Analysis of "ImageNet Classification with Deep Convolutional Neural Networks"

A brief statement of the problems addressed in the paper in my own words.

The authors, Alex, Ilya, and Geoffrey made a deep convolutional neural network that (by their own account) performed admirably on the ILSVRC 2010 competition, and also beat the other contenders in the contest by a sizable margin.

What I agree with/like in the paper and why.

I really appreciated how they employed a variety of tactics to ensure that they were not leaving any performance on the table. They artificially augmented their data(Which in itself is pretty cool). They ran their GPU's in SLI mode, (which really isn't a thing anymore). They used ReLU's. They used overlapping pooling, and dropout. While I was reading the paper, it was kind of like they started by finding some net architecture that kind of worked for them. Once they found an architecture they liked, they started adding in all the little pieces and ideas others had come up with to try and improve the performance. I feel like this point is important. Start with something that works well enough, then augment, improve, and expand upon to get as much improvement/performance as you possibly can.

What I disagree with/dislike in the paper and why.

I can't really think of anything in the paper that I really disagree with. It may be that that idea is a product of how current the paper is, but their ideas, and methodology seemed sound, and it didn't feel foreign or out of date. Which helped me to be able to relate to it more than other papers.

There is one line of thought I have been thinking about, and I might as well put it here. In class, the idea of implementing as close as possible the human brain seems to carry some weight with it. My question is: "Is it really important to have as close a replica as we can to the human brain?" or is it just "good enough" to have some net that can properly classify images and audio? Granted, in the right place and the right time, those capabilities could be major game changers in the world. I guess my main question is "Is it better to try and replicate the human brain as best we can, or just make some new thing to solve some new problem?"

Any inspirations I found in the paper.

With my limited exposure to research papers, I am impressed with how much work is put into these things. A group of researchers just doesn't come along and say "How about we do this…" More often than not, it seems like they get some idea and work with it for a very long time to try and turn it into something really impressive and meaningful. After much work and many years they may or may not succeed, or through sheer dogged perseverance they force their idea into reality and just make it work regardless of how good it was in the first place.

Pay attention to the results section and ask yourself impartially how much trust you, as a researcher, place in them.

I am not sure what to say here. I have never asked myself how much I "trust" the results of some research paper or another. The fact that you asked us to address this issue of trust implies that some papers may not be entirely truthful when reporting their findings. With respect to this paper in particular, I would say that I don't have any reason as of yet to distrust their results. In my mind it would be such an easy thing to verify. They mentioned that they entered the ILSVRC competitions for the years 2010, and 2012. The results of competitions would obviously be published, and with how popular this paper is somebody would have already called them out if their results were misleading/fabricated. To answer the question, "How much trust do I place in those results?". I trust them until I have a reason to not trust them.