Rotation Invariant Shape Contexts based on Feature-space Fourier Transformation

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➢ What's the paper's content?

This paper is about a novel shape descriptor using Fast Fourier Transformation (FFT). It is a minor improvement on the commonly used shape contexts descriptor. In this paper, the authors first address the properties of shape contexts. The advantages and drawbacks of shape contexts are discussed briefly. One of the problems is that in shape contexts, the rotation invariance depends on the tangent at every boundary point, which makes the method unstable to outliers and error-prone perceptual organization of the boundary points. To overcome this, the authors use FFT to obtain a rotation-invariant shape contexts descriptor and give proof of invariance. Experiments are also given for confirmation of the new method and it is show that this improvement is robust to noisy points or outliers.

➢ What's the main contribution?

As mentioned above, the main contribution of the paper is the approach of using FFT in state-of-the-art shape contexts descriptor. The improvement makes the shape contexts more robust to noisy points by keeping rotation invariance and therefore solves the shift problem.

➢ Is the paper well motivated, i.e. are there sound justifications of the approach?

Although the improvement is minor, the paper is clear and well organized. The problems are given and analyzed solidly, and the improvement based on FFT is convincible, since FFT is a mature method and has very wide applications in all kinds of areas such as computer vision and signal processing.

➢ Is the paper technically sound?

Yes, I think so. As discussed above, it is using a simple and mature method (FFT) to solve existing problems in a widely used method.

➢ Are the references sufficient?

No, only three references. This is far not enough for a journal article (here I assume that *Pattern Recognition Letters* is a journal), even for conference papers. For three references, it is hard to believe that the authors have done a thorough survey in this topic. Although it shows somewhat satisfactory results in this paper, it lacks of most recent results in this field. Hence, as a reviewer, I cannot judge how much improvement, or to what extent, the authors have accomplished in their work since I have no idea about what the other researchers have done in the same topic. So the degree of the contribution remains a skeptical issue.

➢ Is the paper readable?

Yes, it is quite readable and easy to understand, since the idea is rather simple and straightforward, and the method is also mature and commonly used.

➢ Are the experiments and results convincing?

No, the experiments are so few. Only a Chinese character '福' is used and only three groups of experiments are given for validation. This is not enough. First, this character is rather simple, we may image that the results should be good because a very easy data set is used. So experiments on different data sets should be performed. Second, number of experiments is few. Three groups of experiments are not convincible. The authors should use different parameters and give different results based on these parameters with different data sets, to make some conclusion. Third, a thorough analysis is missed. Based on the above discussion, the authors may find some further discoveries and problems if more experiments are implemented.

- **Would you suggest publishing the paper? (Ranking: accept as is, major revision, minor revision, reject)**

My suggestion is rejection. As a 'reputable' magazine, it should include papers with novel ideas and somewhat big contribution. Although the idea of this paper is interesting, it lacks of thorough analysis and big improvement. The method is somewhat too straightforward and simple. Also, a lot of work can be done and the results are not convincible enough. Besides insufficient experimental results and references (related work) as mentioned above, I have several more comments:

1) The paper does not give any comparison between its method and state-of-the-art strategies. Therefore, the reviews may have difficulty in judging the work in this paper.
2) The authors mention several problems in traditional shape contexts in introduction and argue that the proposed method can solve them, but only one problem is solved at last.
3) There are three parameters in the method. The authors do not give any hints of setting the parameters. Hence the reviewers have no idea of whether there is a relationship between the performance and the parameters. A successful strategy or method should not be too dependent on parameters. Usually I reject a paper (if I were a reviewer) with more than five parameters in the method, without any explanation or analysis.
4) In Eq. (9), I think it should be sl_{ij} in the last item, rather than l_{ij}.