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Dr. Tara Sainath

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IEEE Transactions on Speech and Audio Processing

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Dear Dr. Sainath,

Please find, attached, a revised draft of our manuscript “ASR for Under-Resourced Languages from Probabilistic Transcription.” I and my co-authors have discussed all reviewer comments, and have modified the text of the manuscript in response to each. These modifications are described, in detail, in our itemized responses to reviewer comments below. Please forward the revised manuscript, and this letter, to reviewers of the manuscript. If there are any reviewer concerns that can be addressed quickly in a short correspondence, I hope you will forward them to me so that I may address them without delay.

Faithfully yours,

Mark Hasegawa-Johnson

Professor, ECE, University of Illinois

# Responses to reviewer comments

Reviewer comments are in blue font. Responses are in black font.

## Reviewer 1

There is only one hour data used for each language in the experiments. It is hard to argue that trying to transcribe one or a few hours speech data for one language by native speakers is really beyond the capability of that language community. What is really beyond the capability is to transcribe massive data by native speakers. So I think the paper would be much more convincing if the experiments could be conducted using more data (e.g. 100 hours or even 10 hours of speech data for each language) and it is also important to see if the performance and impact of PT will change in such scenarios.

The first paragraph of the paper has been changed to explicitly mention the difficulty of finding native transcribers willing to transcribe even one hour of speech audio: “Doing so is beyond the resources of most under-resourced language communities; we have found that transcribing even one hour of speech is beyond the reach of some under-resourced language communities. In order to create the databases reported in this paper, we sought paid native transcribers for the seventy languages in which we have untranscribed audio data. We found transcribers willing to work in only eleven of those languages, of which only seven finished the task.”

Section 5.1 has been changed to explicitly mention the long-term goal of using larger corpora: “It is desirable to test the ideas in this paper with corpora larger than one hour per language, but larger corpora involve problems orthogonal to the purposes of this paper, e.g., the Babel corpora are telephone speech, and therefore contain far more acoustic background noise than the podcast corpora used in this paper.”

Discriminative training of GMM-HMMs. It is a little disappointing that only ML models are investigated for GMM- HMMs as discriminative GMM-HMMs such as MPE or BMMI models are well known to outperform ML-trained GMM- HMMs. Putting aside PT, with deterministic transcriptions discriminative models are the state of the art in GMM-HMM- based acoustic modeling. I would speculate that transcriptions with probabilities probably are more suitable to generative model such as ML-based GMM-HMMs. Since discriminative models are simply better models, it is worth investigating how PT will impact under such conditions and it would be more valuable to show impact on the best models available.

MMI, MPE and sMBR criteria were used to train cross-lingual baseline systems. Table IV now presents PERs of the MPE and sMBR systems. Text has been added to describe these results: “Three different types of discriminative training were tested. Maximum mutual information (MMI) performs consistently worse than minimum phone error rate (MPE) and structural minimum Bayes risk (sMBR), and is therefore not listed in Table IV. Averaged across all languages and systems shown in Table IV, development-test PERs of ML, MPE, and sMBR training are 73.43%, 73.04%, and 72.98% respectively; differences are not statistically significant, therefore only the ML system was tested on evaluation test data.

MMI, MPE and sMBR criteria were used to train PT-adapted GMM-HMM systems. Results of MPE and sMBR training are shown in Table IV. The following text was added to discussion: “PT-adapt GMM-HMM systems were trained using four different training criteria: ML, MMI, MPE and sMBR. MMI training consistently underperformed MPE and sMBR, and is therefore not shown. MPE training of PT-adapt systems improves their PER by a little more than 1% on average, comparable to the improvement provided

to CL baseline systems.”

Sequence training of NNs. It has been observed that techniques that give improvements at the cross-entropy level may give little gain after sequence training. Therefore, sequence training using PT worth looking into.

Authors agree with the reviewer, but the time allocated for this manuscript revision was insufficient to implement sequence training using PTs. Future research may pursue this possibility.

How are the GMM-HMM configured? How many states and Gaussians? What's the feature space?

The following text has been added to the new section VII.A: “All systems used tied triphone acoustic models, based on a decision tree with 1200 leaves. Each GMM-HMM used a library of 8000 Gaussians, shared among the 1200 leaves.”

How are the NN-HMM configured? What's the input? How many hidden layers? How are the activation functions chosen?

The following text has been added to the new section VII.A: “Each NN-HMM used five hidden layers with ReLU nonlinearities, and with 512 nodes per hidden layer, followed by a softmax output layer with 1200 nodes.”

Is it possible to also present WERs along with PERs?

The following text has been added to the new section VII.B: “Phone error rates are reported instead of word error rates because, in order to compute a word error rate, it is necessary to have either native transcriptions in the target language (thereby permitting the training of a grapheme-based recognizer) or a pronunciation lexicon in the target language. These resources are used by the monolingual topline, but not by any of the baseline or experimental systems.”

On EGG. This is an interesting piece of side information for acoustic modeling. But from what is reported, its impact seems to be quite marginal. To my understanding, it is only used in the interpolation in Table III or am I missing something here? Would appreciate it if the authors can elaborate a little bit more on its impact to ASR.

The reviewer is correct: the results of EEG are only demonstrated in Table III, and are not used at all for ASR training. The article demonstrates (1) that EEG may be used to improve PTs (the new Section 6), (2) that PTs may be used to train ASR (the new Section 7) --- both of these findings are new, and have not been previously published in any journal article (the former result has never previously been published in any paper anywhere; the latter has only been published in the ICASSP paper by Liu et al. cited in the bibliography). We intend eventually to connect these two ideas, but we expect this task to require many years, because EEG is hard to scale. This fact is now explicitly acknowledged by the new structure of sections 5, 6, and 7, and by the following text in the article: “Probabilistic transcripts based on EEG were not used to adapt ASR, because it is not yet possible to use EEG to generate probabilistic transcripts on a scale sufficient for ASR adaptation.”

## Reviewer 2

Why should we use IPA phone set to achieve model parameter sharing ? Looking at results reported in Tables III and IV, IPA universal phone set based model training yields much worse results. Besides, IPA universal phone set based multilingual training is ineffective, leading to worse results compared with language dependent phone set system. What if we use language dependent grapheme letter as phone set ? grapheme based lexicon should be much simpler in terms of building ASR system for low-resource language. Besides, it can also realize model parameter sharing, if we have a transducer system similar to EGG. Grapheme system is also effective. In Babel program, Swahili grapheme ASR system can produce comparable results with the ASR system built with expertise lexicon.

The new section VII.A now includes the following text: “Native transcriptions in the target language were used in order to train the monolingual topline system. CL and PT-adapt systems are initialized using native transcriptions in other languages, but are not permitted to see any native transcriptions in the target language. In order to make it possible to transfer acoustic models from training languages (which have transcriptions) to a test language (that has no native transcriptions), the phone set must be standardized across all languages; for this purpose, the phone set was based on the international phonetic alphabet (IPA).”

In addition to introducing IPA phone set, the paper has widely used G2P models, not only for phone language modeling, but also for misperception probability estimate. See Figures 1 and 4. This is not practical. As we know G2P model training needs a lot of supervised lexicon data that should be prepared with expertise knowledge. This contradicts the low-resource condition assumption. Besides, G2P can introduce errors that has never been mentioned in the paper.

The new section VII.A now includes the following text: “Similarly, in order to transfer acoustic models from training languages (which have transcriptions) to a test language (that has no native transcription), the training transcriptions must be converted to phonemes using a grapheme-to-phoneme transducer (G2P). G2Ps were therefore assumed to be available in all training languages, but not in the test language.

The paper uses EEG to estimate misperception probability, however its effectiveness has not been clearly demonstrated, since no baseline has been provided. I guess, data-driven based machine translation method can be adopted to do the same thing, correct? Shall we compare the efficacy of the two methods?

The new Section VI demonstrates that the use of EEG improves PTs. The baseline is a mismatch model using only distinctive feature distance; the improved system uses EEG-based distinctive feature distance. These points were somewhat obfuscated by the old structure of the paper. In order to make these points more clear, all EEG methods and results have been moved to the new Section VI.

How can we do low-resource speech recognition? I think we should sufficiently take advantage of diversified rich-resource multilingual language data and unlabeled target language data simultaneously (I agree there are lots of low-resource languages, but I also agree there should exist a lot of rich-resource languages as well). Looking into the paper, we find the multilingual data is extremely tiny, about an hour.

The cross-lingual system was trained using 240 minutes of data: 40 minutes per source language. The limitation on source language data is only important for symmetry of the experiment: it permits each source language to also be treated as a test language, in a leave-one-out paradigm.

Besides, it used IPA phone set for each source language to train multilingual DNN, which yields much worse results than the case with language-dependent phone sets.

IPA acoustic models are necessary, we believe, in order to allow phone recognition in a test language in which the test-set ASR has (1) no native transcriptions, (2) no lexicon, and (3) no G2P.

Other potential rule that I can think of to develop low-resource ASR: the less human intervention, the better it is. However, the paper contradicts this principle. A lot of human power is used to transcribe the data, and a lot of human power is involved in EEG experiments to estimate the probability of phone transducer. I guess if people use a sharper grapheme based ASR recognizer to transcribe a target language, then use machine translation method to learn the mapping rules between source and target graphemes of languages, it should be using less human intervention.

The paper uses PT to adapt cross-lingual(multilingual trained) DNN. However, little knowledge about how the PT affects the performance is known except for Figure 7. Even in Figure 7, it is not straightforward to understand. For instance, phone alternatives in each ``sausage" on average versus your oracle LPER is not revealed?

In the last column of Table IV, do you use phone lattice or one-best sequence to tune your DNN? If you use phone lattice, you should provide one-best results as your baseline to show the benefit of using PT.

In VII discussion section, you said you one-best phone sequence has 29-49\% PER. From my experience, even this ``worse" hypotheses still can yield improved results, given some simple data selection method in Kaldi. Besides, you should give your oracle results if you use true phone sequence to tune your DNN. Not only will this show

your effectiveness of PT training, but also it will show the effectiveness of your multilingual training. Normally, we would expect it will yield better results compared with results in the first column.

## Reviewer 3

II.D: the authors always talk about "the listeners" and "their responses". In the experimental section, it turns out that there was only "one monolingual" listener. It would be interesting to have at least two listeners, to get an idea if the EEG results are generalisable across speakers. Either the authors should add a second listener or correct the text (also abstract) and state "the listener" or "a listener".

III.A: did the authors use any confidence score during the self-training?

III.A / IV.E: for self-training real-valued targets perform better, but for the PT-NN, forced alignment is preferable? Are the differences between real-valued targets and forced alignment significant? In case of yes, do the authors have an idea why this is the case?

III.B: how do the authors estimate the phone prior with a bigram phone language model?

Related to that question, in IV.C: how is the G2P of the under-resourced language obtained/trained?

Sections V and VI seem less well structured than the first sections of the paper. Probably it would make sense to move the baseline section V.D directly into section VI.C.

At the moment, it is not very clear how Table III and numbers at the end of VI.B should be compared. The baseline of Dutch with phone bigram (68.61) is the same as the EEG-interpolated model?

In the baselines section, the authors talk about oracle experiments. It seems that they are only present in Table IV. It may help to add them to Table III as well. Section V does not talk about the neural network structure at all. The authors may want to add some information about the architecture of the NN that was used.

Minor detail: can the authors say something about where the 24414Hz come from in V.C?

In the analysis of VI.A, the entropy estimates seem not very clear. I.e. how is an entropy of 0.5 bits per segment achieved?

VI.B: the system was trained on English. The EER for English seem relatively high for the reader who is not familiar with EEG signal processing. Should such a result be expected?

In the last paragraph: how should the numbers be compared with Table III. What was the optimal value for alpha? Are the differences between the three numbers significant?

VI.C: Was the neural network trained from scratch on the 40 minutes or was an already trained NN adapted?

Table IV in general: In the abstract and introduction, the authors always separate mismatched crowdsourcing and EEG distribution coding. "Adaptation using mismatched crowdsourcing significantly outperformed self-training": can that statements be seen in Table IV, i.e. is the effect of mismatched crowdsourcing "alone" visible?

VII: "In PT adaptation, however, entropy is unavoidable, and quantizing the forced alignment doesn’t necessarily help." in IV.E, the authors say ... forced alignment also improves the accuracy...

"Table III showed that the 1-best path through the PT is only correct for 29-49% of all phones, depending on language". Could the authors explain where do theses numbers come from, and how to read that from Table III?