More comments on CDS

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The rebuttal I posted on Nov. 21 was written in September after I received Chen-Donaldson-Sun's note on Sept. 11, 2013. Apparently, they made some changes in their posted version though all the charges stayed the same. Here I will show with evidence that their accusations are without merit. I will focus on the existence problem of Kähler-Einstein metrics on K-stable Fano manifolds, simply referred as the existence problem in the following, since this is the center of this controversy.

For the readers' convenience, I list references discussed here. I use their numbering.

[9] arxiv post 19/12/2012. Kahler-Einstein metrics on Fano manifolds, II: limits with cone angle less than 2.

[10] arxiv post 01/02/2013. Kahler-Einstein metrics on Fano manifolds, III: limits as cone angle approaches 2 and completion of the main proof.

[11] arxiv post 20/11/ 2012. K-stability and Kahler-Einstein metrics.

[12] arxiv post 28/01/2013. K-stability and Kahler-Einstein metrics.

[13] K-stability and Kahler-Einstein metrics, I. Lecture one at Edinburgh on July 8th..

On p5 of their note posted on Nov. 21 of 2013, they wrote

"that we feel that there is no evidence that Tian was in possession of anything approaching a complete proof at the time of his announcement [6] in Stony Brook;"

This is a strange reasoning. When Perelman posted his first paper on geometrization of 3-manifolds, no one knew that he was working on such a big problem. In fact, I have worked on the existence problem for many years. In the email included in my previous response and dated on Sept. 19 of 2012, I said "...I discuss the extension to conic Kahler-Einstein metrics. I think I have a proof." (of partial C^0-estimate for conic Kahler-Einstein metrics). We all know that the partial C^0-estimate is the key. For many years, I have told people, including Xiuxiong Chen, the implications of the partial C^0-estimate and showed techniques which may be used to prove it. These techniques are indeed used in recent solution of the existence problem. I know these techniques well and am responsible for developing some of them.

On p6, they continue to write:

"that both arXiv versions [11], [12] of his paper have serious gaps and mistakes;"

This is not true. I have addressed them in my previous response. They may hope to have more details for something they do not know well. I am willing to explain my proof to some of them over a basis of mutual respect or to a group of experts on the subject who can play fairly.

On p6, they further wrote:

"that, insofar as these gaps and mistakes have been partially filled and corrected (in comparing [11], [12], [13]), many of the changes and additions made reproduce ideas and techniques that we had previously introduced in our publicly available work [7], [8], [9], 10], without any kind of acknowledgement."

In making these changes and additions, I did not rely on their work and thus no acknowledgement is necessary. [13] is only a talk which is based on [12] which appeared before they completed their series. [12] is a refinement of [11] which contains all the ideas for my proof. Except an appendix and one extra lemma I quoted from my former student's thesis, all the lemmas, propositions and theorems were already in [11]. In the middle of February, I submitted my paper which is essentially [12] with another appendix which includes an outlined proof of my previous result with B. Wang. This is just for readers' convenience. Later, after receiving feedbacks and referee's report, I added some more details for better presentations. All the lemmas et al stay the same as in [12]. Also as I said above, I knew all the techniques and developed their extensions needed in proving these lemmas, propositions and theorems. The main extension is to generalize the compactness of Cheeger-Colding-Tian to conic case. I already discussed how to do this extension in my lecture on Oct. 25 when I announced my solution. There is no way I need ideas or techniques from them.

There are two specific places they charge I copied their ideas. Let me show by facts that their charges are without merit.

1. On p9 top, they wrote:

"These assertions are blatant copying without attribution. This is almost half a year since the appearance of our third paper [10], in which the detailed proof of the reductivity is provided, based on the uniqueness theorems proved by Berndtsson and Berman-Boucksom-Essydieux-Guedj-Zeriahi, and the technical difficulty in extending the usual proof of the Matsushima theorem is pointed out."

This charge concerns Lemma 6.3 in [12] and refers to my talk [13] in July, 2013:

"This can be deduced from the uniqueness theorem due to Berndtsson and Berman. There is also a more direct proof.

There is also a remark:

Remark: If M_∞ is smooth, then by standard arguments, one can prove that the group is reductive. But if M_∞ is singular, one needs to pay attention to a technical problem caused by the singularity."

Since Berndtsson and Berman et al's paper came first and I thought that Lemma 6.3 is an easy consequence of their result, I felt I should mention them first. Then I said there is also more direct proof which means the first one I used in my paper. It is clear unless I did not write English right since I am not a native speaker. My direct proof follows the arguments of Matsushima.

The remark says that they become standard if M_∞ is smooth and need more attention if M_∞ is singular. What is wrong with this remark? I do not see the logic of their conclusion: "These assertions are blatant copying without attribution."

In any cases, as I said in my previous rebuttal, my paper only contains two proofs and neither has anything to do with CDS.

2 On p9 bottom, they wrote:

"In Tians second written version [12], which appeared a month and a half after our second paper [9], more than 10 pages were added to prove Lemma 5.8, (from page 25 to page 29 and the whole appendixpages 38 to 45). In the main context the proof of Lemma 5.8 (Page 26, Line 11) is not finished since he made an assumption A1. The proof in the appendix depends on a local Hörmander argument, which are very similar to Section 2.5-2.7 of our paper [9]. This is a refinement of the Hörmander argument to prove the partial C^0 estimate for smooth Kähler-Einstein metrics (Section 2 above). The latter has only appeared recently, and this clearly contradicts what he claimed above that the proof of Lemma 5.8 has been known to him for quite a while (Page 25, Line 6). Also in the appendix Page 45, Line 1, he made use of the lower bound on the volume density of the divisor and this has never been mentioned in his first written version or his announcement. In sum, what he has added in the appendix is so similar to our second paper [9] that we feel this amounts to copying."

This concerns Lemma 5.8 in [12].

1. As I said in my previous response, it is easy if the singular set is a subvariety and the key is the partial C^0-estimate and its local version in general case;

2. Contrary to what they claimed, I already mentioned the idea of the proof in [11]. On p22, I wrote:

"This is rather standard and has been known to me for quite a while. This is based on the fact that the Poincare metric on a punctured disc has finite volume. The proof for smooth S_x is particularly elementary,

.....

In particular, S_x can be taken as $\mathbb{C}^{n-1}$ whenever $x \in S_{2n-2}$ of M_∞. Then one can show that it satisfies all the conditions in the above lemma. To deal with $x \in \bar{S}$ which is of codimension at least 4, one can use what we already know for S_{2n-2} and a local version of Theorem 5.9 around such xs (a typo here) to prove the lemma."

This is short and not a detailed proof because of time pressure I had, but I did mention the idea of proving Lemma 5.8. Theorem 5.9 gives the subvariety structure of a singular set in our context and can be derived from the partial C^0-estimate. This idea was due to me and is also in my expository paper in 2010. Clearly, its local version means that we can understand the structure of singularity near where we have partial C^0-estimate around that singularity. More precisely, for $x \in \bar{S}$, if $y \in S_x$ has tangent cone C_y whose singular set is $\mathbb{C}^{n-1}$ (See Lemma 5.5 in [11]), or equivalently, $y \in (S_x)_{2n-2}$ in the natural stratification of S_x, then this is a situation identical to the case that $x \in S_{2n-2}$, so we can apply partial C^0-estimate near y. That is how I did in [12]. My paper [11] was ahead of their second paper for a month, so I already had the idea. In fact, the idea can be traced back to an earlier time as I mentioned in my last response. I know how to prove Lemma 5.8 as early as August. I had discussed with my former collaborators the idea how to determine the analytic structure of the singular set assuming partial C^0-estimate or its local version in much earlier time.

3. The key to proving Lemma 5.8 is the partial C^0-estimate. The Hörmander argument is different from the partial C^0-estimate. It is only a tool in the proof and indeed needed. By saying a refinement of Hörmander argument, I feel that it may not be fair towards my and other's previous works. The compactness is a crucial tool here, too. It is a common sense that it is usually hard to prove a property with uniformity. In our situation, partial C^0-estimate is a uniform estimate. The uniformity comes from the compactness;

4. My proof for Lemma 5.8 does not use the lower bound on the volume density. Actually, my proof ends on p43 (3rd paragraph from bottom) and I wrote clearly: "Thus, the proof of Lemma 5.8 is completed". So my proof has nothing to do what they said: "Also in the appendix Page 45, Line 1, he made use of the lower bound on the volume density of the divisor and this has never been mentioned in his first written version or his announcement. In sum, what he has added in the appendix is so similar to our second paper [9] that we feel this amounts to copying." I do not see any justification for their charge. Indeed, after I finished the proof, I mentioned other natural approaches. One involves the lower volume estimate. But it is not the proof I used. Even for volume estimate, people often do for geometric problems.

In conclusion, all their charges are invalid.