

Emina Soljanin
Distinguished Member of Technical Staff
Math of Networks & Communications Research
Bell Labs

Rm. 2C-353
600 Mountain Avenue
Murray Hill, NJ 07974
+1 908 582 7933
emina@alcatel-lucent.com
<http://ect.bell-labs.com/who/emina>

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To the Information Theory Society Paper Award Committee

Re: member report on the nominated paper

Cuff, P.W., Permuter, H.H., and Cover, T.M., “Coordination Capacity,”
IEEE Trans. Inform. Th., Vol. 56 , No. 9, 2010 , Page(s): 4181 - 4206

The paper claims to “develop elements of a theory” of what the authors call “cooperation and coordination in networks”. The most important contribution of the paper is, in my opinion, in that it tries to go beyond the fundamental problem of communication, as propounded by Shannon, as that of reproducing at one point either exactly or approximately a message selected at another point. Here the decoders’ goals are e.g., to generate joint empirical types (weak coordination) or the joint probability distributions (strong coordination).

The authors list a number of problems arising in e.g., combinatorics or quantum computation. In each case, they briefly mention the problem and then proceed to say how their problem is different. I find this approach somewhat confusing. I kept wondering why were those problems mentioned to begin with. It would be much more useful to identify a set of problems which have been worked on, and which belong to the class being addressed here, and to point out how exactly that is the case. That way the paper would actually provide a framework rather than a not very convincing generalization.

That said, the paper is definitely one of the rare concerned with problems that are nontraditional but should be further looked into. It is well written. The results are not hard to derive by anyone with good skills in information theory, but that aspect of the paper does not bother me at all. Given that the main contribution of the paper is a new formulation, the math (unless trivial) is irrelevant. It is the formulation I have problems with. I would prefer to see models arising from real independent problems (e.g., quantum compression) rather than the standard information theoretic models and topologies looked at in a different way. I find the latter self serving.

In summary, the paper deserves the award because it could “stimulate interest in and encourage contributions to fields of interest of the Society”. It would be more likely to do so if it took somewhat less confined approach in treating its topic.

Sincerely,

Emina Soljanin