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Priming and Attentional Control of Lexical and Sublexical Pathways in Naming: A Reevaluation

Sachiko Kinoshita
Macquarie University

Stephen J. Lupker
University of Western Ontario

The authors report 3 naming experiments using J. D. Zevin and D. A. Balota's (2000) multiple prime manipulation. They used 2 sets of nonword primes (fast and slow) and low-frequency exception word primes to separate the effects of prime speed from those of prime type. The size of the regularity effect was unaffected by prime type. Relative to the low-frequency exception word prime condition, the frequency effect was reduced in the fast, but not in the slow, nonword prime condition. Lexicality effect size was reduced in both nonword prime conditions, a result consistent with the lexical checking strategy described by S. J. Lupker, P. Brown, and L. Colombo (1997). The authors suggest that these results are better explained in terms of S. J. Lupker et al.'s time-criterion account than J. D. Zevin and D. A. Balota's pathway control hypothesis.

An issue that is currently much debated in the field of visual word recognition is the question of whether it is possible to strategically control the processes involved in reading aloud (Lupker, Brown, & Colombo, 1997; Monsell, Patterson, Graham, Hughes, & Milroy, 1992; Zevin & Balota, 2000). This question is typically asked within the framework of dual-route models (Coltheart, 1978; Coltheart, Curtis, Atkins, & Haller, 1993; Coltheart, Rastle, Perry, Langdon, & Ziegler, 2001; Paap & Noel, 1991). These types of models postulate that readers have at their disposal two ways of generating the pronunciation of a printed letter string. One way, referred to as the "lexical route," involves the retrieval of whole-word phonology stored in the phonological output lexicon (which is accessed via connections from the orthographic input lexicon). The second way, referred to as the "nonlexical route," computes phonology by means of application of grapheme-phoneme correspondence (GPC) rules. Exception words, words with pronunciations that do not follow the regular GPC rules (e.g., *pint, yacht*), can be read aloud correctly only via the lexical route, whereas nonwords (e.g., *slint*), which are not represented in the orthographic input lexicon, can only be read aloud via the nonlexical route.

The pathway control hypothesis states that the relative reliance on these two routes for generating phonology can be changed strategically to meet task demands. The main technique used to study this hypothesis is to have readers name a selected set of target items in two (or more) context conditions. This is typically accomplished by manipulating the nature of the other ("filler") stimuli in the stimulus list. The assumption is that when a list is composed primarily of exception words, this should encourage reliance on the lexical route, whereas a list composed primarily of nonwords should encourage reliance on the nonlexical route (e.g., Coltheart, 1978). Predicted changes in naming latency (and error rates) for target items as a function of list composition have been taken as support for the pathway control hypothesis (e.g., Baluch & Besner, 1991; Monsell et al., 1992; Rastle & Coltheart, 1999; Simpson & Kang, 1994).

More recently, however, Lupker and colleagues (Chateau & Lupker, in press; Kinoshita & Lupker, in press; Lupker et al.,1997; Taylor & Lupker, 2001) have proposed an alternative interpretation of these list composition effects. According to their flexible time-criterion account, readers do not always initiate articulation as soon as they are able. Instead, they adopt a flexible time criterion such that the articulatory program for the stimulus may be allowed to develop beyond the point where the execution of the program could start, or its execution may be initiated when the program has not yet been fully developed. What primarily determines the position of the time criterion is the naming difficulty of items in a trial block: When a block consists mainly of easy ("fast") items, the time criterion would be set lower than in a block of mainly difficult ("slow") items. Hence, an item would be responded to faster when mixed with fast stimuli than when that same item was mixed with slower stimuli. The time-criterion account, therefore, explains list composition effects in terms of the speed with which the context stimuli within a list are named, irrespective of which pathway (lexical or nonlexical) they engage.

The general question addressed in the present research is which of the pathway control hypothesis or the time-criterion account provides a better account of list composition effects. We begin with a survey of the existing literature. In the interest of brevity, we limit our review to studies that examined the modulation of

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Sachiko Kinoshita, Macquarie Centre for Cognitive Science and Department of Psychology, Macquarie University, Sydney, Australia; Stephen J. Lupker, Department of Psychology, University of Western Ontario, London, Ontario, Canada.

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Correspondence concerning this article should be addressed to Sachiko Kinoshita, Macquarie Centre for Cognitive Science, Macquarie University, Sydney, New South Wales 2109, Australia, or to Stephen J. Lupker, Department of Psychology, University of Western Ontario, London, Ontario N6A 5C2, Canada. E-mail: sachiko.kinoshita@mq.edu.au or lupker@uwo.ca

target type effects (e.g., regularity effects, frequency effects) as a function of filler type. For a review of studies that compared absolute performance for a single type of target in different types of filler blocks (e.g., Monsell et al., 1992; Rastle & Coltheart, 1999), we refer the readers to Lupker et al. (1997) and Chateau and Lupker (in press).

Modulation of Effect Size as a Function of List Composition

The standard manipulation used to study pathway control is to include different types of fillers (e.g., nonwords, exception words) in blocks containing different target types (e.g., regular vs. exception words) and to examine the modulation of the size of a selected target type effect. The target types used in these experiments are chosen so that the difference in their latencies reflects different relative contributions of the lexical and nonlexical routes. As a result, the pathway control hypothesis would predict a modulation in the size of a target type effect as a function of filler type. In contrast, in general (but with some qualifications to be discussed later), the time-criterion account predicts a main effect of filler difficulty on target latencies, but no modulation in the size of the target type effects. We discuss three different target type effects that have been examined this way: the regularity effect, the frequency effect, and the lexicality effect.[1]

Regularity Effect

The regularity effect refers to the finding that exception words (e.g., *pint*) are read aloud more slowly than regular words (e.g., *pink*), that is, those words whose pronunciation can be correctly generated using GPC rules. This effect is generally restricted to low-frequency words (e.g., Seidenberg, Waters, Barnes, & Tanenhaus, 1984; although see Jared, 1997). According to dual-route models, the regularity effect arises because the nonlexical route generates the incorrect "regularized" pronunciation for low-frequency exception words more rapidly than the lexical route can generate the correct pronunciation for those words. As a result, there is a conflict between the lexical and nonlexical pronunciations. Resolving this conflict takes time; hence, naming of low-frequency exception words is delayed. The pathway control hypothesis, therefore, predicts that the regularity effect for low-frequency words should be larger when greater reliance is put on the nonlexical pathway, that is, when the fillers are nonwords in comparison to exception words. In contrast, as stated previously, the time-criterion account predicts only a main effect of filler speed and no modulation in the size of the regularity effect.

To date, most studies reported have not found that the size of the regularity effect for low-frequency words can be modulated as a function of filler type (Coltheart & Rastle, 1994; Kinoshita & Lupker, in press; Woollams & Kinoshita, 1997). Coltheart and Rastle compared low-frequency, disyllabic regular and exception targets (e.g., *comic*, *chaos*) when named in the context of either high-frequency exception word fillers or nonword fillers. Kinoshita and Lupker used low-frequency regular and exception targets with low-frequency exception words, low-frequency regular words, or nonword fillers. Woollams and Kinoshita used Coltheart and Rastle's target words along with low-frequency exception word fillers or nonword fillers. In all of these studies, while both main effects of regularity and filler type were found, no interaction was observed between the two factors. (Content & Peereman, 1992, did report that the size of the regularity effect for high-frequency French word targets was larger with nonword fillers than with high-frequency word fillers. However, they did not find a parallel result for low-frequency French word targets.) This set of results is, therefore, quite consistent with the time-criterion account.

There is one study, however, by Zevin and Balota (2000, Experiment 2), that showed the predicted interaction. In this experiment, disyllabic and monosyllabic low-frequency regular and exception words (e.g., *fennel*, *hospice*) were presented following a series of either low-frequency exception word primes or nonword primes. That is, rather than using fillers in a block of stimuli and presenting the stimuli randomly, the context was manipulated by presenting each target stimulus following five primes of a particular type. The regularity effect (in latency) was larger, and the percentage of errors for exception targets that were regularization errors increased in the context of nonword primes. This finding, therefore, constitutes the only real support for the pathway control hypothesis in the studies that have examined the modulation of regularity effects.

Frequency Effect

According to dual-route models, the frequency effect is assumed to be a lexical effect, reflecting the faster retrieval of whole-word pronunciations from the phonological output lexicon for high-frequency words than for low-frequency words. Therefore, the pathway control hypothesis predicts a diminished effect of frequency when the nonlexical route is emphasized. The time-criterion account, on the other hand, in principle, predicts no modulation. In contrast to the studies manipulating the regularity effect reviewed in the previous section, a number of studies examining the modulation of the frequency effect have reported results consistent with the pathway control prediction.

Simpson and Kang (1994; see also Kang and Simpson, 2001, Experiment 1), for example, embedded high- and low-frequency Korean Hangul words in three different types of fillers: Hanza words (Chinese ideographs that do not have one-to-one mappings between sublexical orthography and phonology), Hangul words and Hangul nonwords. (Hangul is a script having one-to-one character-to-sound mappings.) A large (60-ms) frequency effect

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[1] One other effect, the imageability effect (faster response to highly imageable words, for example, *ghost* than to low-imageable words, for example, *guise*), was investigated by Zevin and Balota (2000, Experiment 4). They reported that an imageability effect was found with low-frequency regular word targets (e.g., *witch, wisp*) when those targets followed a series of low-frequency exception word primes, but it was eliminated when they followed a series of nonword primes. We find this result somewhat puzzling because previous studies have reported that imageability effects in naming are limited to low-frequency exception words (Cortese, Simpson, & Woolsey, 1997; Strain & Herdman, 1999; Strain, Patterson, & Seidenberg, 1995). That is, typically regular words have not shown reliable imageability effects even when they were mixed with low-frequency exception words. It is, therefore, surprising that Zevin and Balota (2000, Experiment 4) found an effect of imageability for low-frequency regular words in the exception word prime condition. In a pilot study, we also failed to find any effect of imageability with low-frequency regular words similar to those used by Zevin and Balota. For these reasons, we do not consider the modulation of this effect further in the present article.

was observed for the Hangul words when they were mixed with Hanza word fillers, but the effect was reduced when the same Hangul words were mixed with Hangul word fillers or Hangul nonword fillers. Similarly, Baluch and Besner (1991, Experiment 3) reported that the frequency effect found with transparent Persian words (those with predictable mappings between orthography and phonology) was diminished when the words were mixed with nonwords.

In addition to these studies conducted with shallow orthographies, there are two recent reports of a similar result using English words with regular grapheme-to-phoneme mappings. Decker, Simpson, Yates, and Adamopolous (1999) found the size of the frequency effect for regular words (e.g., *born*, *clamp*) was reduced when they were presented with regular word fillers relative to when they were presented with exception word fillers. More important for present purposes, Zevin and Balota (2000, Experiment 3), using their "priming" procedure, also reported that the frequency effect for regular words was smaller when nonword primes were used than when exception word primes were used.

While these findings appear to provide nice support for the pathway control hypothesis, Kinoshita and Lupker (in press) argued that the results could also be interpreted within the time-criterion framework (see also Raman, Baluch, & Besner, 2000, for a similar view). What Kinoshita and Lupker noted was that in every single study cited previously, a particular pattern emerged. First of all, latencies were inevitably faster overall in the condition in which the frequency effect was smaller than in the other condition. In addition, consistent with the time-criterion account, the fillers (or primes) in the condition in which the frequency effect was smaller were faster to name than the fillers (or primes) in the other condition. Finally, the reduction in the frequency effect reflected the fact that the latency for the low-frequency words decreased in the faster filler condition while the latency for the high-frequency words either did not change or decreased slightly between conditions. Thus, Kinoshita and Lupker proposed that all of these results could be reconciled with the time-criterion account if it were assumed that both high- and low-frequency words can speed up when the context stimuli are easier to name (because of the adoption of a more strict time criterion), but that the speeding up of the high-frequency words is more limited because of a floor effect.

In line with this interpretation, Kinoshita and Lupker (in press) found that relative to a block containing low-frequency exception word fillers, the frequency effect for regular words was reduced in a block containing low-frequency regular word fillers (which were named much faster than the exception word fillers), but not in a block containing nonword fillers (which were not named faster than the exception word fillers). Moreover, as noted, in the same experiment, the size of the regularity effect was unaffected by filler type. Note that this result is, in general, at odds with the pathway control hypothesis, because that hypothesis cannot explain a modulation of the frequency effect and the absence of a modulation of the regularity effect occurring simultaneously.

Note also that the pathway control hypothesis has difficulty explaining the fact that the reduction in the size of the frequency effect was observed with regular word fillers (which may be read aloud either via the lexical or via the nonlexical route) but not with nonword fillers (which should maximally encourage the use of the nonlexical route). Thus, in general, the findings from studies examining the frequency effect are actually at least as compatible with the time-criterion account as with the pathway control hypothesis.

Lexicality Effect

Typically, even when words and nonwords are matched on various factors (word length, onset phoneme, Coltheart's *N*; Coltheart, Davelaar, Jonasson, & Besner, 1977), nonwords are read aloud more slowly than words (either regular words or exception words). According to dual-route models, lexicality effects are due to the lexical route generally being faster than the nonlexical route. According to the pathway control hypothesis, then, a condition that encourages the use of the lexical route should magnify the size of this effect, and a condition that encourages the use of the nonlexical route should reduce the difference between words and nonwords. In contrast, there is, in principle, little reason to expect a modulation of the lexicality effect according to the time-criterion account. That is, this account would predict a main effect of filler speed, but there is no reason for there to be an interaction with lexicality.

Only one study to date has examined whether there is a modulation of the lexicality effect as a function of the nature of the context stimuli. In that study (Zevin & Balota, 2000, Experiment 1), the results supported the pathway control prediction. Nonword targets were named more slowly than low-frequency exception word targets when they followed a series of low-frequency exception word primes, but the two types of targets were named equally fast when they followed a series of nonword primes. In other words, the lexicality effect was eliminated under a condition that encouraged use of the nonlexical route. It is difficult to accommodate this finding within the time-criterion account.

The Present Research

This survey of the literature shows that the reported modulations of effect size as a function of list composition are generally interpretable within the time-criterion account. Notable exceptions to this, however, are the experiments reported by Zevin and Balota (2000). Their Experiment 2 showed a modulation of the regularity effect (contrary to other studies), and their Experiment 1 showed a modulation of the lexicality effect (which has not been examined in other studies). The question that naturally arises is the following: Is Zevin and Balota's priming procedure simply a more powerful manipulation of context than the more standard filler manipulation or are their results due to some other factor(s)?

As noted, the manipulation used by Zevin and Balota (2000) involved a presentation of five primes of one type (nonwords or low-frequency exception words) in succession that was followed by a target. This procedure is a strong manipulation of context as it ensures that a target is always preceded by a prime of the appropriate type. In contrast, in other studies (e.g., Coltheart & Rastle, 1994), the ratio of fillers to targets was lower (typically one:one), and because the order of trials was randomized, targets were often immediately preceded by other targets. It is possible, therefore, that the effects of context were somewhat diluted in other studies, while Zevin and Balota were more successful in biasing the use of the lexical versus nonlexical pathway.

In addition to these differences, however, there is one other aspect of Zevin and Balota's (2000) procedure that is different from other studies that manipulated list composition. This is the fact that in Zevin and Balota's study, the nonword primes were named faster than the exception word primes. The time-criterion framework would predict that a more lax time criterion would be adopted following a sequence of slower (and more error-prone) exception word primes than following a sequence of faster nonwords (cf. Taylor & Lupker, 2001). Perhaps, then, at least some of Zevin and Balota's results (e.g., their modulation of the frequency effect) were due to the effect of the *speed* of the context stimuli, rather than because they used a stronger manipulation of the *type* of context stimuli.

To test these possibilities, the present experiments used a priming manipulation similar to Zevin and Balota's (2000), with one major modification. In addition to the use of a sequence of (fast) nonword primes and low-frequency exception word primes, we also included a slow nonword prime condition. The rationale here was to separate the effects of the speed of the context stimuli from the effects of the type of context stimuli. The comparison between the fast and slow nonword prime conditions provided a measure of the first, whereas the comparison between the nonword prime conditions and the exception word prime condition provided a measure of the second. We examined modulations of the size of the regularity effect, frequency effect, and lexicality effect in Experiments 1, 2, and 3, respectively.

General Method

Other than the targets used, the three experiments were identical in design, number of participants, apparatus, and procedure, which are described here.

Design

Each experiment constituted a 2 (target type) × 3 (prime type: exception words, fast nonwords, slow nonwords) factorial design, with both variables being within-subject. The dependent variables were naming latency and error rate.

Participants

In each experiment, a different group of 27 psychology students from Macquarie University participated for course credit. All participants were native Australian English speakers.

Materials

In each experiment, the same 80 low-frequency exception word primes and 160 nonword primes were used. All nonwords were phonotactically legal and were selected from the Australian Research Council (ARC) nonword database (Rastle, Harrington, & Coltheart, 2002; this database may be accessed at the Internet address: http://www.maccs.mq.edu.au/~nwdb). Half of the nonword primes were selected with the expectation that they would be faster to name than the exception word primes, and half were selected with the expectation that they would be slower to name than the exception word primes. To maximize the chances that we would observe noticeable latency differences between our two sets of nonword primes, we used short nonwords with many neighbors as the fast nonwords and long nonwords with few neighbors as the slow nonwords. Their characteristics are shown in Table 1, and the items are listed in the Appendix.

Targets

Experiment 1 investigated the modulation of the regularity effect. The targets were 30 low-frequency regular and 30 low-frequency exception words. All words were monosyllabic and four to six letters long. The regular and exception words were matched on initial phoneme, number of letters, Coltheart's *N* (the number of orthographic neighbors of the same length; see Coltheart et al., 1977), and frequency based on both the CELEX database (Baayen, Piepenbrock, & van Rijn, 1993) and Kucera and Francis (1967).

Table 1
Stimulus Characteristics of Targets and Primes Used

Variable	Length	KF	CELEX	*N*
Target				
High-frequency regular	4.57	176.87	2,509.33	5.27
Low-frequency regular	4.60	5.70	117.70	5.03
Low-frequency exception	4.60	7.47	113.97	5.00
Nonwords	4.60			5.07
Prime				
Low-frequency exception	4.59	5.10	109.79	3.73
Fast nonwords	3.74			10.75
Slow nonwords	4.85			1.24

Note. Length = item length (number of letters); KF = Kučera and Francis (1967) written word frequency (per million); CELEX = CELEX written word frequency (per 18 million); *N* = number of orthographic neighbors.

Experiment 2 investigated the frequency effect. The targets were 30 low-frequency regular and 30 high-frequency regular words. The low-frequency regular words were identical to those used in Experiment 1. All words were monosyllabic and four to six letters long. The high- and low-frequency words were matched on initial phoneme, number of letters, and Coltheart's *N*.

Experiment 3 investigated the lexicality effect. The targets were 30 low-frequency exception words and 30 nonwords. The low-frequency exception words were identical to those used in Experiment 1. The nonwords were selected to match the exception words on initial phoneme, number of letters, and Coltheart's *N*. The stimulus characteristics of all the targets are shown in Table 1, and the items are listed in the Appendix.

In each experiment, the targets were divided into three sets, Sets A, B and C, each containing 10 items of each type (e.g., exception vs. regular). The assignment of these sets to the three prime type blocks was counterbalanced across participants so that across every 3 participants, each set of target items appeared in each prime type block once.

There were also 15 practice stimuli and 10 warm-up stimuli in each prime type block that preceded the test stimuli. These stimuli consisted of items of the same type as the prime stimuli used in that block (i.e., either low-frequency exception words or fast or slow nonwords) and practice/warm-up target stimuli. None of these stimuli were the same as the test stimuli.

Apparatus and Procedure

Each participant completed three blocks of trials, each block containing one prime type (fast nonwords, slow nonwords, or exception words) and both types of targets. Each block consisted of 15 practice trials, 10 warm-up trials, and 100 test trials. The practice and warm-up trials contained the same prime type as that used in the test block. Each of the practice, warm-up, and test blocks consisted of sequences of five items: the first four items were primes (fast nonwords, slow nonwords, or low-frequency exception words) and the fifth item was a target item (in Experiment 1, a low-frequency regular or a low-frequency exception word; in Experiment 2, a high- or a low-frequency regular word; and in Experiment 3, a low-frequency exception word or a nonword). Within a list, the targets occurred in the same position, but a different random order of primes was generated for each participant.

The order of the three prime type blocks was counterbalanced across participants so that each prime type block occurred as the first, second, or

Table 2
Mean Naming Latencies (RT; in ms) and Percentage of Error Rates (%E) in Experiment 1

	Target type					
	Regular		Exception		Reg. effect	
Prime type	RT	%E	RT	%E	RT	%E
Fast NW	530 (17)	0.7 (0.5)	556 (23)	20.4 (2.1)	26	19.7
Exc.	550 (16)	1.1 (0.6)	582 (16)	10.7 (2.6)	32	9.6
Slow NW	558 (16)	1.1 (0.6)	581 (21)	17.4 (2.4)	23	16.3

Note. Standard errors are presented in parentheses. Reg. effect = regularity effect; NW = nonwords; Exc. = low-frequency exception words.

third block once across every 3 participants. The counterbalancing of the assignment of sets of targets to the three prime type blocks resulted in three versions of lists. With three different orders of prime type conditions, 9 participants constituted a fully counterbalanced group.

At the outset of the experiment, participants were told that a list of words and nonwords would be shown on the computer screen, one at a time. They were instructed to read aloud each item as quickly as possible without making too many errors.

Participants were tested individually and were seated approximately 40 cm in front of an NEC Multisync 4FG monitor upon which the stimuli were presented. Instructions and stimuli were presented, and reaction time data were recorded to the nearest millisecond, using the DMASTR display system developed by K. I. Forster and J. C. Forster at Monash University, Australia, and the University of Arizona (details of this system can be obtained at the Internet address: http://www.u.arizona.edu/~kforster/dmastr/dmastr.htm) running on a Deltacom 486 IBM compatible computer. Naming latency was measured by an amplitude voice key fitted to each participant and held a constant distance from the mouth throughout the experiment by means of a headset. Latency was recorded by the DMASTR software, and naming errors and possible measurement errors due to inappropriate voice key activation were recorded manually by the experimenter.

Each trial started with the presentation of a target, which remained on the screen for a maximum of 2,000 ms or until the voice key was triggered by the participant's response. Following a blank screen for 800 ms, the next trial started. Participants were given no feedback with respect to either naming latencies or error rates during the experiment.

Experiment 1

Experiment 1 examined whether there was a modulation of the size of the regularity effect as a function of prime type. If Zevin and Balota's (2000) priming manipulation were stronger than those that have been used in other studies and, hence, more successful in biasing the emphasis placed on the two pathways, then, the regularity effect should be magnified in both the fast and the slow nonword prime conditions. On the other hand, the time-criterion account predicts a main effect of prime speed, but no modulation in the size of the regularity effect.

Results

For this and subsequent analyses, the preliminary treatment of data was as follows. Any trial in which a participant or voice key error occurred was excluded from the latency analysis. To reduce effects of outliers, spuriously long or short reaction times were trimmed to the cutoff value of two standard deviations above or below the mean for each participant. Analyses treating participants as a random variable are reported, with an alpha level of .05 used to determine statistical significance.[2]

For the target items in each experiment, for each naming latency and percentage of error rate, we report a 2 (target type) × 3 (prime type) analysis of variance (ANOVA), followed by two interaction contrasts: one between target type and nonword prime speed (fast vs. slow nonword primes) and another between target type and nature of prime (the average of fast and slow nonword prime conditions vs. exception word prime condition). Both target type and prime type variables were within subject. For the primes, we report a one-way ANOVA, with prime type as a within-subject variable, followed by pairwise contrasts (with Bonferroni correction). The mean naming latencies and percentage of error rates from Experiment 1 are presented in Table 2.

Naming latencies. As expected according to the time-criterion account, there was a main effect of prime type, $F(2, 52) = 4.03$, $MSE = 2{,}736.05$; targets were named faster when following faster primes. There was also a main effect of regularity, $F(1, 26) = 33.75$, $MSE = 877.30$; regular words were named faster than exception words. However, there was no modulation of the regularity effect as a function of prime type, $F(2, 52) < 1.00$. The planned contrast testing the interaction between regularity and nonword prime speed was nonsignificant, $F(1, 26) < 1.00$, as was the planned contrast testing the interaction between regularity and nature of prime, $F(1, 26) < 1.00$.

Error rates. Exception word targets were more error prone than regular word targets. Hence, there was a significant regularity effect on the error rate, $F(1, 26) = 85.36$, $MSE = 109.40$. There was also a significant effect of prime type, $F(2, 52) = 4.73$, $MSE = 65.08$, and, unlike the latency data, there was a significant

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[2] In these experiments, the results of an items ANOVA did not always confirm the results of the participants ANOVA because the items ANOVAs were not sufficiently powerful to pick up effects of the sizes reported here. We do not, however, regard this as a problem. The items we used were not randomly selected but were selected on the basis of a particular set of criteria (see Table 1). As such, treating items as a random variable would violate many of the assumptions of the ANOVA model with the impact being to further reduce the power of the analysis. Thus, as Wike and Church (1976); Raaijmakers, Schrijnemakers, and Gremmen (1999); and others (Cohen, 1976; Keppel, 1976; Smith, 1976) have argued, items analyses would clearly be inappropriate in the present situation. Therefore, we report only the results of the participants' analyses and our conclusions are based on those results. We are happy to provide the results of the items analyses for any of our experiments for any interested readers.

interaction, $F(2, 52) = 7.70$, $MSE = 45.51$. This interaction was due to the fact that the regularity effect was reduced in the exception word condition relative to the nonword prime conditions, as confirmed by the significant interaction contrast between regularity and prime type, $F(1, 26) = 10.37$, $MSE = 60.26$, and the nonsignificant contrast testing the interaction between regularity and nonword prime speed, $F(1, 26) = 2.44$, $MSE = 30.77$.

The percentages of trials involving regularization errors to the exception word targets were 11.5%, 8.1%, and 11.5% for the fast nonword, exception word, and slow nonword prime conditions, respectively.[3] When viewed as a percentage of all errors, these represent 56.4%, 75.9%, and 65.9%, respectively. This pattern shows that the *proportion* of errors that were regularization errors was not smaller in the exception word prime condition than in the nonword prime conditions, unlike in Zevin and Balota's (2000) experiment.

Primes. The mean latencies of the fast nonword, exception word, and slow nonword primes were 534 ms, 571 ms, and 631 ms, respectively. These means differed significantly from each other, $F(2, 52) = 36.12$, $MSE = 1{,}786.40$,[4] with all pairwise contrasts being significant.

The mean error rates for the fast nonword, exception word, and slow nonword primes were 0%, 10.1%, and 0.3%, respectively. There was a significant effect of prime type, $F(2, 52) = 176.21$, $MSE = 5.23$, with all pairwise contrasts being significant, except for the contrast between fast nonword and slow nonword primes ($p = .87$).

Discussion

The results of the latency analysis are clear-cut: Although we observed a typical size regularity effect, the size of this effect did not vary as a function of the nature of the prime or prime speed. In particular, unlike in Zevin and Balota's (2000) experiment, the effect was not smaller in the exception prime condition (in fact, numerically, it was slightly larger in that condition). Thus, the latency results provide no support for the pathway control hypothesis.

This absence of a modulation of the regularity effect as a function of prime type found here is as predicted by the time-criterion account and is consistent with all of the previous studies investigating this issue (Coltheart & Rastle, 1994; Kinoshita & Lupker, in press; Woollams & Kinoshita, 1997), with the exception of Zevin and Balota (2000). Because we followed Zevin and Balota's procedure for manipulating stimulus context, their finding of a modulation of the regularity effect does not appear to be due to their procedure simply being more powerful.

What should be noted, however, is that there was some evidence that prime type did affect error rates. In particular, the regularity effect in error rates was smaller in the exception prime condition. One could try to interpret this effect in terms of pathway control by arguing as follows. The exception prime condition did induce readers to shift attention away from the nonlexical route with the result being that they were then able to avoid competition from that route. Hence, they named the exception words more accurately. Such an explanation, however, carries with it a second prediction. If the reduction in error rates were due to eliminating competition from the nonlexical route, the types of errors that would have been eliminated would have been regularization errors (those errors created by the nonlexical route providing the incorrect, regularized phonological code). As such, the prediction is that the percentage of errors that are regularization errors would be smaller in the exception prime condition than in the other conditions. As noted previously, however, this is the opposite of what occurred. The proportion of errors that were regularization errors was actually higher in this condition than in the other conditions. Thus, like the latency data, the error data provide little support for the pathway control hypothesis.

Experiment 2

The focus of Experiment 2 was the frequency effect for regular words. Based on the assumption that frequency effects reflect the operation of the frequency-sensitive lexical route, the pathway control hypothesis predicts that the frequency effect for regular word targets should be greater in the exception word prime condition than in either of the nonword prime conditions. In contrast, the time-criterion account predicts either that the frequency effect would remain constant or that it may be reduced only in the fast nonword prime condition. The latter prediction is based on the analysis of the previous findings reviewed in the introduction indicating that a reduction in the frequency effect has been observed only when fast context stimuli are used and may, therefore, be due to a floor effect for high-frequency targets.

Results

The mean naming latencies and percentage of error rates from Experiment 2 are presented in Table 3.

Naming latencies. As in Experiment 1, the main effect of prime type was significant, $F(2, 52) = 6.83$, $MSE = 1{,}948.40$, as was the main effect of frequency, $F(1, 26) = 23.20$, $MSE = 1{,}019.24$. Target latencies were faster with faster primes, and high-frequency words had shorter latencies than low-frequency words. More important, these two variables interacted significantly, $F(2, 52) = 3.38$, $MSE = 422.42$. The planned contrast testing the interaction between frequency and nonword prime speed was significant, $F(1, 26) = 6.63$, but the interaction between frequency and nature of prime was not, $F(1, 26) < 1.00$.

Error rates. The main effect of prime type was nonsignificant, $F(2, 52) < 1.00$, $MSE = 0.62$, as was the main effect of frequency, $F(1, 26) = 2.74$, $MSE = 8.12$. The interaction between these two variables was significant, $F(2, 52) = 5.20$, $MSE = 13.89$. The significant interaction arose because the interaction between frequency and nonword prime speed was significant, $F(1, 26) = 9.60$, but the interaction between frequency and nature of prime was not, $F(1, 26) < 1.00$. The pattern of interaction (the increase in error rate in the fast nonword prime condition relative to slow nonword prime condition for low-frequency words, but not for high-frequency words) was opposite to the latency data. This result is consistent with the idea that the greater speedup for the low-frequency words (and, hence, the smaller frequency effect in the latency data) comes with a slight cost in terms of errors.

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[3] Coltheart et al.'s (1993) GPC rules were used to determine which types of errors were regularization errors.

[4] One exception word prime (*waltz*) was consistently mispronounced. Hence, it was excluded from the latency analysis.

Table 3
Mean Naming Latencies (RT; in ms) and Percentage of Error Rates (%E) in Experiment 2

	Target type					
	High frequency		Low frequency		Freq. effect	
Prime type	RT	%E	RT	%E	RT	%E
Fast NW	490 (16)	0.0 (0.0)	503 (16)	3.3 (1.2)	13	3.3
Exc.	506 (13)	1.5 (0.7)	531 (14)	1.5 (0.9)	25	0.0
Slow NW	510 (14)	2.2 (0.8)	543 (16)	1.1 (0.6)	33	−1.1

Note. Standard errors are presented in parentheses. Freq. effect = frequency effect; NW = nonwords; Exc. = low-frequency exception words.

Primes. The mean naming latencies of the fast nonword, exception word, and slow nonword primes were 508 ms, 556 ms, and 616 ms, respectively. They differed significantly from each other, $F(2, 52) = 40.92$, $MSE = 1{,}916.46$, with all pairwise contrasts being significant.

The mean error rates for the fast nonword, exception word, and slow nonword primes were 4.1%, 14.9%, and 6.2%, respectively. They differed significantly from each other, $F(2, 52) = 49.15$, $MSE = 11.54$, with all pairwise contrasts being significant.

Discussion

The results of Experiment 2 show that the modulation of the frequency effect was in line with the pattern expected within the framework of the time-criterion account: Relative to the exception word prime condition, the size of the frequency effect was reduced in the fast nonword prime condition, but not in the slow nonword prime condition (in fact, numerically, it was actually largest in the slow nonword prime condition). The observed pattern of data is consistent with all previous studies that examined the modulation of the size of the frequency effect, in that the reduction in the size of the effect was observed only when the context stimuli could be named rapidly. This description also applies to Zevin and Balota's (2000) Experiment 3, in that their nonword primes were named faster than their exception word primes in that experiment. Thus, in opposition to the predictions of the pathway control hypothesis, it appears to be the inclusion of rapidly named context stimuli and not the inclusion of nonwords that produces a reduced frequency effect. We conclude, therefore, that the time-criterion account (with the additional assumption that there is some impact of a floor effect for high-frequency words) would appear to provide a better explanation of the modulation of the frequency effect than the pathway control hypothesis.

Experiment 3

Experiment 3 examined the modulation of the lexicality effect. The pathway control hypothesis predicts that the lexicality effect would be larger in the exception word prime condition than in either the fast or the slow nonword prime conditions. The time-criterion account, on the other hand, predicts a main effect of prime speed, and a constant size lexicality effect in the three prime conditions.

Results

The mean naming latencies and percentage of error rates for the targets from Experiment 3 are presented in Table 4.

Naming latencies. As in previous experiments, the main effect of prime type was significant, $F(2, 52) = 4.43$, $MSE = 3{,}772.90$.[5] There was also an effect of lexicality, $F(1, 26) = 10.18$, $MSE = 1{,}609.68$, with words being named faster than nonwords. Most important, there was a significant interaction, $F(2, 52) = 8.53$, $MSE = 1{,}131.83$. The contrast testing the interaction between lexicality and nonword prime speed was nonsignificant, $F(1, 26) = 1.62$, $MSE = 1{,}833.49$. In contrast, the interaction between lexicality and nature of prime was significant, $F(1, 26) = 15.71$, $MSE = 17{,}777.78$. That is, consistent with the prediction of the pathway control hypothesis, there was a larger lexicality effect in the exception word prime condition than in the two nonword prime conditions.

Error rates. Consistent with the latency data, the error rate data also showed that the nonword targets, but not the exception word targets, benefited from the nonword prime environment, although for error rates, this occurred only in the fast nonword prime condition. Nonetheless, the main effect of prime type was nonsignificant, $F(2, 52) < 1.00$. In addition, there were overall fewer errors to nonword targets than exception word targets, $F(1, 26) = 4.73$, $MSE = 300.27$, and the interaction between lexicality and prime condition was significant, $F(2, 52) = 3.95$, $MSE = 148.01$. The contrast testing the interaction between lexicality and nonword prime speed was significant, $F(1, 26) = 7.23$, $MSE = 148.01$, but the contrast between lexicality and nature of prime was not, $F(1, 26) < 1.00$.

As in Experiment 1, we examined the rate of regularization errors made to the exception word targets. The percentages of trials involving regularization errors to the exception word targets were 20.4%, 16.6%, and 15.6% for the fast nonword, exception word, and slow nonword prime conditions, respectively. When viewed as a percentage of all errors, these represent 76.3%, 81.3%, and 72.3%, respectively. As in Experiment 1, this pattern shows that the proportion of regularization errors was slightly larger in the exception word prime condition than in the nonword prime conditions.

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[5] One exception word target (*seize*) was consistently mispronounced. Hence, it was excluded from the latency analysis.

Table 4
Mean Naming Latencies (RT; in ms) and Percentage of Error Rates (%E) in Experiment 3

	Target type					
	Exception word		Nonword		Lex. effect	
Prime type	RT	%E	RT	%E	RT	%E
Fast NW	585 (19)	26.7 (2.3)	583 (17)	13.3 (3.2)	−2	−13.4
Exc.	593 (18)	20.4 (2.5)	643 (19)	16.7 (2.5)	50	−3.7
Slow NW	601 (18)	21.5 (3.2)	613 (18)	20.7 (3.3)	12	−0.8

Note. Standard errors are presented in parentheses. Lex. effect = lexicality effect; NW = nonwords; Exc. = low-frequency exception words.

Primes. The mean latencies of the fast nonword, exception word, and slow nonword primes were 545 ms, 578 ms, and 637 ms, respectively. These means differed significantly from each other, $F(2, 52) = 35.40$, $MSE = 1{,}658.59$, with all pairwise contrasts being significant.

The mean error rates for the fast nonword, exception word, and slow nonword primes were 2.7%, 14.2%, and 5.4%, respectively. These percentages differed significantly from each other, $F(2, 52) = 87.03$, $MSE = 11.28$, with all pairwise contrasts being significant.

Discussion

The results of Experiment 3 are different from the results of the previous two experiments in that the modulation in the size of the lexicality effect depended on the nature of the prime (exception word or nonword) rather than on prime speed. Specifically, nonword targets slowed down when preceded by exception word primes (in comparison to nonword primes), regardless of whether the nonword primes were fast or slow. The low-frequency exception word targets, on the other hand, showed an effect of prime speed consistent with the time-criterion account: They were named more slowly when preceded by slower primes, irrespective of whether those primes were exception words or nonwords.

The overall pattern of results is, of course, generally consistent with the pathway control hypothesis as well as being a virtual replication of Zevin and Balota's (2000) result. However, if the modulation of the lexicality effect really were due to pathway control, it becomes rather difficult to reconcile this result with the results in Experiments 1 and 2. That is, if our exception word primes do cause readers to rely less on the nonlexical route than our nonword primes do, one should have observed reduced regularity effects and increased frequency effects following exception word primes in those experiments. (One should also have observed slower, not faster, latencies for the exception word targets in the exception word prime condition than in the fast nonword prime condition in Experiment 3.) Thus, it is certainly possible that the results observed in Experiment 3 were due to something else.

The observation that there is a specific cost to nonwords when they are mixed with low-frequency exception words has been reported previously (Lupker et al., 1997, Experiment 1; Monsell et al., 1992, Experiment 2). In these experiments, exception words and nonwords were named either in pure blocks or in mixed blocks. In Lupker et al.'s experiment, their low-frequency exception words and nonwords had similar latencies in pure blocks. Nonetheless, the nonwords were named more slowly when mixed with low-frequency exception words (while the low-frequency exception words were named more rapidly when mixed with nonwords). Lupker et al. noted that this result could not be explained by the time-criterion account. Thus, they proposed that the results were due to a second strategy that readers can invoke, a strategy that they referred to as "lexical checking."

According to this idea, prior to emitting a naming response, readers have the option of consulting the phonological output lexicon in order to determine whether the code generated by the phonological coding process matches a code in the output lexicon. Although this process produces a small time cost when there is a phonological code in the output lexicon, it can be useful when a lot of low-frequency exception words are contained in a block (in terms of correcting errors). With nonwords, however, the strategy is not only useless (because nonwords do not have a representation in the phonological output lexicon), it is also counterproductive because of the length of time it takes to conduct an unsuccessful check. Thus, this strategy would, presumably, be used to a substantially larger degree in a pure block of exception words, to a lesser degree in a mixed block, and not at all in a pure block of nonwords. As a result, nonword latencies should be slightly faster in a pure block than in a mixed block while exception word latencies would be a bit faster in a mixed block than in a pure block (although errors could increase as well), just as Lupker et al. (1997) observed.

This type of account would provide a viable alternative account of the results of Experiment 3. That is, in the exception word prime condition, all of the primes and half of the targets were low-frequency exception words. Thus, 90% of the stimuli had representations in the phonological output lexicon. Furthermore, because the words were all low-frequency exception words, the phonological coding process for these words would tend to be a bit error prone. Thus, if lexical checking is a viable strategy, it should have been used much more in this condition than in either of the two nonword prime conditions (in which 90% of the stimuli were nonwords). As a result, one would expect to see a noticeable cost for the nonwords (because of the fact that the search of the phonological output lexicon would have been long and, ultimately, unfruitful), as was observed.

We note that the lexical checking strategy is a different notion from pathway control because it does not assume that it is the relative reliance on the lexical versus nonlexical routes that is affected by context. Rather, lexical checking is an extra process that takes place *after* a phonological code has been generated, and it can be bypassed when such a strategy is counterproductive (e.g., when the block consists primarily of nonwords).[6]

The results of Experiment 3 are, therefore, compatible with both a lexical checking strategy account and the pathway control hypothesis. On the basis of the entire pattern of data presented here, we would favor the lexical checking strategy account. The reasoning is that although this interpretation is also compatible with the results of Experiments 1 and 2, as noted, the pathway control hypothesis is not. That is, because the targets used in Experiments 1 and 2 were words, the degree to which the lexical checking strategy was used should not have differentially affected the different types of targets in those experiments (low-frequency regular and exception words in Experiment 1; high- and low-frequency regular words in Experiment 2). Hence, it is not at all surprising that Experiment 3 was the only experiment that showed a modulation in effect size as a function of prime type. In contrast, the pathway control hypothesis cannot simultaneously explain why the effect sizes were modulated by prime type only in Experiment 3 and not in Experiments 1 and 2 or why exception words were named faster following fast nonword primes than following exception word primes (in Experiments 1 and 3).

General Discussion

Past research investigating the modulation of target type effect sizes (e.g., regularity, frequency) as a function of the nature of the context stimuli (i.e., either fillers or primes) has been interpreted in terms of either the pathway control hypothesis or the time-criterion account. In all cases, these experiments were designed so that the target type effect reflected different relative contributions of the lexical and nonlexical routes. Thus, the pathway control hypothesis predicted a modulation in the size of these target type effects as a function of whether the context stimuli encourage the use of the lexical route or the nonlexical route. In contrast, the time-criterion account states that any effects of context are due to the difficulty of naming the context stimuli. Therefore, it predicts that, in general, the size of any target type effect would not be affected by the qualitative nature of the context stimuli.

A survey of the relevant literature showed that virtually all findings in this area are interpretable in terms of the time-criterion account, with the notable exception of the results reported by Zevin and Balota (2000). The goal of the present research was therefore to test whether Zevin and Balota's results were really due to their procedure being a stronger manipulation of context, as they themselves suggested, as well as to separate the effects due to the speed of the context stimuli from the effects due to the nature of the context stimuli. We tested the three target type effects investigated by Zevin and Balota and found that (a) the regularity effect was not modulated as a function of either the speed or the nature of the context stimuli (Experiment 1), (b) the frequency effect was reduced only in the fast nonword prime condition relative to the exception word prime condition (and the slow nonword prime condition) (Experiment 2), and (c) the lexicality effect was reduced in the two nonword prime conditions relative to the exception word prime condition (Experiment 3). Only the last finding is at all inconsistent with the time-criterion account. We suggested, however, that it is consistent with the lexical checking strategy proposed by Lupker et al. (1997), which states that the specific cost observed for nonwords presented in a block consisting primarily of exception words reflects the process of checking to determine whether the generated pronunciation matches a stored code in the phonological output lexicon and failing to find one.

Note that at an empirical level, the present results represent a close replication of two of Zevin and Balota's (2000) three main results. That is, in their experiments, their nonword primes were always named faster than their exception word primes. Thus, the relevant comparisons in the present experiments are between the fast nonword prime condition and the exception word prime condition. These contrasts revealed that both the frequency effect and the lexicality effect were reduced in the fast nonword prime condition, paralleling Zevin and Balota's results. The only empirical discrepancy between the two sets of results concerns the regularity effect. While Zevin and Balota observed a larger regularity effect and a higher percentage of regularization errors in their nonword prime condition than in their exception word prime condition, we failed to observe even a trend in that direction.

As noted, our inability to modulate the size of the regularity effect is consistent with other results in the literature (e.g., Coltheart & Rastle, 1994; Kinoshita & Lupker, in press; Woollams & Kinoshita, 1997). However, the question still remains as to why Zevin and Balota (2000) were able to produce a modulation in the size of their regularity effect using essentially the same procedure as we did in our Experiment 1, whereas we were not.

Our Experiment 1 was, as noted, not an exact replication of Zevin and Balota's (2000) Experiment 2. One difference was that Zevin and Balota used five primes before each target word, whereas we only used four. While one could argue, therefore, that Zevin and Balota's context manipulation was stronger than ours, logically, it seems unlikely that the addition of one more prime could have produced an effect when there was no hint of one otherwise. Furthermore, the fact that we were able to replicate Zevin and Balota's findings in our other two experiments by using four primes rather than five indicates that the context created by four primes was no less powerful than the context created by five primes would have been.

A second difference between our experiment and Zevin and Balota's (2000) was in the set of primes and targets used. With respect to the primes, it is not impossible that, for example, our fast nonword primes differed from Zevin and Balota's nonword primes in terms of things like number or length. One could then conjecture that these differences may have made our primes less effective than Zevin and Balota's, producing our failure to replicate in Experiment 1. Once again, however, because of the fact that we were able to replicate Zevin and Balota's results in Experiments 2 and 3, it seems unlikely that our failure to replicate their results in Experiment 1 could have been due to differences in the properties of the primes. That is, if our primes were problematic, the obvious implication is that those same primes would have produced a failure to replicate in the other experiments as well.

With respect to the targets, one difference was that, whereas our words were all monosyllabic (the more standard situation in the literature), more than half of Zevin and Balota's (2000) words, both regular and exception, were multisyllabic. A second differ-

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[6] Interestingly, a similar idea to the lexical checking strategy has been used to explain the slowdown of nonword naming when they are mixed with pseudohomophones (nonwords, which sounds like words when read aloud, for example, *brane*) in contrast to when they are mixed with standard nonwords (Borowsky, Owen, & Masson, 2002).

ence was that a number of Zevin and Balota's words (e.g., *lapel*, *canker*, *sap*) were unfamiliar and, hence, possibly even unknown to some of their participants. Recent results reported by Kinoshita and Lupker (2001) showing that lexical decisions for Zevin and Balota's words were much slower and more error prone than for the words used in the present Experiment 1 provide support for this conjecture. If, indeed, a number of Zevin and Balota's words were not actually in the lexicons of their participants, this would have had a rather unpredictable impact on any lexical checking processes that those participants would have been performing. Whether these aspects of Zevin and Balota's target stimuli could explain their pattern of results (i.e., the latency for their regular word targets was shorter with nonword primes, whereas the latency for their exception word targets was unaffected by prime type) remains a question for future research.

Conclusion

With the exception of those results that appear to be due to the use of a lexical checking strategy, the present results all appear to be easily explained in terms of the time-criterion account. Furthermore, with the exception of Zevin and Balota's (2000) Experiment 2, most results in the literature that were produced using a "list composition" manipulation have been consistent with the time-criterion account. Our claim is, therefore, that while the speed with which the context stimuli can be named clearly affects target-naming latency, there is still no compelling evidence that target-naming latency is affected by the qualitative nature of the context stimuli in the way proposed by the pathway control hypothesis.

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Appendix

Stimuli Used in Experiments

Target stimuli

Low-frequency exception words (Experiments 1 and 3)			Low-frequency regular words (Experiments 1 and 2)			High-frequency regular words (Experiment 2)			Nonwords (Experiment 3)		
beau	chef	chord	bait	shun	carve	black	ship	claim	pell	romph	poil
cough	deaf	dough	cream	doom	droop	case	dance	drink	wult	suiff	curlt
fete	guild	hearse	flip	glide	hoarse	fact	girl	hair	grung	sheem	sace
hood	pear	realm	helm	plum	roach	hand	point	range	carge	hosh	werg
seize	suite	wolf	surge	sleek	wink	serve	street	wife	rilch	falp	clase
bowl	blown	caste	bulb	broth	coach	blue	bring	clay	sloob	ghief	barv
chasm	comb	debt	creep	cage	dump	cattle	club	deep	huiche	kogue	blint
feud	guise	hearth	fuss	goose	huddle	film	green	help	dimp	cume	derb
pint	plaid	reign	pulp	prune	roast	plane	peace	reach	bule	prape	detch
shove	soot	womb	sheen	sock	wipe	shape	stop	wish	shif	firb	morque

Prime stimuli

Low-frequency exception words			Fast nonwords			Slow nonwords		
hoof	weird	psalm	heam	weg	sneet	honge	wourge	salph
dwarf	yacht	thumb	dife	yeg	tink	dirp	yoam	thyth
gird	malt	chute	gunt	mosh	shool	gube	meich	kurch
mould	sieve	weir	mib	sloy	wib	merb	splype	thwal
tomb	pearl	gnome	tey	poom	nane	trich	pudd	norb
swap	heir	gist	starn	hish	jarn	cilm	yirge	julge
mauve	choir	sweat	mive	kice	steet	murf	clett	snalph
leapt	naïve	vase	lep	nong	vid	lauce	nount	vique
wool	wand	drought	wep	woat	dup	wuth	pheem	deace
coup	swarm	pier	cag	sim	pid	cleeth	slont	peum
brooch	axe	sword	bap	ack	spoom	bymn	phliz	spewt
isle	knoll	niche	jole	noil	nim	iche	naise	nirm
ghoul	sew	fiend	gog	steck	fitch	garr	swolve	frerch
sheik	broom	pique	sheel	bab	parn	shouf	blalf	peph
dove	trough	watt	delp	trog	yane	dorce	teign	woath
shoe	swan	sewn	shep	stin	sen	shouge	smoob	celch
suede	lamb	dual	sep	lig	diss	scral	leint	drizz
plait	lieu	ounce	pem	lish	oop	prith	lerg	owse
worm	swamp	scent	wid	stell	cench	phelf	sponch	skonch
aisle	flown	knot	ank	fet	nid	owth	fruzz	nurf
limb	glow	kneel	lum	groon	nig	lowth	ghegg	nalc
bind	steak	chic	bice	sar	shig	blegg	snaich	chich
rogue	sown	waltz	roil	sosh	weck	reuth	sweil	weff
tsar	flood	warp	trock	fap	wob	trewt	flerb	ceeth
sponge	geese	wrath	sug	gac	rorm	slafe	gwisc	rerf
yolk	glove	crepe	yop	glam	cug	yurk	jewth	cralph
gnaw	aunt		nunch	fliss		nult	skorgue	

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