DEPARTMENT OF COMPUTER SCIENCE & ENGINEERING, 0404   9500 GILMAN DRIVE
FAX: (858) 534-7029   LA JOLLA, CALIFORNIA 92093-0404

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Subject: Review of Alex Gerganov's thesis

To whom it may concern:

First, let me say that I enjoyed reading this thesis! Alex's model for perceptual/categorical learning is so much more elegant than previous models. Perhaps he should call it CPLUS+! Also, I thought the experiments were very well motivated.

Below I cover the 8 areas in the rubric for reviewing the thesis.

1. Significance of the research topic in terms of basic science and applied science.

The topic is the interplay between perceptual and categorical learning. I've always felt that Psychology spends too much time putting artificial boundaries between its sub-areas. This thesis is a step towards integrating the two areas.

The thesis does two main things. It presents a computational model of perceptual and categorical learning that is very simple, yet it is able to cover the data quite well. This is one of the main goals of cognitive models – to explain data in the simplest way possible, but no simpler. It is a model that is elegant in its structure, and it clearly is a step forward from previous models, such as CPLUS, that seem overly complex and in parts, implausible. Alex's model neatly skirts the issues that plague CPLUS – most importantly, that small changes in parameters (the number of competitive units) will completely change its behavior from abstraction to memorization.

Another nice feature of the model is that it bridges between the sides of the debate concerning whether perceptual learning involves feature changes, or simply re-weighting of the features. Of course, the answer could easily be "both," and this model is an embodiment of that idea.

Finally, the model has potential to address questions of featural versus holistic processing of faces. It has a nice mechanism for binding features together through long-range hebbian learning, that could lead to holistic effects.

The second main piece of the thesis is the behavioral experiments. If category learning has a strong, bottom-up perceptual learning component, then it should be location-specific. Hence it is very important to test this. One expects that it will not be location specific, yet it did show effects of location, especially in reaction time. One alternative hypothesis here should be considered, though – that the input from a different location just has to travel over a more noisy channel, as no input like this has been experienced at that location. That could explain the data (say, with a diffusion model of reaction time), without hypothesizing a component of perceptual learning.

2. Rationale and motivation for the goals and research objectives in the thesis.

The thesis is very well-motivated – its goal is to unify these two areas of psychology – one uses the methods of psychophysics and the other categorization learning. Why should psychophysicists limit their stimuli to ones so simple, and can their work be viewed as training a kind of low-level categorization? What if categorization is tested in the same manner as the psychophysics experiments, that show location-dependent learning? These are very interesting questions, with broad appeal in the community.

3. Appropriateness (suitability) of the methodology and techniques chosen for the research in terms of the goals and research objectives of the thesis.

The neural network modeling is particularly suitable for this particular domain. Goldstone has been developing connectionist models of this phenomenon for a long time. Here, Gerganov shows that very simple mechanisms, involving competitive and hebbian learning, can explain the behavioral data in both adults and babies quite well. He shows how Gestalt principles can emerge from very simple training stimuli, and how those same principles can be overcome by biasing the training stimuli, just as they are overcome in babies.

In the behavioral experiments, the methods are quite sound, using eye tracking to ensure that subjects are fixating the fixation point, so that training can proceed in just one location on the retina. Gerganov feels that there are issues in that many subjects find the task frustrating and refuse to continue the experiment, but this is essentially a very difficult task, and it needs to be in order to fulfill the requirements of category learning of complex categories. He is trying to replicate category learning experiments in what is essentially a psychophysical paradigm, where it is common for the authors of the paper to be the subjects, and to run the experimental trials for up to hours. The authors are clearly more motivated to perform these tedious and sometimes frustrating tasks than University undergraduates, and it is commendable that he was able to get a reasonable number of subjects to complete the very difficult task he gave them in the last experiment.

4. Basic and applied contributions of the thesis (description and evaluation).

I think I have already covered this above. Generally, this research does not, to my mind, have an applied component; it is basic science about learning.

5. Assessment of the publications related to the thesis – their number and the nature of the publication venues.

The number of publications is quite small, and they are in the proceedings of the Cognitive Science Society Conference, which is not an archival venue. I would like to see both the modeling and the behavioral experiments submitted to journals. The model, for example, should be of interest to researchers in the Cognitive Science community, and seems quite appropriate for the journal *Cognitive Science.* I challenge Gerganov to have this submitted to a journal by the time I arrive in Sofia. The psychological experiments seem appropriate for *JEP:HPP* or *JEP:LMC*. I expect the reviewers will ask for more experiments, but if so, I am sure Gerganov can carry them out.

6. Citations by other authors, scientific media reactions, etc.

The model (as published in the 2007 Cognitive Science Society Proceedings, with Gerganov as first author), has 11 citations. Another paper first authored by Goldstone has 16 citations. These are an indication that the work is of interest, but it would garner more citations if it was published in a journal.
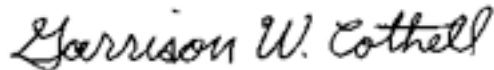
7. Opinions, recommendations, notes.

I gave extensive comments on the thesis itself. One opinion I have is that I find his proposal for a way for learning to transfer between positions to be fairly implausible.

8. Conclusion with a clearly formulated positive or negative evaluation of the thesis.

This is a really nice thesis, and I was glad to read it. It is certainly worthy as a Ph.D. thesis. Again, not to belabor my points, but he creates a very elegant model, that I certainly find more plausible than CPLUS, and he tests a very interesting question behaviorally – whether category learning can be position-specific.

Sincerely,

Garrison W. Cotrell

Garrison Cottrell,
Professor
Department of Computer Science & Engineering,
UCSD