman 1986). For example, a word's letters might be used to "retrieve" its pronunciation and/or meaning. This second process is consistent with our early conceptualization of the completion of lexical access. This distinction between a rapid sense of familiarity and a less rapid "retrieval" process has a venerable history in cognitive psychology (*dual-process theory:* Atkinson & Juola 1973; 1974; Mandler 1980; see Yonelinas 2002 for a review) and has recently been supported by physiological evidence (Davachi et al. 2003).

The second manner in which the two stages of word identification can be conceptualized is in terms of the time course over which orthographic, phonological, and semantic information becomes available as a printed word is identified. We think it is still an open question both about how the phonological code is activated (e.g., the roles of "assembled" and "addressed" phonology) and whether orthographic information accrues appreciably faster than phonological information. That is, there is now research from our lab (Lee et al. 1999; Pollatsek et al. 1992; Rayner et al. 1995; 1998b) and other labs (Perfetti & Bell 1991) indicating that phonological information is extracted early in word encoding – even before the word is fixated. Therefore, we didn't intend to posit, as **Huestegge et al.** claim (see also **Sereno et al.**), that phonological encoding is restricted to the second stage of word identification. However, it seems clear there must be significant activation for orthographic or phonological codes (or both) prior to anything like full semantic activation. An alternative conceptualization of the familiarity check, therefore, is that it corresponds to the availability of a word's form (i.e., its spelling and/or sound), whereas the completion of lexical access corresponds with the availability of a word's meaning. (Yet another possibility is suggested by **Osaka**: that the first stage of word identification is mediated via the phonological loop component of working memory.)

Of course, these different ways of thinking about the two stages of word identification that are postulated in our model are not mutually exclusive. For example, a recently developed instance-based model of word identification (Reichle & Perfetti 2003) is based on MINERVA 2 (Hintzman 1984) and consequently inherits the dual-process distinction between fast familiarity and slow retrieval operations. Because the grapheme-to-phoneme mappings tend to be more consistent than the arbitrary mappings between spellings and meanings, however, word form information is also generally available more rapidly than semantic information.

Hence, our decision to use "L_1" and "L_2" rather than "familiarity check" and "completion of lexical access" reflects our desire to leave open the possibility that either (or both) of these conceptualizations of word identification may play important roles in eye-movement control. That is, the decision about when to initiate a saccadic program may reflect the overall "sense" that a word is familiar and hence likely to be soon identified, or it may reflect the availability of a word's form, or it may reflect both. We think that an interesting area of future study is to try to understand whether there is a consistent difference in how variables affect gaze duration (largely controlled by L_1 in our model) and how they affect "spillover" (largely controlled by L_2 in our model). If the pattern differs markedly, it would indicate that there may be two fairly discrete stages of word processing. If the pattern in the two types of effects is fairly similar, then it would suggest that L_1 (and the signal to move the eyes) may merely be a result of a lower threshold of word identification than L_2.

In this regard, we posited one distinction between L_1 and L_2: that predictability (as measured by cloze task probabilities) affects L_1 less than L_2. Our justification for this assumption was that higher-level linguistic processing (the effects of which are estimated by our predictability measure) should influence the earlier stage of lexical processing less because it is more data-driven. As **Raney** indicates, there is evidence consistent with this notion: Two recent studies from his lab suggest that higher-level linguistic variables can have isolated effects on L_2 (Raney et al. 1996; 2000). We contend that these results, in conjunction with our preliminary simulations exploring the loci of morpheme effects (Pollatsek et al. 2003) and lexical ambiguity effects (described in our target article), demonstrate the usefulness of our model and support Raney's conjecture that E-Z Reader "provides a transparent (i.e., definable) architecture for explaining how high-level language processes influence eye movements."

R4. Parafoveal-on-foveal effects

A number of the comments deal with so-called parafoveal-on-foveal effects, which is not surprising, given that it is currently a heated topic of debate. A "parafoveal-on-foveal effect" refers to a situation in which one would observe some effect on the fixation time on the fixated word ($word_n$) produced by a parafoveal word that has not yet been fixated (usually the word to the right of the fixated word, i.e., $word_{n+1}$). **Inhoff & Shindler**, **Kennedy**, **Murray**, and **Vitu** all discuss this issue and claim that such a phenomenon is quite damaging to the E-Z Reader model, which assumes that encoding of words is strictly serial. Before discussing the data in some detail, we think two theoretical points need to be made. The first is that the serial assumption about word encoding in E-Z Reader does not preclude the possibility that processing $word_{n+1}$ can affect the fixation time on $word_n$. In fact, a later section of our response (sect. R7) will deal with one such effect: the effect of skipping $word_{n+1}$ on the fixation time on $word_n$. Second, because our model predicts occasional saccadic errors, it also predicts that the eyes will occasionally land on $word_n$ in cases where the intended saccade target is $word_{n+1}$. Such targeting errors allow for some parafoveal-on-foveal effects, although these will be admittedly small, as the percent of such mistargeted saccades will be small.

Now let us consider whether there is substantial evidence that there are parafoveal-on-foveal effects that would require a substantial change in our modeling approach. First, let us consider whether there are consistent effects on the fixation times on $word_n$ (i.e., the fixated word) attributable to the processing of the meaning of the word to the right of fixation ($word_{n+1}$). If so, one would expect that the frequency of $word_{n+1}$ would have an effect, but it does not (Carpenter & Just 1983; Henderson & Ferreira 1993; Rayner et al. 1998c). There have been other studies that have manipulated various properties of $word_{n+1}$, but it is far from clear that these effects can be reliably obtained

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(see Rayner et al. 2003c). For example, White and Liversedge (in press) found no such effect. Furthermore, Hyönä and Bertram (in press) reported five different studies in which they looked for such effects and found rather inconsistent evidence for them. In their experiments (which used long Finnish words), they varied the frequency of the first constituent or the frequency of the whole word. In one experiment (which varied the frequency of the first constituent), they found that the gaze durations were slightly longer when the first constituent of the word to the right of fixation was low frequency. Yet, in another experiment with exactly the same manipulation, they found the opposite effect. There were also no effects on gaze duration in the experiments in which whole word frequency was manipulated, but they did find an effect on the final fixation before moving to the target word. Likewise, the other measures that were examined (skipping rate and probability of a refixation) yielded variable and inconsistent effects. Clearly, beauty is in the eye of the beholder: Hyönä and Bertram argued that parafoveal-on-foveal effects can occur under some circumstances, but, given that most of their measures yielded no effect, a skeptic could argue that such effects are highly unreliable and seldom if ever occur.

To their credit, Kennedy et al. (2002) attempted to account for discrepancies in parafoveal-on-foveal studies in a systematic manner. However, one of the key experiments has been somewhat difficult to replicate (even by the lab that first reported the effect; see **Murray**'s commentary), and the fact that similar manipulations can yield either facilitation or inhibition is somewhat troubling. Another problem is that much of the evidence for parafoveal-on-foveal effects comes from tasks that may approximate reading, but aren't reading. Instead, subjects engage in what seem to be closer to visual-search or pattern-matching tasks than reading. Because it is known that frequency effects disappear when the task changes from reading to visual search (Rayner & Fischer 1996; Rayner & Raney 1996), one has to be concerned about whether these parafoveal-on-foveal results (even if they are reliable) can generalize from these nonreading tasks to reading. A final problem is that most of the experiments that have claimed to show such parafoveal-on-foveal effects in reading have manipulated some orthographic aspect of the text (e.g., used highly unusual and/or orthographically illegal letter combinations at the beginning of word$_{n+1}$). In some cases, these manipulations are highly visible and may disrupt the normal reading process (e.g., the reader might conclude that part of the task is to notice these strange letter combinations). We do acknowledge that there are a few studies in which subjects were reading for meaning and semantic parafoveal-on-foveal effects were observed. For example, Inhoff et al. (2002) reported such an effect. However, other similar studies from the same lab (Inhoff et al. 2000a; Starr & Inhoff, in press) did not yield these semantic effects; instead, the only effects were attributable to orthography.

Given these various points, we continue to feel that the validity of parafoveal-on-foveal effects remains questionable, and despite **Murray**'s protestations, we continue to think that proponents of such effects need to demonstrate that they are relevant to the task of normal reading (i.e., reading text for meaning). We do agree, however, that, if large consistent effects of the *meaning* of word$_{n+1}$ on the fixation duration on word$_n$ could be demonstrated, then such a demonstration would be problematic for the E-Z Reader model. In contrast, we are not convinced that accounting for effects of the orthography of word$_{n+1}$ on the fixation time on word$_n$ would be so damaging to the model. Related to that issue, we discuss below (sect. R8) the effect of orthographic properties at the beginning of the word that is to the right of fixation with respect to landing positions, and also argue that the visual processing stage that has been incorporated into the most recent version of E-Z Reader could account for such effects in a manner similar to how the attention gradient in SWIFT (Engbert et al. 2002) would handle such effects. (However, for reasons of parsimony, we are reluctant to do so unless the data forces such a revision.)

R5. Do cognitive processes drive the eyes in reading?

At some level, we would have thought that the answer to this question has already been conclusively answered in the affirmative. Rayner (1998) identified well over 100 published articles that have shown some influence of cognitive processing on eye fixations and eye movements. Indeed, all of those researchers who use eye movement data to study some aspect of language processing implicitly assume that the link between eye movements and cognition is quite tight. Furthermore, as we noted in the target article, the majority of the viable existing models of eye-movement control assume such a link. Yet, **Vitu** and **McConkie & Yang** still question this basic assumption.

Vitu's statement that "E-Z Reader may not be a serious candidate" as a realistic model of eye-movement control in reading is a non sequitur. Of course it is a serious candidate (and many other commentators are quite willing to acknowledge this point, even those who favor other models). We believe that, as with some of the other commentators, much of Vitu's argument rests on points that are disputable. She stresses that cognitive activities are not the engine driving saccades in reading; she further suggests that the landing position in the word is more important than frequency. This claim is quite puzzling and reverts to research on the optimal landing position effect. In a series of experiments with words in isolation, O'Regan and colleagues (see, for example, O'Regan et al. 1984) demonstrated that: (a) the probability of refixating a word increases dramatically as the initial landing position moves further from the optimal landing position; and (b) there is a processing cost in gaze duration of about 20 msec for each letter that the eyes deviate from this optimal letter position. From these data, O'Regan (1992b) argued that oculomotor effects related to where a given word is fixated is the most important determinant of how long the eyes remain in place. However, Vitu's argument is quite puzzling because her own work (see Vitu et al. 1990) clearly demonstrated that, during natural reading (i.e., when reading text, as opposed to isolated words), the refixation effect remained, but that the processing cost was greatly attenuated or disappeared (see also Rayner et al. 1996). That is, the gaze durations on words were relatively flat across different landing positions during reading.

To support her claim that cognitive processes don't guide eye movements in reading, **Vitu** now cites a study by Vitu et al. (2001) which reported that single fixations were longer when the eyes landed in the middle of a word than when they landed near the ends of the word, giving an in-

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verted V-shape function. Vitu et al. correctly noted that our model does not predict this effect; however, it is also inconsistent with O'Regan's (1988) Strategy-Tactics model (which inspired much of the work on landing-position effects in the first place). Vitu also notes that this result is inconsistent with results reported by Rayner et al. (1996) that showed relatively flat functions due to landing position. However, she doesn't mention two important points: First, both Rayner et al. (1996) and Vitu et al. (2001) found an effect of word frequency that was independent of where the eyes landed on the word in single fixation cases; that is, single fixations on low-frequency words were consistently longer than on high-frequency words. Second, the inverted V-shape function reported by Vitu et al. (2001) is inconsistent with data reported by Vitu et al. (1990). Specifically, Figure 6b of Vitu et al. (1990) shows gaze durations on target words separately for cases when two fixations were made on the target word and cases when only one fixation was made. The latter curve (like that reported by Rayner et al. 1996) is relatively flat across different fixation positions in the word. (**Hyönä & Bertram** also claim to have replicated the Vitu et al. 2001 finding of an inverted V-shaped function; however, they refer to first fixations, and the point under debate involves single fixations.) So, at this point, there is clearly some inconsistency in Vitu's own data, and it seems reasonable to conclude that there isn't a systematic effect of landing position on fixation times.

McConkie & Yang argue that there is "counterevidence" to the notion that saccades are triggered by a cognitive event. We would like to know what counterevidence they have in mind other than their own work. In their experiments (Yang & McConkie, 2001; in press), text is replaced by some other pattern (strings of X's, random letters, other text, etc.) on selected fixations. Based on this paradigm, they claim that cognitive processes have an effect only very late in fixations because only very long fixations are influenced by this manipulation. They further make the interesting proposal that cognitive events only inhibit saccades, not trigger them.

In contrast to this evidence, there is an overwhelming amount of data from many labs indicating that word frequency is an important determinant of the gaze duration on a word (see Rayner 1998); obviously, the decision of when to move on to the next word is an important element of eye-movement control in reading. Moreover, contrary to **McConkie & Yang**'s claim that effects like word frequency work only by inhibiting saccades (which would mean that only the longer "tail" of the fixation-duration distribution would be affected by frequency), Rayner (1995) demonstrated that the effect of word frequency was to shift the distribution, with its shape remaining largely the same for high- and low-frequency words.

Vitu and **Brysbaert & Drieghe** attempt to undercut the claim that word frequency is an important determinant of eye control in reading by suggesting that word-frequency effects are largely word-length effects in disguise (because word length and word frequency are highly correlated in text; see also the section on the determinants of skipping, R7). Their argument rests largely on correlational analyses using a corpus of text in which neither frequency nor word length is controlled experimentally (Brysbaert & Vitu 1998). In these analyses, word length is found to predict a higher percentage of the variance than a "cognitive load" variable that reflects a mixture of word frequency and predictability. On this basis, they concluded that word-frequency and predictability effects are actually word length effects in disguise. However, the problem with these correlational analyses is that they seduce people into thinking one can infer causal relationships when in fact there is *no way* to infer how much of the "common variance" predicted by word frequency and length is attributable to one variable or the other.

By contrast, experimental manipulations allow one to do just that (i.e., unconfound these two variables). There are many studies (see Rayner 1998) in which word length is equated and where word frequency produces sizeable effects on gaze duration (and other immediate measures of eye movements). With respect to this, it is worth noting that E-Z Reader not only predicted the fixation duration means for several frequency categories of words (in which frequency and length are confounded), but also predicted the means for key target words in two studies (Rayner et al. 2003a; Schilling et al. 1998) in which word length was equated for words of differing frequency. This indicates that word-frequency effects aren't word-length effects in disguise.

We should also point out that evidence for cognitive control is not limited to effects of word frequency, as the predictability of a word in text also influences the gaze duration on a word (Ehrlich & Rayner 1981; Rayner & Well 1996). In addition, the frequencies of the component morphemes of words (for which the frequency and length of the words are equated) also have significant effects on the gaze durations of words (Andrews et al., in press; Hyönä & Pollatsek 1999; Juhasz et al. 2003; Niswander et al. 2000; Pollatsek et al. 2000). Indeed, in the Hyönä & Pollatsek (1999) and Pollatsek et al. (2000) studies using Finnish compound words, the frequency of each component had almost a 100-msec effect on gaze durations – hardly a negligible effect!

We should quickly add that we are not denying that word length influences the time needed to identify a word, although we do think that these "length" effects are also largely related to cognitive processing. Consider an extreme case of a 15-letter word. It is clear that the length of this word will slow the identification of the word considerably because it will take at least two fixations to encode all of its letters (unless the word is so orthographically unusual that one doesn't have to process most of its letters). The current version of the model does incorporate an assumption that has the potential to explain such an effect: Letters are more difficult to process the further they are from fixation. At present, we admit that this part of the model is rough and doesn't fully handle all the factors that influence the legibility of letters (see the section on word encoding, R3). We also agree with **Hyönä & Bertram** that it is still an open question whether our model can satisfactorily handle all word length effects, such as their data indicating an interaction between word length and the effect that is attributable to the frequency of the components of Finnish compound words (see sect. R3).

Let us now return to the issue of what can be concluded from **McConkie & Yang**'s paradigm (in which the text disappears after a few milliseconds on some fixations). We are quite skeptical that the results obtained in this paradigm reflect normal processing activities during reading, and we suspect that people develop special strategies for dealing with this situation. Consider the following analogy: If you ride your bike down a certain road and a large dog jumps

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out from behind a bush, then we suspect that the next time (and every other time) you ride down that particular road you may do so much more carefully than you did previously; that is, you would adopt a strategy for dealing with the possibility of the lurking dog. Likewise, we suspect that readers adopt non-normal strategies when reading in Yang and McConkie's text-replacement paradigm, and that this partly explains why their conclusions are so much at odds with all the data discussed above.

This does not necessarily mean that conclusions from any study in which there is a display change can't be used to infer something about the process of normal reading. For example, in most of the moving-window and boundary experiments that we (and others) have conducted, readers are seldom aware of the display changes, and are therefore unlikely to adopt special strategies. Furthermore, in those moving-window paradigms in which readers are aware of the display changes (e.g., when all the letters outside of the moving window are replaced by X's), their behavior is approximately the same as when they are unaware of any such display changes. This leads us to speculate that display changes that are apparent to the reader, but which occur on every fixation (as in studies by Rayner et al. 1981), may be less disruptive than those that occur only occasionally. However, the more general point is that, when there is a display change, some argumentation and/or evidence is needed to provide assurance that the results can be generalized to normal reading.

Finally, we note that a recent experiment by Rayner et al. (2003b) involving a display change early in a fixation seems to provide compelling evidence that the cognitive activities associated with processing a word controls when the eyes move, even when the visual information has disappeared. In this experiment, each word that was fixated disappeared after 60 msec and did not reappear until the reader moved to another word. The situation is thus much like the Rayner et al. (1981) and Ishida and Ikeda (1989) studies in which a visual mask obscured the text after 50–60 msec of each fixation. Rayner et al. (2003b), like Rayner et al. (1981) and Ishida and Ikeda (1989), found that reading proceeded quite normally under this situation (i.e., reading rate in the disappearing-text condition did not differ from a control condition in which text was presented normally). Again, this result supports our argument that regular disruption of the text may adversely change the eye movement behavior of readers much less than periodic disruption of the text (as happens in Yang & McConkie's paradigm). Moreover, Rayner et al. (2003b) found that there was still a frequency effect when the fixated word disappeared. Therefore, although the fixated word was not visible after 60 msec, the reader's eyes remained in place longer when the word was low frequency than when the word was high frequency. (The size of the frequency effect was also identical in the disappearing-text and normal-text conditions.) These results appear to be rather good evidence for cognitive triggering of saccades. Therefore, given this and the other evidence discussed above, we continue to think that the basic idea that cognitive events trigger saccades in reading makes quite a bit of sense.

R6. Assumptions about saccadic programming

As stated in the target article, our model's assumptions regarding saccadic programming were largely motivated by the work of (1) Becker and Jürgens (1979), which distinguished between the early and late stages of saccadic programming and indicated the timing of these stages; (2) Morrison (1984), which incorporated the notion of parallel programming into a model of eye control in reading; (3) O'Regan (1990; 1992b) and Rayner (1979) on landing position effects, which suggested that saccades are directed towards the center of words; and (4) McConkie and his colleagues (McConkie et al. 1988; 1991), which examined the relationship between saccadic accuracy and length, and the way this relationship is affected by the duration of the fixation on the launch site. In implementing the model, we tried to be conservative. For example, we used parameter values that make the time needed to program saccades in our model relatively long (i.e., $M_1 + M_2 = 240$ msec), under the assumption that readers may be slightly slower programming their eyes in the context of natural reading than in the relatively simple tasks (e.g., the double-step paradigm used by Becker & Jürgens 1979) that have been used to determine the minimal saccadic latencies. Likewise, we adopted parameter values as suggested by the literature (e.g., a preferred saccade length of 7 character spaces; McConkie et al. 1991) even though these values would undoubtedly vary across different readers and materials (**De Graef & Germeys** seem to concur with this conjecture). And in response to **Doré-Mazars, Vergilino-Perez & Collins** (**Doré-Mazars et al.**), we have used these same metrics for all saccades – initial fixations into words as well as refixation within words.

Nevertheless, several of the commentators raise issues about the timing of events in our model, and, in particular, the timing of saccades. For example, **Findlay & White**, **Liversedge & White**, **Radach**, **Deubel & Heller** (**Radach et al.**), and **Sereno et al.** point out that, given our model's parameter values, there is seemingly very little time during which to complete the first stage of lexical processing on one word ($word_n$), initiate the labile stage of a saccadic program to move the eyes to $word_{n+1}$, complete the second stage of lexical processing on $word_n$, and then complete the first stage of lexical processing on $word_{n+1}$ so that the labile stage will – with the appropriate frequency – be canceled, causing $word_{n+1}$ to be skipped. Indeed, there is evidence that there isn't much time for cognitive operations to influence fixation durations as, for example, sometimes word-frequency effects go away on first fixation durations when there is no preview (Inhoff & Rayner 1986). Nevertheless, cognitive operations do influence fixation times on a word. Furthermore, the commentators seem to ignore the fact that the parameter values that determine the durations of these processes (i.e., L_1, L_2, M_1, and M_2) define their mean durations, and that the actual durations of each of these processes are quite variable, being distributed as gamma distributions having standard deviations equal to 18% of their means.

Osaka also objects to our assumption that the boundaries of an upcoming (parafoveal) word are used to direct the eyes to that word in our model. We did not wish to suggest that other variables are not important determinants of where the eyes move (as we stated in the introduction), and it may well be the case that word boundary information is not as important in some languages (such as Japanese) as it is in English. Instead, we simply wanted to provide an explanation that is reasonable given what we know about the rapid availability of word boundary information in the

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parafovea (McConkie & Rayner 1975; Morris et al. 1990; Pollatsek & Rayner 1982). Interestingly, though, **Inhoff & Shindler** report an experiment that was designed to test this assumption, and the results of this experiment do provide evidence that is consistent with the idea that word boundaries are important determinants of saccade targets during the reading of English.

Finally, **Beauvillain & Pouget** and **Sereno et al.** note that there is evidence that visual processing is enhanced at those spatial locations that are the intended targets of upcoming saccades (Reynolds et al. 2000; Yeshurun & Carrasco 1998). Our model is not inconsistent with these results; as we stated in the target article (see also sect. R2), we distinguish between two types of attention (based on Treisman 1969) – *input selection,* which is used to orient the oculomotor system during the planning of saccades, and *analyzer selection,* which facilitates lexical processing. The finding that visual processing is enhanced in saccade-target locations is readily handled by E-Z Reader 7 if one simply assumes that shifts of input selection enhance processing in the model's early visual-processing stage.

R7. Determinants of skipping

A number of the commentaries (**Brysbaert & Drieghe**, **Engbert & Kliegl**, **Kliegl & Engbert**, **Radach et al.**) deal with the issue of word skipping. They raise two general points. First, Brysbaert & Drieghe (see also **Vitu**) argue that the most important variable affecting word skipping is word length, and they further suggest that we don't adequately deal with word length in the E-Z Reader model. Second, Engbert & Kliegl, **Kliegl & Engbert**, and Radach et al. argue that a key prediction of E-Z Reader regarding skipping (i.e., that there should be some cost associated with skipping a word) is inconsistent with the empirical data. We will address these points in turn.

With respect to the issue of word length, we agree with **Brysbaert & Drieghe** that word length is a powerful variable with respect to word skipping. Indeed, an earlier paper by Rayner and McConkie (1976) showed quite clearly that as word length increased, the probability of fixating the word increased (and conversely, the probability of skipping it decreased). So, there is no argument there. However, Brysbaert & Drieghe criticize our model on the grounds that we don't explicitly include word length as a separate parameter in the model. As we indicated above, though, word length does have its own effect in the model because, the longer a word, the slower the processing of the component letters (a result of acuity limitations). Analogous to what we argued earlier (sect. R5) it isn't quite fair to focus on the 60% skipping rate difference between very high-frequency and very low-frequency words because this is clearly contrasting words of different lengths. When we look at words that are matched in length but vary in frequency, there are clear skipping effects (Rayner et al. 2003a; Schilling et al. 1998).

As with our comments about gaze duration in the prior section, we would like to point out that a few of the commentators (**Brysbaert & Drieghe**, **Feng)** seem to be under the impression that the only data that have been simulated are the global properties of the sentences used by Schilling et al. (1998). This is not correct. Not only was the model compared with the full set of words in the Schilling et al. sentences, comparisons were also made for specific target words (which varied on frequency) in the Schilling et al. sentences. Furthermore, in some very recent work (Rayner et al. 2003a), E-Z Reader was compared with human data in a study in which target words varied in frequency and predictability but were matched on word length (with lengths varying from 4 to 8 letters). Although the model had to be modified to account for the fixation times, both the original model and the modified model could account for the skipping data. Moreover, as noted in section R5, most of the evidence purporting to show that word frequency has little influence on skipping comes from correlational analyses in which it is impossible to separate the influence of the two variables.

With respect to the issue of fixation costs as a result of skipping, E-Z Reader does indeed predict that, when readers skip a word, the fixation on the word preceding the skip and/or the fixation on the word following the skip should be inflated compared with the fixations on those same words in the absence of skipping. Indeed, there wasn't originally any controversy (that we were aware of) with this issue because we (Pollatsek et al. 1986; Reichle et al. 1998) first reported these effects. More recently, though, Radach and Heller (2000) and Kliegl and Engbert (2003) reported small or nonexistent effects on the fixation prior to a skip (though in their commentary, **Kliegl & Engbert** note that they did find an increased fixation following a skip – as predicted by E-Z Reader). However, we (Rayner et al. 2003a) recently again found a 26-msec effect on the fixation prior to a skip, and Pynte et al. (in press) likewise reported a 43-msec effect on the fixation prior to a skip.

Kliegl & Engbert do have a point: that the size of the skipping-cost effect obtained in the empirical data is typically smaller than that predicted by E-Z Reader. However, there is a problem with negative findings on this point that is related to our earlier discussion about what can and cannot be inferred from correlational analyses. That is, all of these analyses (i.e., both the findings agreeing with E-Z Reader's predictions and those that do not) are correlational, in the sense that the experimenter does *not* control whether the reader skips or doesn't skip a given word. As a result, these data are gathered over different locations in the text, different readers, and readers in different states of alertness. This means that, in the ecology of natural reading, places where lexical processing is likely to proceed most rapidly (because the text is easier and/or the reader more efficient) will produce more skipping and shorter fixation times. This in turn means that, if there is no inherent causal relationship between skipping and lengthening of the preceding fixation, then one would expect that fixations that precede skips would be shorter than fixations that do not precede skips, and this would actually work against the model's prediction by producing the opposite relationship between the two variables. As a result, one would not expect consistent results on this issue because the model's predictions apply to the "ideal case," which has not been produced experimentally (and may not be possible to produce).

R8. Landing position effects

A number of the commentaries deal with landing-position effects. We have already addressed **Vitu**'s points above (see sect. R5). However, **Hyönä & Bertram**, **Liversedge & White**, and **Underwood** also raise this issue in their com-

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mentaries. And, although Underwood and Liversedge & White note that we relegate discussion of these findings to a note in our target article, we did not mean to suggest that this issue in not important; indeed, we have two comments that we would like to make regarding this issue.

First, we acknowledge that there is now quite a bit of data suggesting that the initial landing position in a word can be influenced by the orthographic characteristics of a parafoveal word. However, it should also be acknowledged that the sizes of these effects are often rather small. For example, in White and Liversedge (in press), the effect amounted to about one-fourth of a character space (i.e., the landing position when the next word begins with an orthographically illegal letter string is about one-fourth of a character space to the left of the landing position that is observed in the control condition). Frankly, some people find such small effects to be of only minor interest (although we do not agree with such an assessment, if the effects are reliable).

The second, and more critical, comment is simply that, in the most recent version of E-Z Reader, we envisioned that the low-level visual-processing stage we posited would be able to account for such effects. Although we did not implement the specific assumptions necessary to do this, we suspect that doing so would be a relatively trivial endeavor. Nevertheless, certain aspects of how this would be done need to be carefully considered. For example, although **Liversedge & White** focus on our suggestion that our model's early visual-processing stage might be sensitive to word shape considerations, we don't necessarily think that orthographical legality translates into word shape. Rather, the letters themselves would have to be processed at an early filtering stage.

R9. Determinants of refixations

Doré-Mazars et al., **Hyönä & Bertram**, and **Pacht** all raise questions about the model's ability to deal with refixations. We agree that this is perhaps the weakest part of the E-Z Reader model. This is clearly an aspect of reading that is difficult to model, as it's clear that many variables influence the pattern of refixations. As we noted earlier, there is considerable evidence that low-level factors, such as the location of the initial fixation, influence both the initial fixation duration on a word and whether the word is refixated. Higher-level factors, such as word frequency, also influence whether a word is refixated (Rayner & Pollatsek 1987). Our model attempts to explain how these factors influence refixation by positing automatic refixations that can be canceled by subsequent lexical processing (in a manner that is directly analogous to how the cancellation of previously programmed saccades produces skipping).

We think that the current version of the model gives a reasonably plausible account of some of the refixation data. What has become clear, though, is that word encoding during reading is considerably more complex than the relatively simple notion that two stages of word identification are the only processes that influence eye movements and (more generally) the speed with which the reader progresses through the text. As we pointed out in section R3, it is now clear that parts of words (e.g., their morphemes) influence the speed of cognitive processing during reading, as do other aspects of the word, such as the number of "neighbors" that it has and/or the frequency of these neighbors. It therefore seems clear that these factors (and others) need to be taken into account in explaining refixations. Therefore, it doesn't seem worthwhile to discuss in any detail whether we currently have an optimal account of how the first two factors (i.e., the initial fixation location and word frequency) influence refixations, because the data we have been trying to model are undoubtedly also influenced by many other factors (e.g., a word's neighbors) that our model simply ignores. Our attempt to explain refixations should definitely be viewed as work in progress.

R10. Higher-level processing and regressions

As we noted earlier, some commentators maintain that there is too much emphasis on cognitive events influencing eye movements in the E-Z Reader model, whereas others argue that not enough emphasis is put on such events influencing eye movements. **Liversedge & White, Murray**, **Perea & Carreiras**, and **Raney** all seem to be arguing for the latter. Indeed, Liversedge & White and Murray each point out that when readers read syntactically ambiguous sentences (so-called "garden path" sentences) that the fixation times are often inflated when the word that disambiguates the intended meaning of the sentence is first encountered. We can't disagree with this claim because the first such data were collected in our laboratory (Frazier & Rayner 1982; Rayner et al. 1983a). Furthermore, the data that have been collected in our lab (Schustack et al. 1987) clearly show that post-lexical integration processes can also affect the fixation times on a target word, and the data that Raney describes also seem consistent with higher-level processing influencing fixation times.

Let us therefore make three points relative to this issue. First, these higher-level effects sometimes show up immediately, but often, they are delayed (Frisson & Pickering 1999; Garrod & Terras 2000; Pickering & Frisson 2001) and in fact can sometimes be quite elusive in the eye movement record. Second, a recent modification of the E-Z Reader model (Rayner et al. 2003a) incorporates postlexical integration activities as a factor influencing fixation time, and this assumption was relatively easy to add into the model. Finally, the kinds of effects that arise from syntactic processing would be much more difficult to incorporate into the model since, as we argued above and in the target article, one would need to have a fully implemented model of parsing (including reanalysis procedures) and discourse processing, and we do not think a viable model of these operations is available yet.

This last point brings us to **Perea & Carreiras**. They clearly would like to have a fully implemented model of eye-movement control that includes both parsing and reanalysis procedures. We agree that this would be a worthy – but very difficult – goal. Perea & Carreiras then go on to distinguish between local (i.e., within-word) and global regressions. They do not really define what the latter are, but since they talk about long-range regressions, we assume they are referring to the type of regressions that occur when comprehension processes break down. Indeed, when readers are "garden-pathed" by syntactically ambiguous sentences, they are sometimes fairly accurate in moving their eyes back to that part of the text where parsing would have initially gone awry (see also **Kennedy**'s commentary). Frazier and Rayner (1982) first demonstrated this, though Perea & Car-

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reiras refer to a study by Mesenguer et al. (2002). They are mistaken, however, when they say that SWIFT (Engbert et al. 2002) can account for such global regressions. There is no parsing mechanism in SWIFT, and any regressions generated by the model are attributed to lexical processing. That said, SWIFT would probably overpredict the number of regressions, because the rate of regressions it currently predicts does not include those regressions that would be a result of comprehension problems. We reiterate that while it is a worthy goal to have a fully implemented model of parsing and discourse processing, such a model is not currently available and/or easily incorporated into the context of a model of eye-movement control.

R11. Generalizing, comparing, and evaluating models

The issue of how to evaluate theories and, more recently, computational models, has not been resolved (see Hintzman 1990; Jacobs & Grainger 1994; and the **Hanes & McCollum** and **Reilly & Radach** commentaries). The arguments about what constitutes a good model are complex, and are often philosophical in nature (Uttal 1990), so instead of rehashing those arguments here, we will instead simply say we agree with **Hyönä & Bertram**'s assessment that models should: (1) account for empirical data, (2) be conceptually transparent, (3) be psychologically and neurally plausible, and (4) generate novel predictions. We shall use these criteria to address the criticisms that were raised in the commentaries in the discussion that follows.

Feng disapproves of our method of fitting E-Z Reader to the Schilling et al. (1998) data. We believe his specific criticisms are unwarranted because his analysis is fundamentally flawed. In fitting our model to the data, for example, we normalized the differences between the observed and predicted fixation durations and the observed and predicted fixation probabilities so that both measures could be compared on a common scale. Although we did this using standard deviations rather than standard errors, we did not use the normalized scores to make statistical inferences but instead used them as an index to compare the relative performance of successive versions of our model. Feng also objects that, by fitting our model to aggregate data, our model unfairly takes advantage of multicolinearity, and hence accounts for the data only slightly better than simpler, statistical methods (e.g., principal components analysis using log frequency as a factor). Again, this criticism ignores the fact that our goal was not simply to account for as much of the variance in the data as possible; rather, our goal was to specify the cognitive operations that occur during reading and how these operations give rise to the data. The criticism also ignores the fact that our simulations were not limited to predicting means; our model accounts for other aspects of the data, including their distributions and a variety of other phenomena (e.g., spillover effects), a few of which are affected by dependencies between adjacent words (e.g., fixation costs due to skipping). Finally, this criticism overlooks the fact that our model was designed to account for the data while respecting a variety of constraints (e.g., visual acuity limitations, what is known about the time course and nature of saccadic programming, etc.).

Murray also voiced a concern that our model had not been tested on data sets other than those that had been used in developing the model (i.e., the sentences of Schilling et al. 1998). We hope this concern is lessened by the fact that **Sparrow**, **Miellet & Coello** (**Sparrow et al.**) have been reasonably successful using E-Z Reader to simulate the eye movements of native French readers who were reading their native tongue. Although these simulations did not fit the observed data perfectly, they were done using the same parameter values that were used to fit the Schilling et al. (1998) sentences, which provides an extremely conservative test of the model's adequacy because many of the model's parameters (e.g., β_1, β_2, and Ψ) would be expected to take on different values across subjects and languages. (This point regarding parameters is equally relevant to **Osaka**'s concern regarding the model's capacity to simulate the eye movements of native Japanese readers.) Two other facts are also noteworthy about the work of Sparrow et al. First, the model was fit to a passage of text rather than a set of unrelated sentences, which again shows the generality of our model. Second, the model was also fit to the data of dyslexic readers, which supports **Fischer**'s contention that the model may be useful for understanding individual differences in reading ability and the source(s) of reading impairment.

We should also mention (in response to **Murray**) that the model has been used with other materials and in other contexts. For example, as noted above, the model has been used with sentences containing word targets whose frequencies and predictabilities were orthogonally manipulated (Rayner et al. 2003a). It has also been used to interpret the patterns of eye movements observed when native readers of Finnish encounter compound words (Pollatsek et al. 2003; for a brief description of this work, see **Hyönä & Bertram**). And, as mentioned in our target article, the model has been used to simulate eye fixations on lexically ambiguous words (Duffy et al. 1988). In all three of these cases, the model was not used simply to fit the data but was instead used to evaluate the adequacy of other theoretical assumptions. Again, we cite these examples to show how E-Z Reader can be used as a framework for understanding phenomena that the model was not explicitly designed to explain.

With respect to the second issue, conceptual transparency, we again point out that the main advantage to our minimalist approach is its simplicity. In contrast to many of the models reviewed in the target article (e.g., Glenmore; Reader), E-Z Reader (1) can be described by a few principles, (2) can be represented by a simple box-and-arrow diagram, and (3) can be implemented using a small number of equations and parameters. We also reject **Reilly & Radach**'s notion that transparency is not important, and that its absence can be offset through the enhanced "biological realism" that comes from being implemented as a connectionist network. (**Raney**'s commentary strongly suggests that he also shares our opinion.)

We maintain that the biology of reading can be described at many different levels, and that, at the level of interacting neural systems, the best descriptions are functional ones. Other, more complicated descriptions simply confuse matters. To cite one example: **Shillcock et al.** suggest that our model is inadequate because it ignores the fact that printed words are identified by a visual processing system that is divided between the two cerebral hemispheres. This objection is – in our opinion – simply not warranted. As Shillcock et al. indicate, the fact that the visual field is divided into two hemi-fields is largely irrelevant in normal reading ex-

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cept in that it suggests why the eyes are directed towards the centers of words. Implementing this architectural constraint in our model would therefore do little to further our understanding of eye guidance during reading, and would probably obscure other, more important relationships. For similar reasons, we contend that – although E-Z Reader could be implemented as a connectionist model – doing so would make the model more difficult to understand and would probably do little to further our understanding of eye movements during reading.

With respect to the third criterion, plausibility, it is important to note that E-Z Reader was developed to be a theory of eye-movement behavior during reading. Consequently, we have placed a premium on what has been learned about eye movements during actual reading (for a review, see Rayner 1998) in designing the model. For example, the time needed to identify words (Rayner & Pollatsek 1989) and the timing and accuracy of saccades (Becker & Jürgens 1979; McConkie et al. 1988) have always constrained our model. Additional assumptions have been added only when they were needed to make the model more physiologically plausible (e.g., visual acuity limitations; Rayner & Morrison 1981). And, although previous versions of our model have been agnostic with respect to neural implementation, E-Z Reader 7 represents an attempt to map the functional components of our model onto the neural structures that have been implicated in reading. Although this mapping is admittedly a gross oversimplification (see the commentaries of **Beauvillain & Pouget**, **Sereno et al.**, and **Shillcock et al.**), we believe that it is precise enough to be falsifiable. Moreover, the mapping seems to have some theoretical utility. **Fischer,** for example, suggests that it may "serve as a model of dyslexia," and this conjecture is supported by the preliminary simulation results that were reported by **Sparrow et al.**

The last – and we would argue the most important – criterion for evaluating models is their capacity to generate novel predictions and guide new research. With respect to this issue, **Hyönä & Bertram** ask whether the model is "capable of producing novel predictions?" or whether it instead is "restricted to predicting only the effects that it is designed to simulate?" We would argue that our model can – and already has – generated new predictions. To cite one example acknowledged by Hyönä & Bertram: E-Z Reader predicted that the gaze durations on the words following skips should be inflated because of less parafoveal processing from the more distant launch site word. This finding was confirmed in the Schilling et al. (1998) data (see Note 1 of **Kliegl & Engbert**'s commentary).

Several of the commentaries also contain new predictions. **Engbert & Kliegl**'s analysis of word skipping in our model and SWIFT (Engbert et al. 2002) indicates that they make very different predictions regarding the causes of skipping: In E-Z Reader, skipping depends on the outcome of the "horse race" between the first stage of lexical processing and saccadic programming, whereas in SWIFT, skipping depends only on lexical processing. Similarly, **Hane & McCollum**'s analysis suggests that the basic distinction between L_1 and L_2 in our model has strong implications for the underlying neural architecture of the systems that guide eye movements during reading. Although neither prediction is specified in detail, we are confident they will ultimately guide new research. Finally, **Reingold**'s analysis of the factors that should selectively influence L_1 (e.g., stimulus quality; Borowsky & Besner 1993) has led to a prediction that can be tested: Visual degradation of word$_n$ should slow L_1 and hence increase the fixation duration on that word, but it should not affect the spillover effect observed on word$_{n+1}$. Although this prediction is based on both a specific interpretation of the distinction between L_1 and L_2 (that the former corresponds to orthographic processing; see our discussion in sect. R3) and the assumption that visual degradation will only affect very early processing (e.g., that it won't lead to misidentification of letters), its confirmation would complement the results described by **Raney** and thereby show that the two stages of lexical processing that are postulated in our model can be dissociated.

Finally, we would like to reply to some of the commentaries that have advocated that models of reading should be consistent with both basic eye-movement and neurophysiological data (e.g., **Beauvillain & Pouget**, **De Graef & Germeys**, **Osaka**, **Sereno et al.**, and **Shillcock et al.**). On some level, one cannot disagree with these prescriptions. However, we do think the specific points about how our model is found wanting point to dangers of applying such strictures. First, with respect to the claims about inconsistency with "basic" eye-movement data, virtually all of the claims rely on a hidden assumption, termed the "subtractive" method by Sternberg (1966; 1969; 1975). This assumption is, essentially, that in an apparently simple task, such as maintaining fixation and then moving one's eyes to the sudden onset of a stimulus, one is studying a processing module that is common to eye-movement control in general, and that a task like reading merely engages other, later processes that operate "on top of" the basic processes exposed by these simpler tasks. That is, the assumption is that the eye-movement parts of these nonreading tasks and reading are identical. While this assumption is, of course, logically possible, it is not a given and needs to be defended – at least by a serious task analysis of both tasks. Contrary to **Radach et al.**, for example, we are skeptical that the task of reading, which involves no sudden onsets of stimuli, no requirement to maintain fixation artificially, and no stimuli that require any serious cognitive analysis, is a candidate for literal extrapolations of findings from experiments of this kind, especially with respect to the exact timing that various cognitive processes require (e.g., attention shifts).

The point we would like to make about the relationship between a model of reading and neurophysiological data involves both a similar point and a different one. First, there is a similarity to the point made about task analysis; that is, in most neurophysiological research (e.g., ERP experiments), one typically examines word identification in isolation and then extrapolates from this to the reading of text. Clearly, this should be done with some caution. Perhaps more important, however, is the point that one can't take such neurophysiological findings as being written on stone tablets. For example, **Radach et al.** asserted that ERP data indicating that two curves do not significantly deviate before 130 msec can be used to infer that lexical analysis does not begin until then. This clearly assumes the technique is flawless and that this is a direct "window" into all the relevant neurological structures that underlie word identification – an assumption we find quite hard to take seriously.

More generally, we think such comments seriously misgauge the state of the art in both cognitive psychology and cognitive neuroscience. That is, they seem to be based on an assumption that we really understand all of the "atoms"

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of the cognitive process and have carefully measured all of their relevant properties, and that this information is now ready to be put together into something like a "periodic table." Instead, we would like to suggest that we are still groping around in the dark on many basic issues – for example, after 40–50 years of research we still do not fully understand how people recognize words or objects – and that many of the suggestions that we standardize the way we model (e.g., **Reilly & Radach**) are quite misguided. In fact, we would like to invite those who think otherwise (e.g., **Sereno et al.**) to develop a fully implemented model of eye-movement control that is both neurophysiologically based and neurophysiologically plausible.

R12. Conclusion

In closing, we would like first to restate that we fully acknowledge that our model, E-Z Reader, is not a complete model of reading. Our minimalist approach to modeling will (by definition) insure that whatever model we end up developing will grossly oversimplify the cognitive processes that determine when and where the eyes move during reading. However, we also believe that this is a modest price to pay for having a model that is transparent enough to be used as a heuristic for guiding research. And, as witnessed by both the number of responses to our target article and much of the content of those responses (especially the predictions that were discussed in sect. R11), the E-Z Reader model has already clearly demonstrated its value in this regard.

We would also like to note that – in our opinion – the bulk of the responses to our target article were valuable in at least two ways. First, they provided us with the opportunity to clarify points in the description of our model that may have been incomplete or otherwise vague. Second, and more important, is that the dialogue further clarified many of the issues that are currently the subject of (sometimes heated) debate within the area of reading research. This process forced us to rethink certain aspects of our model, and, in so doing, has already led us to modify E-Z Reader 7. For example, as outlined at the outset of this response, **Finday & White**'s analysis of the time course of lexical processing forced us to re-implement the early stage of visual processing in our model so that visual processing needs to be initiated with each new fixation. (As noted earlier, this modification did not adversely affect the model's performance, and had the desired effect of reducing the overall size of the parafoveal preview benefit to approximately half the value we reported in the target article.) Clearly, we expect that E-Z Reader will continue to develop as our understanding of the determinants of eye movement control in reading continues to grow.

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Letters "a" and "r" appearing before authors' initials refer to target article and response, respectively.

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