

Response: Climate and language: has the discourse shifted?

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Caleb Everett^{1†}, Damián E. Blasi^{2†} and Seán G. Roberts^{3†*}

¹Andrew Carnegie Fellow, Department of Anthropology, University of Miami, Coral Gables, FL 33124, USA

²Zurich Center for Linguistics, University of Zurich, Plattenstrasse 47, Zurich, 8032, Switzerland; Department of Linguistic and Cultural Evolution, Max Planck Institute for the Science of Human History, Kahlaische Strasse 10, Jena, 07745, Germany and ³Max Planck Institute for Psycholinguistics, Wundtlaan 1, Nijmegen 6525 XD, The Netherlands

[†]These authors contributed equally to this work.

*Corresponding author: sean.roberts@mpi.nl

We begin by thanking the respondents for their thoughtful comments and insightful leads. The overall impression we are left with by this exchange is one of progress, even if no consensus remains about the particular hypothesis we raise. To date, there has been a failure to seriously engage with the possibility that humans might adapt their communication to ecological factors. In these exchanges, we see signs of serious engagement with that possibility. Most respondents expressed agreement with the notion that our central premise—that language is ecologically adaptive—requires further exploration and may in fact be operative. We are pleased to see this shift in discourse, and to witness a heightening appreciation of possible ecological constraints on language evolution. It is that shift in discourse that represents progress in our view. Our hope is that future work will continue to explore these issues, paying careful attention to the fact that the human larynx is clearly sensitive to characteristics of ambient air. More generally, we think this exchange is indicative of the growing realization that inquiries into language development must consider potential external factors (see Dediu 2015).

Having said that, much debate remains about our more specific hypothesis vis-a-vis tonality and desiccation. Is desiccation *directly* implicated in the global distribution of complex tonality? The best answer anyone can offer at this point, we think, is ‘quite possibly’. Our distributional data remain highly suggestive but not conclusive (nor could they be), and the skeptical points raised in the comments are insightful but not conclusive either. (As we will demonstrate below.) Some of those

comments do hint that the relationship between climate and tone might be indirect, and that more factors must be considered for the direct relationship we hypothesize to be more convincing. As we noted in our target piece, new kinds of data are required—the synchronic distributional data have probably taken us as far as they can. Yet new kinds of data are also needed to support the hypothesis that language is not ecologically adaptive. Much work remains to explore these issues. As Winter and Wedel (2016) note, this is an exciting period, as new sources of data and associated strands of research present themselves.

Next we address principal objections/skepticisms. We focus first on de Boer’s commentary (2016), which was the only one to engage with the crucial laryngology data. He suggests that (a) water vapor loss is likely a better indicator of the effects of desiccation than specific humidity and that, (b) while the impact of desiccated air on the vocal cords is evident in the laryngology data, the impact in question is minor. Claim (a) is interesting, but our account is not actually predicated on overall dehydration but on the effects of desiccation via air contact with the larynx. (Most of the desiccation/phonation studies we cite relate to ex-vivo or in-vivo exposure of the larynx to desiccated air.) Therefore, water vapor content in the pharynx is a more important factor than overall water vapor loss, and the very study de Boer cites (Cole 1953; Figure 3) actually shows that pharyngeal water vapor is reduced, though in minor ways, even when cold/dry air is inhaled through the nose. (Oral inhalation is crucial, though, as discussed below.) Specific humidity remains the best climatic proximity, we think,

to test the hypothesis. With respect to (b), experimental evidence could resolve this issue, but we would like to make four key points: (1) if the pitch effects were subtle in all cases, they may still impact the evolution of language given the many interactions and interlocutors involved in language change. (2) The overall effects of desiccated air are, we think, very unlikely to be as subtle or imperceptible as de Boer claims. While he acknowledges that speaking entails oral breathing, humans breathe through the oral cavity in many other contexts—for instance when their nasal passageway is blocked they do so continuously. Furthermore, respiration exceeding 40 liters/minute generally requires oral breathing, and cardiovascularly unfit individuals regularly use oral breathing, as do many individuals at high elevations. (And high elevations are drier too.) And frequent oral respiration challenges both vapor recuperation and pharyngeal humidification. Cold air is exceptionally dry regardless of saturation rate/relative humidity, given its very limited water vapor capacity. It is well known that dry winter air desiccates the oral cavity if left open (hence xerostomia). The deleterious effects of cold/dry air on the respiratory tracts of winter athletes are actually well documented (Sue-Chu 2012). Desiccation effects are likely pronounced in many real-world arid contexts, and ubiquitous for a certain segment of the population. Given the well-established deleterious effects of dry air on the respiratory tract in naturalistic contexts, the assumption of subtlety vis-a-vis the larynx is potentially problematic. Furthermore, the subtle effects de Boer (like us) cites are based on studies with relatively brief exposure (in some cases 15 minutes) to dry air. In reality, people may breathe dry air orally for much longer or even continuously, which may explain why singers' pitch variation is definitely impacted by desiccation in natural contexts. (3) A variety of respiratory tract ailments are well known to be more prevalent in cold/dry environments (Koskela 2007). These include laryngitis, which has been described as an associate of dry atmospheric conditions for well over a century (Ingals 1890). This sort of ailment has clear effects on pitch. (4) A consistent effect of desiccation, across a variety of studies, is on *perceived* phonation effort. Even if pitch-based effects were as imperceptible as de Boer suggests, speakers are clearly aware of some influence of desiccation on F0 production. There may be diachronic pressures against over-reliance on sound patterns such as tone that are more heavily dependent on phonation when contrasted to say, voiceless consonants. This alternate possibility is mentioned in our target piece, but goes unmentioned in the commentaries. This includes those of Ladd (2016) and Donohue (2016),

whose understandable objections to our characterization of tonality are much less problematic if this alternate factor is at work.

Ladd notes that, at least in the case of Yoruba and English, there is no obvious basis for claiming that tonal languages require more precise pitch. While we are aware that intra-speaker data show precise pitch in nontonal languages, as Ladd's data suggest, inter-speaker data may suggest greater pitch-precision in tonal languages, as evidenced by the perceptual studies we cite that demonstrate stronger pitch-discrimination by speakers of tonal languages. This includes the heightened pitch-discrimination of Yoruba-like tones by speakers of other tonal languages (Caldwell-Harris et al. 2015). Gussenhoven (2016) also suggests that inter-speaker variation in tonal languages is lower than nontonal languages, comparing two more closely related varieties (British English and Nigerian English). Nevertheless, we take Ladd's point and recognize that this issue requires further examination. We agree with Ladd and Donohue (and Progovic and Ratliff 2016) that our characterization of complex tonality is somewhat simplistic, but this seems an operational necessity at this stage given the current phonological databases available. Hopefully our ideas will eventually be tested with corpora of phonetic data, data that depict the usage of phonation/pitch in the speech stream. Such tests will require the refining of predictions regarding phonation reliance (which we predict to be less in cold/dry environments) and pitch complexity (which we also predict to be reduced in such environments, but admittedly in very broad ways). We recognize(d) that tonal languages like Mandarin have a variety of phonetic correlates. However, these also generally entail modulation of the larynx, modulation that may not be immune to the effects of desiccation over the long haul. Furthermore, the assumed commonality of nonpitch correlates does not preclude a relatively high degree of reliance on pitch precision, nor heightened F0 reliance more generally, in languages with complex tone. (As Maddieson 2013 notes, nontonal languages are relatively likely to allow complex syllables, while languages with complex tone are relatively unlikely to allow complex syllables.) By definition, after all, tonal languages require some pitch differentiation for morpheme and word-level meaning contrast. Ladd also offers a perceptually oriented account of the distribution we describe, suggesting that the pattern may be due to sound attenuation in certain environments. He notes that his and our accounts are not mutually exclusive. We agree, though it remains unclear whether sound attenuation effects are relevant at typical distances between interlocutors.

Next we transition to more statistically oriented discussions. We begin with a simple point. The prediction of the account in [Everett et al. \(2015\)](#) was that languages with complex tonality should be less likely to occur in the world's driest regions. This prediction was certainly borne out. This may be fortuitous coincidence, of course, and whether or not the observed association is statistically significant may depend on the approach one takes to the data, given factors such as the overall climatic distribution of languages. After all, the hypothesis made predictions only for the least humid regions of the world, in which fewer languages exist.

Somewhat puzzlingly, Donohue observes that there are many correlations between tonality and climatic factors at a regional basis. Of course, many regional correlations can be found between many factors in language and the environment, particularly if one does not control for language relatedness (which, crucially, Donohue has not done). While we thank Donohue for his comments, we suggest that such regional, noncontrolled analyses only serve to distract from the core issues and data. We observed a global association while controlling for historical relationships, and only after engaging with findings in laryngology. We would be much less dismissive of the relevance of associations of that sort.

[Hammarström \(2016\)](#) strongly disputes the merit of our proposals and of our data analysis. (More on the latter shortly.) We question several of Hammarström's interpretations of our target piece. He points out that, if ecological adaptation can be found on some level it does not follow that 'sound systems of human languages belong to an adaptive level', and that 'the a priori question need not be determined by a general rule that stipulates everything to (not?) adapt to climate'. We agree with this and did not suggest otherwise. Studies should focus on effects motivated by prior evidence, but determining a priori the size of the 'discernable effect' of an evolutionary pressure in a cultural system is, we think, not as straightforward as Hammarström suggests. This is particularly true in the case of the suggested causal mechanisms at play here, which are contingent on the very real effects of desiccated air on the larynx—effects whose influence on language remain unexplored. The casual dismissal of such potential influence is problematic from our perspective. Additionally, given the adaptations of other communication systems, we think solid evidence should be offered that language is not also adaptive. Either way, this is an empirically explorable arena.

Progovac and Ratliff point to a 'problem of proportion' as part of the reason for the hostile attitudes of

some linguists to explanations of language development outside of language. These attitudes, we endeavored to stress in our target piece, are themselves based on long-held but empirically unsubstantiated assumptions. We agree with Progovac and Ratliff that empirical research on the impact of climate is worthwhile, if it is done as part of a wider, integrated attempt to explain linguistic phenomena.¹ Progovac and Ratliff also see that [Everett et al. \(2015\)](#) was meant to encourage further development of the theory and engagement with the evidence rather than provide absolute proof of a complete explanation. We agree wholeheartedly. In that sense, Hammarström's active engagement with the empirical evidence is part of the progress that needs to be made, even if we disagree on key points.

Several criticisms point toward the need for a more sophisticated statistical approach to providing evidence. The strongest objections come from Hammarström, and we respond to these more fully in a supplementary section. This includes a discussion of the 'corner' hypothesis, a reproduction of the statistical tests of Hammarström as well as extensions to control for language family and geographic area at the same time. We acknowledge that the original paper did not describe the statistical methods in enough detail, but we argue that the results are more robust than suggested by Hammarström, and therefore warrant further investigation.

The commentaries from [Collins \(2016\)](#), [Moran \(2016\)](#) and [Donohue \(2016\)](#) all argue that the histories of languages should be taken into account. Moran points out that climates have changed dramatically within the timescale of the spread of languages, and the present location of the speakers of a language may be far from the language's original homeland. This is of course true, though we think that the synchronic data are an important place to begin such explorations. After all, the predicted association should hold over such a large sample of languages, many of which move independently in different global regions, synchronically. Particularly in the light of the mobility of languages, we

- 1 Progovac and Ratliff ask how we see the effect of climate relating to other proposals such as genetic effects. We see them as nonmutually exclusive, together with well-known linguistic explanations for tonogenesis and leveling. It is quite possible that separate studies may attribute some of the same variation to different causes, but the key to progress on these issues is, as Progovac and Ratliff suggest, considering the interactions between these elements in order to build a comprehensive framework for explanation.

think that the consistency of the tendency across world regions is telling. Donohue suggests that the statistical link between tone and humidity may emerge from wider interactions between social history and broader aspects of climate. This fits with the idea that climate may influence migration, but also means that the direct and indirect effects of climate on language need to be disentangled.

Collins quite convincingly shows that the kind of correlation we observed might derive from historical spread and contact in many individual cases. First, we should stress that we have never claimed that such factors are not also at play, and we suspect they may be at play in interrelated ways to desiccation. But Collins's analysis also demonstrates that it is quite unlikely that humidity and desiccation will associate in different macroregions, as we have observed. His logistic regressions suggest that the pattern holds independently in Eurasia, Africa, and North America. In [Everett et al. \(2015\)](#) we observed that it also held in South America, and in fact the South American distribution is consistent given that languages with complex tone there occur in Amazonia. If the question is whether the pattern is simply consistent in these four macroregions, the answer is yes. A coincidental consistency in four macroregions seems not just 'unlikely' but 'extremely unlikely', and a contact-only approach does not explain it. Also, it should be stressed that these are the only four macroregions for which our account makes any predictions, given that they have inhabitable desiccated regions. Furthermore, Collins's commentary does not address one of the key points in our original study—the observed association holds across geographically distant isolates. Finally, Collins mentions the movement of Niger-Congo languages through Africa, losing tone as they come into contact with dry regions, but also nontonal languages. One open question is why the languages in these regions were dry to begin with. His data are fascinating, we think, and may be consistent, at least in some cases, with the proposed 'borrowing' mechanism we discussed in our target piece. This too requires further exploration.

Winter and Wedel also show some intriguing results suggesting that tonal languages have fewer L2 speakers, hinting that tone systems might simplify under contact. This would fit with Collins' modeling of tone as 'radiating' from an origin and simplifying in the periphery. Taken together, these commentaries hint at the more indirect influence of climate on the spread of tone, which is still some influence. Yet they also hint that direct ecological influences may also be at work, and that the effect of desiccation may be reified in contact situations.

These issues suggest that explicit modeling of language histories is necessary in order to more precisely elicit the most appropriate baseline for comparison. One approach would be a geographic-phylogenetic approach which models the divergence of languages in time and space, with separate models for the evolution of language and climate. We discussed this approach in our target piece. This approach requires expertise in many cutting-edge techniques, and points toward collaboration as the key to developing the statistical approach. Relatedly, Moran is right that statistical work should be accompanied with the data and code for reproducibility. Therefore, we include in this publication the data and code required to reproduce the results from [Everett et al. \(2015\)](#) (see also <https://github.com/seannyD/ToneClimateJoLE>).

Gussenhoven points to further opportunities for interdisciplinary collaboration including the investigation of intonation in nontonal languages and extensions to para-linguistic communication and evolutionary psychology. Hammarström points to alternative modes of communication shaped by ecological factors such as whistled languages and signed hunting codes. (We agree, though one difficulty is that there are too few instances of these alternative modes to establish reliable associations.) We also appreciate [Ember's \(2016\)](#) review of prior work on psychological effects of climate on wider cultural behavior.

We recognize that it is vital that social and historical factors, well known to be operative in language change, are incorporated into more complex models of climatic influence. It is also vital that the extent and directness of potential climatic influences be examined. This entails disentangling potential production-related influences such as those we highlight from acoustic-based factors such as those suggested by Ladd and in recent work by [Maddieson and Coupé \(2015\)](#). Indeed, as Coupé notes, the climate is just one part of the wider 'ecology' of language, and statistical methods from ecology can help untangle the different influences. Given the complexity of the factors involved in the exploration of this hypothesis, it is perhaps unsurprising that much work remains. Our broad hypothesis, that ecological factors impact sound patterns, is not a simplistic deterministic one. Testing the hypothesis will remain an exceedingly complex task, one that will require experimental data, new kinds of databases, and so on. Yet it is also, we submit, an essential task in the study of the development of languages. We are excited to see more researchers agree with, and engage with, this general premise. After all, languages are not hermetically sealed from the environments in which they evolve.

Finally, we would like to point out that, in spite of the considerable differences in opinion displayed, the same methodological standards are shared across the board. Regardless of the degree of agreement researchers hold with respect to our proposal, the arguments focused on the analysis of data and the evaluation of concrete hypotheses with the principles of the scientific method. Appeals to what is natural, self-evident or obvious no longer have a place in the language sciences.

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