From: **C. Chui (Applied and Computational Harmonic Analysis)** EviseSupport@elsevier.com
Subject: Revision requested for ACHA_2019_11
Date: June 4, 2019 at 4:50 AM
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CC

Ref: ACHA_2019_11
Title: Fast Algorithms for the Multi-dimensional Jacobi Polynomial Transform
Journal: Applied and Computational Harmonic Analysis

Dear Professor Yang,

Thank you for submitting your manuscript to Applied and Computational Harmonic Analysis. We have received comments from reviewers on your manuscript. Your paper should become acceptable for publication pending suitable minor revision and modification of the article in light of the appended reviewer comments.

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Kind regards,

Dr. Chui
Editor in chief
Applied and Computational Harmonic Analysis

Comments from the editors and reviewers:
-Reviewer 1

- See the attachment.

-Reviewer 2

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The article describes an algorithm to compute the Jacobi coefficients of a function given samples at Gauss-Jacobi points as well as the observation that 2D and 3D Jacobi transforms are Kronecker products of the univariate Jacobi transform. I have high regard for the authors and their research contributions in computational orthogonal polynomials. Unfortunately, this paper is not reflecting on their talents.

NOVELTY. I am struggling to understand the significant novelty in the manuscript. Since Section 2 recaps existing material and sections 3.2 and 3.3 are doing Kronecker products of the 1D transform, I believe the only place to find significant novelty is in Section 3.1. In Section 3.1, the authors present a variant of the algorithm from an unpublished manuscript by Bremer and Yang in 2018 for the 1D Jacobi transform with an unquantified computational improvement and a side-remark about increasing the range of the Jacobi parameter (a,b) (section 4.1.2 is not precise). It feels incremental to me because nothing concrete is stated or observed. The literature is crowded in this area with the work of Daniel Potts, the work of Jianlin Xia, the work of Nick Hale, the work of Mikael Slevinsky, the related work of Akil Narayan, the alternative approaches of Arieh Iserles, and many others. In such a crowded space, one would like to see a novelty statement that is clearly stated and demonstrated.

MAJOR QUESTIONS

- Is it practical? A reader needs to be convinced that this is faster than the direct algorithm for practical transform sizes. Section 4 is a little unconvincing because (1) The 2D transform in Figure 3 is only shown for N <= 1024 (a regime where one can do kron(J,J)vec(X) = JXJ^T using the naive algorithm), and (2) On my laptop I can construct the discrete Jacobi transform matrix for N = 1024 and compute the 2D transform in a naive way in 0.09 seconds. Assuming timings are measured in milliseconds (the manuscript does not give units for time in any of the figures), Figure 3 suggests that the fast transform needs 2^15 milliseconds = 32 seconds. I am a little confused because the authors say that they have 28 processes but they don't mention how or if they are using them.

- What is the theoretical complexity? Throughout the manuscript there are complexity statements that involve the numerical rank of the matrix A_n^{(a,b)} in (42). How does r grow with n (the paper suggests r = O(log(N)), but is this observed not proved)? It would be nice if the paper made the theoretical complexity precise by bounding r. In Section 4, I think we learn that the rank of the matrix A_n^{(a,b)} is about 1000 for N = (2^6)^2 = 4096, which is barely low-rank structure in my mind. Does that mean the constants in this algorithm are computationally prohibitive? I am confused by Figure 1, bottom-left, it seems to suggest that the rank is growing linearly.

- What are the numerical results showing us? I find the numerical results highly confusing. Why is the N^4 trend line plotted on the left column of Figure 3? Figure 2, bottom-row, makes it look like you have a bug in your CHEB algorithm for a=b=0.8 and a=b=0.9 (why did it go from 8-digits to 1-digit so quickly?). Why can one not slightly modify the CHEB algorithm to make it work in the (-1,1)\(-1/2,1/2) regime? Why is everything for such small N? The authors say that r = ceil(2*log2(N)), but then say that they plot r from (52) in Figure 3, but it isn't ceil(2*log2(N)). The times also need units. Also, I am assuming that "N" in the figures of Section 4 is "n" in Section 3.

- What about the Jacobi-to-Chebyshev transform? There are other approaches for computing the Jacobi coefficients, which are fast and make the inverse problem easier. For example, one could first evaluate a function at Chebyshev points, compute Chebyshev coefficients (using DCT), and then convert to Jacobi coefficients. In this direction, Slevinsky has a paper on writing the Chebyshev-to-Jacobi transform using a sum of diagonally scaled discrete sine and cosine transforms (Section 1.1 of (Slevinsky 2017) is a good review of the literature). There's also a paper by Townsend, Webb, and Olver that shows that the Chebyshev-to-Jacobi transform has a T-dot-H structure.

MINOR REMARK 1. The authors mention several times that the inverse for a nonuniform Jacobi transform requires solving a highly ill-conditioned linear system. While strictly correct, the authors may want to be aware of the relations in Potts, Steidl, and Tasche's paper from 1998 (see page 1578). In particular, that paper shows them that WJ is the inverse of J in the uniform case, and generalizes this observation to other nodes from interpolative quadrature rules.

MINOR REMARK 2. In Section 3.2, the authors say that the 2D nonuniform transform can be written as a Kronecker product. While this is true for the transform in (5.3), the fully nonuniform Jacobi transform involves points (x1,y1),...,(xN,yN) without tensor-product structure. Here, the authors have restricted themselves to nonuniform tensor product grids. It might be worth explicitly stating this restriction.

TYPOS. Here are a few typos that I spotted while reading:

- p.2, line -3: "referred as" should read "referred to as". Same error on p.13.

- p.7, just before (28): "barcyentric" should read "barycentric". Same error on p.2, line -5 and p.8 just after (34).

- p.11, line 11: "to demonstrate the superior of the new method" should read "to demonstrate the superiority of the new method"

- p.13, just before (57): "we consider the the tensor" should read "we consider the tensor"

- Section 4 uses "N" for the size of transforms, while the rest of the manuscript uses "n".

- p18, conclusion: This is the first time that you tell the reader that the range of interest for (a,b) is -1<a,b<1.

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-**Reviewer 3**

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