

Peer Review

Review of: "An Information Geometry Approach to Analyzing Topic Evolution in Scientific Networks: From Physics to International Relations"

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Overview: This paper presents an innovative methodology for analyzing scientific topic evolution using information geometry and entropy-based metrics. The approach combines information theory with computational topology to study knowledge networks, offering a sophisticated framework for understanding how scientific concepts evolve and interact over time.

Major Strengths:

1. Theoretical Foundation:

- The methodology is well-grounded in information theory, particularly through the use of the variation of information (VI) metric.
- The mathematical framework is rigorous and clearly explained, with detailed equations and derivations.
- The approach successfully bridges information theory and network topology, providing a novel perspective on knowledge networks.

2. Methodological Innovation:

- The introduction of normalized velocity matrices offers a new way to quantify network dynamics.
- The geometric interpretation of topic volumes as simplicial complexes is particularly creative.
- The framework effectively captures both temporal evolution and structural relationships.

3. Practical Implementation:

- The study demonstrates practical application across different domains (physics and international relations).
- The methodology scales well, as shown by its application to both ArXiv (421,524 papers) and JSTOR (10,370 documents).
- The computational requirements are reasonable, with processing times under an hour on standard cloud infrastructure.

4. Validation and Results:

- Results are well-visualized through multiple figures showing different aspects of topic evolution.
- The findings about knowledge network compression and stabilization patterns are convincing.
- The quantification of institutional and national impacts provides valuable insights.

Areas for Improvement:

1. Methodological Limitations:

- The country-level analysis is limited to corresponding authors only, potentially missing important collaborative influences.
- The paper doesn't address potential biases in the dataset selection or the impact of database coverage.
- The choice of parameters ($n=5\%$, $b=0.98$) needs more justification and sensitivity analysis.

2. Validation Gaps:

- The paper would benefit from a comparison with existing bibliometric methods to demonstrate added value.
- There's limited discussion of potential failure cases or limitations of the approach.
- The robustness of the results across different time periods and disciplines needs more exploration.

3. Technical Clarity:

- Some mathematical notation could be more clearly defined, particularly in equations 6 and 7.
- The relationship between normalized and non-normalized metrics could be better explained.
- The algorithm for topic selection (mentioned but not fully detailed) should be more explicitly described.

4. Practical Applications:

- The paper could provide more concrete examples of how the findings can be applied by stakeholders.
- The computational requirements for larger-scale applications need more discussion.
- Guidelines for parameter selection in different contexts would be valuable.

Recommendations:

1. Essential Revisions:

- Include a more detailed discussion of methodological limitations and potential biases.
- Add a comparison with traditional bibliometric approaches.
- Provide complete details of the topic selection algorithm.
- Clarify the parameter selection process and its impact on results.

2. Suggested Improvements:

- Expand the validation across more disciplines and time periods.
- Add case studies showing practical applications of the findings.
- Include sensitivity analysis for key parameters.
- Discuss computational scalability for larger datasets.

3. Minor Points:

- Improve figure captions for clarity.
- Add a more detailed explanation of the notation used.
- Include a glossary of technical terms.
- Provide more context for the JSTOR dataset selection.

Overall Assessment: This is a strong paper that makes a significant contribution to the field of scientific knowledge analysis. The methodology is novel and well-founded, and the results are convincing. With the suggested revisions, particularly regarding validation and practical applications, this paper would be even more valuable to the research community.

Recommendation: Accept with Major Revisions

The paper shows great promise but needs additional work to address the identified limitations and strengthen its practical impact. The revisions suggested above would significantly enhance its contribution to the field.

Declarations

Potential competing interests: No potential competing interests to declare.