View Reviews

Paper ID	9
Paper Title	Scalable Searching and Ranking for Melodic Pattern Queries
Track Name	Papers

Reviewer #1

Questions

2. The title and abstract reflect the content of the paper.
Agree

3. The paper discusses, cites and compares with all relevant related work.
Disagree

4. The writing and language are clear and structured in a logical manner.
Agree

5. The references are well formatted.
Yes

6. The topic of the paper is relevant to the ISMIR community.
Agree

7. The content is scientifically correct.
Agree

8. The paper provides novel methods, findings or results.
Agree

9. The paper will have a large influence/impact on the future of the ISMIR community.
Agree

10. The paper provides all the necessary details or material to reproduce the results described in the paper.
Agree

11. The paper provides reusable insights (i.e. the capacity to gain an accurate and deep understanding). Such insights may go beyond the scope of the paper, domain or application, in order to build up consistent knowledge across the MIR community.
Agree

12. Please explain your assessment of reusable insights in the paper.
10 and 11 cannot be evaluated without access to same tools.

17. Main review and comments for the authors.
Clearly presentable, comprehensible, and well explained. Please describe intended repertory. Please consider including more encoded repertories--kernscores (c.2500 works), MuseData (c.1200 full scores), and MIDI (often not fully reliable but numbers run into six figures). Most MEI data is not available in open-source versions. MusicXML is derived from MuseData, with Humdrum (kern) as a basis for its timewise transformations. Myriad translations between all these formats exist. On scalability and two-factor searchs, please consult Sapp et al (ISMIR, 2004). On fuzziness and the minutiae of "normalization", please consult Müllensiefen (numerous dates). Also consider a pilot test of the ranking system. Ranking results do not always make intuitive sense.

Reviewer #2

Questions

2. The title and abstract reflect the content of the paper.

Agree

3. The paper discusses, cites and compares with all relevant related work.

Disagree

4. The writing and language are clear and structured in a logical manner.

Agree

5. The references are well formatted.

Yes

6. The topic of the paper is relevant to the ISMIR community.

Strongly agree

7. The content is scientifically correct.

Agree

8. The paper provides novel methods, findings or results.

Disagree

9. The paper will have a large influence/impact on the future of the ISMIR community.

Disagree

10. The paper provides all the necessary details or material to reproduce the results described in the paper.

Disagree

11. The paper provides reusable insights (i.e. the capacity to gain an accurate and deep understanding). Such insights may go beyond the scope of the paper, domain or application, in order to build up consistent knowledge across the MIR community.

Strongly disagree

12. Please explain your assessment of reusable insights in the paper.

The authors barely compare their approach with other existing methods. Furthermore, they do not evaluate their approach with respect to retrieval quality. (Instead, they evaluate runtime.) Overall, the paper provides no deep insights in the problem nor the benefits of their method.

17. Main review and comments for the authors.

The paper describes a symbolic music search engine. A search query is a monophonic melodic pattern and the database consists of symbolic encodings of polyphonic scores. The contribution is the implementation within the Elasticsearch framework, which is probably quite efficient (however, I cannot judge the efficiency, because no comparisons are made.) However, there are a couple of downsides for this paper, which are listed below.

1. The paper does not discuss its scope and limitations:

For example, in this symbolic music search scenario, a score is regarded as a collection of voices. A search query is a monophonic melodic pattern that is searched in each of the voices. As far as I understand, such an approach is intrinsically not capable of finding cross-voice melodies. For example, it won't be able to find the "Ricercar a 6 voci" by Anton Webern because the theme is split across several voices. It is okay, if such a case is outside of scope, but a scientific paper should critically discuss its scope.

2. Evaluation

The paper reports on experiments in terms of runtime and number of matching scores. However, there is no discussion if the matching scores are relevant at all. Neither a qualitative nor a quantitative evaluation of the retrieval quality is made. The paper's focus is more the implementation and not an improvement of melodic search algorithms, which is okay. However, the reader gets no feeling on how well the algorithms are performing. (It may be okay to have a lower retrieval quality for faster runtime, but that is not discussed at all!) For the runtime experiments, no comparisons are made.

3. Comparison with related work:

The paper mentions a bit of related work, but it does not really compare. For example, the classical ThemeFinder (20 years old!) is mentioned, but the reader should get a feeling, to what extent that is similar and how it is different to the proposed approach. Other, more recent related work is not discussed at all, e.g. [1–3].

4. Several smaller issues:

- The encoding of rhythm is unclear to me (8 means eight note, 3 means dotted half, 6 means dotted quarter). Maybe that is a standard encoding, but its name could be mentioned for the non-experts.

- The author's perform experiments with 4,950 polyphonic scores. However, they do not mention at all, which data set this is. If it is a self made data set, they should give at least some statistics (number of composers, average length of piece, etc).

- The mathematical notation is sloppy, e.g. the notation for the list of n-grams on page 4 (e.g. "n" is italic as subscript, but not in interval, etc.)

- The paper explains that it avoids costly music similarity search for whole database in the matching stage because of runtime issues. That is understandable. However, after filtering out the relevant documents, one could apply a more costly music similarity algorithms from the literature (occurrences or ranking stage). The paper explains not why that is not done.

[1] Julián Urbano, Juan Lloréns, Jorge Morato, Sonia Sánchez-Cuadrado: "Melodic Similarity through Shape Similarity", CMMR 2010. (Winner of the MIREX Symbolic Melodic Similarity task in 2010–2015!)

[2] Camélia Constantin, Cédric du Mouza, Zoé Faget, Philippe Rigaux: "The Melodic Signature Index for Fast Content-based Retrieval of Symbolic Scores", ISMIR 2011.

[3] Klaus Frieler, Frank Höger, Martin Pfleiderer, Simon Dixon: "Two Web Applications for Exploring Melodic Patterns in Jazz Solos", ISMIR 2018.

Reviewer #3

Questions

2. The title and abstract reflect the content of the paper.
Strongly agree

3. The paper discusses, cites and compares with all relevant related work.
Strongly agree

4. The writing and language are clear and structured in a logical manner.
Strongly agree

5. The references are well formatted.
Yes

6. The topic of the paper is relevant to the ISMIR community.
Strongly agree

7. The content is scientifically correct.
Strongly agree

8. The paper provides novel methods, findings or results.
Agree

9. The paper will have a large influence/impact on the future of the ISMIR community.

Agree

10. The paper provides all the necessary details or material to reproduce the results described in the paper.
Strongly agree

11. The paper provides reusable insights (i.e. the capacity to gain an accurate and deep understanding). Such insights may go beyond the scope of the paper, domain or application, in order to build up consistent knowledge across the MIR community.
Agree

12. Please explain your assessment of reusable insights in the paper.
I think the use of indexing in (music) content-based retrieval with search engines will (again) be a popular research topic.

17. Main review and comments for the authors.
I enjoyed reading this paper and found the topic interesting and related with ISMIR.

The structure of the paper and the language were easy to follow and the use of figures (definitions, algorithms, graphs) was very informative.

Pattern matching is not a new problem and n-gram sampling has been studied before so I think the main contribution of this work goes to standardization and scalability.

The availability of the system is a positive feature, and the utilisation of generic search frameworks is a sign of good software development.

I have some concerns about the implied certainty that all the scores have voicing information. Even notated voices sometimes do not correspond with musical voices (e.g. streaming). It is not clear how this information is obtained.

Although the task is described sufficiently for a 6 pages document,
I would be interested to see a closer relation with works from monophonic pattern matching and methodological issues. For example, voice transformation have similarities with viewpoint representation.

The results seem justified and since scalability is on focus, also appropriate.

TYPOS:
pitchs -> pitches (4§1)
gobal -> global (6.6§5)
'not yet found their in ...' , (7§3, last sentence)