I have received a decision from the Board of Editors regarding the paper you submitted in July.

The two reviewers had different recommendations. Both reviewers, however, agree on their main impression about the paper: You and your coauthor describe potentially interesting facts, but fail to situate these facts within the context of the literature that exists on the topic. The Board agrees with this evaluation. In order to be publishable in *Linguistics*, this paper would need to undergo major changes in essentially all sections. In particular, the contribution that this paper makes to our knowledge regarding  the structure of syllables in the languages of the world needs to be highlighted, with adequate reference to relevant work by other authors. The goal of the paper cannot be limited to "verifying to what extent Persian behaves in accordance with the universal patterns observed in languages of the world with respect to the sonority hierarchy". Instead, it should be to point out that some facts of Persian challenge certain views regarding the sonority hierarchy and to propose an analysis that accounts for the Persian facts in the context of a theoretical framework of general applicability.

If  you and your  coauthor feel that you can do this, the Board is willing to reconsider a thoroughly revised version of your artilce. I shall then require a doc file, a pdf file, and a file containing a list of changes/reply to the reviewers.

I am appending the reviews upon which this decision has been made.

Best wishes

Ann

**Review A:**

**Summary:**This paper presents a study of the sonority contours of syllable patterns occurring in

natural speech in Persian. The coda clusters observed in a corpus of spoken language were coded

according to a four-point sonority scale. An analysis of the sonority combinations in these codas

revealed that for the syllables with clusters exhibiting unequal sonority, just over half (~57%)

had patterns conforming to the predictions of the Sonority Sequencing Principle (SSP). The

authors take this as tentative support for the SSP.

**General comments**

This study is well-motivated and the findings are potentially quite relevant to the literature on

sonority and phonotactics. In this area of research (sonority and phonotactics), there is a dearth

of quantitative studies conducted on non-European languages. The focus on Persian in this paper

is a welcome change in this regard. This study also uses a novel approach, examining syllable

patterns within a corpus of natural spoken language rather than relying on data from a dictionary

or lexicon. This too is a welcome shift with respect to previous studies of these issues.

The finding that only a little over half of the relevant data conform to the SSP is an important

one. In fact, I think that the authors are not bold enough in their interpretation of these results,

which do not provide “tentative confirmation” of the SSP so much as they question its validity in

accounting for the natural language data at hand.

There is clearly a lot of scientific value in this study. That being said, several aspects of the paper

need substantial revision before it can be accepted for publication. First, the authors need to

situate the study more firmly in the existing literature. This includes not only quantitative/

typological studies supporting the SSP, but also alternative accounts and especially the growing

body of data which challenges the validity of the SSP. Second, the authors need to clarify their

methodology so that it is absolutely clear what their frequency figures are measuring and so that

the reader can compare these to previous findings. Finally, the authors should provide a much

more substantive interpretation of their results. I believe that the data presented here warrant far

more discussion than what is given.

The authors may wish to consult the studies in references [1]-[3], which I’ve listed at the bottom

of this review.

Another general concern is that the authors do not ultimately connect their findings to the

motivations for the study. This makes it hard for the reader to follow the line of argument and to

find it convincing. In some places, reorganization of paragraphs/ideas could improve the flow of

the paper. In other places (Discussion and Conclusion), the implications of the findings for the

initial research question should be more explicitly discussed.

My specific comments are laid out below by section.

**Comments by section**

***1. Introduction***

I appreciate that the authors are brief in their general presentation/discussion of sonority, since

the main focus of the paper is on the specific phonotactic patterns of natural speech in Persian.

However, the discussion in the introduction should be expanded to better situate the study in the

existing cross-linguistic research. In particular, the authors should address the following issues,

even if briefly:

(i) There are many ways in which researchers have attempted to define sonority independently

of the linear order of segments in a syllable, which is a circular definition of the property.

What definition of sonority are the authors using? This issue should be acknowledged and at

least briefly discussed.

(ii) Give example(s) of a syllable or word (in Persian or another language) illustrating the SSP.

(iii)Provide a more substantive discussion of the cross-linguistic data supporting the SSP,

referencing studies by name and summarizing some of the qualitative and quantitative

findings regarding specific patterns. The authors mention in the abstract and introduction that

the SSP finds support in the typological and acoustic research, but they do not elaborate any

further on this point. The reader needs to know what the relevant cross-linguistic findings are

in order to meaningfully compare them to the findings in the current study. By including this

discussion, the authors can also provide a clearer, more concrete motivation for their

hypothesis, which refers to “universals” that are not fully elaborated upon in the introduction.

(iv)Similarly, acknowledge the growing body of literature which presents challenges to the SSP

and indeed the validity of sonority as a property with a consistent cross-linguistic definition.

This not only shows the reader that you are aware of these other lines of research, but also

gives you a point of reference for situating the later findings (since nearly half of the relevant

data did not confirm the SSP). Some recent studies examining challenges to sonority can be

found in [1] and [2] below.

(Point (i) could additionally be discussed in the methodology in section 3, and parts of points (iii)

and (iv) could alternatively be addressed in the review of literature in section 2)

***2. Review of literature***

This section lays out the results of several recent studies of Persian phonotactics. However, it

was sometimes difficult for me to determine how the results were comparable to each other or

relevant to the current study. For example, the description of Rahimi et al. (2014) doesn’t state

what kind of data was used. Sometimes the discussion mentions results which are not relevant to

the current research question (eg., the support for Licensing by Cue in Rahimi et al. 2014: while

this result is interesting, it does not help set the stage for the current project).

The studies are currently presented as a (mostly chronological) list. It would be easier for the

reader to follow the line of argument if the studies were presented in a more thematically

coherent way. For example, the studies could be organized by type of data used (lexicon versus

corpus), or by the degree to which they provide support for the SSP.

***3. Methodology***

The description of the establishment of FarsDat in lines 114-117 could be omitted, as it is not

directly relevant to the methodology.

line 118: perhaps a citation for TIMIT here?

The description of the visual setup of the Microsoft Office Excel worksheets is unnecessary and

can be omitted.

As mentioned above, there are various ways to define sonority, and a four-point sonority scale

for consonants (glides, liquids, nasals, and obstruents) is just one of many different

categorization schemas. The authors should state clearly in the methodology section that they

have chosen to use a four-point scale (glides > liquids > nasals > obstruents), and perhaps even

give brief definitions here for each class of consonants under consideration. In other words,

while the four-point scale above is often used in studies of sonority, it should not be assumed that

readers will recognize this to be the universally standard scale of sonority.

Finally, the authors should state explicitly in this section what kind of frequency is being

analyzed. My initial impression from the first sentence in section 4 is that the authors are

measuring coda cluster type frequency within the set of syllables with complex codas

represented in the corpus. However, this is never directly stated, and the wording in lines

187-188 and 224 suggest that it is actually coda cluster *token*frequency which is being

examined. The authors should make it very clear in this section what they are counting, perhaps

even using one of the smaller data sets from section 4 to illustrate exactly how they are counting

frequency.

***4. Results***

I find it helpful that the authors present tables for each of the sonority combinations under

examination, and that illustrative examples are given for each occurring coda cluster type.

\_3

Section 4.1.3: the discussion of subcategorizations of sonorant and obstruent classes should be

moved to the methodology section. Here it distracts from the presentation of results.

Section 4.1.6: the authors state that the nasal+obstruent patterns are similar to the

liquid+obstruent patterns reported in section 4.1.5. While the two patterns do show similar

*frequency*in the corpus, I was surprised that the authors did not remark upon the very different

distribution of cluster types in the two tables. The nasal+obstruent clusters appear to be largely

limited to homorganic clusters (e.g., labial+labial), while the liquid+obstruent clusters are much

more varied in their combinations. This is a place where the inconsistency between the wording

and the text could cause confusion for the reader.

***5. Discussion***

lines 247-250: this is an interesting observation about permutations, and an account which I

haven’t seen very often in accounting for sonority plateaus. The authors seem to be suggesting

that statistical probability and sonority are competing principles in producing cluster patterns. If

this is indeed their intention, they should provide some convincing rationale for this

interpretation. This is not a common claim and will strike some as provocative, especially

without any discussion of external supporting evidence (such as from other languages or crosslinguistic

studies).

lines 260-268: Perhaps a table would be a more appropriate way to present this data. I think the

authors’ focus on the one *contradicting*pattern (the class of nasal-liquid combinations) here

detracts from the far more interesting pattern in the data. That is, there are certain sonority pairs

in Persian which strongly favor the SSP (e.g., obstruents and glides), and some which have a

much more ambiguous pattern (e.g., nasals and liquids, obstruents and liquids, obstruents and

nasals). That result is, I believe, the more important contribution of this study and should be

emphasized. In doing so, the authors could springboard into a more substantive discussion of

what their findings imply for SSP and alternative (e.g., acoustic) accounts of phonotactics.

As it is, the discussion lacks sufficient depth to satisfy the reader’s curiosity about the

interpretation of and accounts for the patterns in the data. In particular, I was hoping that some or

all of the following questions would be addressed:

(i) How does the distribution of SSP-conforming vs. SSP-non-conforming coda patterns in the

current study compare to previous studies of Persian? I understand that the various studies

deal with different methodologies, making direct comparisons difficult, but it seems like a

core question in the current study is whether the prevalence of SSP-non-conforming clusters

is higher or lower in natural speech than in dictionary/lexicon data. The answer to this

question bears important implications for the explanatory power of SSP.

(ii) In the introduction the authors make the important point that the phonological word does not

highly correspond to the syntactic word in Persian, thus motivating a study of phonotactic

patterns in natural speech rather than a lexicon. Yet the authors never return to this point in

the discussion. Can they present any observations (even impressionistic) regarding the

morphological status of frequent cluster types in the data (either conforming or nonconforming)?

Are there particular grammatical morphemes or constructions that contribute to

high frequencies of certain kinds of sonority combinations in the data? Some (e.g., Dressler

and colleagues, [3]) have claimed that SSP-non-conforming clusters are more likely to be

morphologically complex, coming about through processes of inflection, specifically. If the

data in the current study provide evidence for or against such a claim in Persian, then that is a

finding which should be emphasized and presented as a contribution.

(iii)How do the patterns observed reflect common processes in language use, such as the liaison

and elision processes discussed in the introduction? In their data collection, did the authors

note such processes affecting any particular sequences, which in turn might have had an

effect on the cluster distributions observed? Even if a detailed analysis of this data cannot be

presented, I think that this issue is relevant and should at least be briefly visited by the

authors. After all, the natural speech aspect of this study is what makes it stand apart from

other similar studies of syllable patterns.

Of course there are other routes the authors could take in their discussion, depending on how

they seek to account for and reconcile the patterns in their data with those of previous studies of

Persian and other languages. But it is essential that the authors make *some*effort to show how

their study relates to the existing literature. They should also make it clear how their findings

relate directly to their research questions and the motivations for conducting the study the way

they did.

***6. Conclusion***

lines 278-281: I don’t think that the authors have sufficiently shown that the SSP is confirmed in

the data. At best, there is extremely weak confirmation, but as mentioned above, this is based on

a very coarse frequency analysis and is not discussed in light of previously-established patterns

in the sonority literature. They also do not touch on SCL at all in their analysis or discussion, so

the statement regarding SCL should be removed entirely from the conclusion.

A more substantive discussion in section 5 will lead to an improved conclusion and allow the

authors to make bolder statements regarding their findings and the contributions of their study.

The discussion of limitations of the current study and suggestions for improvement/expansion of

the data collection process are good.

**Typos/Proofreading Issues**

line 2: “universal patterns of the linear order…” sounds strange to me. Perhaps change “of” to

“in”.

line 16: sonority sequence profile/SSP

line 28: *ezafe*: since this is a term specific to Persian linguistics, this term should be defined

(maybe in a footnote) or omitted in favor of a more generic term.

lines 121-3: Omit either “since” or “however”; the use of both of these makes the sentence

difficult to process.

line 135: omit “carried on with” or “based on”

line 150: “relashionship(s)” > “relationship(s)”

lines 166, 171: Note that Tables 1 and 2 have slashes // around the illustrative syllable examples,

while the other tables in this section do not.

line 195: the wording “pretty much similar” seems overly casual for a journal publication. “Very

similar” or “largely similar” would be more appropriate.

line 208: “as long as” should be “as”

line 213: I think you mean “precede” rather than “follow”

line 213: omit “only” in “the only Persian glide” (it has been established earlier in the paper that

Persian only has one glide)

line 284: “the Persian sole glide” > “the sole Persian glide”

**References**

[1] Parker, Steve (ed.). 2012. The sonority controversy. De Gruyter Mouton

[2] Ball, Martin J. and Müller, Nicole (eds.). 2016. Challenging sonority: cross-linguistic

evidence. Equinox Publishing Ltd.

[3] Dressler, Wolfgang U. & Katarzyna Dziubalska-Kołaczyk. 2006. Proposing morphonotactics.

Rivista di Linguistica 18(2): 249-266.

**Review B:**

**General comments:**

The paper reviews the literature on syllable structures in Persian, which are mainly based on the structure of words in isolation, and proposes to review coda clusters realizations in the context of connected speech. Though the issue is relevant and holds good potential, the conclusions are poor, too descriptive; they do not really shed new, interesting light on the topic. The article is not well balanced either and does not take into account recent work on perceptual cues that could shed new light on the results (e.g., Wright’s article and others in Hayes, Kirchner and Steriade, eds., 2004: *Phonetically Based Phonology*, CUP).

**Specific comments**:

**Comments on §2. Review of literature**

P. 3: “…, which supports the idea that Perceptual Cue Salience…” (lines 65-66): This is only true for the sentence concerning obstruents in onsets (line 65) . As it is stated, it seems that sonorant consonants occurring more often no matter where in the syllable (line 64) also sustain the Perceptual Cue salience theory, which is not the case.

**Comments on §3. Methodology**

The author does not indicate some important data about the corpus used: for example, page 5, lines 118-123: it is not mentioned how many sentences over the total amount of 6,080 were analyzed (though see the comment below).

Page 5, lines 135-137: At this point, I am not sure if the author collected his/her own new material for the corpus or if part of the above mentioned FarsDat sentences were also used in the corpus. If FasDat is no taken into account, then I do not think it makes much sense to spend one page explaining its characteristics.

**Comments on §4. Results**

In the previous section, the author says that 561 CVCC syllables; but the total amount of instances reported sum up 562: 8 (G+L), 4 (G+N), 19 (G+O), 5 (L+N), 76 (L+O), 83 (N+O), 217 (homogeneous) + 1 (x twice the same item) (O+G) + 7 (N+L) + 67 (O+L) + 75 (O+N) = 562 (563 if we count the word occurring twice: Do all other occurrences appear in different words? This point is not mentioned in the paper). This issue slightly affects the percentages given in section 5.

Page 7, line 187: “/ln/ is not possible and /rn/ is extremely rare both due to the component phonemes’ same place of articulation” (Samare, 2006)”: Is this a special restriction of Persian? (As far as I know, in many languages /rn/ is more common than /rm/.) It must be stated more clearly (as done later on page 10, lines 228-229). At this point, it is not clear if the restriction operates only on L+N sequences, since in L+O sequences there seem to be ‘same-place-of-articulation’ clusters: e.g., page 8, Table 5/rt/, /ld/, /rd/, /rs/, /rz/; Table 6 /nd/. Without a table of the consonants of Persian with their specific point of articulation, it is impossible to foresee the scope of the restriction.

Page 9, lines 203-205: Here the author clearly states that there is a Persian specific restriction on “liquid clusters composed of different segments” to ban /lr/ and /rl/ clusters. Since /jj/ does not exist either, is this a more general restriction on sonorant clusters?

Pages 8-9, w.r.t homogeneous sequences: Here a table is necessary to see which ‘homogeneous’ combinations occur, especially since the author puts together plosives and fricatives in the obstruent group: one wonders if there are more instances of fricatives followed by plosives, or the other way around.

**Comments on §5. Discussion**

Page 11, lines 246-247: Since the author groups together all obstruents, the total amount (217 cases) includes true *plateau* combinations as well as fricative+plosive clusters, which can be alternatively considered sonority decreasing. I think that this distinction among obstruents is important to be taken into account in this work, at least to contrast the results of both possibilities. The calculation given below (lines 251-253, i.e. 195 in favor of the universal decreasing pattern vs. 149 against it) could be significantly altered with the proposed distinction.

The discussion only presents the percentages, without any sign of interpretation of the results, as it is expected for a journal such as *Linguistics*: Why, for instance, “it is only in the case of the nasal-liquid order that the cases contradicting universal patterns [7 occurrences] outnumber those contributing to their validity [5 occurrences]” (lines 269-270)? (I think the number of occurrences is anyway too low to make a strong claim about the results.)

**Comments on §6. Conclusion**

I agree with the author that the study provides “a rather tentative confirmation …” (page 12, line 278).