View Reviews

Paper ID
6433

Paper Title
A Lighting-Invariant Point Processor for Shading

Reviewer #1

Questions

1. [Summary] In 3-5 sentences, describe the key ideas, experiments, and their significance.
This paper presents a (mostly) theoretical study of a new illumination-invariant point processor. The main idea is to restrict the number of compatible shapes locally and to use the image value at a particular location to generate such a compatible shape set (the shapes themselves are quadratic surface patches). The authors define a coupled pair of neural networks to approximate this shape set. Finally, they describe two potential applications of the presented methods/theory.

2. [Strengths] What are the strengths of the paper? Clearly explain why these aspects of the paper are valuable.
1. The paper is written well.
2. The ideas appear novel.
3. While outside of my main area of expertise, the described problem is interesting and should be of interest to many at CVPR.

3. [Weaknesses] What are the weaknesses of the paper? Clearly explain why these aspects of the paper are weak. Please make the comments very concrete based on facts (e.g. list relevant citations if you feel the ideas are not novel).
1. For a nonexpert in this specific area, the paper was at times difficult to read. I think a bit more intuition should be incorporated into the writing to ensure that the paper is more accessible to a broad audience.
2. While I understand that this is a preliminary study, there were very few practical results.

4. [Overall rating] Paper rating (pre-rebuttal)
Borderline

5. [Justification of rating] Please explain how the strengths and weaknesses aforementioned were weighed in for the rating. Please also mention what you expect to see from the rebuttal that may change your rating.
While the paper was clearly written, as a nonexpert in this area, I found it difficult to read at times due to lack of explanations of various terminology/concepts. Also, while this is primarily a theoretical study, there were very few practical examples.

Reviewer #2

Questions

1. [Summary] In 3-5 sentences, describe the key ideas, experiments, and their significance.
This paper addresses the problem of recovering shape under unknown lighting. Unlike conventional shape-from-shading approaches, the proposed method makes some simple assumptions of the local surface about a point and uses the Lambertian model to characterise the set of possible shape solutions per point. A neural network is trained to estimate weights in a parametric model that helps resolve the ambiguity of shape inherent to the

photometric model developed in the paper. Example applications are provided.

This is an elegant paper that tackles the difficult ambiguity of shape-from-shading my a careful set of surface assumptions and a learnt model for resolving residual ambiguities. As a result, the paper provides a fruitful way of combining known physical models in vision problems with learning methods.

2. [Strengths] What are the strengths of the paper? Clearly explain why these aspects of the paper are valuable.
I particularly like the paper for its careful choices in marrying known physics that governs surface illumination models and neural networks to disambiguate the residual ambiguity present after the modeling assumptions are applied. Apart from proving an important solution to the difficult 'shape-from-shading' type scenario (the paper does not exactly address the classic problem of using a single image), this paper is an important reminder to the vision community in that the recent breakthroughs in deep learning can be made more fruitful if one can figure out ways of moving beyond regression that does not incorporate knowledge of the problem.

Instead of working with very general assumptions, the paper makes some simple assumptions on the surface shape (only upto second-order derivatives). This is a very useful assumption as the paper subsequently shows how such a "quadratic" surface assumption (per point) ramifies through the model.

3. [Weaknesses] What are the weaknesses of the paper? Clearly explain why these aspects of the paper are weak. Please make the comments very concrete based on facts (e.g. list relevant citations if you feel the ideas are not novel).
While I like the paper a lot, I feel that the authors have some loose ends to tie up in their exposition. Firstly, there are number of ideas (algebraic varieties, automorphisms, implicit function theorem) that many in the vision community are unfamiliar with. There needs to be a big picture statement in the beginning of the paper that will walk the reader through the thicket of individual steps that follow. Also how are the polynomials in Eqns 3-5 arrived at ? They are stated and proved in a somewhat mysterious form.

There is much ambiguity in the description of the neural network training (this is a typical problem with most deep learning papers). For instance in Sec 4.2, we are told nothing about the nature of the "ground truth" lighting samples used in training. Obviously the generalisation capability of the trained network will depend on the nature of the distribution of L*. More importantly in line 609, the authors use the term "light source direction L*". From the assumption used (L* is in R^3), it is obvious that L* should admit any general lighting function, including a sum of point-sources-at-infinity and beyond. So is the network trained and tested on single direction light sources L* ? If yes, then how well can we expect the networks weight estimate to generalise. Additionally, why is the surface patch used for training f* (lines 609-610) quadratic alone. Wouldn't it be useful to test as to what happens when the input data has some higher-order derivatives in the surface ? Admittedly, your model admits only quadratic forms, but it would be important to test for the stability of the method when the data violates the assumptions.

4. [Overall rating] Paper rating (pre-rebuttal)
Weak accept

5. [Justification of rating] Please explain how the strengths and weaknesses aforementioned were weighed in for the rating. Please also mention what you expect to see from the rebuttal that may change your rating.
I like the paper and believe that it makes important contributions. In particular I like the careful combination of physical models and neural networks. I would revisit my rating based on the rebuttal feedback.

6. [Detailed comments] Additional comments regarding the paper (e.g. typos, any suggestions to make the submission stronger).
Looking beyond the current submission, consider the following thought exercise. Instead of using continuous representations of (f_x,f_y), say we use a set of discrete normals sampled from the unit sphere surface. Then the method would provide weights (maybe just 0,1 selection weights) for which of these discrete normals are admissible pointwise. These can then be input to an MRF to develop a "global" surface estimate that can incorporate other assumptions such as smoothness, discontinuities etc.

Reviewer #3

Questions

1. [Summary] In 3-5 sentences, describe the key ideas, experiments, and their significance.
This paper presents a new theoretical view of the shape-from-shading problem. The key insight is a method for transforming local intensity and derivatives into a space of possible local surfaces such that the space always contains the correct surface regardless of the illumination. This theoretical idea is rigorously derived and tested on some basic synthetic test cases.

2. [Strengths] What are the strengths of the paper? Clearly explain why these aspects of the paper are valuable.
This is a really interesting read with a new take on a classical problem. I really like the idea of an illumination invariant mapping from local intensity/intensity derivatives to a set of candidate local surfaces - the "point processor" - and the fact that this mapping can be approximated by a neural network.

The development of the idea is well motivated and clear. The notations are generally readable and I followed the main theoretical parts of the paper. All derivations are given in full, enhancing the rigour of the work.

3. [Weaknesses] What are the weaknesses of the paper? Clearly explain why these aspects of the paper are weak. Please make the comments very concrete based on facts (e.g. list relevant citations if you feel the ideas are not novel).
The 2-jet representation requires second derivatives of intensity. This is presumably highly sensitive to noise. Since there are no results on real images and no discussion about noise, we are left wondering if the approach really could have any practical value. In the rebuttal, I wonder if some comments could be made on this or discussion of whether any preliminary experiments were tried along these lines.

Obviously, the biggest weakness is the theoretical focus of the paper. If there was also a fully worked out, robust algorithm and an evaluation on real data, this would be an outstanding paper. As it is, it's really a "theoretical teaser" for a new direction that will need taking forward before we know if it's really useful.

4. [Overall rating] Paper rating (pre-rebuttal)
Weak accept

5. [Justification of rating] Please explain how the strengths and weaknesses aforementioned were weighed in for the rating. Please also mention what you expect to see from the rebuttal that may change your rating.
I think the theoretical developments presented are interesting and significant enough to outweigh the lack of a developed robust algorithm or application to real data. Other reviewers may well disagree with me and this is not a fashionable sort of paper to accept to CVPR but I believe a healthy conference should encourage a diversity of types of work. The ideas in this paper are begging to be picked up by someone else and integrated into a working algorithm and presentation at CVPR might enable this to happen. I do have a concern that the presentation doesn't do enough to make the work accessible. There are a lot of exhaustive mathematical derivations shown without any intermediate explanation and I found some of this hard to follow. Maybe some of this could be moved to the supplementary material so that more of the intuition or practical implementation details could be in the paper. Overall, I am excited by the direction of the work.