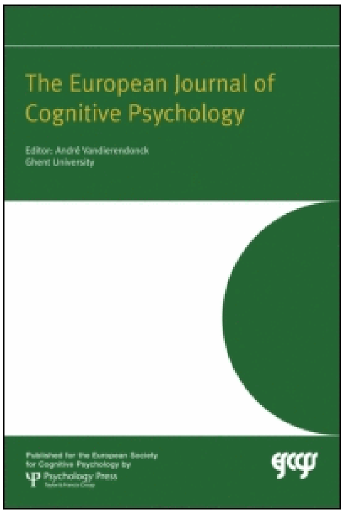

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Orthographic effects in rhyme monitoring tasks: Are they automatic?

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Orthographic effects in rhyme monitoring tasks: Are they automatic?

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Over the last 30 years or so, various findings have been reported which suggest that the perception of spoken words may involve the automatic coactivation of orthographic properties. Here we assessed this possibility in auditory rhyme judgement tasks and replicated a classic finding reported by Seidenberg and Tanenhaus (1979), showing that orthographic similarity between stimuli facilitated responses on rhyming pairs, but had the opposite effect on nonrhyming pairs. However, Experiments 2 and 3 showed that manipulating the nature of the nonrhymes, or adding a large proportion of filler items, eliminated the effects of orthographic match or mismatch. These findings suggest the involvement of strategic factors in the emergence of orthographic effects in rhyme judgement tasks.

Keywords: Speech perception; Orthographic effects; Rhyme monitoring.

In psycholinguistic research, the issue of how various subsystems involved in language (i.e., semantic, syntactic, phonological, orthographic) interact in any given language task has been one of the dominating themes. In the case of reading, the interaction between orthography and phonology is relatively well-established (see, for instance, various masked priming studies which provide evidence of fast and automatic activation of phonology from print, e.g., Rastle & Brysbaert, 2006), and there is growing evidence that activated phonology feeds back onto orthography prior to written word identification (e.g., Pexman, Lupker, & Reggin, 2002). The reverse case, namely the potential role of phonological–orthographic interactions in speech perception/comprehension, is much less studied. Nevertheless, over the last 30 or so years, a number of articles provide a growing body of evidence of orthographic influences on the perception of spoken words (e.g., Chéreau, Gaskell, &

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Dumay, 2007; Dijkstra, Roelofs, & Fieuws, 1995; Donnenwerth-Nolan, Tanenhaus, & Seidenberg, 1981; Hallé, Chéreau, & Segui, 2000; Jakimik, Cole, & Rudnicky, 1985; Muneaux & Ziegler, 2004; Racine & Grosjean, 2005; Seidenberg & Tanenhaus, 1979; Taft, Castles, Davis, Lazendic, & Nguyen-Hoan, 2008; Ziegler & Ferrand, 1998; Ziegler, Ferrand, & Montant, 2004; see also Pattamadilok, Perre, Dufau, & Ziegler, 2009; Perre, Midgley & Ziegler, in press; Perre & Ziegler, 2008, for evidence derived from electrophy siological measures). These results suggesting that interactions between the orthographic and the phonological system are quite general and robust.

One of the earliest and most influential set of studies to support the claim that orthographic access may be a mandatory and automatic component of lexical access in spoken perception was reported by Seidenberg and Tanenhaus (1979) in rhyme judgement tasks. They presented participants with a visual or auditory cue word, followed by a list of five auditory target words. Participants pressed a button as soon as they detected a word that rhymed with the cue. Perhaps not surprisingly, the results showed that with visual presentation of the cue word, responses were faster when cue and target were orthographically similar (stroke-joke) than when not (stroke-soak). More importantly, however, a similar pattern was found with auditory presentation of the cue. In a further experiment, using a simpler procedure, they asked participants to perform rhyme judgements on word pairs that were either visually or auditorily presented. Both rhyming and nonrhyming pairs could be either orthographically similar (dune-tune; tease-lease) or dissimilar (dune-moon; tease-piece). For the rhymes, aver ages were 779 ms for orthographically similar pairs, and 878 ms for dissimilar pairs, hence similar word pairs were judged 99 ms faster than dissimilar pairs. For the nonrhymes, averages were 961 ms for orthographically similar pairs, and 903 ms for dissimilar pairs, hence similar pairs were judged 58 ms slower than dissimilar ones. This pattern was reflected in a significant interaction between rhyme status and orthographic similarity. Although Seidenberg and Tanenhaus did not report simple effects analyses on this interaction, they provided means for individual items (Table 3) and noted that the obtained pattern held more consistently for rhymes than for nonrhymes, which according to the authors may be attributable to the fact that items were rotated across two experimental lists. The authors concluded that "both visual and auditory stimuli may be encoded in terms of both visual and auditory features ... auditory encoding does not always occur to the exclusion of visual information" (p. 554).

Seidenberg and Tanenhaus's (1979) findings had a major impact on current thinking about how spoken perception works: The article is highly cited (as of the date of writing, the ISI Web of Science lists 138 citations), and the results are regularly described in major textbooks on psycholinguistics (e.g., Caplan, 1996). The findings are impressive because the empirical effects are substantial and the method, particularly the rhyme judgement performed on word pairs, is straightforward (certainly a lot simpler than some of the more recently published articles that have looked at the issue). Curiously, however, we know of no published replication of their findings. This is regrettable as some of the design features of the original study were perhaps less than ideal. First, by completely crossing phonological and orthographic relatedness in their experiments, and due to the fact that in English spelling and sound are highly interrelated, the nonrhyme word pairs (e.g., tease-lease) are still relatively related. This rendered rhyme decisions quite difficult, and may have directed participants' attention to other stimulus dimensions such as orthography. Second, no filler items were included, which may render the manipulation relatively transparent to participants. Each of these factors (and perhaps both) may have helped in generating influences of orthography that may not be genuine to spoken comprehension per se.

Indeed, in the literature there is some evidence that a salient orthographic manipulation can induce sensitivity to orthographic variables that is not present when the manipulation is less obvious. For instance, Cutler, Treiman, and van Ooijen (1998) showed that in a phoneme monitoring task performed on English words, word-initial target sounds which have consistent spelling (/b/, /m/, /t/) were detected faster than targets that are inconsistently spelled (/f/, /s/, /k/). Crucially, however, this was only the case when many irregularly spelled filler items drew participants' attention towards orthographic properties, but not otherwise. Although these findings do not rule out the existence of genuine orthographic effects in speech perception, they certainly warrant caution with regard to experiments in which the orthographic manipulation may have been so salient as to possibly induce strategic effects.

In the experiment presented here, we initially aimed at capturing the pattern of rhyme judgement results reported in Seidenberg and Tanenhaus (1979), and subsequently we modified various aspects of the design in an attempt to reduce the likelihood of any strategic effects. The original results reported by Seidenberg and Tanenhaus should be interpreted as reflecting automatic access to orthography in speech perception only if the effects are robust to these changes.

EXPERIMENT 1

The first experiment attempts to duplicate Seidenberg and Tanenhaus's (1979) Experiment 3 in all important aspects such as stimulus selection, procedure, conditions, number of trials, etc. We simply aimed to determine whether their results can easily be replicated.

Method

Participants. Sixteen undergraduate students at the University of Bristol, all of them native English speakers, participated in this experiment for course credit.

Materials and design. Stimuli were taken from Seidenberg and Tanenhaus' (1979) Experiment 3 (see Appendix) and consisted of 28 monosyllabic word triplets, each with one target, and two cue words. In 14 triplets, cues and targets rhymed, but only one cue was orthographically similar to the target (moon-tune; target: dune). Median spoken Celex frequencies (Baayen, Piepenbrock, & Gulikers, 1995) for targets and orthographically similar and dissimilar cues were 7, 16, and 13 per million (18, 32, and 41 per million median written Celex frequencies). The two types of cues were somewhat, but not perfectly, matched regarding the degree of phonological overlap with the target, 2.14 phonemes and 1.93 phonemes for similar and dissimilar items ($p = .082$). In the other 14 triplets, cues and targets did not rhyme, but again one cue was orthographically similar to the target (piece-lease-tease). Median spoken frequencies for targets and orthographically similar and dissimilar cues were 15, 26, and 47 per million (23, 45, and 70 per million median written frequencies). The two types of cues had phonological overlap with the target of 1.04 and 1.00 phonemes for similar and dissimilar items ($p = .336$).

From these materials two lists were created such that orthographic similarity was counterbalanced across participants. For the analyses of variance, list was entered as a dummy variable to reduce the estimate of random variation (see Pollatsek & Well, 1995). Hence, effects involving the list variable will not be reported.

Apparatus. Stimuli were presented from an IBM-compatible computer. Cues and targets were recorded by a female speaker, digitised with a sampling frequency of 16 kHz, and presented to participants at a comfortable volume level over Sennheiser HD450 headphones.

Procedure. Participants were tested individually. They were first instructed as to the nature of the task, and were presented with six practice trials on three of which cue and target rhymed. Subsequently the experimental items were presented in a single block. A new random sequence of trials was generated for each participant. On half of the trials, cues rhymed with the target; on the other half, they did not. Within each group, half of the cues and targets were orthographically similar, and the other half were not. The entire testing session consisted of 28 trials, and took approximately 10 min.

On each trial, participants heard a cue word presented over headphones, followed after 2000 ms by a target word. Participants pressed one of the two shift keys on the computer keyboard if the two words rhymed, and the other shift key otherwise. Response keys were counterbalanced within each list. Each trial was followed by a 2000 ms intertrial interval.

Results and discussion

One item triplet was excluded from analysis because in British English pronunciation, the target does not rhyme with one of the cues (lance-dance-pants). Response times on trials on which participants had made errors (5.1%), as well as latencies longer than 2000 ms or shorter than 200 ms (1.2%) were excluded from the response time analysis. Average response times and errors are shown in Table 1, showing a substantial facilitatory effect of orthographic similarity on rhyme pairs, and a smaller inhibitory effect on nonrhyme pairs. Analyses of variance (ANOVAS) conducted on latencies, with the factors rhyme (rhyme vs. nonrhyme) as a within-participants and between-items variable, and orthography (similar vs. dissimilar) as a within-participants and -items variable, showed a main effect of rhyme, $F1(1, 14) = 4.28$, $p = .058$; $F2(1, 23) = 7.64$, $p = .011$, with rhyme responses 90 ms faster than nonrhyme ones. The effect of orthography approached marginal significance in the analysis by participants, $F1(1, 14) = 2.79$, $p = .117$, and was significant by items, $F2(1, 23) = 6.68$, $p = .017$, with similar responses 38 ms faster than dissimilar ones. The interaction between rhyme and orthography was highly significant, $F1(1, 14) = 12.66$, $p = .003$; $F2(1, 23) = 11.69$, $p = .002$. Simple effects of the factor orthography, performed separately for each level of the factor rhyme, showed that for rhyme pairs, orthography exhibited a significant

TABLE 1
Response latencies (error percentages in brackets) for Experiments 1–3

	Orthographically similar	*Orthographically dissimilar*	*Difference*
Experiment 1			
Rhyme	838 (0.9)	938 (9.7)	+100 (+8.9)
Nonrhyme	990 (2.7)	965 (7.1)	−25 (+4.4)
Experiment 2			
Rhyme	980 (3.6)	996 (4.9)	+16 (+1.3)
Experiment 3			
Rhyme	912 (6.4)	911 (3.9)	−1 (+2.5)
Nonrhyme	977 (8.6)	987 (6.4)	+10 (+2.2)

effect, $F1(1, 14) = 14.81$, $p = .002$; $F2(1, 25) = 4.79$, $p = .038$, but this was not the case for nonrhyme pairs, $F1$ and $F2 < 1$.

Parallel analyses conduced on the errors showed no effect of rhyme, $F1$ and $F2 < 1$, but an effect of orthography, $F1(1, 14) = 6.40$, $p = .024$; $F2(1, 23) = 9.75$, $p = .005$, with similar items 6.6% less errors than dissimilar ones. Here, rhyme and orthography did not interact, $F1 = 1.20$, $p = .292$; $F2 < 1$.

In sum, the results largely replicate those reported by Seidenberg and Tanenhaus. On rhyming trials, orthographic similarity has a substantial facilitatory effect, whereas on nonrhyming trials, a smaller effect in the opposite direction was found. As outlined in the introduction, Seidenberg and Tanenhaus (1979) did not report inferential tests of the effects of similarity for rhymes and nonrhymes separately; they did, however, note that their effect appeared more consistent for rhymes than for nonrhymes (11 out of 14 rhyme pairs showed a facilitatory effect of orthographic similarity, but only 8 out of 14 nonrhyme pairs showed an inhibitory effect of similarity). This pattern generally agrees with our results: In our data, 14 out of 14 rhyming pairs showed the predicted facilitatory effect of orthographic similarity, but only 6 out of 14 nonrhymes showed the inhibitory effect visible in the means.

We additionally found an effect of orthography on errors, which, however, contrary to the latency results, did not interact with the rhyme variable. The overall error rate in Seidenberg and Tanenhaus (1979) was 3.0%, which according to the authors was distributed approximately evenly across the conditions; however, the authors did not report an inferential analyses of the errors. In our own results, the absence of an interaction in the errors is to some degree counterintuitive: Encountering a nonrhyme pair which is orthographically similar (foot-toot) should hypothetically have resulted in a higher chance of making an error than an orthographically dissimilar pair (foot-suit). Why the opposite is the case is at present unclear. Nevertheless, the latency results capture the findings reported by Seidenberg and Tanenhaus rather well.

In Seidenberg and Tanenhaus's original experiment (1979) as well as in Experiment 1 here, the variables rhyme and orthography were fully crossed. As a result, half of the nonrhyme pairs were orthographically similar (e.g., leaf-deaf; tease-lease). This may have served to direct participants' attention to the manipulation of orthographic relatedness, and potentially invoked strategic effects. In the second experiment, we obscured the orthographic manipulation by focusing exclusively on the "rhyme" condition that had produced a significant effect of orthography in Experiment 1. To this aim, we now treated nonrhymes as fillers, and made cues and targets in this condition quite distinct. If the claim holds that orthographic properties of spoken words are automatically evoked, then the facilitatory effect of orthographic match on rhymes should be unaffected by the properties of the nonrhymes.

EXPERIMENT 2

Method

Participants. Twenty undergraduate students, none of whom had been in the first experiment, participated in this experiment for course credit.

Materials, design, and procedure. For trials that had previously been in the nonrhyme condition, we randomly combined targets with cues such that they were always phonologically and orthographically unrelated, and hence now constituted filler items that were not further analysed. Consequently, only the variable orthography was included in the design, but the variable rhyme was not. All other procedural and design aspects were identical to the first experiment.

Results and discussion

Responses on error trials (4.3%) and those longer than 2000 ms or shorter than 200 ms (1.3%) were excluded. Table 1 shows the results, exhibiting only a very weak effect of orthography. ANOVAs with the factor orthography showed no significant results on either latencies or errors, $F1$ and $F2 < 1$.

The fact that the orthographic effect on the rhyme trials almost entirely disappears is certainly surprising. However, it could be argued that elimination of the original nonrhyme items which were relatively similar in phonological properties, may have made the task very easy: Participants could have focused on relatively "shallow" form properties of the items (e.g., determining whether the central vowel matches or mismatches), and carried out their decision without proper lexical access. If this was the case, however, overall response latencies of the second experiment should have been faster than the first one. The fact that overall speed is quite similar gives no reason to suspect that the two experiments could have differed in overall difficulty.

In the next experiment we reverted back to the full design of Experiment 1 (i.e., phonology and orthography were fully crossed), but added a large number of filler trials. Half of the filler pairs were phonologically and orthographically related, and half were unrelated. The addition of the filler trials should aid in obscuring the properties of the critical items (see, e.g., Radeau, Morais, & Dewier, 1989, for the use of fillers in phonological tasks in order to prevent the involvement of strategic factors).

EXPERIMENT 3

Method

Participants. Twenty undergraduate students, none of whom had been in the first two experiments, participated in this experiment for course credit.

Materials, procedure, and design. The materials and design were identical to the first experiment. However, we added a further 64 randomly interspersed filler trials. On half of these, cue and target rhymed and were orthographically similar; on the other half, they were dissimilar. The entire experiment hence consisted of 92 trials, and took approximately 15 min to administer.

Results and discussion

Responses on error trials (6.3%) and those longer than 2000 ms or shorter than 200 ms (3.8%) were excluded. Table 1 shows the results. An ANOVA showed a main effect of rhyme, $F1(1, 18) = 9.70$, $p = .006$; $F2(1, 23) = 3.89$, $p = .061$, with rhyming responses 71 ms faster than nonrhyming responses. Orthography was not significant, $F1 < 1$; $F2 = 1.56$, $p = .220$, and neither was the interaction between rhyme and orthography, $F1$ and $F2 < 1$. An analysis on the errors showed that neither rhyme, $F1 = 1.43$, $p = .247$; $F2 < 1$, nor orthography, $F1 = 1.40$, $p = .252$; $F2 = 1.22$, $p = .282$, nor the interaction between them were significant, $F1$ and $F2 < 1$. These results show that simply adding a large number of filler trials to the critical items eliminates the effect of orthography altogether.

GENERAL DISCUSSION

Three experiments investigated orthographic effects in auditory rhyme judgement tasks. Our results show that although it is possible to replicate the orthographic effects originally reported in Seidenberg and Tanenhaus (1979), these tend to be dependent on a specific form of the experiment: When nonrhyme stimuli are orthographically and phonologically similar (as in Seidenberg & Tanenhaus, 1979, and Experiment 1 here), orthographic overlap has an effect on latencies, but when they are not (as in Experiment 2 here) the effects of orthography disappear. Likewise, if the original items are embedded within a large number of filler items, orthographic effects cannot reliably be obtained. These results warrant scepticism about an account of the original results in terms of auditory encoding involving the automatic activation of spelling properties.

Error percentages were 5.1% in Experiment 1, 4.3% in Experiment 2, and 6.3% in Experiment 3. These are slightly higher than the overall error rate of 3.0% reported in Seidenberg and Tanenhaus's original study (1979). Given that the task involves a judgement on spoken stimuli, such differences in accuracy in across the studies could have arisen from variations in how clearly the stimuli were pronounced by the speaker, in the quality of the audio recordings themselves, or in the quality of auditory reproduction during the experimental session.

Of course, given the growing body of evidence suggesting this type of effect (see introduction), our data should not be taken as evidence against the claim that genuine orthographic effects in speech perception exist. And indeed, we would not want to interpret the results as calling into question the more general claim that orthography can affect spoken perception. However, they certainly warrant caution about the particular set of results on which this claim was originally based, and furthermore highlight the need for an explicit investigation of the possibility of strategic effects in experiments of this type. Specifically with regard to the rhyme judgement task, it is now clear that the required explicit comparison between pairs of words invites various matching strategies, and so this task is not ideally suited for the investigation of automatic processes. Alternative procedures which have more recently been used in the literature and which are largely immune to strategic factors are hence clearly preferable, and the existing evidence is strong that speech perception is constrained by orthographic variables. For instance, Chéreau et al. (2007) used rhyme primes that could be orthographically similar or dissimilar to subsequent targets. However, participants did not explicitly compare prime and target, but instead were simply asked to ignore the prime and to perform a lexical decision on the target. A large proportion of filler items was included. Reliable facilitatory orthographic priming effects were obtained, which are very unlikely to be the result of response strategies such as those we suggest are present in auditory rhyme judgements. Additionally, a recent electrophysiological study using the task by Chéreau et al. (Perre et al., in press) suggests an early and nonstrategic locus of the orthographic effect.

It is worth noting that in spoken production the possible role of orthographic codes is much less studied than in perception. Even the limited set of data available at present suggests that reliable orthographic effects are difficult to obtain. For instance, Damian and Bowers (2003) used a form preparation paradigm in which a small number of responses, typically elicited by prompt words, was produced repeatedly within an experimental block, and the presence or absence of form overlap between the responses was manipulated. They demonstrated a reliable priming effect in the homogeneous condition in which all response words shared initial sound and spelling, but no such priming effect in an inconsistent condition in which all response words shared initial sound, but differed in spelling. These results were interpreted as supporting the possibility that spoken word production involves parallel orthographic activation (see also Gaskell, Cox, Foley, Grieve, & O'Brien, 2003, for related evidence from a different task). However, subsequent studies with the form preparation paradigm, but conducted with Dutch (Roelofs, 2006) and French (Alario, Perre, Castel, & Ziegler, 2007) speakers failed to replicate the originally reported effect. It is presently unresolved which variables underlie the divergent results, but it is clear that it is more difficult to obtain orthographic effects in speaking than originally envisaged.

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APPENDIX

Stimuli used in Experiments 1–3 (from Seidenberg & Tanenhaus, 1979)

	Rhyme primes			*Nonrhyme primes*	
Target	*Similar*	*Dissimilar*	*Target*	*Similar*	*Dissimilar*
cure	pure	tour	base	phase	raise
curt	hurt	dirt	bash	wash	gosh
dune	tune	moon	bead	dead	fed
fad	glad	plaid	bomb	tomb	room
fate	mate	freight	cough	tough	stuff
fox	box	rocks	foot	toot	suit
glue	clue	crew	goose	choose	cues
lance	dance	pants	gown	blown	moan
loose	goose	juice	hood	mood	rude
ride	hide	guide	howl	bowl	roll
tie	Pie	guy	leaf	deaf	ref
toe	foe	row	pose	lose	Jews
turn	burn	learn	tease	lease	piece
wise	rise	lies	ward	card	guard