the program “Investissements d’Avenir” (ANR-11-IDEX-0007) of the French government operated by the National Research Agency (ANR).

References


Commentary: Is the effect of desiccation large enough?

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I certainly agree with the authors (Everett et al. 2016) that the question whether there is an environmental influence on the sound systems of languages (or on other aspects of language) is worth investigating and that the
position that there is no such influence is not empirically founded. Their point that individual counterexamples do not invalidate statistical tendencies is also valid and important. However, it is also essential to keep in mind the \textit{a priori} plausibility of the proposed correlation, and this may be problematic.

The main evidence of the influence of ambient humidity on the vocal folds—provided in this article and in Everett et al. (2015)—is the work by Hemler et al. (1997) as it presents quantitative data that is directly relevant to the relation between humidity and control over pitch. Other referenced papers mainly focus on the relation between overall hydration (i.e. through water intake) and pitch or on the relation between mouth breathing (which dehydrates the vocal folds more effectively than nasal breathing) and phonation threshold pressure (Sivasankar and Erickson-Levendoski, 2012).

Hemler et al. (1997) show that after orally breathing extremely dry air (2.1 per cent relative humidity) at 23°C for ten minutes, absolute jitter (random variation of timing in the vocal fold vibrations) increases from an average of 40–53 μs. There is also an increase of relative shimmer (random variations in amplitude) from an average of 3.45 per cent to 3.72 per cent and an increase in noise to harmonic ratio, which corresponds to what is expected when jitter and shimmer increase. At the maximal fundamental frequency involved in the measurements (220 Hz for female speakers), the increase in jitter corresponds to an increase of the uncertainty of the precise pitch from 0.88 per cent to 1.17 per cent (for reference, 1.17 per cent pitch difference is approximately one-fifth of a semitone). These numbers would be correspondingly lower for lower frequencies. Although these differences are significant, they are very small. But perhaps the level of desiccation—and its corresponding influence on the vocal folds—in real life would be even higher?

The authors correctly point out in Everett et al. (2015) that one should not use relative humidity when estimating desiccation. For a given value of relative humidity, moisture content depends on temperature. As air is heated up to body temperature when inspired, colder air absorbs more moisture, and thus tends to have a stronger desiccating effect. However, the airways are a very effective exchanger of humidity so that moisture that is lost on inspiration is partly recuperated on expiration, and this happens more effectively at lower temperatures. I would therefore argue that one should not use specific humidity as a proxy for desiccation (as the authors have done), but actual water loss from the airways. There is a surprisingly complex relation between temperature, relative humidity, and water loss.

Relevant data can be found in Cole (1953; figure 7) (although these data are for breathing through the nose, to which I will come back). For completely dry air, water loss increases linearly with temperature from 320 ml/24 h for ambient air of −30°C to 520 ml/24 h for +50°C. For completely moisture-saturated air, the water loss is equal to that of dry air between −30°C and −10°C (this means, incidentally, that the correlation between absolute humidity and desiccation is actually the inverse of what the authors propose in this temperature range) and then drops off parabolically to no water loss at all at 37°C (body temperature). What this means is that water loss (and by implication, desiccation of the airways) is relatively constant between −30°C and +20°C, decreasing from 320 ml/24 h to 290 ml/24 h for moisture-saturated air (for reference, for this temperature range, specific humidity increases from 2.3 × 10⁻⁴ to 1.5 × 10⁻²) and increasing to 450 ml/24 h for completely dry air. It also means that water loss can be very different for equal values of specific humidity, depending on temperature. Only for temperatures above +20°C does moisture content have a strong influence on water loss. Hence one would expect the authors’ proposed explanatory mechanism only to be valid in warm climates. However, because these data are from nasal breathing, it can be argued that they are only relevant for estimating the hydration of the vocal fold mucosa before starting to speak.

When speaking, breathing occurs through the mouth, which results in less effective warming and humidification. Sivasankar and Erickson-Levendoski (2012) report a higher phonation threshold after fifteen minutes of mouth breathing, compared with nasal breathing, indicating a negative effect (due to dehydration) of mouth breathing on the vocal folds. Interestingly, they find no effect of ambient humidity (temperature is not specified, but I assume it is room temperature). As Hemler et al. (1997) do find a significant effect of extremely low humidity, I assume that Sivasankar and Erickson-Levendoski’s lower humidity level (between 7 per cent and 30 per cent relative humidity) is not yet low enough to observe an effect.

It is therefore indeed likely that long periods of speaking in very dry air result in desiccation of the vocal folds, and thus influence voicing. However, if we assume that water loss curves for oral breathing are similar to those in Cole (1953) (except with higher water loss) it must be concluded that desiccation would be less extreme than in Hemler et al.’s (1997) experiment for lower temperatures and by implication, the influence on the vocal folds would be less extreme. In addition, desiccation would be mitigated by vocal fold rehydration.
during periods of nasal breathing. Therefore, I do not expect for vocal fold dehydration to result in more than 0.3 per cent extra uncertainty in pitch production over the range of realistic environments and situations in which speech is used. As the just noticeable difference of pitch in speech is in this range (0.3 Hz at 120 Hz (0.25 per cent) for level pitch, and 2.0 Hz at 120 Hz (1.67 per cent) for falling pitch, Klatt 1973) it is questionable whether such a difference would even be perceivable.

**Funding**

This work was funded by the European Research Council starting grant project ABACUS, grant number 283435.

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**Commentary: Culture mediates the effects of humidity on language**

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Does (the presence or complexity of) tone inversely correlate with dryness of climate? The authors (Everett et al.) suggest that the absence of ambient humidity in the air negatively correlates with the presence of (complex?) lexical tone, partly because of the effect that dry air has to increase the difficulty in achieving precise articulatory targets.

There are two main problems with the argumentation used.

1. Conflating ‘tone’ with ‘pitch’ or ‘fundamental frequency’, and mistaking ‘complexity’ with a syllable domain for tone assignment;
2. conflating ‘dry climate’ with the absence of humidity.

The authors are not guilty in an absolute sense of these problems, acknowledging that there are complications.

Their reliance on pitch contrasts as a proxy for tonal category contrasts, and the use of air humidity rather than (easily available) climate information for the ranges of different languages means that the authors are dealing with ephemeral correlations between proxy features.

In the next two sections, I will critique the use of tone primarily to refer to distinctions realised by pitch, and the use of humidity as a powerful explanatory for the existence of tone categories.

1. **Tone is not simply pitch/fundamental frequency, and complexity is not brevity**

The authors acknowledge that ‘many non-pitch phenomena are associated with the production of tone, including ancillary laryngealization and duration...