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Pattern Recognition Letters 27 (2006) 707–708
Pattern Recognition Letters
www.elsevier.com/locate/patrec

Editorial

Recent submissions in linear dimensionality reduction and face recognition

During the last few years the interest for linear dimensionality reduction (LDR) raised significantly in the pattern recognition domain. Especially the small sample size case (more dimensions than examples) received intense attention. Many papers are illustrated by examples and experiments on pixel-based image recognition, most of these specifically aim at face recognition, often using a public domain face image database. These papers can be roughly divided into the following groups:

1. Mathematical methodology of finding good subspaces: PCA yields spaces in which the data fit well, but does not use class information. LDA optimizes for linear separability between c classes, but finds in its plain form at most $c-1$ dimensions. There are papers discussing various compromises, e.g., de-correlation of the LDA features or the information content of the null-space. Some papers change the LDA criterion such that more features can be retrieved. Others discuss the relation with highly related approaches like the Foley–Sammon transformation, singular value decomposition, partial least-squares and canonical correlation analysis.
2. 2D-representation (in fact a special case of the first group). In the traditional representation the pixels of an $m \times n$ image are combined into a vector of length mn. This is partly the cause of the high dimensionality, which causes accuracy and computational problems. A new approach, related to an old idea, is the so-called 2DPCA technique in which the image is represented by an $m \times n$ matrix. This proposal generated a number of other papers discussing variants (e.g., 2DLDA), adding understanding or focusing on computational issues.
3. Image transformations. Before a PCA or LDA subspace is found some image transformations may be applied enhancing certain aspects (e.g., some frequency bands) or dealing with invariants like light conditions or face poses. As far as these preprocessing steps are linear, they may be combined with or integrated into linear feature extraction.
4. Computational issues. Speed and memory usage, both, during training as well testing, may be important, depending on the procedure, especially in using larger databases.

As these topics are partially correlated, new proposals for one of them often generate studies w.r.t. the consequences of issues in one of the other topics. This partially explains the exploding number of studies. Some of them are rather obvious and straightforward, others need more discussion and experiments.

Without doubt, a number of interesting studies and proposals have been published recently. However, many other papers have only a minor significance, or low citation value, as others have treated the same point. A pattern on these papers consists of the following: a set of mathematical derivations is presented which are largely similar between different papers, then the same benchmarking datasets are used for illustration, but often, unfortunately, only a small subset is used for each experiment, or a different way of preprocessing has been chosen, so that across the papers the results are not comparable. For the readers the situation might be very confusing as it will take them time and effort to find the material that is of importance to them.

The editorial boards of journals that are suddenly confronted by a heavily increased interest in a small area have to face serious problems. They need to find good, experienced, independent referees, that preferably have not participated in the hype, but are still able and willing to review a large number of submissions. Just fractions of these are of acceptable quality. However, every referee sees a limited set of submissions and may not be aware of many other related submissions. Often referees try to assist the authors to improve the papers, w.r.t. mathematics, language, structure of the paper, experimental evidence, etcetera. This is a lot of work, for the referee as well as the authors, but the result is still a series of highly overlapping, related papers that do not refer to each other as they are simultaneously in the process. The task of the editors

0167-8655/$ - see front matter
doi:10.1016/j.patrec.2006.01.002

who handle the topic is to supervise the situation and coordinate the decisions made for individual submissions.

In the specific case of LDR, the problem was detected at PRL at the end of 2004, after a number of papers were already published or being revised or reviewed. From that moment, we decided to have all papers on the topic handled from a central point and reviewed by a very small group of coordinated anonymous referees. In this group, it was observed that, although some submissions made good points, many were just discussing the topic without a clear message, and many others have heavily overlapping contents. Here we thank the referees cordially for their enormous efforts.

Our experience with papers on LDR suggests that it is worthwhile to reiterate what papers are desirable for publication in PRL:

(1) Papers must make a clear and significant contribution, not only in terms of improved accuracies, but also that from a conceptual point of view, important progress or insight should have been shown. In particular, it should not be a repetition of a known message in a slightly different terminology. The contribution should already be stated in the abstract and in a more elaborate form in the introduction, and be made comprehensible to anybody with basic training in pattern recognition.
(2) Known mathematical derivations should not be repeated. Instead, proper citations should be made to standard textbooks or classic, preferable older papers that have been in wide circulation. It is not the task of the referees to find such references for the authors.
(3) Experiments should show significant results, including error bars or reported standard deviations. They should be properly designed, with independent training and testing sets separated by, e.g., a holdout procedure or cross-validation. Fair comparisons should be made with existing methods. In particular, it is not good to compare different procedures for arbitrary or unmentioned choices of their parameters like retained dimension or variance. It is also a bad practice to report just the best result of a proposed procedure over a set of method parameters.
(4) When the papers claim to contribute to an area of application, such as face recognition (the claim being made by, e.g., using it as a key word or in the title), they should not report results only on a simple, small database like ORL, but should use larger ones that are believed to contain sufficient variations occurring in the real world. They should also discuss issues that may not have been addressed in the benchmarking database.

Submissions that do not fulfil these criteria will be rejected without a full review or on the basis of a single review. In the past, we tried to guide authors in publishing a good paper. In situations as described above, such detailed guidance is no more possible. We hope that the future authors will understand.

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