Emmanuel Dauce <edauce@gmail.com>

Mauvaise nouvelle

1 message

Frédéric Henry <frederic.henry@etumel.univmed.fr> 26 September 2008 15:51
To: Emmanuel Dauce <edauce@gmail.com>

Salut,

mauvaise nouvelle: l'article pour neurocomputing a été refusé.

Je copie/colle les commentaires (j'ai pas encore tout lu).

à+
Fred

Please find below the referee reports. Based on these and the corresponding recommendations, we have to reach the sad conclusion that your paper

Emergence of a selective synchronous response through STDP in recurrent neural networks

cannot be accepted for publication in Neurocomputing. We hope that the referees' comments and suggestions are nevertheless useful and can help you improving your scientific work and/or presentation.

Hereby we would like to thank you for submitting your work to Neurocomputing and welcome you to consider us again in the future.

Sincerely,

Frank-Michael Schleif
Special Issue Editor

Tom Heskes
Editor in Chief
Neurocomputing

Reviewers' comments:

Reviewer #1: This paper presents interesting simulation results on the effect of a particular implementation of the STPD learning rule in a recurrent neural network made up of leaky integrate-and-fire neurons with random transmission delays. The main contribution of the paper is the report of the emergence of a selective synchronous response through STDP, i.e. a regular and synchronous pattern of activity is yielded when a learned stimulus is presented to the net while the net response remains unstructured when unlearned stimuli are presented. However, the authors recognize that this behavior is not robust and depends on a fine tuning of some parameters (namely, the plasticity coefficient and session duration); an inadequate choice of these parameters may lead either to forget some previously learned stimuli or to saturate the network making it insensitive to the presented stimuli.

As the authors state in the discussion section, "this study only presents the first stages of a neural encoding mechanism that may take place in perception processes", and I would add that the results of this study are not conclusive and do not address some important questions that are commented below. In my opinion, the study, in its present form, although interesting and well reported is not complete enough to deserve its publication in the Neurocomputing journal. In particular, I miss a deeper analysis of the following items:

1) the distinctiveness among the encoded stimuli. When several stimuli are learned to be encoded, the encoding test only addresses whether it is possible to discriminate between the response to one of them and the response to the rest of unlearned stimuli (based on the respective estimations of the #DOF). For encoding/decoding purposes, it is also desirable to discriminate the responses to each learned stimulus, if possible. Hence, a test on this matter or at least some ideas about how to do it would be very welcome.

2) the sensitivity of the reported selective behavior to some parameters of the learning process. On one hand, a rule of thumb relating the plasticity coefficient and session duration parameters with the network size N and number of patterns P could be investigated. On the other hand, a regulation mechanism for STDP could be devised or an alternative learning rule could be adopted to make the selectivity process more robust.

3) the comparison between the reported study and previous related work in the literature. As the authors mention in Section 1, there are contradictory results on the effects of STDP on recurrent networks regarding both structure development and synchronization. A comparative study among different STDP rules and neuron models which shed light to these apparent contradictions would be quite helpful. The reported study just adds a new combination of STDP rule and neuron model but does not address the causes of the contradictory results.

Some typo errors and minor comments:

- p.3: "garantees" -> "guarantees";
- p.5: "ah internal period" -> "an internal period";
- p.6: "srong inhibition" -> "strong inhibition";
- p.8: "higher than a critical threshold" I think should be "lower than a critical threshold"
- p.8, in the equation, "p=1..k" I think should be "p=1..k-1";
- p.9: "unknow stimuli" -> "unknown stimuli";
- p.9: "able encode" -> "able to encode"
- last paragraph of Section 5 (pages 9 and 11 and Figure 5): a linear relationship between N and the capacity of the network is assumed, but is this linear relationship extendable to higher values of P and N or a logarithmic capacity may appear?
- p.12 "wether" -> "whether".

Reviewer #2: In this article, the authors study the effect of a STDP learning rule in a recurrent neural network assuming random axonal delays. In particular they show that the network can become selective to input patterns, i.e. the network elicits a high spiking synchrony when a given input pattern is presented and a low synchrony when other patterns are presented.

%%%%% MAJOR COMMENTS %%%%%

1. In the introduction, the authors emphasise on the apparent contradiction in the literature on STDP and synchronization. On one hand, there are papers showing that STDP enhances spiking synchrony (Suri and Sejnowski, 2002; Cassenaer and Laurant, 2007) and on the other hand there are papers (Lubenov and Siapas, 2008) showing that STDP desynchronizes the network spiking activity. Unfortunately, the paper does not try to solve the apparent contradiction. Does the apparent contraction comes from the different STDP learning rules, the different axonal delays, the different network structure,…? An analysis or at least a strong discussion on the topic would be highly appreciated.

2. In section 4, the authors recognized that emergence of selectivity (which is the main message of the paper) " relies on a fine tuning of both the coefficient value and the plasticity session duration". The absence of robustness in the selectivity process makes the message of the paper relatively weak. Would it be possible to change the learning rule to make the selectivity process more robust (e.g. with a triplet learning rule, c.f. Pfister and Gerstner 2006 with potentiation or depression coefficient being some function of the postsynaptic activity)? For example, the BCM learning rule (Bienenstock et al. 1982) has a built-in mechanism (the non-linear dependence upon a low-pass filtered version of the postsynaptic firing rate) that regulates automatically the amount of plasticity. Furthermore, the BCM learning rule does not require (as opposed to the ad-hoc plasticity switch assumed in this paper) plasticity to be switched on when the desired pattern and switched off when the other patterns are presented.

3. The Neuronal model assumed in this paper is a Leaky-Integrate-and-Fire one. I am a bit surprised that the network input is modelled as a superimposed voltage instead of a current. The unconventional way of treating the input rises two problems. First it is not biologically realistic and second it prevents comparing the results with other studies (c.f. major comment 1). The motivation presented for this "potential-based" input is not convincing since it is still possible to guarantee that a subset of input is active in a presence of input currents.
4. I like the idea that the two regimes (high synchrony and low synchrony) could coexist. Would it be possible to make a study on the bifurcation? What is the bifurcation parameter?

%%%%% MINOR COMMENTS %%%%%

1. I find the #DOF quantification of synchrony interesting. However, it should be clarified how the vectors used for the PCA are defined. Does each element of the vector correspond to the number of spikes within a given bin size. If yes, what is the bin size? Does the bin size matter for the analysis?

2. On p5, "The result is a global facilitation …." I would rather use potentiation since facilitation is normally used for short-term plasticity.
3. On p5, it is emphasized that the rule is "antisymmetrical". If we plot the weight change as a function of the time difference between the pre- and postsynaptic spike, (it could be a good idea to add this figure) the result is not antisymmetrical because of the axonal delay. The whole curve is shifted to the left (if Dt = t_pre - t_post).

4. On p7, why is STDP applied between the 12th and 13th second and not the 4th and 5th second?

5. On p8, "… the #DOF estimation is higher than a critical threshold". I guess the word "higher" should be replaced by "lower".

6. On p11. "… several statistically equivalent stimuli" should be replaced by something like "… stimuli that are drawn from the same distribution".

Reviewer #3: NEUCOM-D-08-00438
Title: Emergence of a selective synchronous response through STDP in recurrent neural networks

The manuscript presents recurrent self-feeding neural network model trained with STDP. It learns to differentiate by phase transition between one stimulus from a set of static and statistically similar stimuli through synchronised patterns of activity

The idea of synchronised activity in STDP networks is not new. The (marginal) novelty of this manuscript is in the type of stimuli presented. Although the proposed model differentiates one stimulus for other similar inputs, the manuscript does not propose a solution for the 'forgetting' problem - that this learnt pattern is forgotten once a new one is presented. The manuscript is therefore neither clear enough not novel enough to warrant publication.

The discussion is rather superficial. To argue that the presented simulations are a model of perception is an aver statement not supported with any evidence. No attempt is made to link the presented model to other similar work on synchronised activity in STDP networks e.g. Shen et al, Neural Computation 2008; Borisyuk Biosystems 2002; Panchev; Panchev and Wermter Neurocomputing 2004; Masuda and Kori Computational Neuroscience 2007, or put the work in the context of real neural systems e.g. Shadlen Newsome J Neuroscience 1998.

The syntax of the manuscript is generally poor and the sentence structure is often difficult to follow (and difficult to evaluate) with typos in various places. The manuscript would benefit from a proficient speaker of English to proof read it, and, perhaps, to help simplify the structure of some excessively long sentences.

Other comments
Given the fundamental role of synchrony in this work, the introduction would benefit from a brief description of the terms "synchronization of a neural network" and "distinguish by synchrony".

A reference or a note to justify the choice of Tau_r = 2 ms, Tau_m = 10 ms would be useful.

Check square brackets in equation (1)
Small s is not defined in equation (1)

The square brackets in several places, for example "The weights sum standard deviation is set in the range [2-3]." Could be confused for references.

The listing of author names in the reference list is inconsistent. Some are in first name + surname others are initials + surname. Check required reference format.

" The stimuli are qualified as "static" i.e. when a stimulus is presented to the network, it lasts for a duration which is greater than the mixing time of the system in order to converge to a stationary response". The "mixing time" and duration of presentation need to be qualified more concretely.

The relationship between the weight update equation (top of page 5) and the trace need to be made more explicit.

Figure 2C. Activity of the network in the first 10s of the STDP process, but the x-axis indicates 5000-150000 ms. What is the 'message' of this plot? That synchrony emerges?
Figure 2 appears to represent the activity pattern from one network. It is not clear if this patter is reproducible.

Last paragraph in page 6, this qualitative description is not backed by sufficient data to support it. For example, it is not clear how reproducible is the described patter of activity, or at least under what

circumstance does it reproduce.

Page 7: We also verified that such a periodic shaping is systematically observed for other parameter configurations, even if the patterns of activity are not always synchronous. [What does this mean?]

Page 7: Some complexity decrease for the other stimuli,
The manuscript is not clear what is meant by complexity in this context or its relationship with DoF

Figure 3 Does the drop in DoF correspond to the stimulus presentation?