NIPS
Neural Information Processing Systems
8-11th December 2014, Montreal, Canada

Reviews For Paper

Paper ID	666
Title	Adaptation of Supervised Deep Convolutional Models with Limited Training Data

Masked Reviewer ID: Assigned_Reviewer_25
Review:

Question	
Comments to author(s). First provide a summary of the paper, and then address the following criteria: Quality, clarity, originality and significance. (For detailed reviewing guidelines, see http://nips.cc/PaperInformation/ReviewerInstructions)	The paper evaluates 3 methods to adapt a deep CNN to a new dataset with limited training data, i.e., 1) Deep and Frustrating Easy (DFE), 2) Deep Late Fusion (DLF), and 3) Deep Subspace Alignment (DSA), by a case study adapting the deep CNN trained on ImageNet to the Office dataset. The Office dataset includes 31 categories with 3-8 labeled images per category in 3 domains referred as Amazon, DSLR, and WebCam. The paper also confirms the last fully-connect layer shall be used in the adaptation by comparing the A-distance between the source and target data. The paper studies a very interesting problem, how to adapt a deep CNN to a new data domain with very limited training data. The major contribution is to present some empirical results comparing 3 methods using the ImageNet and Office datasets as a case study. As a benchmark or evaluation paper, there are two major concerns: 1) How representative is the Office dataset as a new task in a target data domain? It is unclear if 3-8 labelled samples per category is a typical target case for adapting a deep CNN model. 2) As shown in the experiments, the improvements of these 3 methods vary depending on how similar of the source and target domains. What are the conclusive observations or good practice the readers can learn from these experiment when tackling with their new transfer problems? As a technical paper, the novelty and contributions are a little bit thin for NIPS.

	Some recent relevant references: Learning and transferring mid-level image representations using convolutional neural networks, CVPR 2014 CNN Features off-the-shelf: an Astounding Baseline for Recognition, CVPR Workshop, 2014.
Please summarize your review in 1-2 sentences	As a technical paper, the novelty and contributions are a little bit thin for NIPS. As a benchmark paper, a single case study adapting the deep CNN model trained on ImageNet to the office does not leave conclusive guidance for future practice.
Quality Score - Does the paper deserves to be published?	4: An OK paper, but not good enough
Impact Score - Independently of the Quality Score above, this is your opportunity to identify papers that are very different, original, or otherwise potentially impactful for the NIPS community.	1: This work is incremental and unlikely to have much impact even though it may be technically correct and well executed.
Confidence	4: Reviewer is confident but not absolutely certain

Masked Reviewer ID: Assigned_Reviewer_41

Review:

Question	
	Contribution: The paper proposes new methods for domain adaptation in discriminatively-trained deep networks. As these networks require very large datasets for training, the key challenge is to transfer to the target domain using only a few example images a deep model that was pre-trained on a source domain from million of labelled examples. The paper proposes (1) a way of selecting which level of the deep network to use for domain adaptation and (2) the use of three different adaptation methods, largely taken off-the-shelf, in order to do so. Pros: The problem is clearly an important one and the paper is timely, as deep features transferred from ImageNet are the new state-of-the-art in most vision applications.

Comments to author(s). First provide a summary of the paper, and then address the following criteria: Quality, clarity, originality and significance. (For detailed reviewing guidelines, see http://nips.cc/PaperInformation/ReviewerInstructions)	The empirical evaluation is nice, covering many different use cases and alternative strategies for adaptation. Adaptation results are very good. Cons: A) A contribution of the paper is the use of the A-distance to select which layer of the CNN to adapt. Unfortunately: 1- at l.228 the authors note that in all cases the layer with smallest A-distance is the last one 2- the criterion is validated in Fig. 3.c, but not in a convincing way. Here we see that performance goes up with successive layers of a CNN and that the A-distance, conversely, goes down. However, this may be entirely due to the fact that higher level provide better features rather than features that adapt better. Most importantly, this is insufficient evidence that the A-distance is a good predictor of which layer to use for adaptation. Overall, There is no strong evidence that the A-distance is useful. B) The paper is a nice evaluation of existing adaptation methods, applied to deep CNNs. DFE and DSA were proposed before. I am not certain about DLF, but it seems a minor variation of DFE. Hence, technical novelty feels very limited. C) Compared to previous work, and as noted above, results are very remarkable. However, it is unclear whether this is due to the power of the proposed (off-the-shelf) adaptation techniques or, rather, the use o significantly better baseline features than other papers. There is only one deep-learning baseline (DLID), which compares favourably in one case, and is outperformed by the authors in many others -- however, even in this case the baseline features are probably not as good as the one used here. D) The paper could use some

	restructuring to present its content in a more obvious and organised manner. There are many methods and scenarios compared, and these are a bit entangled in the current presentation.
Please summarize your review in 1-2 sentences	The paper is timely and include a nice comparison of simple/existing adaptation techniques on ImageNet-pre-trained features. Unfortunately, beyond the empirical evaluation, the technical contribution is very limited. The layer selection method, based on the A-distance, is perhaps the most innovative component of the paper; unfortunately the authors could not convincingly demonstrate its utility. While noting that domain adaptation works well with the latest generation of deep features is useful, I am not convinced that this is sufficient novelty or impact for NIPS.
Quality Score - Does the paper deserves to be published?	5: Marginally below the acceptance threshold
Impact Score - Independently of the Quality Score above, this is your opportunity to identify papers that are very different, original, or otherwise potentially impactful for the NIPS community.	1: This work is incremental and unlikely to have much impact even though it may be technically correct and well executed.
Confidence	3: Reviewer is fairly confident

Masked Reviewer ID: Assigned_Reviewer_6

Review:

Question	
Comments to author(s). First provide a summary of	This paper proposes a domain adaption approach that uses A-distance to select a layer in a deep CNN, which provides strong adaption performance. The proposed method outperforms existing approach in a various of experimental studies. This paper also conducted some studies about the affect of training size to domain adaption. Clarity: This paper is well written. The proposed

the paper, and then address the following criteria: Quality, clarity, originality and significance. (For detailed reviewing guidelines, see http://nips.cc/PaperInformation/ReviewerInstructions)	method is simple and intuitive, and its performance is confirmed by extensive studies on the Office dataset and ImageNet. Originality and significance: Deep neural network is a popular technique in image classification. Deep models tend to perform even better when they are supplied with more training samples. This paper addresses an important problem that whether domain adaption is still necessary, given deep models and large training set.
Please summarize your review in 1-2 sentences	This paper address an important problem of applying domain adaption to deep learning models. Experiments show that the proposed method outperforms existing approaches by a large margin.
Quality Score - Does the paper deserves to be published?	7: Good paper, accept
Impact Score - Independently of the Quality Score above, this is your opportunity to identify papers that are very different, original, or otherwise potentially impactful for the NIPS community.	2: This work is different enough from typical submissions to potentially have a major impact on a subset of the NIPS community.
Confidence	3: Reviewer is fairly confident