
Edward William Naim

May 4, 2004

Ed is to be congratulated for a very thorough and mature piece of innovative research. This thesis represents the culmination of many months spent thinking long and hard about how to make a local search algorithm incentive-compatible (with high probability) for combinatorial auctions. The main result is a method to take an LP-based local search algorithm and make it non-manipulable with high probability, using a consensus function approach to make the search direction independent (w/ high probability) of the bids of any single agent. As such, the Consensus mechanism provides a nice counterpoint to the GrowRange mechanism (which uses systematic-search algorithms, but cannot leverage information within agent bids in determining how to branch). Experimental results suggests that Consensus often outperforms or matches the anytime performance of Cassanova, which is generally viewed as a state-of-the-art (but non incentive-compatible) mechanism for combinatorial auctions. It is also intriguing that the thesis uncovers a local search algorithm— LP-based hill-climbing —that seems to outperform Cassanova handsomely across most domains.

For me, the most interesting aspect of the analysis demonstrates the effect that the choice of 'q' (the consensus probability) has on the run-time performance of Consensus. Not only does Consensus reduce to hill-climbing for q=0, but the performance actually improves as q increases towards 0.5, with the voting and robustness providing by the consensus function seeming to help in making decisions. As expected, in moving from q=1.0 to q=0.5 the performance improves, as Consensus becomes less like a random walk and more like a directed local search. By the way, I found the discussion (around p.56) of the 'q=0' case as random hill-climbing a little confusing. I would refer to 'q=0' as hill-climbing (I didn’t see what was random about it). Anyway, the bottom-line is exactly as described on p.53, that the choice of q makes a tradeoff between performance and non-manipulability.

I thought the discussion (p.32) of why strategyproof considerations are important was useful. Actually making this argument often gets overlooked. I also appreciated the good use of examples throughout the thesis. The discussion (around p.35) that compares Consensus with GrowRange was clear and insightful. The theoretical analysis (e.g. Claim 3) is fairly
clear, but I think it would have been better to break this down into two claims. First, prove strategyproofness for a random-walk VCG-based/LP-based local search. This could improve a precise proof of why the VCG-based method would be anytime strategyproof in this case. (Also, I don’t think anytime strategyproofness was every precisely defined). Second, one could then use this to combine with a Lemma that the sequence of states was agent-independent with some probability that depends on ‘q’ (assuming \(\rho\)), to give the main result.

The experimental results are interesting, especially in that they demonstrate competitive performance with Cassanova in many domains. It was also nice to see the large effect that \(\rho\) has on performance, for a fixed ‘q’, and the analysis to demonstrate reasonable values of \(\rho\). I was a little confused about why you chose (p.66) to ”select q to be near-optimal”. I would have thought that ‘q’ should be defined to give a particular probability of manipulation, rather than optimized for performance? Good next steps would be to repeat the analysis for a fixed ‘q’, run on Boutilier’s data to do a good comparison with Cassanova, and also to compare with GrowRange.