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papers_0164 - Geometric Modeling Using Focal Surfaces

Rejected

Review Summary:

The topic of this paper is interesting idea which has not been explored so far: use the focal surfaces (meshes) for various geometry processing tasks. The initial results presented in this paper, especially on focal-surface guided subdivision and parts of the focal surface estimation method, are promising.

However, it was felt that the current state of research does not yet justify publication at Siggraph. The derivation and motivation of the subdivision scheme (section 3.1) is not rigorous (demonstration is in 2D, though the spatial situation can in general not be limited to the planar case) and not symmetric. Actually one wants to have that all normals are ''tangent'' to both sheets of the focal mesh, which can hardly be achieved by looking at one of the two focal meshes only.

Another serious concern is that the paper tries to do too much. Instead of focussing on one or two applications and elaborating them in detail, the authors present a number of possible applications, all of which did not get sufficient discussion and elaboration.

Reviews are sorted by questions with individual responses separated by dashes.

1) Briefly describe the paper and its contribution to computer graphics and interactive techniques. Please give your assessment of the scope and magnitude of the paper's contribution.

------------ Reviewer 1 -----------------------

The notion of focal surface (loci of the principal curvature centers) is used to model smooth surfaces (via subdivision and interpolation) and to estimate principal curvatures and directions from triangle meshes. Several contributions are listed (subivision, interpolation, curvature estimation).

Original Answer:

------------ Reviewer 2 -----------------------

The paper introduces simplicial focal surfaces of simplicial surfaces and derives a novel subdivision scheme based on focal surfaces. This is a very interesting theoretical contribution to the toolbox of discrete differential geometry but the practical usefulness has only partially been demonstrated.

------------ Reviewer 3 -----------------------

The paper proposes a method for computing discrete focal surfaces, along with principal curvatures and principal curvature directions for meshes. Additionally it describes a subdivision scheme using focal surfaces.

------------ Reviewer 4 -----------------------

This paper uses the geometry of focal surfaces to develop algorithms for triangle mesh subdivision and discrete focal surface estimation. The subdivision in inspired by the principle of preserving the focal surfaces. The algorithm takes as input a triangle mesh together with given vertex normals, and produces a consistent refinement of vertex positions and normals; it is able to generate a smooth normal field capturing effects such as specular inflection. The algorithm for constructing a discrete focal surface from given mesh positions and normals is demonstrated to be a very effective albeit time-consuming method for estimating shape operators.

------------ Reviewer 5 -----------------------

This paper describes how to find `focal meshes' for a given mesh. (i) It describes their use in a subdivision process which generates surfaces with a high quality normal vector field, and (ii) their usefulness for computing curvatures and principal directions. An application of the surfaces generated by (i) is rendering of reflections. This is a paper which studies geometry in a nice

way, however I think that
the scope and magnitude of the paper's contribution may be limited.
------------ Reviewer 6 -----------------------
The paper points to the important role that focal surfaces (loci of principal
curvature centers) are playing in various tasks of geometric modeling and
computer graphics. The contributions are focal-surface guided subdivision
surfaces, the superior performance of focal-surface based interpolation
for rendering specular and reflective surfaces, and a method for curvature
estimation. It is also shown how to compute focal meshes.

2) Is the exposition clear? How could it be improved?

------------ Reviewer 1 -----------------------
The exposition is clear.
Original Answer:

------------ Reviewer 2 -----------------------
The exposition is extremely clear.

A minor comment: I suggest replacing the supplementary material
on standard differential geometric properties of smooth focal surfaces (where
a reference to a standard textbook would suffice) with some more intermediate
steps of the calculations done in the paper.
------------ Reviewer 3 -----------------------
The exposition is not clear, please see remarks below.
------------ Reviewer 4 -----------------------
The exposition is overall clear; due to the subject
matter, it will be perceived as dense by most SIGGRAPH readers, but
this should not be counted against the work.
------------ Reviewer 5 -----------------------
yes
------------ Reviewer 6 -----------------------
The paper is well written

3) Are the references adequate? List any additional references that are needed.

------------ Reviewer 1 -----------------------
Yes.

Note that not all mehtods listed as such estimate tensors using finite differences (eg Cohen-Steiner and Morvan is based upon normal cycle theory, ie mesure theory).
Original Answer:

------------ Reviewer 2 -----------------------
OK.
------------ Reviewer 3 -----------------------
Some references seem to be incorrect, are used in unexpected ways or mischaracterized.

Zorin and Schroeder 2005 should be Grinspun et al 2005, and has little if anything
to do with subdivision; Vlachos et al 2001 is hard to interpret as extending subdivision
to interpolate normals. Zorin 1996 is a certainly about subdivision, but hardly a primary
reference, unless the authors have interpolating subdivision in mind.
Cohen-Steinter and Morvan do not derive their curvature estimates using finite differences.

References to some of the recent work on curvature estimation are missing, e.g.

Yang et al, Robust Principal Curvatures on Multiple Scales (SGP 2006)

Estimating Curvature on Triangular Meshes, Timothy Gatzke and Cindy Grimm. International Journal of Shape Modeling, 12(1): 1-29, 2006.
------------ Reviewer 4 -----------------------
Yes.
------------ Reviewer 5 -----------------------
yes
------------ Reviewer 6 -----------------------
The autors added a reference to conical meshes, but
more relevant would be the following paper which is available on the web:

H. Pottmann, J. Wallner: The focal geometry of circular and conical meshes.
Geometry Preprint 163, TU Wien, 2006.

It deals with focal meshes of principal meshes (circular or conical
meshes). In fact, the authors address this topic in the future research
part, and thus may not be aware of this paper.

**4) Could the work be reproduced from the information in the paper?
Are all important algorithmic or system details discussed adequately?
Are the limitations and drawbacks of the work clear?**

------------ Reviewer 1 -----------------------
I think it would be difficult (see comments below)
Original Answer:

------------ Reviewer 2 -----------------------
YES.
------------ Reviewer 3 -----------------------
With difficulty, some aspects are unclear (see below).
------------ Reviewer 4 -----------------------
It's hard to say whether sufficient detail is given. I believe that there is sufficient detail in the math, so that one could eventually program this method. However, I feel that this is the kind of paper that could certainly benefit from an associated technical sketch---I would not be surprised if there are subtle details and degeneracies that must be treated in practice.
------------ Reviewer 5 -----------------------
yes, except the reader does not know how the authors computed their normal vector fields.
------------ Reviewer 6 -----------------------
A good graduate student may be able to reproduce the work.
The limitations and drawbacks are not really discussed,
except for issues of computational efficiency.

**5) Please rate this paper on a continuous scale from 1 to 5, where:
1 = Definitely reject. I would protest strongly if it's accepted.
2 = Probably reject. I would argue against this paper.
3 = Possibly accept, but only if others champion it.
4 = Probably accept. I would argue for this paper.
5 = Definitely accept. I would protest strongly if it's not accepted.
Please base your rating on the paper as it was submitted.**

------------ Reviewer 1 -----------------------
3.0
Original Answer:

------------ Reviewer 2 -----------------------
3
------------ Reviewer 3 -----------------------
2
------------ Reviewer 4 -----------------------
4.4
------------ Reviewer 5 -----------------------
3.3
------------ Reviewer 6 -----------------------
3.3

7) Explain your rating by discussing the strengths and weaknesses of the submission. Include suggestions for improvement and publication alternatives, if appropriate. Be thorough -- your explanation will be of highest importance for any committee discussion of the paper and will be used by the authors to improve their work. Be fair -- the authors spent a lot of effort to prepare their submission, and your evaluation will be forwarded to them during the rebuttal period.

------------ Reviewer 1 -----------------------
The main idea proposed in the paper is novel and would deserve more investigation.

My main concern for me not to champion the paper is the lack of clarity in the contributions: the authors want to do "a bit of everything" like subdivision, curvature estimation and rendering, GPU-based estimation, without exploring deep enough one of them. For example, it would be valuable to prove that the curvature estimator converges in the limit when the mesh is refined (under certain meshing and sampling conditions), to prove that the subdivision scheme converges to a smooth surface, etc.

Another concern is the sensitivity to noise. Some methods are clearly very sensible to noise (eg normal cycle), while others are less (based eg
upon tensor voting). Still even the sensible estimators can take one parameter which is related to the "measuring area", eg a line density of tensors on the mesh edges. In this case the principal directions are smoother.

From the algorithmic point of view, what happens if the focal surface is degenerate or close to degenerate? infinite? the issue of flat points is also not fully solved.

The exposition is sometimes confusing:
- other techniques are said to be inconsistent: explain with more details
- the algorithm needs vertex normals as input, and the narrative says that it computes them.

My suggestion to improve the paper would be to... split it into two or more papers.

Original Answer:

------------ Reviewer 2 -----------------------
I consider the contributions of this paper novel and interesting for the
community.
PL focal surfaces are surely a novel idea and a fruitful extension of
the
set of tools from discrete differential geometry.

On the other hand, the practical applications of the paper are not fully
convincing.
1. I am missing a theoretical investigation how respectively why
interpolation via the focal
surface should improve the order of interpolation of the PL surface. If
the order
of interpolation is improved, then there should be some reasoning behind
this idea.
2. The dependence on the PL normal vector is a critical issue. Since the
whole method
relies on second order terms (principal curvatures) the dependence on a
first order
normal vector is highly critical, especially if the normal vector
computation is
not part of the presented technique but obtained from elsewhere.
3. I am not sure if the switching between Gamma1 and Gamma2 at singular
regions does not lead to discontinuities of the subdivided surface.

Based on my critical comments (and I hope they can be answered) I am
giving a rather
low rating for this SIGGRAPH submission. On the other hand I would
strongly vote
for a publication of this paper, including answers to questions 1.-3.,
for example, in a journal publication!

Minor issues and typos:
page 1, col 2, -6: can <<be>> computed
p2, c1, -24: as the --the-- surface
p2, c2: Say some words on orientation of normal vectors ("take outer
normals")
p2, c2: why the dot on S in (1)?
p2, c2, -12: The normal<<s>> ...
p3, c2, -7 in eq (3): I think the denominator should be:
area(v*,v2,f1,f2)
p3, c2, -6: sin a ---> sin(a)

The references are a bit sloppy and need revision, for example,
- Grinspun 06: missing journal
- Hildebrand 04: cited?, another major shape operator
- Meyer 03: missing publisher
- Vlachos 01: capitalize pn ----> PN

I suggest replacing the references to SIGGRAPH course notes
with references to the original (and published) work.
------------ Reviewer 3 -----------------------
The main idea of the paper is interesting and novel, and I think worth exploring.
However, the paper is trying to do two things at once (focal surface construction and
subdivision), and the algorithms, experimental validation and presentation appear too
raw to me -- I believe with more work this will be an excellent
paper (probably two separate papers).

The fundamental issue with using and computing focal surfaces, is that focal surfaces
may have unbounded sheets, with points at infinity corresponding to some points on the
surface, and may degenerate into curves. While this problem is discussed in the paper
at several instances, I did not see a consistent approach to this problem and related
numerical instabilities. E.g. the authors say "In this case [zero curvature] we associate
the second focal mesh with the base mesh". It is quite unclear to me how this can work.
Also, suppose we have a surface close to a Dupin cyclide; this means that its focal
surfaces will be close to degenerate, which is also likely to cause all sorts of numerical
problems (if not, I would like to see experimental evidence that this is not the case).
Perturbing the surface at flat points (randomly?) also seems to be dangerous.

The description of the algorithms is hard to follow and is structured in a nonintuitive
way. First, the subdivision algorithm is described, assuming that the focal surface
is given.

On p. 3 "inserting a new vertex ... using Phong interpolation". Should this be linear interpolation?

On p.4 "our algorithm uses one of the two focal surfaces .. " I do not really understand this paragraph. How does the result depend on the choice, in what sense are the results similar? How does one switch from one surface to another at the points where one is closer than the other? what happens when the focal surface is degenerate or the denominators in

A complete step by step description of the subdivision algorithm, starting with the control mesh, would help.

A lot is known about the properties of the limit surfaces of conventional schemes (Loop, Catmull-Clark). What can be said about the surface generated by focal subdivision? At least some experimental exploration should be attempted.

I find some parts of the discussion of Loop subdivision difficult to understand. "Loop subdivision does not consider consistency between the resulting surface and the given focal meshes." There are no given focal meshes for Loop subdivision. I do not see how one can obtain focal meshes (from the coarse control mesh? why is this a good idea?) "Loop subdivision ignores differential geometry features". Of what? It starts with a control mesh and produces an almost everywhere C2 surface, which features does it ignore?

Based on the video and some images in the paper, it appears to me that the authors compare their technique with PN triangles and Loop in the context of approximating an analytic surface from a mesh sampled from this surface (If I understand correctly focal meshes If this mesh is used directly as a control mesh of the Loop surface, this comparison is unfair, as both focal subdivision and PN triangles are interpolating. A quasi-interpolation or fitting procedure similar to Halstead et al 94 has to be used.

Similarly, comparison to Cohen-Steiner and Morvan(I assume the simplest single-ring version) is hardly a fair way to evaluate a curvature estimator given that it is one of the worst ones for noisy meshes (it works quite well in the context of surface optimization problems). There are many better ones to compare to including those in cited references. I am far from sure that the relatively expensive process needed to estimate curvature as described in sec 5 is competitive with other techniques.

------------ Reviewer 4 -----------------------

This is a thought-provoking and inspiring paper, and it deserves a place in this year's SIGGRAPH proceedings. The paper (as I outline below) is by no means perfect, but despite its imperfections, it is likely to spur considerable thought and to inspire further activity in geometry processing. Therefore, I recommend that it be accepted as-is, although I encourage the authors to improve the presentation, as outlined below.

The strength of this paper lies in its harnessing of the beautiful and (in graphics) relatively unexplored structure relating surfaces to their evolutes. The authors succeed in demonstrating that what may be considered somewhat abstract objects and theorems of smooth differential geometry can have significant impact in the computational domain.

The work requires additional strengthening in some respects, but I feel it would have good value even published as-is.

One important limitation of the work is laid out in the text---the method as presented requires given vertex normals (possibly estimated using some other technique). However, the importance of this limitation is obfuscated by other phrases in the text, e.g.,

"Her method requires accurate estimates of differential surface features (particularly consistent normals), whereas ours derives such features."

This appears to imply that the proposed method derives vertex normals. Rather, the method is given vertex normals. Please clarify.

"Phong shading is still flawed due to inconsistencies between surface points and their normals. As a result, it fails to reproduce many noticeable phenomena."

This is a correct statement, of course. However, one must clarify to the reader how this notion of "inconsistency" does not affect the proposed method, which clearly is subject to any inconsistencies between the input normals and input positions. (The clarification can be made by referring to inconsistencies introduced during interpolation, rather than any inconsistencies existent in the quantities given at the vertices).

There are other such clarifications that are needed---search for every occurance of "normal" that compares to another method, and think about whether it may incorrectly conclude that the proposed method does not require input normals.

The method does not support flat points on the surface. Of course, many mechanical and architectural geometries contain flat points or flat regions. How can the method be extended to support flat points? It should be clarified that the approach mentioned here---perturbing the surface, is acceptable when the surface has an unintended flat point, but probably not acceptable if the surface has an intended flat region.

The subdivision algorithm appears to make a somewhat ad-hoc decision about which focal surface to use; the decision is made, justifiably, based on numerical considerations, however, it would be appealing to make the decision in a way that becomes symmetric w.r.t. the two evoltes when they are equidistant from the midsurfaces. Otherwise, the statement that "when the surface has no parabolic points, using either of the two focal surfaces produces similar results" is a rather weak qualification---in particular, are there any artifacts that arise along the curve on the surface that demarks the discrete transition between the use of one evolute versus the other?

There is no comparison to curvature estimation based on fitting of polynomial jets.

The plots of the distribution of curvatures are instructive, and they illustrate that the method generates a smoother (and more consistent) estimation of the shape operator than a very local method such as Meyer's. Two questions arise:

1) is the consistency due to using an optimization? Or is it due to using discrete focal meshes? There are other alternative formulations one could use for optimization. (This is not a critique of the method, but an interesting question).

2) how does the estimation compare to more expensive operators with larger support? For example, Polthier's operator using the star of a vetex? Cohen-Steiner over a larger neighborhood? Using a larger support is computationally more expensive, but yields more consistent (and smooth) curvature lines; therefore, from a numerical/computational perspective it would provide a closer comparison to the proposed method (persumably here the proposed method could "win" because it does not require larger support and therefore it can capture finer surface details).

"An objective of discrete differential geometry is to derive higher-order local surface properties that are simultaneously consistent with the given sampled surface mesh as well as some underlying smooth surface" --- another view is that DDG seeks to develop discrete analogues of smooth structures, such that important theorems or invariants are preserved. This latter view seems to be consistent with the approach adopted in this work, in which properties relating surfaces and their evolutes in the smooth setting are used as the foundation of algorithms in the discrete setting.

In summary, while there is no doubt that this paper has its shortcomings, that the execution was not perfect, and that the narrative requires some further improvement, I feel that the paper is thought-provoking, invites us to take new perspectives on geometry processing, and invites us to learn more mathematics and geometry because of its real and exciting applications to computer graphics. For these reasons, I feel that the overall perspective presented here is deep and worth bringing to light at a large venue such as SIGGRAPH.

------------ Reviewer 5 -----------------------

I see the strengths of the paper in a subdivision method which refines both the base mesh and the focal meshes at the same time, thus generating normal vector fields of high quality. The paper introduces a new way of computing principal curvature centers (i.e., the vertices of the focal meshes).

As to the use of focal meshes for geometry processing (for computing curvatures and principal directions), apparently the normal vector field which is the basis of computations is very important, and the outcome relies on the normal vector field's smoothness. Thus one could argue that the present paper does not address the most sensitive part of the overall processing pipeline, but only the part after that.

------------ Reviewer 6 -----------------------

I really like the use of focal surfaces. The paper shows that there is some potential in this classical concept and it also addresses a few applications for which this should be true.

However, I am not so sure that the paper represents significant progress in this interesting direction. Some constructions are quite ad-hoc and not really convincing. Details on this claim and some additional comments, in the order of the occurrence in the paper, are given below:

1. Page 2, 2.1: The focal surfaces of polynomial base surfaces are two degrees less continuous. 'Polynomial' is not essential, as soon as the surface is C2, one can compute principal curvature centers and hence a focal surface. So only sufficient differentiability is required.

2. In section 2.2., the reference to focal meshes of circular or conical meshes should be provided. Clearly, the work of Hahmann is much less relevant, since Hahmann's focal surfaces are something different.

3. Figure 3 is very problematic. The sketch of the focal surface shows focal points on the silhouette. Hence, the normals should be tangent to the silhouette. I know that it is not easy to sketch this situation, but a better figure would be necessary since otherwise this may confuse the reader. In fact, also Fig. 1 is not really nice.

4. The subdivision rule is only partially motivated, not symmetric (uses only one focal sheet) and there is no proof that the procedure generates smooth surfaces.

5. The use of the 'slits' is unusual. The results of sections 4.2 and 4.3 are well known in the geometry of line congruences. Aren't the slits just infinitesimal line segments on the principal curvature axes? (axes of the osculating circles of the principal normal sections).

6. Section 5 is a nice part, but I am missing a clear explanation of the metric (18), which is announced to be part of Supplemental material A, but the reviewer could not find it within Suppl. A. Instead, we find there derivations which are actually known.

7. The authors claim that the focal surface approach is less sensitive to noise than other methods of curvature estimation. Did you also compare with methods such as tensor voting (Tong, Tang, IEEE PAMI, 2005) or integral invariants (Yang et al, SGP, 2006)?
   It seems strange that a method which depends so much on normal estimates is robust against noise. How do you compute the normals?

8. The reviewer does not understand the sentence: "Our focal mesh model can also be viewed as the conjugate to the conical mesh representation which is constructed using the principal directions". What do you mean by 'conjugate'? I guess a main difference is that focal meshes of conical or circular meshes are based on precise counterparts of the classical smooth theory, whereas the present focal meshes arise from some local approximation.
   They are a result of numerical geometry rather than discrete differential geometry.

8) List here any questions that you want answered by the author(s) during the rebuttal period.

------------ Reviewer 5 -----------------------

It would appear that one should compare a method of computing curvatures which relies on a given normal vector field only with methods which use that same normal vector field. On the other hand apparently your method together with a suitable way of computing normal vector works well. Could you comment on that?

------------ Reviewer 6 -----------------------

Could you comment on the derivation of the metric (18)? Moreover, comments on a proof of smoothness for the proposed subdivision scheme would be really nice.