View Reviews

Paper ID
2576

Paper Title
C2N: Practical Generative Noise Modeling for Real-World Denoising

Reviewer #1

Questions

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

This paper proposes a method for realistic noise generation for image denoising. The proposed noise generator consists of different layers designed specifically for simulating signal-independent, signal-dependent, and spatially-correlated noises. The generator is trained with unpaired clean-noisy images. Then, the generator is used to generate paired clean-noisy images for training image denoisers. The visual results show good performance in simulating real noise and improving image denoising.

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

- The proposed noise simulation model with unpaired clean-noisy images seems to be novel.
- The results show fair performance in terms of visual characteristics of simulated noise.
- Training image denoisers with the simulated clean-noisy image pairs seems to improve the denoising performance of such denoisers.

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 paper lacks in some aspect including experimental validation, as discussed next.

- It is unclear how to evaluate the noise samples generated by the proposed model? Why did not the authors use metrics adapted in related works, such as the KL divergence, to compare the generated samples to the real ones? The visual results are okay; but they look visually different from real samples, especially in Figures 1 and 5.

- The major contribution of this paper the noise generator. Why wasn’t the proposed generator compared to existing ones, such as noise flow, GCBD, GAN2GAN, etc.?

- It is unclear how noise level/strength is controlled while training the proposed noise generator? The assumption here is that a real dataset would contain various noise levels, and hence, this should be taken into account while training the noise generator. Is this variation in noise level being learned implicitly? If yes, then how to validate it?

* Other remarks

- The adapted noise model throughout the paper, and in Equation 2, is a heteroscedastic Gaussian noise model, not a Poisson-Gaussian noise model. There is no Poisson distribution in equation 2.

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.

The proposed noise generator has some novelty. The visual results show good imitation of real noise. The simulated noise can improve denoising performance. However, there is a sever lack in the experimental validation. The proposed noise generator is not compared to any existing noise generators. The evaluation of simulated noise is based on visual inspection only, there is no quantitative/statistical evaluation.

Reviewer #2

Questions

1. [Summary] In 3-5 sentences, describe the key ideas, experiments, and their significance.
The authors propose a real noise generator based on the GAN framework. The noise generator includes signal-dependent and signal-independent components. Sampled random values and the input image are feed into the noise generator network. The noise generator network is learned with the GAN framework.
Then, the blind-denoising network is trained with the noisy images generated by the noise generator.
They experimentally demonstrate that the proposed denoising framework outperforms existing algorithms.

2. [Strengths] What are the strengths of the paper? Clearly explain why these aspects of the paper are valuable.
They use the GAN framework so that they can train the noise generator with unpaired image data.
The proposed noise generator explicitly involves the signal-dependent and signal-independent components.
The stabilizing loss to avoid the color shift looks important.

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).
My concerns are (1) robustness against noise level and camera model, (2) learning procedure, and (3) ablation studies.

(1) robustness against noise level and camera model
I think the authors train the noise generator for a specific camera with a specific noise level. Even if they trained with some noise level and some camera types, they missed the discussion of the robustness against the noise level and different camera model.

(2) learning procedure
It is unclear the learning procedure of the feature extractor in Fig. 3. I could not get the information on the distribution of r map.
The signal-dependent noise component includes the sampling process. Then, I wonder how the feature extractor is trained.

(3) ablation studies.
One of the key components of the proposed noise generator is the signal-dependent noise component.
However, the ablation studies of Table 2 did not include clearly to validate the effect of the signal-dependent noise component. It may include the algorithm without G^{I} and G^{I}_{3x3} and the algorithm without G^{D} and G^{D}_{3x3}.

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.

The GAN-based noise generation approach is positive. However, as mentioned in the introduction, the generation-based approach is not the originality of this paper. One of the key components of the proposed approach is the signal-dependent noise component. However, ablation study and clarity of the learning procedure of that part are insufficient.

Reviewer #3

Questions

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

The paper presents an unsupervised-learning denoising approach that relies on a neural network to generate real-world noise. Then, a denoising network learns to remove the generated noise from the image. The generator network is trained adversarially based also on real noisy images, while the denoising network relies on the generator's supervision. The evaluation of the approach to the SIDD database shows better performance than the prior work.

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 follow. The related work is complete. Also, the method is well-explained.

+ The approach is evaluated on a standard large-scale benchmark where it shows promising results.

+ The approach shows better results than supervised learning.

+ The proposed generator network integrates signal-independent pixel-level transforms.

+ The ablation study clearly presents the performance of all model components.

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).

- Related work: The following papers could be also discussed:
- Unprocessing images for learned raw denoising.
- When awgn-based denoiser meets real noises.

- Table 3 does not show the results of DIDN only. They would be useful for comparing with the C2N + DIDN.

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

Strong 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.

Overall the paper presents a novel generator network to synthesize noise. The idea is novel and delivers promising results on the SIDD dataset. The paper is already in a very good status to be accepted. There are a few minor points to be addressed in the final version.