I agree that my inflation argument in the 2001 paper *Symplectomorphism groups and almost complex structures* has a gap in the proof of the inflation lemma 3.2. I assume here that for every ω-tame J and every J-holomorphic curve C one can find a family of normal planes that is both J invariant and ω-orthogonal to TC. But this is true only if ω is compatible with J at all points of C. And if it is, then the result of inflation is a symplectic form that tames J. Therefore, instead of getting an inclusion $\mathcal{A}_u \to \mathcal{A}_{u'}$ we get an inclusion $\mathcal{A}_u \to \mathcal{J}_{u'}$, where $\mathcal{J}_{u'}$ is the set of all almost complex structures tamed by some form in the class u. It seems to me that for anything I wanted to do in my paper this is good enough since the inclusion $\mathcal{A}_{u'} \to \mathcal{J}_{u'}$ is a homotopy equivalence.

I don't understand your proof well enough to know if this would also do for you. But, if not, it would be good to explain why.

Your proposed remedy looks fine except that it is very high powered. Since you are just proving a result on homotopy type, perhaps you don't need such deep results?

I agree that you may need to use Condition 1 (though I'd like to know why). But Thm 3.1 on the identity $\mathcal{K}^c_J = \mathcal{K}^t_J$ is a different matter. Do you really need this?

Is the hypothesis $\chi(M) \leq 12$ really necessary? I understand that the 10 point blow up of $\mathbb{C}P^2$ does contain a nonembedded rigid symplectic sphere (in a class C with $C^2 = 1, c_1(C) = -1$), namely take rational cubic in $\mathbb{C}P^2$ (which has a node) and then blow up 10 points on it. (and it might be nice to gove this explicit example for nonexperts, to help them understand what is going on). But why does the presence of this sphere mess things up? We cannot reduce its symplectic area by inflating along it, but we can change this area by altering the sizes of the exceptional divisors. In any case, if there is a real problem in this case that prevents Thm 1.1 from holding it would be nice if you gave an expalnation.

There seems to be a general argument that $U_{u,\mathcal{C}}$ is a cooriented Frechet manifold (with no condition $\chi(M) \leq 12$); but then you need this to conclude that $\mathcal{A}_{u,\mathcal{C}}$ are submanifolds, i.e. you seem to be using the fact that the classes in $\mathcal{C}$ are represented by embedded spehres rather than by immersed curves of arbitrary genus. Your proof here is very obscure. Can you please explain better?

Here are a few other comments on tbe paper.

1. You say that for a rational surface $\mathcal{T}_u$, the space of forms in the class u is path connected – but I think this is unknown. There could be diffeomorphisms ψ that act trivially on $H^*(M)$ and are such that $\psi^*(\omega)$ is NOT isotopic to ω. I don't know what is in [LM96] but the new edition of INTRO is probably more reliable here; see Ch 13.1 and specially p 535. You do need to assume that the forms are compatible with homotopic rulings, or that a syplectic sphere representing the fiber class lies a a distinguished connected component of the space of embedded spheres.

2. There is something funny about the reference to [LLei] on p 4 top – on the arxiv I can only find one paper with this title 1611.07436, it has Wu also as an author and it does NOT seem to have a lemma 2.16...

3. The discussion of inflation in sec 3.2 is far too oblique, and it is totally unclear what you are trying to say. It would be much better explicitly to point out what is wrong with Lemma 3.1 [McD01] ; eg say that the proof makes the unwarranted assumption that for every ω-tame J and every J-holomorphic curve C one can find a family of normal planes that is both J invariant and ω-orthogonal to TC. This is true only if ω is compatible with J at all points of C. Then state the correct version.

from Dusa, November 23.