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Citation for published version:

Link:
Link to publication record in Edinburgh Research Explorer

Document Version:
Peer reviewed version

Published In:
INTERSPEECH 2015 16th Annual Conference of the International Speech Communication Association

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Are we using enough listeners? No!
An empirically-supported critique of Interspeech 2014 TTS evaluations

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Abstract

Tallying the numbers of listeners that took part in subjective evaluations of synthetic speech at Interspeech 2014 showed that in more than 60% of papers conclusions are based on listening tests with less than 20 listeners. Our analysis of Blizzard 2013 data shows that for a MOS test measuring naturalness a stable level of significance is only reached when more than 30 listeners are used. In this paper, we set out a list of guidelines, i.e., a checklist for carrying out meaningful subjective evaluations. We further illustrate the importance of sentence coverage and number of listeners by presenting changes to rank order and number of significant pairs by re-analysing data from the Blizzard Challenge 2013.

Index Terms: Subjective evaluation, text-to-speech, MOS test

1. Introduction

It is common to illustrate the performance of, for example, speech synthesis systems or voice conversion methods by presenting objective error measures such as mel cepstral distortion and the likelihood of the training set [1–3]. Although these give an indication of how well the synthesis model represents natural speech, automatically measuring the perceptual quality of synthetic speech is a challenge even when a reference natural speech signal is available, which often is not the case [4]. Although non-intrusive measures (measures that do not require a reference speech signal) have been proposed for synthetic speech [5, 6], subjective listening tests remain the gold standard for a true measure of quality.

The most commonly used listening tests are Mean Opinion Score tests (MOS) or Differential MOS (DMOS), preference tests, ABX-tests, transcription tasks, and MUSHRA tests. The synthesis attributes measured by these tests can range from quality to naturalness, intelligibility, similarity, expressiveness, pleasantness, and even emotions.

Through the years there have been many papers giving listening test guidelines [7–11]. However, contemporary evaluations of synthetic speech frequently do not take these guidelines to heart when designing and carrying out listening tests. This paper intends to present good practice in designing listening tests for subjective evaluation of synthetic speech systems, e.g., statistical parametric speech synthesis (SPSS), unit-selection, hybrid methods, and voice conversion. We detail some common shortcomings of current subjective evaluations and illustrate the importance of a sufficient amount of test material and participants in listening tests by an example using real data.

The paper is organised as follows: Section 2 begins by presenting a checklist of elements that must be considered when designing a good listening test. Following on from the checklist, we inspect the state of affairs pertaining to subjective evaluation at last year’s Interspeech. Next, in Section 4, the importance of sentence coverage and number of listeners is illustrated by means of a re-analysis of a portion of the Blizzard 2013 data [12]. We conclude by discussing how the results may be interpreted and by presenting our final recommendations.

2. A checklist for successful testing

There are many factors to consider when designing a subjective evaluation. The first question to ask oneself is: “What do I want to measure?” This should be followed by: “How do I get the answer to my question using listeners?”

To help you answer the above two questions we present a checklist of points/questions you need to consider when designing a test for subjective evaluation. The checklist consists of a list of questions, with comments and references supporting the relevance of each item. There is no one correct answer to any of the questions, but if these points are addressed every time a listening test is designed it will result in more meaningful subjective testing of synthetic speech.

• What test to use? MOS, MUSHRA, preference, intelligibility, and same/different judgements all fit different situations.
• Which question(s) to ask? This should be followed by: “How do I get the answer you get?”
• Which data to use for testing? Factor out aspects that affect the evaluation, but which are unrelated to the research question studied.
• What type of listeners? Native vs. non-native? Speech experts vs. naïve listeners? Age, gender, hearing impairments? Different listener groups can lead to different results [14–17].

See Section 4.1 for an analysis of the effect of listener type.
• Is a reference needed? Considering giving a reference or adding training material, particularly for intonation evaluation [18].
• Also consider the case for including other anchors.
• How many listeners to use? See Section 4.1 for an analysis of the effect of listener numbers on Blizzard 2013 data.
• How many datapoints are needed? Section 4.2 investigates the effect of the number of datapoints.
• Is the task suitable for human listeners? Take into consideration listener boredom, fatigue, and memory constraints, as well as cognitive load [19].
• Can you use crowdsourcing? The biggest concern here is how to ensure the quality of the test-takers [20–22].
• How is the experiment going to be conducted? With headphones or speakers, over the web or in a listening booth?
• Is the evaluation material unbiased and free of training data?

In short, think before you test! Don’t treat subjective evalu-
Table 1: Number of speech synthesis studies at Interspeech 2014 using a particular amount of listeners.

<table>
<thead>
<tr>
<th>Number of listeners</th>
<th>Number of studies</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Preference test</td>
</tr>
<tr>
<td>1–10</td>
<td>10</td>
</tr>
<tr>
<td>11–20</td>
<td>5</td>
</tr>
<tr>
<td>21–30</td>
<td>0</td>
</tr>
<tr>
<td>31–50</td>
<td>4</td>
</tr>
<tr>
<td>&gt; 50</td>
<td>3</td>
</tr>
<tr>
<td>Not stated</td>
<td>2</td>
</tr>
<tr>
<td>Total studies</td>
<td>24</td>
</tr>
</tbody>
</table>

Table 1: Number of speech synthesis studies at Interspeech 2014 using a particular amount of listeners.

<table>
<thead>
<tr>
<th>Number of listeners</th>
<th>Number of studies</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>MOS</td>
</tr>
<tr>
<td>1–10</td>
<td>8</td>
</tr>
<tr>
<td>11–20</td>
<td>5</td>
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<tr>
<td>21–30</td>
<td>1</td>
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<tr>
<td>31–50</td>
<td>5</td>
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<tr>
<td>&gt; 50</td>
<td>3</td>
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<tr>
<td>Not stated</td>
<td>0</td>
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<tr>
<td>Total studies</td>
<td>22</td>
</tr>
</tbody>
</table>

Table 1: Number of speech synthesis studies at Interspeech 2014 using a particular amount of listeners.

The lack of relevant detail in papers could suggest that little thought has gone into designing the experiments. Following the checklist in Section 2 would largely remedy such issues.

4. Re-analysing the Blizzard Challenge

To illustrate the importance of sentence coverage and the number and type of listeners, we re-analysed listener response data from the Blizzard Challenge 2013 evaluation [12]. This particular year was chosen since it is the most recent challenge involving English synthetic speech. We focus on results of MOS tests for naturalness and similarity on the main task (EHI).

In 2013, the Blizzard Challenge evaluated eleven systems, including natural speech. Each listener scored each system five times for naturalness and once for similarity, except natural speech, which was only scored three times for naturalness. The final scores published in [12] were obtained with 50 paid participants (EE), 92 volunteers (ER), and 52 speech experts (ES). Paid participants were native English speakers performing the task using headphones seated in sound isolated booths. Volunteers and speech experts were recruited online and took the test over the Internet, with no control over their listening conditions or native status.

To assess the robustness of MOS test conclusions to the number of participants and sentences used, we re-analysed progressively larger subsets of the Blizzard data. For each analysis, we computed two things: the number of significantly different system pairs, and the rank correlation between the ranking given by the current data subset and the ranking obtained when considering all participants for the test in question. To compute the number of significantly different pairs we used Bonferroni-corrected pairwise Wilcoxon signed-rank tests at a 1% level. This is the same procedure used to analyse MOS test data in Blizzard [23]. To calculate the correlation between two rankings we used the Kendall $\tau$ rank correlation coefficient [24].

4.1. Participants

To begin with, we consider the effect of the number of test participants, as well as how results differ depending on the type of listener used. To quantify how the number of listeners affects the ability of the test to discriminate between different systems, we computed the number of system pairs that were found to be significantly different when gradually increasing the number of listeners included in the analysis. To eliminate potential effects of sentence material, listeners were subsampled such that all system-sentence combinations were always covered. The results are presented in Fig. 1, where each point is an average across independent 1 000 resamplings (hence the minor amount of sampling noise). In this, as in all our graphs, cubic interpolation has been used between datapoints to better visualise the shapes of the curves. Solid curves correspond to naturalness results, while dashed graphs refer to the similarity task.

From Fig. 1, it is clear that the Blizzard similarity tests overall resulted in fewer significant differences than the naturalness evaluation. We will investigate the cause of this difference in the next section. There also appear to be some differences between the various types of listeners, particularly for naturalness.

Apart from the discriminative power of the test, it is also important that appropriate distinctions are made. To assess this, we calculated the rank correlation between system rankings based on the subsampled data and the final ranking obtained when averaging the full dataset including all listener types. (Since data is shared between the two rankings, the results may be biased to be overly optimistic, especially for large
numbers of listeners.) The results are displayed in Figs. 2 and 3 for naturalness and similarity, respectively. The shaded bands have widths of one standard deviation, estimated from 1 000 re-
samplings as before.

We see that rank correlations at first improve rapidly with the number of listeners, but that the rate of growth generally decreases as higher listener numbers are reached. For the nat-
uralness task in Fig. 2, using 30 paid participants was sufficient to achieve strong correlation (more than 0.98) with the final ranking. The minor correlation gap when using 30 rather than 50 paid participants was due to frequent rank changes between two pairs of Blizzard systems: (I, L) and (H, F). For simi-
arity (Fig. 3), the correlations are generally a bit lower, it takes a larger number of listeners to make the rank correlation rise to similar levels, and the results never quite reach stability.

It is also interesting to contrast different types of listeners. For this purpose, the graphs in Figs. 1 through 3 have all been broken down across the three different Blizzard listener types. In terms of rankings, paid participants (EE), despite being the smallest group, correlated the best with the full-data rankings for both naturalness and similarity. For naturalness ratings, vol-
unteers (ER) consistently gave low rank correlations and the least number of number of systems tested, and the set of listeners that listen to exactly the same system-sentence combi-
nations (the same stimuli) will here be referred to as a listener group.

To illustrate the importance of covering all system-sentence combinations we computed the average score of each system for each listener group (wherein everyone scored the same stimuli). These scores are presented in Fig. 4. It can be seen that the judg-
ments change substantially between listener groups, particularly for the similarity scores.

The need to adequately sample both listener and sentence variation puts a lower bound on the number of data points re-
quired. With too few samples, stochastic variation perturbs rankings and makes it impossible to confidently tell systems apart. To investigate the effect of the size of the statistical mate-
rial, and to put the naturalness and similarity results on a more equal footing, Fig. 5 graphs the number of significant pairs for the two tasks as a function of the total number of ratings used per system. Like before, the plots are averages over a large number of data subsets, but for convenience and to get better granularity, this figure was created by successively adding en-
tire listener groups (in all possible combinations), rather than selecting a certain number of listeners within each group as in
not necessarily helpful, either. It is likely of importance how well the stimulus variation correlates with the listener’s internal perceptual model, to which synthetic stimuli are compared when a listener scores them. As an example, the artefacts in unit selection synthesis (e.g., bad joins) are typically quite distinct from artefacts in SPSS (e.g., vocoder buzz), and arguably mostly orthogonal to each other. Since preferences and internal perceptual models may vary from listener to listener, having many listeners is important in order to accurately sample the space of internal listener models and converge on the population average. The Blizzard Challenge analysed here is an example of a test with a highly heterogeneous pool of systems, and the associated stimuli may be acoustically quite distinct.

All things considered, the results of the Blizzard Challenge re-analysis strongly suggest that synthetic speech naturalness evaluations, particularly MOS tests, should include more listeners compared to the numbers commonly used today (cf. Section 3). For reliability, we would recommend using at least 30 listeners. Moreover, each listener should listen to several examples of each system evaluated. 150 total judgements per MOS value computed should probably be considered a minimum.

The above numbers are for paid participants in carefully controlled conditions. In less controlled scenarios, such as crowdsourcing, behaviour closer to the online volunteers (ER) in Blizzard may be expected. In these situations, our advice would be to collect significantly more data and listeners. Even so, the power to draw conclusions may be limited, for instance because participants may not be using proper listening equipment and therefore not be able to discern minor differences between systems. For expert listeners, as may be recruited in a lab of speech researchers, one should keep in mind that their preferences may differ from those of the general public.

The numerical analysis in this paper has mostly focussed on requirements for identifying significant and stable differences. Of course, statistical significance is not the same as a practical significance, and the end goal is not to always tell all systems apart. Somewhere in the long tails of our figures, it makes sense to stop testing, and instead direct resources towards improving the systems involved. However, this is not an excuse for using an unsound testing methodology. Moreover, being able to identify many significant differences is generally a pre-requisite for accurately estimating effect sizes. Effect sizes quantify the subjective advantages of one system over another, and so are a step towards a more meaningful difference measure.

For truly meaningful results, a system should be evaluated for the task and context where it is ultimately used. This ideal is however at odds with the plight of the researcher or engineer making a general-purpose speech synthesiser, who wants to achieve results that are as broadly applicable as possible. Typical speech synthesis systems are in a sense designed for any task, and thus, paradoxically, for no task at all. While more meaningful tests are conceivable, they generally require a prohibitive amount of time and resources.

Until improved benchmarks and better objective measures arrive, differences in generic measures such as naturalness ratings remain our best indicators of synthesis adequacy. However: to get the answers we seek, and to convince fellow researchers and practitioners of their validity, we have to get better at asking the right questions, in the right way, to a good set of listeners. In other words, we need to pay attention to the points in Section 2. Only when improved benchmarks and better objective measures arrive, differences in generic measures such as naturalness ratings remain our best indicators of synthesis adequacy. However: to get the answers we seek, and to convince fellow researchers and practitioners of their validity, we have to get better at asking the right questions, in the right way, to a good set of listeners. In other words, we need to pay attention to the points in Section 2.

Acknowledgements This work was supported by the EPSRC Programme Grant EP/I031022/1 (Natural Speech Technology).
6. References


