

# View Reviews

## Paper ID

1539

## Paper Title

Non-appearance-preserving Image Smoothing

## Reviewer #1

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### Questions

#### 1. [Summary] In 3-5 sentences, describe the key ideas, experiments, and their significance.

This paper proposes a novel image smoothing framework which can be easily combined with multiple related state-of-the-art methods to enhance their performance in a variety of computer vision applications for image decomposition and editing. It indicates that directly preserving appearances would make the decomposition task inadequate, so it does not import strict constraints on appearance preservation, but instead, a few supporting anchors are selected adaptively to approximate image structure. This facilitates the decoupling of image content.

#### 2. [Strengths] What are the strengths of the paper? Clearly explain why these aspects of the paper are valuable.

The authors provide a reasonable framework to acquire supporting points and the observation of non-appearance preserving smoothing is interesting.

Generally, the proposed algorithm is easy to follow and use. While the theory is simple, the validation is solid.

The paper is well organized and motivated.

A lot of applications are shown and the results look impressive and time cost is reasonable.

#### 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 explanation of "using a limited set of supporting positions" to "approximates source structure" is not intuitive enough. Why choose points instead of other structures? More descriptions should be given.

The assumption that appearance term contains structure information is still heuristic to me, illustrated only using Fig. 1. Data statistic is preferred and would make this work more convincing.

The paper mainly compares the texture removal and decomposition results with L0 and L1. Can the related work "Hojin Cho, et al. Bilateral texture filtering, TOG2014 [8]" be adopted into the framework? Additionally, I think the authors should compare with it on texture removal task, if possible w/o NIS.

What is the limitation of this proposed framework? Would the increase of supporting point size apply a negative impact to the decoupling results?

#### 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.

The proposed framework is effective and simple. It can meliorate multiple current image smoothing methods without big modification and shows its benefit on a variety of applications. I thus lean towards an accept to this work.

## 11. Final rating

Weak accept

## 12. Explanation of final rating. Describe the rationale for your final rating, including notes based on the rebuttal, discussion, and other reviews.

I think the motivation for this paper is intuitive and effective. It is happy to see an easy framework but with useful functionalities. I think the authors should modify their claims and emphasize their core contributions. It is not a big problem of how hard the contributions are.

## Reviewer #2

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### Questions

#### 1. [Summary] In 3-5 sentences, describe the key ideas, experiments, and their significance.

This work deals with the problem of optimization-based image smoothing. The authors propose to replace the classic appearance-preserving fidelity term by sparse control points computed using 0-1 knapsack. The potential of this approach is demonstrated on numerous image smoothing applications e.g., structure-texture decomposition, intrinsic image decomposition, specular removal, etc.

#### 2. [Strengths] What are the strengths of the paper? Clearly explain why these aspects of the paper are valuable.

- Nicely written, clear paper which is easy to follow.
- Very solid experimental evaluation.
- General framework, applicable to a wide variety of image processing problems.

#### 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).

- Idea is not really novel, references towards appropriate mathematical imaging literature lacking.
- Organisation of the paper is very unbalanced, most of the content being experiments.
- Somehow superficial mathematical treatment of the problem.

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

Weak reject

#### 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.

This is overall a good application paper which is interesting to read. It is pleasant to observe the wide variety of applications of the proposed non-appearance-preserving fidelity term, and the very strong evaluation is convincing. Nevertheless, the core idea of the paper - replacing a global quadratic data term by a quadratic term on sparse control points - is, unlike what is announced, not novel. Therefore, the paper essentially comes down to evaluating (in an extremely solid way, though) this known approach on a wide variety of problems. Such a solid experimental evaluation is valuable as such, yet in view of the lack of fundamental novelty I cannot recommend acceptance.

#### 6. [Detailed comments] Additional comments regarding the paper (e.g. typos, any suggestions to make the submission stronger).

1- My major concern regarding this work lies in what is announced as the core contribution: replacing quadratic appearance preserving of the full image domain ( $\|X-Y\|_{L2}^2$  in Eq. (1)) by quadratic distance to some control points ( $\sum_{i \in P} \|X_i - Y_i\|_2^2$ ). This idea has long been proposed in many mathematical imaging works. For instance, for diffusion-based image compression [Galic et al., Image Compression with Anisotropic Diffusion, JMIV, 2008]. There also exist advanced mathematical tools for finding the sparse control points, see e.g. [Hoeltgen et al., An Optimal Control Approach to Find Sparse Data for Laplace Interpolation, EMMCVPR 2013] and related papers. One may argue that such works provide much stronger theoretical insights than the proposed 0-1 knapsack solution, which remains somehow heuristic and would fail for instance in the presence of noise (such a discussion on limitations of the method is by the way missing).

2- Therefore, the added value of the current paper lies in showing that such sparse fidelity terms can be applied for many image processing tasks. This impression of a work biased towards experimental evaluation is confirmed by the relative importance of the contribution wrt to the rest of the paper. The paper starts with 2 pages of introductory discussion (which contains some flaws, see next item), and ends with 5 pages of experimental evaluation. This leaves only a single page for the presentation of the actual contribution !

3- The authors state that researchers in the field have widely studied regularization terms, but all optimization-based approaches rely on a quadratic appearance preserving term on the full image domain. This is not correct, and an appropriate coverage of the state-of-the-art regarding the choice of the fidelity term must definitely be incorporated. Optimization-based approaches can be viewed as the MAP solution to image smoothing. Therein, the fidelity term is derived from the likelihood. And, likelihood is deduced from the knowledge of the actual physics involved in the acquisition of data. Obviously, if data is close to ground truth with negligible noise, noise can be modeled as a zero-mean Gaussian process and one indeed gets the usual quadratic term on the full domain. But, a lot of research has been devoted to exploring other fidelity terms. For instance, in CT reconstruction one shows from the physics that the correct fidelity term is Poisson-based (see, e.g., the book [Buzug, Computerized Tomography, Springer]). Or, impulsive noise (e.g., salt-and-pepper or other sparse corruptions) is often considered Laplacian, and one gets the L1 fidelity term for which various solvers exist, see e.g. [Yang et al., An efficient TVL1 algorithm for deblurring multichannel images corrupted by impulsive noise, SIAM JSC, 2009]. One could also mention historical approaches which do not even include any fidelity term, as for instance diffusion methods. See the book [J. Weickert, Anisotropic diffusion in Image processing, 1998].

5- The mathematical treatment of the problem is deceiving, and some equations do not make sense. (1,2,3,...) involve terms like  $\sum ||X-Y||$ : should be something like  $\sum_{i} ||X_i-Y_i||$  for this to make sense. Contribution 1 states Cauchy-Schwarz inequality, but (4) is directly deduced from the optimality condition associated with (3): there is no Cauchy-Schwarz inequality here.

6- Some additional discussion on the claims are expected. For instance, the weight (9) will fail to appropriately detect the control points in the presence of noise in the input image. Thus, the approach cannot be used for the most classic image smoothing problem: image denoising. Stating the assumptions on the inputs is thus necessary. Regarding the discussion in 1.2 and Figure 1, one may also argue that using an I0 (or even I1) regularizer on the gradient, one would get exactly the same solution as the proposed one. So, this discussion is very incomplete and does not show that "it is impossible for appearance-preserving smoothing methods to completely remove or decompose desired patterns" (contribution 1).

## 11. Final rating

Weak accept

## 12. Explanation of final rating. Describe the rationale for your final rating, including notes based on the rebuttal, discussion, and other reviews.

In view of the other reviewers' comments, I change my rating to weak accept. I still believe that the paper contains a limited amount of fundamental novelty, and the rebuttal only partly addressed this important issue. However, the effectiveness of the approach, demonstrated on a wide variety of applications, is of interest for the community.

## Reviewer #3

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## Questions

### 1. [Summary] In 3-5 sentences, describe the key ideas, experiments, and their significance.

This paper presents a Non-appearance-preserving Image Smoothing (NIS) technique that does not strictly require the preservation of the source image. To this end, a 0-1 Knapsacks problem is formulated to find a set of supporting positions for structure approximation. Experiments demonstrate that existing image smoothing

methods can be meliorated under the proposed NIS framework, and the NIS approach can help boost various image manipulation tasks such as texture removal and intrinsic decomposition.

**2. [Strengths] What are the strengths of the paper? Clearly explain why these aspects of the paper are valuable.**

The paper is well-written and easy to read. The proposed NIS framework can not only enhance the performance of existing image smoothing methods, but also benefit various image manipulation tasks.

**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 problem and motivation part in section 1.2 confused me. It seems that the perfect energy term assumes the known of the structure  $\omega$ . So, why you design the quadratic energy function in Eq. (3) when structure  $\omega$  is already known? In my opinion, the problem and motivation section is misleading, since the structure  $\omega$  is a unknown variable and we will never be able to judge whether the underlying structure  $\omega$  is perfectly reconstructed. Hence, this section fails to validate the significance of non-appearance-preserving image smoothing.

I guess that the effectiveness of the proposed NIS approach mainly comes from the iterative implementation of specific image smoothing method. In fact, iteratively implementing any existing image smoothing methods (e.g. L0 gradient minimization, RTV, and WLS) would achieve similar results, since the iteration process implicitly perform a supporting position selection by smoothing out other non-prominent constituents. The proposed 0-1 knapsack inherently maintains the most salient constituents indicated by previous image smoothing operation and abandons other less salient constituents. This indeed somewhat improves the performance, but at the cost of introduction a core parameter  $u$ , and some other parameters  $l_p$ ,  $t$  and  $\sigma$ . It is foreseeable that the adjustment of  $u$  would be challenging, since it is difficult to determine the amount of salient constituents to preserve (indicated by  $u$ ) in each iteration.

According to above analysis, the comparisons to existing image smoothing methods are unfair. For instance, Figure 3 compares six existing image smoothing methods and its NIS versions. This is unreasonable, since the NIS versions iteratively implement the compared image smoothing methods, while the compared methods only implement once. To achieve fair comparison, the compared methods should also undergo the same number of iterative implementations.

**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.**

Image smoothing is a fundamental technique applicable to various image manipulation tasks. This paper introduces a new perspective for improving the performance of image smoothing. Various applications are conducted and the results are impressive. However, the effectiveness and contributions of the proposed NIS approach are not adequately analyzed and stated.

**6. [Detailed comments] Additional comments regarding the paper (e.g. typos, any suggestions to make the submission stronger).**

I would like to see more detailed illustration of the 0-1 Knapsack formulation. In its current version, it is not clearly analyzed why the item value and weight are designed as them in Eq. (8) and (9). In addition, I would like to see ablation study that validates the effectiveness of the designed item value and weight.

**11. Final rating**

Borderline

**12. Explanation of final rating. Describe the rationale for your final rating, including notes based on the rebuttal, discussion, and other reviews.**

I would like to keep my initial rating, since my concerns on the method's effectiveness was not completely addressed by the rebuttal.

