Statistical learning of orthotactic constraints: evidence from typing

Author Note

All the data, the analysis code and the complete outputs are publicly available on OSF, through the following link: (OSF link is hidden due to anonymous review)

Abstract

Possible spoken and written sequences of a language are determined by phonotactic and orthotactic rules, respectively. Adult speakers can learn both simple new phonotactic rules (e.g., "/k/ is always an onset in a syllable), and more complex second-order rules (e.g., "/k/ is an onset only if the vowel is /æ/, but a coda if the vowel is /ɪ/"). However, the learning timeline for more complex rules is less consistent across populations and languages. In this paper, we investigate the learning of parallel orthotactic rules in typing. We first show that adults quickly learn new first-order constraints in typing similar to those in speaking (Experiment 1). Next, we show that they also learn second-order rules, and with a timeline similar to learning such phonotactic rules in speaking (Experiment 2). We further find that the second-order constraint is learned for the coda, but not the onset, suggesting that learning new rules of sequencing is carried out by a chaining-type mechanism. Finally, we show that while phonology clearly influences orthography, orthotactic learning cannot be reduced to phonotactic learning (Experiment 3). Collectively, these data support strong similarities between the statistical learning of orthotactic and phonotactic constraints, pointing to the domain-generality of the incremental learning principles across different modalities of language production.

Introduction

Phonotactic and orthotactic rules govern the possible spoken and written sequences in a language. For example, /ŋ/ can never appear at the beginning of a syllable (onset position) in spoken English, but it can in Vietnamese. Similarly, /h/ cannot be a coda in English, but it can in Farsi. While phonotactic constraints have been studied extensively in English, relatively little attention has been paid to orthotactic constraints. Even though they are related, the two are not the same. For example, /v/ is an acceptable coda in spoken English (e.g., /dʌv/, /daɪv/, etc.), but it is rare for English written syllables to end with “v” without a following vowel (e.g., “dove”, “dive”, etc.). In this paper, we examine the learning of orthotactic constraints in typing. Because this process involves extracting patterns of letter occurrences in specific positions, we refer to it as statistical learning. By imposing artificial orthotactic constraints, we first examine the parallels between the statistical learning of phonotactic and orthotactic rules for first-order constraints (e.g., “t” can only be an onset and “s” only a coda; Experiment 1) and second-order constraints (e.g., “f” is an onset if the vowel is “a” but a coda if the vowel is “i”; Experiment 2). We then test specific hypotheses about the nature of such learning by comparing statistical learning of constraints in onset vs. coda positions. Finally, we test whether orthotactic learning can be dissociated from phonotactic learning (Experiment 3). Collectively, these results shed light on the nature of orthotactic learning in English.

Phonotactic learning

Adult speakers rarely make mistakes that violate the ground rules of their native language. For example, /kɪŋ/ may slip into /nɪŋ/, but is highly unlikely to slip into /ŋɪŋ/, because /ŋ/ is not an acceptable onset in English. Since speakers do not intentionally make speech errors, these errors reflect the implicit processes underlying language production, including how people incorporate sequencing rules of a language. Error patterns in studies with artificial phonotactic constraints have shown that adult speakers not only follow the established phonotactic rules of their language but also quickly learn new artificial rules. Dell and colleagues (2000) had participants read aloud and recite sequences of four consonant-vowel-consonant (CVC) syllables such as “*ges meng fen hek”* and recorded their speech errors. Unbeknownst to the participants, artificial phonotactic constraints were embedded in the syllables, e.g., /f/ was always the onset and /s/ always the coda. An error was considered “legal” if the constrained phonemes preserved their syllabic positions (e.g., *“ges mes fen hek”*). Otherwise, it was deemed illegal (e.g., “*ges* ***s****eng fen hek”).* Unsurprisingly, speech errors were 100% legal for the language-wide constraints, such as the coda /ŋ/. Interestingly, errors involving artificial constraints were also legal 98% of the time, whereas errors involving unrestricted consonants were legal only 68% of the time, showing fast and robust learning of the new phonotactic rules (see also Taylor & Houghton, 2005; Warker & Dell, 2006; Warker et al., 2009). A number of follow-up studies showed that such learning was unaffected by participants’ awareness of the new rule, pointing to the incremental and implicit nature of phonotactic learning (see also, Warker & Dell, 2006; Smalle et al., 2017).

Further work showed that speakers could also learn artificial second-order constraints, e.g., /f/ is an onset if the vowel is /æ/ but a coda if the vowel is /ɪ/ (Warker & Dell, 2006). However, learning of these more complex rules was only evident on the second day of training. In a clever study, Gaskell et al. (2014) showed that delayed learning was critically dependent on sleep. Participants who had a 90-minute nap in between the two sessions demonstrated learning, while those who watched a video for 90 minutes instead of sleeping did not. The delay in the learning of second-order constraints is, however, not universally replicated. For example, Smalle et al (2017) showed that 9-10-year-old Dutch-speaking children learned the second-order constraints on the first day. Also, in a more recent study, Smalle and Szmalec (2022) found that French-speaking adults also learned the second-order constraints on the first day. It was argued that in French, compared to English, vowels are generally more predictive of consonant positions, leading to faster learning of second-order constraints in the former compared to the latter. In a different study, Muylle et al. (2021) reported that older adults were also able to learn second-order constraints on the first day. The authors also noted a trend toward learning in young adults on the first day, although it did not reach statistical significance. The difference was tentatively attributed to different numbers of errors; since younger adults made fewer errors, detecting an effect may have been more difficult.

The mechanisms underlying the new phonotactic learning have also been investigated to some extent. For example, Anderson et al. (2019) investigated whether phonotactic learning was incremental or similar to learning a rule. After training participants on a certain novel rule (e.g., /s/ is always onset), they reversed the constraint (i.e., /s/ is now only coda). If learning is incremental, reversal must gradually undo the learning of the original constraint and slowly build the representation of the second constraint. If, on the other hand, learning is rule-like, once the original rule is learned, it should be quickly reversible to its opposite. The results supported the incremental account. Moreover, Warker and Dell (2015) investigated whether a system that learns the mapping between phonemes and positions through error-based learning explains the effects. If so, training the model with a constraint (e.g., /f/ is onset) in the context of a certain syllable (e.g., /fæk/) should not only promote slips like /gæk/ to /**f**æk/, but also slips in untrained syllables, such as /gæm/ to /**f**æm/. This is what was observed. Recall that children showed faster learning of the second-order constraints than adults (Smalle et al., 2017). Since children also make more speech errors than adults (e.g., Budd et al., 2011; Hanley et al., 2016), it is possible that the greater number of errors provides more opportunities for error-based learning, leading to the quicker acquisition of the new constraints. Although a higher error rate cannot be the sole reason behind the faster acquisition of second-order constraints (e.g., Smalle et al., 2021 reported fast learning under cognitive load without an increase in error rates), it remains a possible factor in faster learning worth investigating.

Another way to investigate the mechanism underlying statistical learning of second-order contingencies is to examine if such learning is different for consonants in the onset vs. coda position. If constraints are learned in a sequential manner, one would expect learning in the coda —which follows the constraining vowel— but not in the onset, which precedes it[1]. This pattern would suggest a chaining-like mechanism

[1] Note that this does not apply to first-order constraint learning because there learning does not depend on the preceding or following segments.

(Washburn, 1916; Wickelgren, 1965; Lewandowsky & Murdock, 1989; Botvinick & Plaut, 2004). In chaining accounts, there is no separation between content and frame. Sequencing is achieved through the retrieval of each unit based on the preceding, but not the following, context. Alternatively, constraints may be learned hierarchically, i.e., relative to a positional frame (Dell et al., 1997). Positional frame accounts posit a separation between an abstract frame with a predefined number of slots and content, i.e., phonemes or letters, each of which is linked to a specific slot in the positional frame (e.g., Hepner, Pinet & Nozari, 2018; Houghton, 2018; McCloskey et al., 1994). Consequently, these accounts have a much richer representation of the relationship between position and content, which does not strictly depend on the preceding context. Although assuming a positional frame is common in models of language production, including orthographic production (e.g., Houghton, 2018), certain findings also indicate a role for chaining mechanisms. For example, Snyder and Logan (2014) reported that primes facilitated target typing the most when primes and targets overlapped in initial sequences, and the greater this overlap, the larger the magnitude of the facilitation.

Finally, phonotactic learning has been studied from the angle of domain-generality of principles of processing (Nozari & Martin, 2024) by comparing whether findings from speech studies are generalizable to nonspeech studies (Anderson & Dell, 2018; 2019; Rebei, Anderson, & Dell, 2019). In nonspeech studies, instead of vocalizing a syllable, participants learn an arbitrary mapping between sounds and buttons on a button-box and must respond by pressing buttons. The results have largely replicated those of phonotactic studies, including rapid learning of the first-order constraints and slow reversal of that learning when encountering opposite constraints. Interestingly, while Rebei et al. (2019) replicated superior learning of second-order constraints on the second compared to the first day, they also found evidence of learning on the first day, contrary to some of the previous reports in speech studies, potentially pointing to the role of the domain.

To summarize, studies of phonotactic constraints in spoken production have shown that novel first-order constraints can be acquired quickly even in adult speakers who have experienced the phonotactic rules of their native language from infancy, pointing to the continuous nature of learning in language production (Dell et al., 2021). More complex phonotactic rules can also be learned, although the timeline of such learning is less consistent across populations and languages. One of the most interesting dichotomies reported in this vein is between speech and nonspeech studies, with the former showing slower learning than the latter. There could be two reasons for this difference: the modality of production (speech vs. button-press) or experience (highly practiced speech production vs. newly acquired arbitrary mappings). Typing provides a good testbed for disentangling these two factors. If modality is a determining factor, typing should show a pattern similar to the button-press task. If, on the other hand, those results stem primarily from less-practiced arbitrary mappings, then we would expect typing to pattern more closely with speaking, because in both cases participants would be executing well-learned mappings as opposed to newly acquired ones. Moreover, while positional frame accounts have been successful in explaining many aspects of sequencing in language production, there is some evidence for the role of chaining, especially in typing, further motivating the study of positional constraint learning in this modality.

Typing as a window to language production

A critical question in studying typing is whether it reflects language production in the same way that speech does. Language production is essentially the process of mapping meaning to motor commands, and such commands can be executed in different modalities. There is much evidence that typing and speaking share common processing principles. For one thing, typing errors show the same linguistic categories as speech errors (Pinet & Nozari, 2018). Moreover, factors that affect speech errors also affect typing errors. For example, Pinet and Nozari (2018) replicated the "repeated phoneme effect" (Dell, 1986) as the "repeated letter effect" in typing, by showing that sharing a letter increased the probability of migration of other letters within the pair, e.g., p (fig tip → **t**ig tip) > p (fig top → **t**ig top). This finding shows feedback between lexical and sublexical layers in both spoken and typed production.

The similarity between typing and speaking is not limited to error patterns. Other findings, such as the interference induced by the segmental similarity between the target and the context (e.g., cat, mat), also extend from speech to typing (Breining et al., 2016; Harrison et al., 2020; Nozari et al., 2016; Pinet & Nozari, 2023). Importantly, such effects point to fundamental processing principles, such as incremental learning (Breining et al., 2019) in the language production system, which manifest in all production modalities. Finally, electrophysiological evidence also points to similarities between spoken and typed production and ties them both to more general processing principles. For example, Pinet and Nozari (2020) reported the same EEG components for monitoring in typing previously reported in speaking and, more generally, in action monitoring. These and other studies provide strong evidence for parallels between spoken, handwritten and typed production, suggesting typing as a useful medium for examining questions that focus on how people learn sequencing rules in language production (see Nozari, Pinet, & Muylle, 2025, for a review).

However, despite the similarities across language production modalities, the differences are not negligible. The timeline of production is different between spoken and typed modalities. The motor systems involved in speaking and typing are different. Typing involves more discrete units (key presses) than speaking, and phonotactic rules do not translate directly to typing constraints; a coda /h/ is not pronounced in English, but the letter “h” does appear at the end of typed syllables, e.g., “Noah”, or as part of digraphs, e.g., “dish”. Conversely, /v/ is a pronounceable coda, but rarely appears at the end of a typed syllable without a following vowel. Collectively, these similarities and differences make typing a unique candidate for studying sequencing rules in language production: on the one hand, differences allow the examination of orthotactic constraint learning, which may be separable from phonotactic constraint learning. On the other hand, similarities motivate the investigation of the general principles of implicit learning shared between production modalities. Finally, findings can help fill in the gaps left open by studies of phonotactic learning, as discussed in the previous section.

Current study

The present study examined the statistical learning of orthotactic constraints in typing. In a design similar to that used in studies of phonotactic learning (e.g., Dell et al., 2000), participants heard sequences of four syllables (e.g., "*dex vef ten kes")* and typed them. There were three types of letters; those representing language-wide constraints (e.g., "x" is always coda), those representing artificial experiment-wide constraints (e.g., "s" is always coda), and control letters that were unconstrained with regard to their syllabic position. We investigated the learning of new first-order (Experiment 1) and second-order (Experiment 2) orthotactic constraints by comparing the rate of legal errors on the experiment-wide letters to those of control and language-wide letters.

Our first goal was to compare the pattern of orthotactic learning with that reported in previous phonotactic learning studies to determine the extent of generalization of learning mechanisms across different modalities in language production. If orthotactic rules are learned similarly to phonotactic rules, we would expect the learning of both first-order and second-order constraints in typing, at least as quickly and efficiently as phonotactic constraints in spoken production. Our second goal was to shed further light on the nature of the mechanisms underlying sequence learning in language production. We posed two questions: (a) is the delayed learning of second-order rules in nonspeech vs. speech studies due to modality or experience? If learning positional constraints in the orthotactic domain is fundamentally different from the phonotactic domain, we expect typing to pattern with button-press studies and show earlier learning. If, on the other hand, experience determines the speed of learning, we would expect typing and speaking, both of which involve well-learned mappings, to pattern similarly to each other and differently from a button-press task with newly-acquired arbitrary mapping. (b) Are second-order constraints learned differently for onset and coda positions? If constraint learning follows a chaining-like mechanism (Washburn, 1916; Wickelgren, 1965; Lewandowsky & Murdock, 1989), we would expect learning to be confined to the coda consonant in Experiment 2 (but not in Experiment 1). Alternatively, if the second-order constraints are learned hierarchically, we would expect learning for both onset and coda consonants in Experiment 2 (as well as in Experiment 1). The logic for (b) is as follows: simple chaining is a mechanism by which simple transitional probabilities are learned in a feed-forward manner: “s” comes frequently after “a”, so retrieving the constraining vowel “a” increases the likelihood of the retrieval of a subsequent “s”. Since “s” never comes after the alternative vowel “i”, retrieving "i" is unlikely to retrieve "s". This manifests as the learning of the coda constraint in our experiment. But chaining will not work for the onset constraint because the constrained consonant “s” comes *before* the constraining vowel “a”. Therefore, an asymmetry in onset and coda learning would be compatible with an influence of simple chaining. A hierarchical account, on the other hand, is not constrained by forward or backward transitional probabilities. It can represent various constraints between letters and positions as bindings between a positional frame and letters. As such, it would not predict strong asymmetries in onset vs. coda learning for second-order constraints.

Finally, we investigated if any learning observed in typing reflects true orthotactic learning or merely phonotactic learning (Experiment 3). We studied constraint learning on the letter “s” when the set contained the opposite constraint on the letter “c”, which also had the sound /s/. If constraint learning is phonological, the opposing constraints on the sound /s/ invoked by letters “s” and “c” in different positions should cancel each other out, leading to no learning. Conversely, if learning is truly orthographic, then the “s” constraint should be learned in the orthographic space, despite having the opposite constraint in the phonotactic space. Disentangling orthographic and phonological influences are important, as phonology is known to influence typing (Pinet & Martin, 2024; Muylle et al., 2024; see Nozari et al., 2025, for a review).

Experiment 1

Methods

Participants

Most prior experiments on phonotactic learning have used a small number of participants (e.g., four in Dell et al., 2000; eight in Taylor & Houghton, 2005) and have not reported the indices of variability necessary for calculating the effect size. We thus used the values reported in the first experiment of Warker et al.’s (2009) study. The study also manipulated the congruency of heard and spoken constraints, which is not relevant to the current study. We based our estimates on the congruent condition, which returned a Cohen's D of 1.8. Given this large effect size, with $\alpha = 0.05$ and a power of 0.95, a sample size of only seven participants should be sufficient to detect an effect using the Wilcoxon Signed Rank test for matched pairs. However, there may be differences between speaking and typing, which would make this estimate inapplicable. Moreover, the current study was conducted online, which could also lead to noisier data. Finally, we were also interested in separately investigating onset and coda positions, which requires more statistical power. Therefore, to avoid running into a power issue, Experiment 1 aimed for a sample several times larger than the estimated required sample size. Twenty-four native speakers of English (12 females, $M_{age} = 21.42$, $SD = 1.64$) were recruited through the Prolific platform (Palan & Schitter, 2018) and received payment for their participation. Inclusion criteria consisted of passing a headphone check (Milne et al., 2021) to make sure the experimental material was heard clearly, and a typing proficiency test to ensure that participants were touch-typists, i.e., they could type quickly without having to look at their hands (Pinet & Nozari, 2021; see Procedures). Participants who could not pass either the headphone check or the typing proficiency test did not proceed to the experiment. The study was approved by the Carnegie Mellon University Institutional Review Board (IRB).

Materials

The stimuli were modeled after the studies of phonotactic constraints (e.g., Dell et al., 2000, Experiment 1). Ninety-six sequences, composed of four CVC syllables, were generated using the vowel "e"(/ɛ/) and eight consonants. The consonants were divided into three categories: *language-wide constraints* ("v" and "x"), *experiment-wide constraints* ("t" and "s"), and *unrestricted* ("k", "n", "f" and "d"). Each of these consonants appeared only once within a sequence (e.g., "*dex vef ten kes").* Although "v" and "x" can occasionally appear as coda and onset in English (e.g., "listserv", "Xanax") such occurrences are rare. To show this objectively, we analyzed the 3,000 most frequent words in the English lexicon (Brysbaert & New, 2009). This analysis showed that the probability of "v" and "x" appearing at the end and the beginning of words was 0.005 and 0, respectively. In contrast, the experiment-wide constraints showed no such strong restriction in this corpus. Letters “t” and “s” appeared with a probability of 0.38 and 0.21 in the onset and 0.30 and 0.34 in the coda positions, respectively. We also ensured that “t” and “s” and the four unrestricted consonants were matched in frequency (pi.math.cornell.edu). Additionally, “t” and “s” were both typed with the left hand and had a uni-manual transition to the vowel (Scaltritti et al., 2016). The four control letters were selected such that two of them were typed with the left and two with the right hand, with uni-manual and bi-manual transitions to the vowel, respectively. In half of the participants, “t” appeared only in the onset position and “s” only in the coda position. In the other half, the assignment was reversed. Unrestricted consonants were presented equally often in both positions. The 96 sequences were presented as auditory stimuli generated using Descript (www.descript.com) with the voice of a native female speaker of American English. Each sequence was 2s long, with the four syllables produced in 500 ms intervals.

Procedures

The experiment was developed in the jsPsych library (de Leeuw, 2015) and administered through JATOS (Lange, Kuhn & Filevich, 2015). A typing proficiency test (Pinet & Nozari, 2021) was administered to include participants who could type quickly without looking at their hands. In the first part of the test, participants typed 15 words, one at a time, without any time pressure. In the second part, they typed an additional 15 words with a 6-second time limit for each word. Participants passed the test if they had an accuracy of 80% for the words without a time limit and 50% for the words with a time limit.

Each trial consisted of an “acquisition phase” and a “test” phase. In the acquisition phase, participants first heard each of the four syllables and typed them one at a time without time pressure. If participants made a mistake, the correct syllable appeared on the screen, and they typed it again until it was correct. In the test phase, they heard the whole sequence followed by a beep and typed the syllables with a deadline of 4s. This was repeated three times. Once they were ready, participants initiated the next trial by pressing the space bar. The use of backspace was not allowed. Participants first watched an orientation video and completed two practice trials. They then completed three experimental blocks of 32 trials, with a break in between the blocks. The trial order within each block was randomized for each participant. This experiment was administered in a single session that took roughly an hour. The keystrokes in the test phase were registered for analysis.

Analysis

Participants' typing errors were coded by a trained coder blind to the hypotheses of the study and double-checked by a second coder. Each error was coded as legal or illegal. Legal errors were defined as letters migrating to the same syllabic position as the target within the sequence, e.g., "*tek des fen vex"* → "*tek **t**es fen vex"*. Illegal errors were defined as letters migrating to a different syllabic position from the target (within the sequence, e.g., "*tek des fen vex"* → "*tek de**t** fen vex").* To decide whether an error involving unrestricted consonants was legal or illegal in a given sequence, its position in that particular target sequence was used as a reference. For example, if the unrestricted consonant "k" appeared in the coda position in the target sequence ("*tek des fen vex"),* it was coded as a legal error if it moved to another coda position and illegal if it moved to an onset position.

Analyses were carried out in R version 4.0.3. Linear mixed effect models (LMEMs) were fit using the lme4 R package version 1.1-26 (Bates et al., 2015). A logistic version of the model was used to model a categorical DV (legal vs. illegal errors). P-values were estimated via Satterwhite approximation using the lmerTest package version 2.0- 33 (Kuznetsova, Brockhoff, & Christensen, 2016). For the analyses that included comparing multiple non-orthogonal contrasts, the multcomp R package version 1.4-16 (Hothorn, Bretz & Westfall, 2010) was used, which returns Tukey-corrected p-values for pairwise comparisons. The random effect structure was kept maximal according to the recommendation by Barr et al. (2013) unless the model did not converge. In such cases, the random effect structure was reduced by first eliminating the slopes for items and subjects. As most models did not tolerate random slopes, unless stated otherwise, we included the random intercept of subjects and items in all models for consistency. All the critical results were double-checked by the more conservative non-parametric Wilcoxon signed-rank tests, which make no assumptions regarding the underlying distributions. P values from these non-parametric tests are reported after the Bonferroni correction for multiple comparisons to avoid type I error.

All the data, the analysis code and the complete outputs are publicly available on OSF, through the following link: (OSF link is hidden due to anonymous review)

Results

There was a total of 27,648 opportunities for producing syllables. Missing 252 syllables, participants produced 27,396 syllables in total. The average error rate at the syllable level was 26%. The decisions on error coding were adopted from studies of phonotactic learning (Dell et al., 2000; Warker et al., 2009). Only strings containing three letters or fewer, with a vowel in the second position, were included in the analysis to ensure a clear structure for coding positions and determining the legality of errors. Therefore, syllables with different structures (e.g., *"tekd", "dx", "veen",* etc.) were excluded. Errors including lexical shifts (e.g., *tek dex ven fes* → *tek* ***ven dex*** *fes*) were also excluded from the analysis (4.6% of the produced syllables). In addition, letters that were not part of the sequences (e.g., "*z", "q",* etc.) were excluded, which corresponds to less than 1% of the total 78,107 letters in the remaining syllables. Table 1 shows the profile of the analyzed errors after these exclusions.

Table 1: Number of errors in the three conditions for the onset and coda positions, and their proportion out of the total number of letters produced in that category.

Condition	Onset	Coda	Total	Proportion
Language-wide	574	546	1120	8%
Experiment-wide	421	635	1056	8%
Unrestricted	1152	1689	2841	10%

Figure 1a shows the results. Error legality was assessed as a function of letter type with three pairwise comparisons of interest (language-wide vs. unrestricted, experiment-wide vs. unrestricted, and experiment-wide vs. language-wide), with Tukey-corrected p-values for multiple comparisons. As expected, the proportion of legal errors on letters with language-wide constraints was significantly higher than the unrestricted (β = 2.67, z = 12.975, $p < .001$). Importantly, the proportion of legal errors on letters in the experiment-wide condition was also significantly higher than unrestricted (β = 2.87, z = 12.673, $p < .001$) and comparable to language-wide (β = 0.20, z = 0.69, $p = .687$). These results were further supported by the non-parametric Wilcoxon tests after Bonferroni correction[2].

[2] The proportion of legal errors on letters in the language-wide condition was significantly higher than unrestricted ($z = 3.91$, $p < .001$). Also, the proportion of legal errors on letters in the experiment-wide condition was significantly higher than the unrestricted ($z = 4.15$, $p < .001$) and comparable to the language-wide condition ($z = 1.19$, $p = 0.699$).

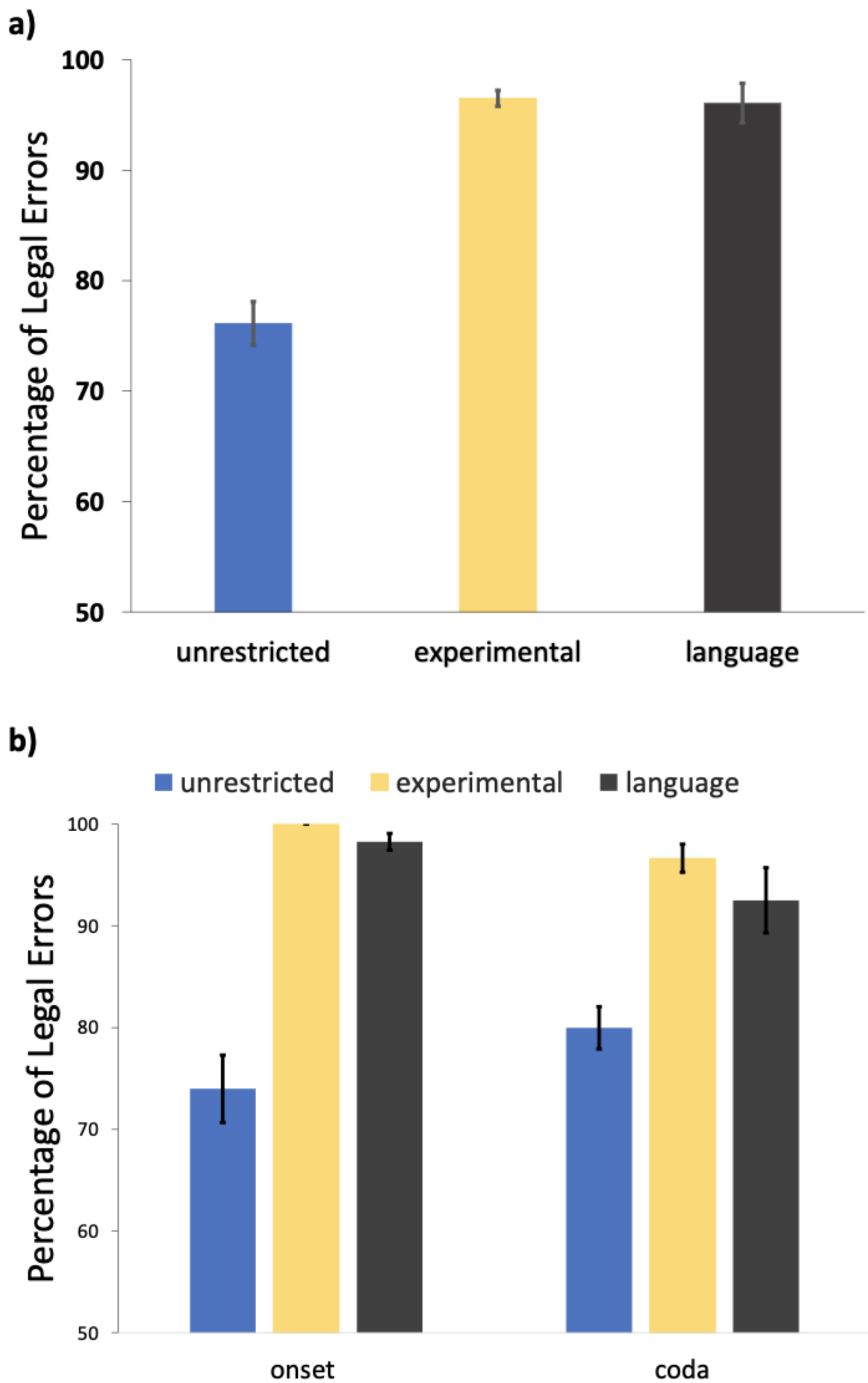

Figure 1: a) Mean proportion of legal errors ± SE of the subject means in the unrestricted, experiment-wide (experimental) and language-wide (language) conditions. b) Mean proportion of legal errors ± SE broken down by onset and coda positions for each of the three letter types.

Figure 1b shows the breakdown of legal error proportions by onset and coda positions. To examine the possible differences between learning the new constraints in the onset vs. coda positions, we conducted a second analysis predicting error legality as a function of letter type, position, and the interaction between the two. Letter type was contrast coded as experiment-wide vs. unrestricted and experiment-wide vs. language-wide. Position was center-coded (-.5 vs. .5) for onset vs. coda. Table 2 reports the results of this analysis. As expected, the effect of letter type was significant for the contrast between experiment-wide vs. unrestricted, but not for the experiment-wide vs. language-wide. There was also a significant effect of position ($\beta = 1.46$, $z = 3.65$, $p < .001$), with slightly more legal errors in the onset compared to the coda position. Importantly, we also found a significant interaction between the contrast for the experiment-wide vs. unrestricted conditions and position ($\beta = 2.79$, $z = 2.72$, $p = .007$). To unpack this interaction, post-hoc tests were conducted separately on onset and coda datasets. The results showed learning in both positions with a larger magnitude of learning (see β) in the onset ($\beta = 5.22$, $z = 5.25$, $p < .001$) than the coda ($\beta = 2.38$, $z = 9.84$, $p < .001$) position. The results were further again supported by the non-parametric Wilcoxon tests[3].

Table 2. Results of the analysis of Experiment 1.

Fixed Effects	**Estimate**	**SE**	**z**	**p value**
Intercept	3.469	0.228	15.24	< .001
Experiment-wide vs. unrestricted	3.713	0.513	7.244	< .001
Experiment-wide vs. language-wide	0.572	0.595	0.960	0.337
Letter Position	1.457	0.399	3.654	< .001
Experiment-wide vs. unrestricted x Letter Position	2.787	1.025	2.718	0.007
Experiment-wide vs. language-wide x Letter Position	0.542	1.192	0.455	0.650

[3] Non-parametric tests after Bonferroni correction showed significantly more legal errors in the experiment-wide vs. unrestricted conditions in both onset ($z = 4.18$, $p < .001$) and coda ($z = 3.85$, $p < .001$) positions.

Discussion

Results of Experiment 1 showed that participants were able to learn new arbitrary orthotactic constraints. The above-chance legality of errors in the unrestricted condition shows that typing errors, similar to speech errors, respect syllabic positions (Nooteboom, 1967, 1969). Moreover, the near-ceiling proportion of legal errors on experiment-wide letters, which was significantly higher than the unrestricted letters and comparable to language-wide letters, shows learning of artificial constraints with a magnitude and time-course similar to studies of phonotactic-constraint learning in spoken production(e.g., Dell et al., 2000). Finally, we found robust learning in both onset and coda positions, with a larger magnitude of learning in the onset position. While the strength of onset vs. coda effects in first-order constraint learning has not been examined before, the asymmetry observed here is compatible with generally stronger effects on initial segments in typing, for example Snyder and Logan (2014) showed greater facilitation for prime-targets overlapping in initial (e.g., "busy", "burn") than final (e.g., "busy", "easy") segments. Experiment 2 investigated the learning of second-order constraints in typing.

Experiment 2

Methods

Participants

Cohen's D for second-order constraint learning, estimated based on Warker and Dell's (2006) Experiment 1a, was 1.45. With $\alpha = 0.05$ and a power of 0.95, the required sample size for replicating the effect was nine. Experiment 1 showed that the results of prior studies can be replicated well using typing and online platforms. Nonetheless, since we were interested in separately investigating onset and coda positions, we roughly doubled the estimated sample size to avoid low power. Twenty native speakers of English (12 females, M_{age}= 22.35, SD = 1.96), recruited through the Prolific platform (Palan & Schitter, 2018), participated for payment. None had participated in Experiment 1. As in Experiment 1, all participants were required to pass the headphones check and the typing screening test. The study was approved by the Carnegie Mellon University Institutional Review Board (IRB).

Materials

Materials were modeled after the second-order phonotactic constraints studies (i.e., Warker and Dell, 2006, Experiment 1). Ninety-six sequences, composed of four CVC syllables, were generated. As in Experiment 1, "v" and "x" were the language-wide constraints, always appearing in onset and coda positions, respectively. Two vowels, "a" (/æ/) and "i" (/ɪ/), were used to restrict the two experimental consonants, "k" and "f", in alternating sequences. For half of the participants, "k" was always an onset and "f" was always a coda when the vowel was "a" (e.g., "*naf vat kas dax")* and the assignment was reverse when the vowel was "i" (*e.g., "sid vik tix fin").* The other half of the participants were exposed to the opposite assignments. An additional four letters in the unrestricted group ("t", "s", "n" and "d") were paired equally often with the two vowels in onset and coda positions. The auditory syllables were generated in the same manner described in Experiment 1.

Procedures

This experiment consisted of two sessions, completed on two days 24-48 hours apart. The structure of the two sessions was identical to one another and to that of Experiment 1. Sequences were presented pseudo-randomly for every participant, with the constraint that the two vowels alternated between trials. The keystrokes in the test phases of both sessions were registered for analysis.

Results

Missing 676 and 706 syllables, participants produced 22,364 and 22,334 syllables on the first and second day, respectively. The average error rate at the syllable level was 31% (Day 1) and 22% (Day 2). As in Experiment 1, lexical shifts and unacceptable syllable structures (5% on Day 1 and 3% on Day 2) were excluded from the analysis. Also, letters that were not part of the sequences were excluded (1% of the total 63,006 and 64,022 letters in the remaining syllables, produced on days 1 and 2, respectively). Table 3 shows the profile of the analyzed errors.

Table 3: Number of errors in the three conditions for the onset and coda positions on days 1 and 2 and their proportions out of the total number of letters produced in that category.

	Day 1				**Day 2**			
Condition	Onset	Coda	Total	Proportion	Onset	Coda	Total	Proportion
Language-wide	560	491	1051	8%	475	438	913	7%
Experiment-wide	546	611	1157	8%	505	588	1093	8%
Unrestricted	1197	1679	2876	10%	992	1307	2299	8%

As in Experiment 1, each error was coded as legal or illegal, based on whether the migrated letter maintained its syllabic position within the sequence or not. Figure 2a shows the data. In a first-pass analysis, we examined the legality of errors as a function of letter type with three pairwise comparisons of interest (language-wide vs. unrestricted, experiment-wide vs. unrestricted, and experiment-wide vs. language-wide), with Tukey-corrected p-values for multiple comparisons.

As expected, there were significantly more legal errors in the language-wide vs. unrestricted condition ($\beta = 1.80$, $z = 16.30$, $p < .001$). Importantly, there were also more legal errors in the experiment-wide vs. unrestricted condition ($\beta = 0.25$, $z = 3.84$, $p < .001$), although there was a significant difference between the rate of legal errors in the experiment-wide and language-wide condition ($\beta = -1.55$, $z = -13.04$, $p < .001$. The non-parametric test also supported the difference between the language-wide and unrestricted conditions[4].

[4] Wilcoxon test showed a significant difference between the language-wide and unrestricted conditions ($z = 4.23$, $p < .001$), but did not show a significant difference between the experiment-wide and unrestricted condition ($z = 0.97$, $p = .990$). It did show a significant difference between the experiment-wide vs. language-wide condition ($z = -4.76$, $p < .001$).

While both LMEM and Wilcoxon test confirmed the greater legality of language-wide errors, their results differed with regard to the critical test of Experiment 2, namely, the learning of second-order constraints. This implies that this finding is not robust. Additionally, the analyses above did not take into account the learning day, which phonotactic studies suggest plays an important role in the emergence of the effect. Therefore, we next tested a model with letter type, day, and the interaction between the two as the fixed effects. Letter type was contrast coded as experiment-wide vs. unrestricted and experiment-wide vs. language-wide. Day was center-coded (-.5 vs. .5) for day1 vs. day2. Table 4 reports the results of this analysis.

The critical difference between the experiment-wide and unrestricted conditions was not significant in this model. Instead, there were significantly more legal errors in the experiment-wide vs. language-wide condition (β = -1.69, z = -11.04, p <.001). There was also a significant effect of day (β = 0.34, z = 4.08, p <.001) with more legal errors on day 2 compared to day 1. Importantly, there was a significant interaction between the contrast for the experiment-wide vs. unrestricted conditions and day (β = -0.51, z = -3.92, p <.001), implying different learning for day 1 vs. day 2. To further investigate this interaction, post-hoc tests examined the legality of errors separately for day 1 and day 2. Tukey-corrected contrasts showed that on day 1, the rate of legal errors in the experiment-wide condition was comparable to the unrestricted condition (β = 0.03, z = 0.37, p = .924), and significantly lower than the language-wide condition (β = -1.67, z = -10.84, p < .001). On day 2, however, the rate of legal errors in the experiment-wide condition was significantly higher than in the unrestricted condition (β = 0.52, z = 5.15, p < .001), even though it was still lower than the language-wide condition (β = -1.42, z = -7.58, p < .001). These results were supported by the non-parametric tests[5].

[5] The Bonferroni-corrected Wilcoxon tests showed that, on day 1, the rate of legal errors in the experiment-wide condition was comparable to the unrestricted condition (z = 0.86, $p \approx 1$), and significantly lower than the language-wide condition (z = -4.62, p < .001). On Day 2, however, the rate of legal errors in the experiment-wide condition was significantly higher than the unrestricted condition (z = 2.92, p = .014), even though it was still lower than the language-wide condition (z = -3.76, p = .001).

Table 4. Results of the analysis of Experiment 2.

Fixed Effects	**Estimate**	**SE**	**z**	**p value**
Intercept	1.855	0.109	17.061	< .001
Experiment-wide vs. unrestricted	0.023	0.085	0.267	0.790
Experiment-wide vs. language-wide	-1.694	0.154	-11.034	< .001
Day	0.339	0.083	4.078	< .001
Experiment-wide vs. unrestricted x Day	-0.509	0.130	-3.917	< .001
Experiment-wide vs. language-wide x Day	0.306	0.240	1.280	0.201

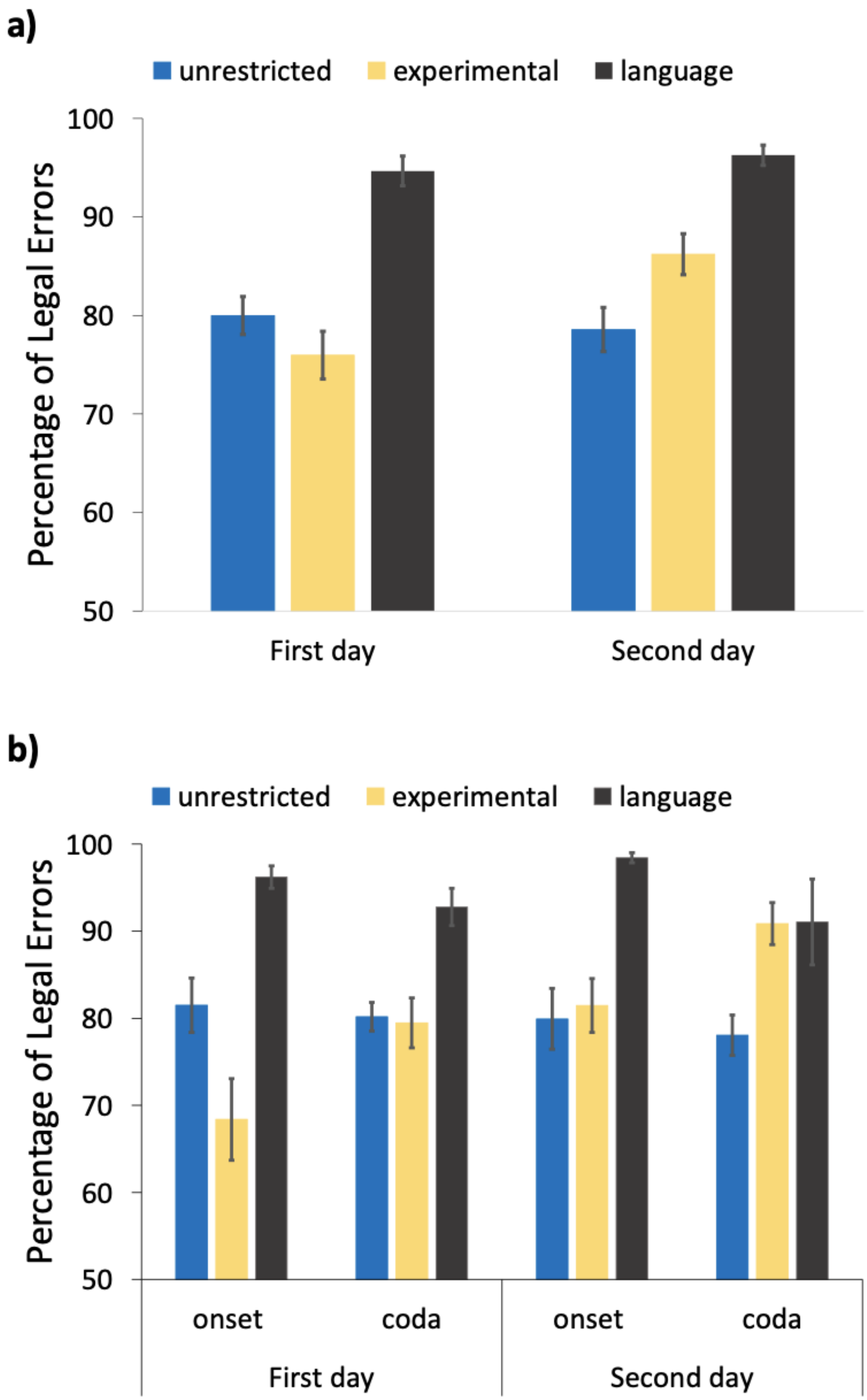

Figure 2: Results of Experiment 2. Percentage of legal errors on day 1 and day 2 ± SE (a), and the breakdown by onset/coda positions (b).

Next, we turn to the issue of position-dependent learning. Figure 2b shows the pattern of second-order constraint learning separately for onset and coda. Since the previous analyses showed learning only on day 2, we focused our critical investigation of positional learning on day 2. We first tested a model with letter type (experiment-wide vs. unrestricted and experiment-wide vs. language-wide) and letter position (onset vs. coda) and the interaction between the two as the fixed effect structure. Table 5 reports the results of this analysis. There were significantly more legal errors on experiment-wide vs. unrestricted letters ($\beta = 0.85$, $z = 5.51$, $p < .001$), although not as many as on the language-wide letters ($\beta = -0.53$, $z = -2.32$, $p = .021$). There was also a marginal effect of position ($\beta = 0.33$, $z = 1.99$, $p = .047$), with slightly more legal errors on the coda. Critically, there was also a significant interaction between the contrast for the experiment-wide vs. language-wide condition and position ($\beta = -2.36$, $z = -4.88$, $p < .001$) and for the experiment-wide vs. unrestricted letters and position ($\beta = 0.596$, $z = 2.84$, $p = .005$). To unpack this interaction, two post-hoc tests with Tukey-corrected comparisons were performed separately on onset and coda subsets of data on day 2. In the onset position, the proportion of legal errors in the experiment-wide condition was not significantly different from the unrestricted condition ($\beta = 0.28$, $z = 1.94$, $p = .116$), and it was significantly lower than the language-wide condition ($\beta = -2.93$, $z = -6.88$, $p < .001$). Conversely, in the coda position, the proportion of legal errors in the experiment-wide condition was significantly higher than the unrestricted condition ($\beta = 0.88$, $z = 5.72$, $p < .001$) and only marginally lower than the language-wide condition ($\beta = -0.52$, $z = -2.25$, $p = .061$).

Non-parametric tests supported the differences between onset and coda positions on Day 2. The proportion of legal errors in the experiment-wide condition was not significantly different from the unrestricted condition in the onset position ($z = 0.221$, $p \approx 1$) but it was in the coda position ($z = 2.856$, $p = .017$).

Table 5: Results of the analysis on position-dependent learning on Day 2 in Experiment 2.

Fixed Effects	**Estimate**	**SE**	**z**	**p value**
Intercept	2.129	0.130	16.437	< .001

Experiment-wide vs. unrestricted	0.848	0.154	5.513	< .001
Experiment-wide vs language-wide	-0.532	0.230	-2.317	0.021
Letter Position	0.326	0.164	1.988	0.047
Experiment-wide vs. unrestricted x Letter Position	0.587	0.207	2.841	0.005
Experiment-wide vs. language-wide x Letter Position	-2.360	0.484	-4.879	< .001

Although the earlier analyses did not support learning on day 1, we conducted parallel analyses to those reported above, as a comparison to the positional effect found on day 2. The model with letter type, letter position and their interaction showed that the proportion of legal errors in the experiment-wide condition was marginally lower than the unrestricted condition ($\beta = 0.22$, $z = 1.78$, $p = .076$) and significantly lower than the language-wide condition ($\beta = -0.99$, $z = -5.03$, $p < .001$). There was also a marginal interaction between the contrast for the experiment-wide vs. unrestricted conditions and position ($\beta = 0.34$, $z = 1.95$, $p = .051$) and a significant interaction between the contrast for the experiment-wide vs. language-wide conditions and position ($\beta = -1.51$, $z = -4.60$, $p < .001$). Tukey-corrected post-hoc tests on onset and coda subsets of the Day 1 dataset found no significant difference between the experiment-wide and unrestricted conditions for either the onset ($\beta = -0.15$, $z = -1.24$, $p = .414$) or the coda ($\beta = 0.19$, $z = 1.48$, $p = .294$) position. In keeping with these results, non-parametric tests showed no significant differences between the proportion of legal errors on experiment-wide vs. unrestricted conditions in either the onset ($z = 1.934$, $p = .213$) or the coda ($z = 0.311$, $p \approx 1$) position.

Discussion

The results of Experiment 2 showed that second-order constraints can be learned in typing. When lumped together over both days, evidence of learning was not robust. However, when day was entered into a model, it significantly modulated the effect. Post-hoc tests revealed robust learning on the second day, but not on the first day, replicating the pattern commonly observed in studies of phonotactic learning in spoken production (e.g., Warker & Dell, 2006). It is noteworthy that even on the second day, the second-order constraints were not learned as well as the language-wide constraints, pointing to the greater difficulty of incorporating these more complex rules into the production system, despite the high rate of errors and opportunities for error-based learning in the current study.

The next set of analyses examined the sensitivity of learning to syllabic position. This analysis focused on day 2, which the first set of analyses indicated as the timepoint where robust learning was observed. However, we also reported the analyses of data on day 1 for comparison. Entering the syllabic position in the model significantly modulated the learning effect on day 2. Post-hoc tests indicated robust learning in the coda, but not the onset, position. On day 1, there was weak evidence of the modulation of a possible effect by syllabic position, in the form of a marginal interaction between the critical contrast of experiment-wide vs. unrestricted conditions and position. This interaction is likely due to the lower proportion of legal errors in the experiment-wide condition in the onset position on day 1, which post-hoc tests confirmed was not significantly different from the unrestricted condition. Thus, even though there seems to be a numerical increase in the rate of legal errors in the experiment-wide condition in the onset position from the first day to the second day, the rate is not significantly different from the unrestricted baseline on either day. Similarly, the proportion of legal errors in the experiment-wide condition in the coda position was not significantly different from the unrestricted condition.

Collectively, these findings support the results of the basic analysis: no robust learning was observed on the first day for either the onset or the coda position. In contrast, the constraint was robustly learned on the second day, but only in the coda position. These results are compatible with learning through a chaining mechanism, where the vowel can trigger the learning of the following, but not the preceding, contingency. This difference cannot be attributed to a bias in favor of learning in the coda condition per se. When sequential learning was not an option, as in learning the first-order constraints in Experiment 1, learning was stronger in the *onset* position. Moreover, the stronger learning in the coda position in Experiment 2 cannot be attributed to the weak learning cues for the onset constraint. The alternating sequences made the upcoming vowel completely predictable. Thus, if the prediction was enough to learn the constraint, the onset constraint could have been easily learned. Instead, these results suggest that the *production*, rather than the *anticipation*, or the vowel is the key to driving the learning of orthotactic constraints. The final experiment examined whether orthotactic learning is truly orthotactic in nature or is, instead, driven by phonotactic learning. For simplicity, we focused on the stronger first-order constraint learning.

Experiment 3

Recall that for a constraint to be learned, it must not be strongly opposed. For example, participants can learn "s is coda" if the majority of trials show this constraint. However, if half of the trials contain "s" in the onset position, "s" will become an unrestricted consonant and there cannot be any learning. The opposition logic has been successfully used in the past work to test whether constraints that have different sources (e.g., production vs. comprehension) interact during phonotactic learning (Kittredge & Dell, 2016). In Experiment 3, we use the opposition logic to disentangle phonotactic from orthotactic learning. As before, the experiment contained two experiment-wide restricted ("f" and "g") and four unrestricted consonants. But this time, we also added a critical restricted consonant "s" and set the rule "s is coda". Half of the participants were also exposed to an additional rule "c [sounding as /s/] is onset." (e.g., "*fe**s** reg **c**ep den",* sounding as *"/fɛ**s** rɛg **s**ɛp dɛn/").* If the two rules are treated *orthotactically*, there is no opposition ("s" is coda and "c" is onset). We, therefore, expect the rule "s is coda" to be fully learned. However, if the rule is treated *phonotactically*, there is full opposition (/s/ is coda and /s/ is onset), and we would expect no learning for "s". Due to the possibility of such opposition, we call this condition *opposed*. A control condition was included to control for the letter "c" adding a new constraint. In the control condition, the rule was "c [sounding as /k/] is coda." (e.g., "*fe**s** reg pe**c** den",* sounding as *"/fɛ**s** rɛg pɛ**k** dɛn/").* Note that while the number of constraints, restricted letters and their identity are identical across the two conditions, the control condition is inherently unopposed ("s" /s/ is coda and "c" /k/ is coda[6]). Therefore, we call this condition *unopposed.* This design allows us to generate different predictions for orthotactic and phonotactic learning summarized below:

Orthotactic learning: restricted s-opposed = restricted s-unopposed = restricted f/g > unrestricted (Evidence of better constraint learning in all restricted conditions compared to the unrestricted condition).

Phonotactic learning: restricted s-opposed = unrestricted < restricted s-unopposed = restricted f/g (Evidence of better constraint learning in the s-unopposed and f/g restricted conditions compared to the unrestricted condition. No such evidence for the restricted s-opposed condition).

Methods

Participants

No a priori effect size existed for estimating the sample size for Experiment 3. The sample was thus determined a priori. Twenty-four native speakers of English (9 females, M_{age}= 22.5, SD = 1.98) were recruited through the Prolific platform, who had not participated in the first or second experiments, and received payment for participation. The study was approved by the Carnegie Mellon University Institutional Review Board (IRB). As in Experiments 1 and 2, all participants were required to pass the headphones check and the typing screening task.

[6] It is true that the position of the letter "c" differs in experimental and control conditions, but (a) this is unavoidable, given the pronunciation rules for "c" before and after the vowels used in these experiments. If we wanted to use "c" in the same position, we would have had to change the vowel, which would have been a departure from Experiments 1 and 2. (b) The different position of "c" should not pose a problem though. Earlier analyses have shown that the learning of first-order constraints is not position-dependent. Moreover, we are not comparing the learning of "c" in onset and coda positions. The critical comparison always involves learning "s as coda", when an opposing "c" (/s/) is present in vs. when it is absent from the onset position. The addition of "c"-as-coda constraint in the control condition merely balances the number of critical letters and constraints.

Materials

Ninety-six sequences, composed of four CVC syllables, were generated using the vowel “e” (/ɛ/). Two letters (“f” and “g”) always appeared in the onset or coda position (restricted) and four letters (“r”, “p”, “n”, and “d”) appeared equally often in onset and coda positions (unrestricted). In addition, a critical restricted letter “s” always appeared in the coda position. In the absence of any other manipulations, “s” would be similar to the two other restricted letters (“f” and “g”). But we manipulated the phonotactic constraint imposed by “s” by adding a letter “c”. In the experimental condition, “c” always appeared in the onset position, and was pronounced as /s/ (e.g., “*fes reg cep den*”, sounding as *“/fɛ**s** rɛg **s**ɛp dɛn/”).* Orthotactically, the letter “s” is fully constrained (it only appears in the coda position). However, phonologically speaking, this condition creates opposing phonotactic constraints (/s/ is both coda and onset). We, therefore, call it *opposed.* To control for the appearance of “c” in the string, while ensuring its different pronunciation without changing the vowel, we also created a control condition in which "c" appeared in the coda position and was pronounced /k/ (e.g., *“fes reg pec den”* sounding as *“/fɛ**s** rɛg pɛ**k** dɛn/”).* In this condition, there is no opposition to the coda “s” (/s/), either orthotactically or phonotactically. We, therefore, call it *unopposed.* The auditory materials were generated in the same manner as described in Experiments 1 and 2.

Procedures

The procedure was similar to Experiment 1, except for two changes. First, the target syllables were presented both auditorily and visually in the first two repetitions of the test phase, followed by two repetitions with only the auditory presentation. The sequences appeared in black, Verdana font, 15px and approximately 40px above the response box. Second, participants received feedback not only in the acquisition but also on their first full recitation of the sequence. These changes were made to minimize the auditory confusion between the “s” and “c” sounds when they were interchangeable as in “*fes reg cep den”*. This experiment took approximately one hour. The keystrokes in the last two repetitions of the test phase, which did not include a visual presentation of the sequence, were registered for analysis.

Results

Participants produced 18,363 syllables in total, missing 69 syllables. The average error rate at the syllable level was 11%. As in Experiments 1 and 2 lexical shifts and unacceptable syllable structures were excluded (3%). Also, of the total 53,508 letters in the remaining syllables, letters that were not part of the sequences were excluded (< 1%). After these exclusions, we analyzed 647 errors in the unrestricted category (4% of the remaining letters in this category), 345 errors in the restricted category (3% of the remaining letters in this category), and 146 errors in the restricted letter "s" (3% of the remaining letters in this category).

Legality was coded in the same manner as the previous experiments. Not surprisingly, phonological confusion caused some "c" (/s/) letters to be typed as "s" (25 errors). These errors, while showing a clear influence of phonology on orthography are too ambiguous to serve as a test for positional violation. We, therefore, examined the legality of the remaining 121 errors on critical "s" that were unambiguous. Figure 3 shows the results. Percentage of the legal errors on the unrestricted letters is shown alongside three restricted conditions: "f" and "g", which are unopposed, "s" in the unopposed condition and "s" in the opposed condition.

The first-pass analysis was carried out to replicate the main finding of the previous two experiments, namely that new constraints can be learned, and that the restricted letter "s" generally behaved like the restricted letters. Error legality was assessed as a function of letter type with three pairwise comparisons of interest (restricted "f/g" vs. unrestricted, restricted "s" vs. unrestricted, and restricted "f/g" vs. restricted "s"), with Tukey-corrected p-values for multiple comparisons[7]. As expected, we found significantly more legal errors in the restricted vs. the unrestricted conditions ($\beta = 1.05$, $z = 3.80$, $p < .001$). Moreover, there were also significantly more legal errors on the restricted "s" compared to the unrestricted condition ($\beta = 2.32$, $z = 3.23$, $p = .003$), with no reliable difference between the rate of legal errors between the restricted "s" and restricted "f/g" letters ($\beta = -1.27$, $z = -1.69$, $p = .193$). These results were supported by the non-parametric

[7] The same analysis was carried out with the letter "c" included as a part of restricted letter type group. The pattern was the same (see https://osf.io/gmzfq/, Results, analysis #3.2).

tests[8] . These results replicate those of Experiments 1 and 2 and show that, generally speaking, the restricted "s" behaves like any other restricted consonant.

We next tested whether the difference in the legality of errors was modulated by our manipulation. The legality of errors was predicted as a function of letter type ("s" vs. "unrestricted"), opposition group (opposed vs. unopposed group) and the interaction between the two. Compatible with the previous analysis, we found a main effect of letter type (β = -2.23, z = -3.10, p = .002) but no significant differences in legality across the two opposition groups (β = -0.34, z = -0.43, p = .664) or a significant interaction between letter type and opposition group (β = -0.58, z = -0.40, p = .690; Table 6). These results were supported by the Bonferroni-corrected non-parametric tests[9], which showed that the effect was present in both the opposed and the unopposed conditions.

Table 6: Results of the analysis of Experiment 3.

Fixed Effects	**Estimate**	**SE**	**z**	**p value**
(Intercept)	3.369	0.405	8.328	<.001
Opposition	-0.338	0.780	-4.434	.664
Letter Type	-2.228	0.720	-3.095	.002
Opposition x Letter Type	-0.575	1.439	-0.400	.689

[8] The Bonferroni-corrected signed-rank test results confirmed that the proportion of legal errors on the restricted letter "s" was significantly higher than unrestricted (z = 3.703, p < .001), and it also showed that the restricted constraints (f/g) was slightly lower than both opposition groups (z = -2.550, p = .022).

[9] The proportion of legal errors on the restricted letter "s" was significantly higher than the unrestricted letters in both in the opposed (z = 2.61, p = .027) and the unopposed (z = 2.61, p = .027) group, with no significant difference between the two (z = 0.49, $p \approx 1$).

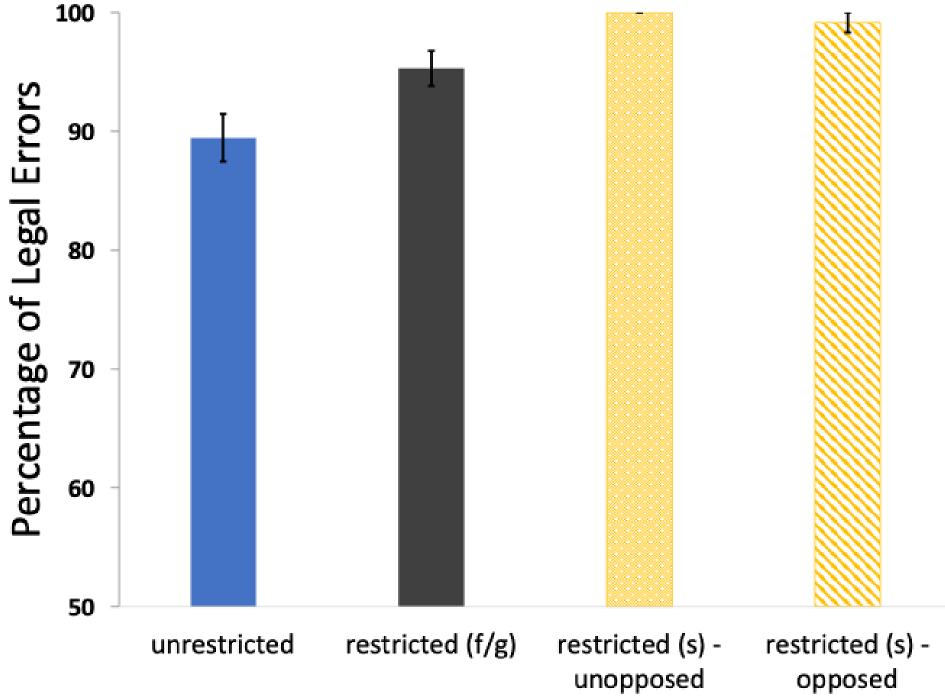

Figure 3: Results of Experiment 3. Mean proportion of legal errors ± SE of the subject means in unrestricted, restricted (f/g)and the restricted (s) conditions, restricted-s letter is shown divided by the opposition status.

Discussion

As expected, and in line with past reports of the influence of phonology on typing (Pinet & Martin, 2024), when the letter "c" sounded as /s/, participants sometimes mistakenly typed it as "s". When these errors were set aside, the remaining errors involving the restricted "s" in the opposed condition were almost all legal. This should not have been the case if learning was primarily phonotactic, since the opposition provided by "c" (/s/) should have simply made the letter "s" an unrestricted consonant with a much lower percentage of legal errors. In short, the results were compatible with the prediction "restricted s-opposed = restricted s-unopposed = restricted f/g > unrestricted", supporting the orthotactic hypothesis.

General Discussion

In three experiments, we investigated the learning of new orthotactic constraints in typing. We found that people were able to learn first-order constraints within an hour (Experiment 1), while the evidence for the learning of the second-order constraints only appeared on the second day (Experiment 2). The results of these two experiments closely mirror the findings on phonotactic learning (Dell et al., 2000; Warker & Dell, 2006), raising the question: Is orthotactic learning fully mediated by phonotactic learning? Experiment 3 ruled out this possibility. Collectively, these results show strong parallels between orthotactic and phonotactic learning of novel constraints, pointing to similar computational principles underlying incremental learning in different modalities of language production.

In addition, we tested two new hypotheses regarding the mechanisms of such learning. The first of these two addressed the discrepancy between the timeline of learning of the second-order constraints across studies. Recall that while some studies have reported learning only on the second day of training (Warker & Dell, 2006; Warker, 2013; Gaskell et al., 2014; Anderson & Dell, 2018), others have reported learning of such complex rules on the first day, at least under some circumstances (Muylle et al., 2021; Smalle et al., 2017; 2021; 2022; Smalle & Szmalec, 2022). Several reasons have been proposed for the discrepancies between different studies on the timeline of second-order constraint learning, including the nature of different languages (Smalle & Szmalec, 2022) and cognitive load (Smalle et al., 2021). Interestingly, children seem to learn the second-order rules faster than adult speakers (Small et al., 2017).

One mechanistic explanation for the faster learning of complex rules in children could be that an immature or noisy production system is more error-prone (e.g., Budd, Hanley, & Nozari, 2012; Hanley et al., 2016;), and more errors provide more opportunities for error-based learning (e.g., Waller, Yurovsky, & Nozari, 2025), which has been proposed as the underlying mechanism for phonotactic learning (Anderson & Dell, 2018). In keeping with past studies showing high error rates in typing (Pinet & Nozari, 2020; 2021, 2022), the error rate in Experiment 2 was three times higher than that reported in phonotactic studies that had shown delayed learning of second-order rules (e.g., Warker & Dell, 2006). Nonetheless, we observed the same delayed timeline of learning reported in those studies, with no evidence of learning on the first day and full-fledged learning on the second day. Therefore, it does not appear that the quantity of errors is a critical determiner of faster learning.

Another series of studies that demonstrated faster learning of second-order constraints were non-speech studies (Anderson & Dell, 2018; 2019; Rebei, Anderson, & Dell, 2019). In those studies, participants responded by pressing buttons with arbitrary links to sounds. Thus, it was unclear whether the modality (button-press vs. speech) or experience (newly acquired arbitrary mappings vs. well-learned mappings) was responsible for the faster learning in the nonspeech condition. Our results can adjudicate between these factors because typing uses the button-press modality but with well-learned mappings. We found a similar pattern between typing and speaking, which rules out response modality as a critical factor. Instead, the findings lend credibility to the hypothesis that less well-formed mappings are more susceptible to change. This would also explain the faster learning observed in children (Smalle et al., 2017). Still, the faster learning demonstrated in older adults and under higher cognitive load shows that well-learned mappings are not immune to fast learning. A possible common element between these two conditions could be reduced explicit attention to maintaining the old mappings.

The second hypothesis we tested concerned the serial vs. hierarchical nature of second-order rule learning. Chaining, i.e., representation of serial order through a chain of directional associations between successive units (Washburn, 1916) and its more sophisticated alternatives, such as compound chaining (e.g., Botvinick & Plaut, 2004, 2006), has been a simple and elegant mechanism proposed for a variety of tasks that require sequencing, from language production to coffee making. Despite differences, all flavors of chaining theories assume that the retrieval of information about the current unit depends critically on prior units and context (but not future units or abstract positions in the sequence). In contrast, positional theories (Conrad, 1965; Crossman, 1961) assume that sequential order is represented by linking units to ordinal positions in an abstract positional frame, called slots, rather than to prior units. While more complex than chaining theories, positional theories are not subject to the same problems raised against chaining theories (Lashley, 1951). The problem of serial order has been studied extensively in language production, with most theories of spoken production adopting some version of positional coding (e.g., Dell, 1986; Glasspool & Houghton, 2005; Houghton, 2018). However, typing has been special in this respect, because the majority of work on typing has focused on the building and execution of a motor program (Logan, 2018; 2021; Logan & Crump, 2009; Yamaguchi, Crump, & Logan, 2013), as opposed to the more abstract treatment of sequences often observed in models of spoken or handwritten production.

Studies of sequencing mechanisms in typing have reported three important findings. First, a full sequence of movements is often prepared before the first keystroke, demonstrated by the effect of sequence complexity and end-state comfort of production on the initiation movements (Keele, 1968; Rosenbaum et al., 1993; Rosenbaum et al., 2007; Rosenbaum, Hindorff, & Munro, 1987). Second, target units are activated in parallel by related primes (Crump & Logan; 2010). Third, there is at least some evidence for chaining as an important mechanism in typing. Snyder and Logan (2014) conducted a series of experiments to test the importance of sequential priming, which directly tests the predictions of chaining models. They found that anagrams (e.g., "ocean") did not prime targets (e.g., "canoe"), showing that the position of primed segments mattered. Critical for chaining models, the authors showed that when the degree of positional overlap was kept the same, the evidence for facilitation in typing the target was stronger for sequential overlap (e.g., "busy"/ "burn") than non-sequential overlap (e.g., "fire"/ "fuse"). Finally, the degree of facilitation was parametrically related to the length of the sequential overlap: it grew progressively stronger when the overlap between the prime and target increased from one segment (e.g., "hair", "hunk") to three (e.g., "hair", "hail"). Collectively, these findings are consistent with a chaining mechanism determining serial order in typing.

The findings of the current study are well-aligned with the findings of Snyder and Logan (2014) and extend them to learning new sequences in typing. Learning of second-order constraints in Experiment 2 showed a strong positional effect. While the dependency of the coda on the vowel was robustly learned, the dependency of the onset on the vowel was not. This difference was striking, because the sequences with different vowels alternated regularly, therefore, even before the beginning of the next sequence, participants knew the upcoming vowel and could, potentially, form contingencies between the onset and that vowel. Moreover, the vowel was kept constant within the trial, therefore, producing it in the first words should have activated it for the following words and repetitions. Finally, when no sequential learning was possible, i.e., in Experiment 1, learning was stronger in the onset position, showing that there is no a priori bias for better learning in the coda position. Nevertheless, there was no evidence that the onset constraint had been learned in Experiment 2. This finding implies a strong reliance on chaining mechanisms for the learning of second-order constraints in typing. Critically, producing, rather than anticipating, the vowel seems to have been necessary to activate the chaining mechanism that formed the contingency.

This conclusion, however, is not meant to reject the relevance of positional theories to typing. For one thing, participants were able to quickly learn the link between letters and specific onset and coda positions in Experiment 1. This is only meaningful in the context of a positional frame. More generally, certain findings in typing, such as gemination errors (e.g., Book → Bokk) are difficult to explain with chaining theories (Hepner, Pinet, & Nozari, 2018; Rumelhart & Norman, 1982). Rather, the differential learning of onset and coda contingencies on the vowel observed in Experiment 2 reflects the critical role of chaining mechanisms when the to-be-learned is about the dependency between the units within the sequence. In English, there is a strong connection between vowels and codas, while a similar association between onsets and vowels is weak (Kessler, & Treiman, 1997; Lee & Goldrick, 2008), prompting the onset/rime syllabification schemes in English. Our findings show how vowel-coda contingencies can be quickly acquired via chaining. Whether the same is true for learning phonotactic constraints in spoken production is an excellent question for future studies. Similarly, whether the evidence of chaining is equally prominent in languages that do not show a strong vowel-coda contingency is a well-motivated question for future research.

Finally, our last experiment examined the critical dependence of orthotactic on phonotactic learning. If we cannot successfully disentangle learning in orthotactic and phonotactic domains, claims of parallel mechanisms and similar computational principles become moot. One could object that the findings across phonotactic and orthotactic experiments are simply two manifestations of learning within the phonological system. In line with past research claiming an influence of phonology over typing, participants in Experiment 3 sometimes replaced the /s/-sounding “c” with an “s. However, when these direct phonological confusions were excluded, we observed near-perfect learning of the constraint; “s” almost always moved to another coda position, despite the opposing “c” (/s/) being always in the onset position. In fact, learning of the “s” constraint was comparable in the opposed and unopposed conditions. This finding would not be expected if learning were phonotactic in nature. Instead, it points to true orthotactic learning. Together with the findings from Experiments 1 and 2, the results of Experiment 3 allow us to conclude strong parallels between processes that mediate implicit statistical learning of sequences in spoken and typed production. Specifically, we have the same domain-general learning principles applied to specific domains of speaking and typing (Nozari & Martin, 2024). More generally, the current findings add to the body of evidence that learning never stops, even in well-formed mature systems.

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