CVPR 2014
IEEE Conference on Computer Vision and Pattern Recognition 2014
June 17-19, 2014, Columbus, Ohio, USA

Reviews For Paper

Paper ID	1058
Title	Anytime Recognition of Objects and Scenes

Masked Reviewer ID: Assigned_Reviewer_10
Review:

Question	
Paper Summary. Please summarize in your own words what the paper is about.	The authors formalize the classification-on-a-budgeted problem in a reinforcement learning setting. They use linear value function approximation to define a policy on feature evaluations that maximizes the expected classification performance on a budget. A policy iteration method is used for training. The authors evaluate their method on three data sets and report good performance.
Paper Strengths. Please discuss the positive aspects of the paper. Be sure to comment on the paper's novelty, technical correctness, clarity and experimental evaluation. Notice that different papers may need different levels of evaluation: a theoretical paper may need no experiments, while a paper presenting a new approach to a known problem may require thorough comparisons to existing methods. Also, please make sure to justify your comments in great detail. For example, if you think the paper is novel, not only say so, but also	See below.

explain in detail why you think this is the case.	
Paper Weaknesses. Please discuss the negative aspects of the paper: lack of novelty or clarity, technical errors, insufficient experimental evaluation, etc. Please justify your comments in great detail. If you think the paper is not novel, explain why and give a reference to prior work. If you think there is an error in the paper, explain in detail why it is an error. If you think the experimental evaluation is insufficient, remember that theoretical results/ideas are essential to CVPR and that a theoretical paper need not have experiments. It is not okay to reject a paper because it did not outperform other existing algorithms, especially if the theory is novel and interesting. It is also not reasonable to ask for comparisons with unpublished papers and papers published after the CVPR deadline.	See below.

Preliminary Rating. Please rate the paper according to the following choices. Oral: these are papers whose quality is in the top 10% of the papers at CVPR. Examples include a theoretical breakthrough with no experiments; an interesting solution to a new problem; a novel solution to an existing problem with solid experiments; or an incremental paper that leads to dramatic improvements in performance. Oral/Poster: these are very strong papers, which may have one weakness that makes you unsure as to whether they should be oral or poster. Poster: these are strong papers, which have more than one weakness. For example, a well-written paper with solid experiments, but incremental; a paper on a well studied problem with solid theory, but weak experiments; or a novel paper with good experiments, but poorly written. Weak Reject: these are papers that have some	Poster

promise, but they would be better off by being revised and resubmitted. Strong Reject: these are papers that have major flaws, or have been done before.	
Preliminary Evaluation. Please indicate to the AC, your fellow reviewers, and the authors your current opinion on the paper. Please summarize the key things you would like the authors to include in their rebuttals to facilitate your decision making. There is no need to summarize the paper or reviews. If you have additional concerns that	In general the paper is well written and technically sound. The abstract and introduction are a bit "fluffy" for my taste, for example "this vital competence is riddled with controversy and remains a mystery still today". Also, in line 78-83, I find it a bit insulting to the intelligence of the reviewer to multiply a runtime with an arbitrary constant to make it sound large (why not 1 billion and 15 years?); the intelligent reader will recognize the runtime as important without this. The proposed method based on reinforcement learning is sound and puts the whole approach on a solid foundation. However, the two mentioned methods [8] and [13] seem quite relevant; in particular [8] seems to assume the same formalization of the problem and differs only in details. It is unfortunate that the authors do not discuss these relevant works in detail or compare against these methods. In line 357 the authors mention that if their loss function g is "unbiased" then maximizing the information gain would produce the minimum loss decisions. I cannot quite follow this; in what sense can the loss be unbiased? I believe what the authors may mean is not bias but the concept of proper scoring rules. (Information gain is just mutual information and corresponds to maximum likelihood for a categorial model; hence the information gain corresponds exactly to the log-loss, which is a proper scoring rule. All proper scoring rules will yield the same decisions asymptotically.) In (3) the authors use a temperature parameter tau. I do not quite understand the purpose: one can always define $\theta' = \theta / \tau$ and learn θ'. There must be a reinforcement learning motivation to use tau, as but it is not discussed. In addition to their own baselines, there are existing classifiers which are not budgeted but do have the property of being sensitive to the input in their feature evaluations: decision trees. I think it would be a good baseline to include decision tree models of different depths to enforce a budget. Figure 4 and 5 are unclear. In Figure 4 the right plots are not readable and I do not know what the top one represents: is it a number of feature paths taken when evaluating the test samples?

were not included in the reviews, please be sure and include them as well.	In Figure 5, the bottom plots are not described except for that they represent a hierarchy of classes. What do I see in the rightmost three plots? What are the colors? Likewise, for "specificity" it would be good to have an equation instead of "normalized information gain in the hierarchy"). In addition, while informative, the experiments are not exactly on tasks where speed really matters. Image classification is important as is power consumption, but it can be always parallelized across multiple machines and feature computation can be performed on suitable hardware. It would be more convincing if the authors could implement their method for a task which does require real-time performance such as face or object detection. Overall the method is promising but a comparison to the existing state of the art is required to appreciate it. Minor details. Line 30, this sentence no grammar. Line 43, "a few attempts". Line 49, "budget". Line 120, what does "this" refer to? Line 315, is $r_i = R(s_i,a_i, s_{i+1})$ here? Line 623, do you mean "objective" instead of "objects"? Reference [14], "of visual perception".
Confidence. Write “Very Confident" to stress that you are absolutely sure about your conclusions (e.g., you are an expert who works in the paper's area), “Confident” to stress that you are mostly sure about your conclusions (e.g., you are not an expert but can distinguish good work from bad work in that area), and “Not Confident” to stress that that you feel some doubt about your conclusions. In the latter case,	Confident.

please provide details.	
Final Recommendation. After reading the author's rebuttal and the discussion, please explain your final recommendation. Your explanation will be of highest importance for making acceptance decisions and for deciding between posters and orals. Include suggestions for improvement and publication alternatives, if appropriate.	Poster
Final Rating. After reading the author's rebuttal, please rate the paper according to the following choices.	Poster

Masked Reviewer ID: Assigned_Reviewer_2

Review:

Question	
Paper Summary. Please summarize in your own words what the paper is about.	The paper proposes an active classification policy by blending reinforcement learning with object detection. The main idea is that a classifier can reach a decision through a sequence of steps, that may be both class- and input- specific. Instead of having a fixed strategy for feature extraction and early reject/accept (as cascades do) the authors here propose a dynamic strategy for assessing a classifier's score by learning a policy for feature evaluation and updating the score of a classifier. In an offline stage they learn how to 'take actions', i.e. sample features in a 'state-specific' manner, namely based on the current classifier score and class. They do this by optimizing an information theoretic estimate of the gain incurred by using a particular feature, aiming at maximizing the expected cumulative reward of the policy (which they set to be the area over a cost-information gain curve). At test time they follow this policy, and use Gaussian data imputation to accommodate (temporarily) missing features.

	demonstrate that both on synthetic data, and on more common vision datasets their method allows to intelligently select features, getting the highest classification accuracy for a given computation budget.
Paper Strengths. Please discuss the positive aspects of the paper. Be sure to comment on the paper's novelty, technical correctness, clarity and experimental evaluation. Notice that different papers may need different levels of evaluation: a theoretical paper may need no experiments, while a paper presenting a new approach to a known problem may require thorough comparisons to existing methods. Also, please make sure to justify your comments in great detail. For example, if you think the paper is novel, not only say so, but also explain in detail why you think this is the case.	I really liked reading this paper; it proposes a genuinely novel combination of reinforcement learning and detection, and comes with well thought-out solutions to many problems that are not too obvious to address (namely: - the data imputation, in the absence of features - the phrasing of the overall, additive reward, in terms that relate to the computation cost and - the parameterization of the state space -the learning of a policy on that parameterized space). In particular I had personally thought about using reinforcement learning to the problem, and could not figure out all of those details. Apart from its theoretical contributions of this paper, the authors also demonstrate its use in a practical (even though a bit limited) setting, which would make it of interest to the practitioners of large-scale classification. I am therefore very strongly in favor of the paper.
Paper Weaknesses. Please discuss the negative aspects of the paper: lack	The paper has certain shortcomings in terms of presentation, that could however be easily dealt with by a proper rewriting. At certain points the authors also use unnecessary complicated language, or heavy statements, e.g.: l.17-18: The time course ... still today. l. 081-083: Is this really necessary to make the point? l. 080 suffices. I also did not enjoy too much the connections with human vision - they may be intriguing, but stating in the abstract that your strategy mimics human perception, is a bit of a stretch, given that you do not provide any real proof that it does, other than a few references to similar papers. The presentation could also be simplified, by omitting unnecessary notations. For instance, the 'budget sensitive loss' is introduced in Sec. 3, but then never reused again (instead the authors turn to maximizing a

of novelty or clarity, technical errors, insufficient experimental evaluation, etc. Please justify your comments in great detail. If you think the paper is not novel, explain why and give a reference to prior work. If you think there is an error in the paper, explain in detail why it is an error. If you think the experimental evaluation is insufficient, remember that theoretical results/ideas are essential to CVPR and that a theoretical paper need not have experiments. It is not okay to reject a paper because it did not outperform other existing algorithms, especially if the theory is novel and interesting. It is also not reasonable to ask for comparisons with unpublished papers and papers published after the CVPR deadline.

reward).
Or in l. 232 a 'primary discretization' is mentioned ; is there a secondary discretization?
Similarily, in l. 364-367 the authors use boldface to say what they could do (but do not actually do - so this is still a wish list for the future).

The figure placement and captions should be improved, too.

Figure 1 is entirely out of place, appearing two pages before the text that explains it. The notation in the figure is not fully explained: it is not clarified that the horizontal axis is H(Y;H_S) - the distinction between B and B_s is missing - and the connection between the symbols in the text (reward function - entropy - cost is not made explicit).

Similarily, for Figure 2 it is not clarified what the width of the arrows stands for (I guess probability of action?); how - and if- the input x is associated with the thickness of the arrows - and what 'budget cut' stands for. I can understand after some thought these things, but the authors should invest a bit more effort in hand-holding.

Finally, for Figures 3/4 the authors do not indicate what the vertical axes of their plots stand for, leaving it to the reader to guess.

When introducing \phi (l. 384) the authors should tell the reader that they will detail it below (or else the reader is lost).
The authors could use different notation for \phi(s) and \phi(s,a), or else explicitly state that they are overloading notation.

l. 459-461: this is a rather awkward description for an algorithm.

l. 494: some disclaimer should be added here about the viability of the Gaussian distribution assumption.

l. 508-519: I did not get this part at all. Presumably you cluster classifiers based on the observed features. But these change dynamically, based on the current state. So my guess is that there are hundreds of observability patterns throughout, so how can you do this clustering into 'K' classes? It turns out this does not buy you much after all, so maybe the authors could omit this altogether?

l. 608-610: how exactly do you perform this? Some additional technical details would be needed here, the authors should not assume the reader knows [6] by heart.

In particular, there are small spelling/syntax errors throughout the text:
l. 30: to-> we
l. 43: a view - > a few
l. 48: contribution -> contributions
l. 49: budged -> budget
l. 50-51: SOTA methods tend to be expensive: which ones?
l. 161 He He -> He
l. 230: the the -> the
l. 309 amount -> the amount
l. 571: identify classify -> classify
l. 627: leads -> lead
l. 622: syntax derails

Preliminary Rating. Please rate the paper according to the following choices. Oral: these are papers whose quality is in the top 10% of the papers at CVPR. Examples include a theoretical breakthrough with no experiments; an interesting solution to a new problem; a novel solution to an existing problem with solid experiments; or an incremental paper that leads to dramatic improvements in performance. Oral/Poster: these are very strong papers, which may have one weakness that makes you unsure as to whether they should be oral or poster. Poster: these are strong papers, which have more than one weakness. For example, a well-written paper with solid experiments, but incremental; a paper on a well studied problem with solid theory, but weak experiments; or a novel paper with good experiments, but poorly written. Weak Reject: these are papers	Oral/Poster

that have some promise, but they would be better off by being revised and resubmitted. Strong Reject: these are papers that have major flaws, or have been done before.	
Preliminary Evaluation. Please indicate to the AC, your fellow reviewers, and the authors your current opinion on the paper. Please summarize the key things you would like the authors to include in their rebuttals to facilitate your decision making. There is no need to summarize the paper or reviews. If you have additional concerns that were not included in the reviews, please be sure and include them as well.	I enjoyed reading this paper: I found it very timely and genuine. The authors could improve their presentation, and should comment on the convergence of their RL algorithm (namely how many epochs of training it takes, how sensitive it is to the cooling schedule, whether they have tried it with different initializations to see whether the quality of the policy varies based on initialization etc). Such an analysis would be needed to assess whether their MDP-based algorithm requires lots of tuning and hacking, or whether it can easily be replicated by other users. Sharing the code would also be great.
Confidence. Write “Very Confident" to stress that you are absolutely sure about your conclusions (e.g., you are an expert who works in the paper's area), “Confident” to stress that you are mostly sure about your conclusions (e.g., you are not an expert but can distinguish good work from bad work in that	Confident.

area), and “Not Confident” to stress that that you feel some doubt about your conclusions. In the latter case, please provide details.	
Final Recommendation. After reading the author's rebuttal and the discussion, please explain your final recommendation. Your explanation will be of highest importance for making acceptance decisions and for deciding between posters and orals. Include suggestions for improvement and publication alternatives, if appropriate.	This was a really nice paper - I could recommend this for an oral, but the authors should cut down on pompous statements and put some effort in righting the paper more clearly, as indicated by all reviewers.
Final Rating. After reading the author's rebuttal, please rate the paper according to the following choices.	Oral/Poster

Masked Reviewer ID: Assigned_Reviewer_9

Review:

Question	
Paper Summary. Please summarize in your own words what the paper is about.	This paper proposes an anytime classification method that dynamically selects features to meet budget while achieving a decent accuracy, given costs of features. It uses a MDP framework to dynamically choose features. In this framework, states are subsets of features; adding features (and thus making transition between states) receives rewards in terms of information gain balanced by feature cost. The goal is to minimize cost-sensitive loss over time.
Paper Strengths. Please discuss the positive aspects of the paper. Be	

sure to comment on the paper's novelty, technical correctness, clarity and experimental evaluation. Notice that different papers may need different levels of evaluation: a theoretical paper may need no experiments, while a paper presenting a new approach to a known problem may require thorough comparisons to existing methods. Also, please make sure to justify your comments in great detail. For example, if you think the paper is novel, not only say so, but also explain in detail why you think this is the case.	The paper addresses an interesting problem that is relevant for many researchers in the community. Additionally the paper is well written, especially when introducing the MDP framework, which is useful for other vision researchers, and the paper cites many relevant and recent work.
Paper Weaknesses. Please discuss the negative aspects of the paper: lack of novelty or clarity, technical errors, insufficient experimental evaluation, etc. Please justify your comments in great detail. If you think the paper is not novel, explain why and give a reference to prior work. If you think there is an error in the paper,	My first main concern is the similarity to the previous work of Benbouzid 2012 [1]. In the related work section, the paper dismisses [1] as "simply extends the traditional sequential boosted classifier with an additional skip action." (l.187-189) However, I found [1] to not be doing that at all. Rather, it is learning a policy of weak learners to execute, where the state is the current estimate of the prediction and the action selects which weak/base classifier to run next. That is, Eq.2 in [1] is not simply enumerated j=1...N where certain weak/base classifiers are skipped. Hence, both works are dealing with learning a policy under budget constraints, with an advantage given to [1] without having to deal with unobserved feature imputation. I would like a more thorough description of the pros/cons between these two works. My next concerns are with the experiments. Although the paper claims the performance of proposed approach matches with Active Classification and exceeds the results given by Greedy Miser, the reported metrics are different across the papers. The performance metric "area under the error versus cost" used in this paper is different from test classification accuracy used in Active Classification, and is different from the metric NDCG (Normalized Discounted Cumulative Gain) used by Greedy Miser. Since the paper doesn't describe the relationship between the metrics, it is had to tell

explain in detail why it is an error. If you think the experimental evaluation is insufficient, remember that theoretical results/ideas are essential to CVPR and that a theoretical paper need not have experiments. It is not okay to reject a paper because it did not outperform other existing algorithms, especially if the theory is novel and interesting. It is also not reasonable to ask for comparisons with unpublished papers and papers published after the CVPR deadline.	which method is better. Additionally, it would be nice to have a direct comparison. To deal with missing features, the paper designs a classification mechanism that can take any subset of features and output label probability. This is achieved by logistic regression with proper feature imputation. This is a nice piece of work. However, it seems that in the experiments instead trains a separate multi-class SVM on each feature channel. Please provide clarification/explanation. In the ImageNet 65-class experiment, the paper only reports the proposed work's own performance after combining a previous approach "Hedging Your Best" without any detailed description. Since the stage-wise comparisons are not shown, it is not clear whether the proposed approach or the previous work plays a key role. This paper would be much better if it compares with previous approaches in the same metric. For example, things could be much easier if the paper shows for different methods, how test accuracy changes with feature cost. This is an intuitive and broad metric on which all the cost-sensitive approaches can show their strength and weakness. Also, providing more details for the experiments (e.g., how each step of the method works) would be useful if replication. Some figures are quite confusing. E.g., In Fig. 1, the yellowish region to the right of the piecewise curve is the reward I_Hs(Y;h_f)(B_s - .5 c_f). This region is very hard to see. At a glance I thought the gray area is the reward. In Fig.3 and Fig.5 (the result figures), I cannot read the legend of figures unless zooming in significantly.
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performance. Oral/Poster: these are very strong papers, which may have one weakness that makes you unsure as to whether they should be oral or poster. Poster: these are strong papers, which have more than one weakness. For example, a well-written paper with solid experiments, but incremental; a paper on a well studied problem with solid theory, but weak experiments; or a novel paper with good experiments, but poorly written. Weak Reject: these are papers that have some promise, but they would be better off by being revised and resubmitted. Strong Reject: these are papers that have major flaws, or have been done before.	Weak Reject
Preliminary Evaluation. Please indicate to the AC, your fellow reviewers, and the authors your current opinion on the paper. Please summarize the key things you would like the authors to include in their rebuttals to facilitate your	The main concerns are: 1) Key differences between this work and [1], which to me are formulated and solved very similarly, but [1] being wrongfully dismissed.

decision making. There is no need to summarize the paper or reviews. If you have additional concerns that were not included in the reviews, please be sure and include them as well.	2) It is not immediately clear that experimental comparisons are fair. And, of course, the relationship with [1].
Confidence. Write “Very Confident" to stress that you are absolutely sure about your conclusions (e.g., you are an expert who works in the paper's area), “Confident” to stress that you are mostly sure about your conclusions (e.g., you are not an expert but can distinguish good work from bad work in that area), and “Not Confident” to stress that that you feel some doubt about your conclusions. In the latter case, please provide details.	Confident
Final Recommendation. After reading the author's rebuttal and the discussion, please explain your final recommendation. Your explanation will be of highest importance for making acceptance decisions and for deciding between posters and orals.	After discussions, I retract my concerns with Benbouzid[1] and agree with the authors/rebuttal (and AC); though, it would be great to see comparisons (as AR10 also notes). Given the enthusiastic interest from the other reviewers, it seems the paper would be well received at the conference.

Include suggestions for improvement and publication alternatives, if appropriate.	
Final Rating. After reading the author's rebuttal, please rate the paper according to the following choices.	Poster