This document contains 3 e-mails I have written:

p2: to E. Ionel, T. Parker, and Y. Ruan before the March 2014 workshop at SCGP in the hope of having a discussion on these papers at the workshop

p4: to E. Ionel and Y. Ruan after the workshop in the hope of finding a reasonable agreeable way to proceed

p6: in response to Ionel-Parker's attempts to distort the situation with their papers.

The March letter referenced several times in the last e-mail is posted on my website as *Comments on Ionel-Parker's Response.*

Aleksey

E-Mail 1

Date: March 14, 2014
From: A. Zinger
To: E. Ionel, T. Parker, Y. Ruan
Cc: G. Tian, D. McDuff, H. Hofer, K. Fukaya, K. Cieliebak, K. Mohnke, K. Wehrheim, M. Abouzaid, M. McLean, J. Li, R. Pandharipande, D. Maulik, M. Tehrani

Dear Eleny, Tom, and Yongbin,

As you know, some people have wondered about some aspects of your symplectic sum papers, including
IP: the extra S-matrix, distinguishing between classes differing by rim tori, no VFC;
LR: how short the argument is.

Over the past 1.5+ years, Mohammad and I have analyzed the IP relative and sum papers and the LR paper. Our understanding is detailed in the hidden link

http://www.math.sunysb.edu/∼azinger/SympSum.pdf

(which also includes a general exposition about the subject). Section 2 lists what we feel are important, very specific issues in these papers. It is self-contained, so the rest can be ignored for now. Some examples are listed below.

The above manuscript is not quite complete, but since you all will be at Simons Center this coming week, it would be great to go over these things. We hope you will share your thoughts on the points listed in Section 2, the presentation in general, and each other's papers. We also hope for some feedback from everyone else. Wednesday afternoon is currently free.

Best regards,
Aleksey

Comments on LR

Excluding the introduction and the applications (Sections 1,2,6), the whole argument is only 30 pages, including all definitions and 10-12 pages of unnecessary things.

In our view, the idea behind LR (to adopt Hofer's SFT type setup) is really nice, symmetrizing the domain and target degeneracies, and should allow to take care of all technical issues very efficiently (as we expect to show in a separate paper).

However, little is actually done with this idea in the paper, with many important points not even mentioned. Even the definitions of relative maps and maps to the nodal fiber (3.14 and 3.18) are incomplete (and the second one is incorrect because it separates the maps into 2 parts). Bubbling and gluing is considered at only one node, which captures the general situation pretty well in the semi-positive case, but not in general.

The most technical arguments, appear either incorrect, incomplete, or unnecessary. For example, the correct statement of Thm 3.7 reduces to Hofer's statements for the circle in a few lines. The argument in the 6-page Section 3.1 is incorrect and in particular uses a Morse Lemma in the infinite-dim setting of Prop 3.4 without any justification and with an inner-product in which the space is not even complete.

Another example is 4 pages about limiting behavior of gluing maps. This is used to show convergence of the integrals (4.50) defining rel GWs, after it had been claimed the regularized spaces define pseudocycles (and so give GWs as in Ruan-Tian, McDuff-Salamon).

Comments on IPrel/sum

We feel that these papers go into all issues needed to prove the symplectic sum formula in semi-positive (and other nice) cases, but go wrong in several important places.

(1) the topology on the twisted targets for the refined relative invariants in Section 5 of IPrel is never defined. If the divisor is not connected, it is usually not possible to lift the relative maps to *any* covering space. In other cases, there are plenty of choices of such liftings, making them unlikely to be useful for the symplectic sum formula. The attempt to use refined rel GWs in IPsum to resolve the rim tori deficiency involves taking intersection product in typically non-compact space in the middle of page 993 in IPsum.

(2) The S-matrix in (0.2) in IPsum appears only because of the forgotten $\mathbb{C}^*$ action on maps with rubber component at the top of p1003; otherwise, these spaces would have lower dim (just as in LR and Jun's paper). The S-matrix acts as the identity in (0.2) anyway, even though it is not the identity in general. Furthermore, the limiting argument at the top of p1003 does not fit with the allowed complex structures in IPrel/sum, but it does fit with the more restricted ones in LR.

As stated at the beginning of Section 8 in IPsum, the key step in the proof is a uniform elliptic estimate on the linearization of the $\bar{\partial}$-operator (or bound on the adjoint).

(3) Part of the proof is the sign arising from the comparison of the two curvatures in (8.7), which allows dropping the negative term in (8.8) and thus to take care of the exploding curvature due to the degeneration of the target (which does not happen in LR). However, the Gauss equation in (8.7) is organized in a rather unusual way and crucially has the wrong sign on the correction terms. Thus, the negative term in (8.8) is actually positive and cannot be dropped. Even conceptually, it seems unlikely the local estimate (8.8) could have this term to be negative.

(4) The first component of the claimed adjoint of D_f in (7.5) does not map to the domain of D_f because it does not satisfy the average condition. It would have to be corrected by an L^2 term, which is not in the domain of D_f. So, the estimates in Section 8 (e.g. Prop 8.3) are not even for the adjoint of D_f.

Of course, there is a correct symplectic sum formula, which was perhaps known in the mid-90s because of Caporaso-Harris. So, in theory, (3) and (4) can in principle be dealt with, perhaps by re-interpreting the LR norms in the IP setting.

E-Mail 2

Date: March 31, 2014
From: A. Zinger
To: E. Ionel, Y. Ruan
Cc: G. Tian, H. Hofer

Dear Eleny and Yongbin,

I am planning to post a somewhat updated version of

$$\text{http://www.math.sunysb.edu/}\sim\text{azinger/SympSum.pdf}$$

by next Monday. This is 4 weekends after my previous e-mail. The main changes are in Section 2, especially the remarks with my own views.

This manuscript is not about going at you or your papers, but about the mathematics of the symplectic sum and relatedly in your papers. The points listed in Section 2 and elsewhere that concern your papers are only about what is stated in your papers, not about what you may have known then or know now and would have done differently, including after my explanations of what needs to be done and how. Nearly everything in the manuscript is factual statements, which I believe to be accurate. If they are not, please let me know and I'll change them; I apologize in advance for any misstatements.

Each of you had reservations about the other's paper(s), and I agree with you in this regard. The remarks stating my personal opinion are labeled as such. Your opinion is likely different, but I am entitled to mine as well and prefer to state it openly instead of behind your back (which is what people normally do these days). This way you are able to disagree with any specific points raised. Ideally I would have liked to add whether you agreed with whatever points in Section 2 and leave to the mathematical community to judge the rest.

If these points concerned a specific gap, I would have contacted you individually so that you could fix it (as I have done with you in the past). However, in the given case, I believe the issues raised leave fairly little of the argument needed to address the main problem (proof of the symplectic sum formula) in LR and almost nothing in IP which is not in LR. The applications are nice and fundamentally correct in my view, though not new in IP and basic special cases of Hu-Li-Ruan in LR. Thus, these papers prevented others (not me) from providing a proof 15 years ago, without actually providing anything approaching a proof in my view or a fundamentally new idea.

It seems to me that it is not about filling gaps at this point, but about whether it is appropriate for your papers to remain in the *Annals* and *Inventiones*, instead of being replaced by a general, semi-expository monograph, with fair credits to and criticisms of the original papers. This is of course a subjective judgment. By deciding that they should stay in the journals, you put your current standing (rather than that 15 years ago) behind what you feel the standards for the *Annals* and *Inventiones* in your field should be.

If you decide to withdraw your papers, there would be no need for stating my personal opinion on this in Section 2 and I would replace it with a statement to this effect. I believe such a decision

would be viewed by others as you choosing to be above any reproach and to uphold high standards for the *Annals* and *Inventiones* articles in the field that you represent as prominent senior people.

I respectfully understand that your views on this may be very different from mine. Other mathematicians will have the opportunity to form their own views based on your papers and the above manuscript, which they can choose to disregard.

Sincerely,
Aleksey

P.S. As I have said before, to you and others, I have found your papers exciting to think about, however correct they are. This applies to the three papers in question as well, but this does not contradict the above. Many conference proceedings raise intriguing questions, but that does not necessarily qualify them for the *Annals* or *Inventiones*. I realize that unfortunately my open attempt to sort out purely mathematical issues may lead to personal conflicts, which is not my intention. However, I do not feel that some of the ways in which things are generally done in mathematics these days are ideal. The *Manifold Destiny* article seems to have had no effect in this regard over the past 7.5 years; most people continue to keep things quiet, while undermining others behind their back. I feel that it would be in everyone's best interest (including the whole field) in the long term to have this resolved quickly and move on without ill feelings instead of dragging this on for years.

Yongbin, I feel sorry about your situation with this paper which is not even relevant to you anymore. This case again appears to be about over-trusting one's coauthors, after sharing important ideas with them (another, much smaller case is your genus 2 paper). While my feelings about the much earlier LR are somewhat ambivalent, I think you are actually less likely to disagree that in retrospect LR falls below your current standards for *Inventiones*.

E-Mail 3

Date: June 14, 2015
From: A. Zinger
To: annals@math.princeton.edu

Dear Dr. Hsiao,

The referee II report you attached is a short excerpt from the letter I wrote in March in response to IP's response to someone else's inquiry (my March letter is attached). As I explained in that letter, this kind of corrigendum would not change anything; my March letter applies without any changes. The issues with IP's papers are not about some mistake that needs an erratum. The symplectic sum paper contains almost nothing new and correct; the relative invariants paper contains little of that, is tied to the symplectic sum, and involves fraud. Both papers should be withdrawn. The longer this situation continues, the more public attention it will attract. If these papers remain in the Annals at this point, I believe many people would interpret this as an explicit endorsement of them by the current editorial board.

IP's corrigendum would at most address the most technical of the problems with the paper (C on p4 of my letter) by a completely different argument from the original, but not the other 3 general issues listed there or the dozens of smaller ones. For example, Professor Tian knows what the first sentence in the excerpt (basically issue A on p4 in the letter) means and he knows that it is not addressed by IP. I thus see no reason for me or anyone else to waste their time on IP's corrigengium. I highly doubt anyone other than me has actually read their symplectic sum paper in the 15 years it has been around.

IP's corrigendum is also an admission that what they had claimed was their main achievement in that paper (summarized by the second sentence in the excerpt) was in fact wrong. In their usual style, they did not even bother to acknowledge who had pointed out the errors they are addressing and play down the significance of the issue they are addressing. The sign error (mentioned in their abstract) destroys their original argument completely and that was the main point of their paper; the main statement of the paper (symplectic sum formula for Gromov-Witten invariants) had appeared well before their arXiv pre-print (and their statement is not even completely correct).

I suggested in my March letter having a videotaped discussion on their papers in Princeton if IP are not willing to withdraw the papers on their own accord. Since it is now clear to me that the authors will not take the responsibility for their papers and the Annals board is unwilling to force them to or to admit their own past failings, I will make my March letter public. If even this will not suffice for these papers to be withdrawn, I feel I would be left with no choice but to make my January letter public in due course. I have tried for 15 months to resolve this issue as discreetly as possible, but this has not worked. The past handling of their papers, in particular at the Annals, has led to IP engaging in outright fraud and to the near destruction of the field of symplectic Gromov-Witten theory (the two letters contain specific, verifiable examples of the former). I do not think IP should be encouraged to destroy whatever remains of this field.

Sincerely,
Aleksey