

**The Infinite Factorial Hidden Markov Model**  
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**Paper Critique 2**  
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## 1 Overview

The authors start their paper by discussing the evolution of the Hidden Markov Model (HMM) to the Factorial Hidden Markov Model (FHMM). The main reason behind this extension is the HMM's limited representational power of latent variables in modeling discrete time series data (e.g. part-of-speech tagging, dialogue segmentation). To overcome this, the FHMM factors each hidden state into multiple hidden (i.e. latent) state variables. In other words, FHMM formally defines  $M$  latent variables as  $M$  latent Markov chains that evolve according to Markov dynamics at each timestep  $t$ . Although this strengthens the representational power of the FHMM for latent variables, it introduces a new free parameter  $M$ . What the authors are after is **learning this parameter from data rather than specifying it beforehand** (Section 1, page 2, end of first paragraph).

The main contribution of the paper is presenting a non-parametric FHMM, Infinite FHMM (iFHMM), that is based on a new stochastic process for latent feature representation of time series called the Markov Indian Buffet Process (mIBP). The mIBP can be derived in three steps (Section 2):

1. Describe a distribution over binary matrices with a finite number of columns.
2. Integrate out the parameters of the model by careful selection of the hyperparameters.
3. Take the limit as the number of features (i.e. columns) goes to  $\infty$ .

The mIBP provides a matrix  $S$  which is interpreted as an arbitrarily large set of parallel Markov chains.  $S$  is then used as the building block of the iFHMM (Section 3).

## 2 Significance and Originality

This work is very recent (2009) so it does not have that many citations yet. However, one can see the significance of the work in the field of modeling discrete time series data (e.g. blind source separation) once one realizes that infinite features (or previously unknown number of features) can be modelled using the iFHMM.

The novelty of the methods presented by the authors stems from relieving the constraint of knowing  $M$  (number of features) beforehand in the FHMM and dealing with infinite (or unknown) number of features in the iFHMM. This has various applications in vision, audio processing (e.g. unknown number of speakers), and natural language processing.

## 3 Model Evaluation

Although the authors presented a very interesting idea (iFHMM), they did not give enough explanation or clarification for some key points in their processes. My only explanation for that is space limits. Here are some of those key points that I think required more details:

- Section 2.1, page 3: how they got to equation 5 from the previous explanation.
- Section 2.2, page 3: the derivation for the size of  $[S]$ .
- Section 2.3, page 4: I did not get how equation 9 shows that the suggested model is exchangeable in the columns and Markov exchangeable in the rows. Going back to reference [6], I am even more confused. If the columns are exchangeable, then it should not matter where the position of a feature in the matrix is. However, this is not true as the probability that a customer orders a dish  $m$  (corresponds to a feature in the matrix) depends on whether the customer in front of him ordered that dish or not.
- Section 3.1, page 5: the authors did not mention what other alternatives other than Laplace(0,1) could be used to IID sample the entries in matrix  $X$ . Using Laplace(0,1) was justified because it fits their data better. What about other types of data?
- Section 3.1, page 6: the authors did not provide further details about how their likelihood calculations satisfy the two technical conditions for proper iFHMM likelihoods.
- Section 3.2, page 6: no details about how to extend the representation of  $S$ ,  $X$ ,  $W$  were given (first step in the iterative sampling).

## 4 Experiment Evaluation

The experimental evaluation done by the authors is very poorly explained and not rigorous enough. My remarks on their experimental evaluation can be summarized as follows:

- Section 3.2, end of page 6: the authors threw some claims without backing them up by empirical evidence, or references to such evidence. For example, poor performance of naive Gibbs sampler; dynamic programming does not perform better than blocked Gibbs sampler; the bulk of computation is used for estimating  $X$  and  $W$ .
- Much more information was needed about the dataset used for the experiments. For example, which speakers were used as the dataset (there're 34 speakers in the original dataset); the randomness of the artificial pauses; how long the sentence were; was there any background noise in the speech.
- The authors did not discuss the complexity of their algorithms, or how much CPU time they consume on average.
- I was very surprised when I did not find any comparison with other existing blind source (or speech in their case) separation algorithms.
- The results obtained using 3 microphones cannot be compared to those obtained using 10 microphones since the authors **subsampling a noiseless version of the data** for the former case (Section 4, end of page 7).

## 5 Related Work

The authors did not mention what other algorithms, methods or techniques are used for blind source separation. They only discussed the work on which they based their processes. It is very interesting to note that 50% (7 out of 14) of the references had at least one of the authors as a co-author. I think they should have discussed other people's work rather self-referencing their own previous work, even though this paper builds on top of it.

## 6 Readability

The paper was mostly well written with few typos and some punctual errors related to the use of contractions (i.e. don't, shouldn't, etc). One confusing readability issue that did not leave me at ease is in (Section 2.2, page 3) when they refer to the **author's website** without giving any reference to it. I found out later (after another reference at the end of Section 3.2, page 6) that they meant the first author whose website was provided as a footnote in the first page.

## 7 Suggestions

I think it was a poor choice to publish this paper as a conference paper rather than a journal paper. Most of my comments are related to missing information that are essential to understand the work of the authors better. In addition, more experiments should be done to evaluate such work and compare it to other blind source separation algorithms.