

On the Li-Ruan Situation
Aleksy Zinger 11/25/15

This document contains:

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Background

Below is some background on the present situation with the 2001 Inventiones paper [LR] on the symplectic sum formula for Gromov-Witten invariants by A.-M. Li and Y. Ruan.

I first brought up [LR] in a discussion with Ruan during a Fields Institute workshop in mid-October 2013; this discussion began with the 2003 and 2005 Annals papers, [IP4] and [IP5], by E. Ionel and T. Parker on the same topic. I told him that I had read [IP4, IP5] carefully (as some people had been asking me to do over the previous decade) and found a number of grievous errors that essentially invalidated the entire approach and that Mohammad Tehrani and I were writing an expository manuscript on this topic, part of which would discuss the IP work in an organized fashion for a streamlined discussion in the future. He seemed happy to hear this. I also told Ruan that for the sake of completeness I would carefully read [LR] as well and that Mohammad's view was that it did not contain much of a proof, something that had appeared to me to be likely after a preliminary look at his paper. He seemed to be taken aback by this, but assured me that *everything was there*, though with distinctly less confidence than usual.

I read [LR] carefully in January 2014 and was shocked by how little of a proof of the symplectic sum formula for GW-invariants or even of the construction of relative GW-invariants it contained. Contrary to the distorted spin on our comments in their arXiv response [LR'14], the issues we brought up were not about the amount of *detail*, but about the lack of most major *statements* needed to establish the claimed results. The arguments for the preliminary convergence statements (Section 3.1) and basic compactness arguments (Section 3.2) had plenty of details whenever they had been copied from Hofer's earlier papers, but they were not adjusted for the differences between Hofer's contact and LR's circle bundle settings. Whenever generally applicable, they could have been cut by two thirds (as is grudgingly acknowledged at the bottom of p12 in [LR'14]). On the other hand, there are cases when Hofer's arguments do not apply without LR realizing so (as in [LR, (3.55)], where the vertical distance is claimed to be controlled by the horizontal energy). However, the content of the entire Section 3 in [LR] is no more than a basic setup, about one-tenth of what is needed for the main claims of the paper. The only other parts that concern these claims are Sections 4 and 5; the remaining half of the paper is about applications. This is opposite to the situation with [IP4, IP5], which actually contain the major statements, but their proofs are completely wrong (in most cases).

Our comments on [LR] were incorporated into a long expository manuscript [FZ] on the symplectic sum formula in GW-theory. I emailed it to Ruan, IP, and several other people on March 14, 2014, just before the first SCGP workshop on *Moduli Spaces of Pseudo-Holomorphic Curves* (March 17-21); I could not find A.-M. Li's e-mail address at that point, but Ruan quickly forwarded the manuscript to him (as I had expected). My hope then was to have informal discussions on these issues during the workshop week. Mohammad and I did have some discussions with IP, but it became clear to me that they would not honestly admit the situation. Ruan simply refused to discuss [LR] with me. He said that he was working on different topics now, did not care about this paper anymore, but could not just withdraw it on his own. He told me that his intention was to extricate himself from this situation and let A.-M. Li deal with this. After the workshop, Ruan kept on trying to convince me not to post [FZ] on arXiv until A.-M. Li's reply was ready. Since it became clear to me that LR's intention was to follow IP's approach in not honestly acknowledging the situation, I posted [FZ] on arXiv on April 7, 2014.

LR's response [LR'14], authored by A.-M. Li under Ruan's authorization (in light of Ruan's decision to delegate the responsibility for the handling of this situation to A.-M. Li), appeared on arXiv on May 15, 2014. Even though I had delayed posting [FZ] by almost 2 weeks to give them more time to consider the situation *after* Ruan had refused to discuss [LR] with me at SCGP, [LR'14] begins by complaining that they had not been given the opportunity to discuss the issues in question privately (middle of p2). It consists primarily of derogatory comments about Mohammad and me (examples can be found in the e-mail beginning on p11), complete distortions of statements in [FZ], implicit claims of being experts in the *neck stretching technique* of the 1990s, and digressions of no relevance to whether [LR] contains anything resembling a proof of the symplectic sum formula (e.g. on the Chen-Ruan orbifold cohomology and FJRW-theory in the bottom half of p2). Nevertheless, LR want the *mathematics itself in this note to speak for itself* (top of p6). Many people can judge this "mathematics", as there is very little of actual mathematics in their *note*. I hope others will take 15-20 minutes to read LR's entertaining response to see what they consider great mathematics and what this might indicate about LR's paper itself (as well as possibly about the "mathematics" in the authors' other papers). LR's response with my occasional comments is available on my website.

The most mathematical part of [LR'14], the top half of p18, delivers an unequivocal double blow against [LR] (and a number of other pre-prints of A.-M. Li); it is far clearer than anything stated in [FZ] regarding [LR]. LR claim that the integral of something over a compactified moduli space, after taking a virtual class with their "integration over the top stratum method" depends only on the virtually main stratum. There are plenty of examples when this is not the case (e.g. the stable maps and stable quotients invariants of the quintic) and in general this statement does not even make sense. This is related to [LR] not taking Jun Li's maps into the singular fiber, but something which does not compactify the space of maps into the smooth fibers of a symplectic sum fibration. This is stated absolutely clearly at the top of p18. Remark 2.1.1 at the bottom of p6 in [LR'14] claims inventing Jun Li's compactification in [CLSZ] (10 years after the published version of [LR] and 13 years after the first arXiv of [LR] highlighted in A.-M. Li's e-mail on p7); this claim was implicitly repeated by Bohui Chen (A.-M. Li's co-author on [CLSZ]) in a videotaped talk at the second SCGP workshop on *Moduli Spaces of Pseudo-Holomorphic Curves* (June 2-6, 2014). Without having Jun Li's compactification or something even more elaborate, there can be no proof of the symplectic sum formula.

At the beginning of September 2015, A.-M. Li's name was added to the colloquium schedule at Stony Brook. I then realized that he was coming to Stony Brook for the SCGP workshop on *Toric Kahler Manifolds* (October 5-9); I do not know if he had known I was supposed to be away at the time he accepted Donaldson's invitation to this workshop. Nevertheless, I suggested to Fukaya and Hofer (one of the two managing editors at the Inventiones) that they host a discussion between A.-M. Li and me during his visit to SCGP (for which I would have come back from Bonn). Fukaya was receptive to having a discussion, as usual. Unfortunately, Donaldson indicated that such a discussion could have detrimental effects on the scheduled workshop, and so Fukaya did not go ahead with suggesting it to A.-M. Li. I contacted him instead myself (the e-mail on p6); A.-M. Li publicly refused to back LR's contemptuous arXiv response in a face-to-face meeting (e-mail on p7).

The paper [LR] essentially consists of two parts: theory (attempted construction of relative invariants and proof of the symplectic sum formula) and applications (changes in GW-invariants under some birational transforms of the target). Each of these parts is roughly 30-35 journal pages, very

lightly written. It is not in dispute that the applications part (about which no material questions are raised in [FZ]) was written by Ruan and the theory part by A.-M. Li. However, this paper is primarily cited for the symplectic sum formula (which the theory part was supposed to establish), while the applications part is rather minor and concerns very special cases later covered by [HLR]. As A.-M. Li and Y. Ruan are the authors of [LR] and have both refused to discuss this paper with me when presented with convenient opportunities, I will no longer differentiate between them in regards to [LR, LR'14]. In my view, both are 100% responsible for [LR, LR'14] for as long as [LR] remains in the Inventiones and [LR'14] remains on arXiv.

Ruan is quite open about not being an analyst and may not have looked at the theory part of [LR] at all. From his reaction to my comments on this part of [LR], it seems to me he never had full confidence in A.-M. Li either. While the latter implies being an expert in the *neck stretching technique* of the 1990s, none of his papers on *MathSciNet* appears to have anything to do with these techniques; most of them are on Riemannian and Kahler geometry. There were a number of Chinese mathematicians affiliated with A.-M. Li and Y. Ruan that attended the March'14 SCGP workshop. At the beginning of this workshop, some of them expressed admiration for my technical soundness and made dismissive comments about [IP4, IP5]. They were rather stunned when in response I mentioned problems with [LR] and e-mailed them the pre-arXiv version of [FZ]. However, I also understand that Ruan did not want to rely just on my view of his paper. So, I suggested asking another person who is considered very technically sound (including in the Chinese mathematical circles) and had read [LR], but was unwilling to say anything about it, except in response to direct questions. It appears Ruan was afraid to hear what that person might say and instead preferred to hide behind whatever actions A.-M. Li might take.

The double blow to [LR] delivered at the top of p18 in [LR'14] (see above) concerns two of the most fundamental conceptual issues for the purposes of [LR]; I have no doubt Ruan understands this. He may not have realized at first what A.-M. Li had written on his behalf, but this has been brought to his attention several times by now. While A.-M. Li did not appear to initially realize the significance of what he had written at the top of p18 in [LR'14], I explained this to Bohui Chen (who is at the same university) at the June'14 SCGP workshop and have no doubt that Bohui diligently passed this on to A.-M. Li. It is thus not completely surprising to me that A.-M. Li declined to have a discussion with me during his visit to SCGP.

It seems both authors realize the general situation with [LR] at this point and simply refuse to acknowledge it. I find this particularly sad in the case of Ruan. He has contributed enormously to GW-theory by bringing in ideas from physics ([LR] is not an example of this, as it arises from [T] and [CH]). However, this has nothing to do with whether [LR] contains anything resembling a proof of the symplectic sum formula. In my view, it has caused huge damage to the field. It has contributed to driving junior people from symplectic GW-theory by demonstrating to them that papers in this field submitted even to the Inventiones are judged not on their content, but on the popularity of the authors. It has also inspired IP's behavior, including their mostly wrong and partially fraudulent Annals papers and the outright fraud in [IP6] (as established at the March'14 SCGP workshop; the video of the relevant discussion session was vetoed by T. Parker). I do not find Ruan's reluctance to acknowledge the situation with [LR] overly convincing in regards to his confidence in the solidness of his other contributions, such as the FJRW theory, either.

Yongbin Ruan is currently the William Fulton Collegiate Professor at the University of Michigan.

Fulton is known for his impeccable integrity and willingness to mediate disagreements that do not involve him directly. In my view, this makes Ruan's approach to dealing with the present situation by hiding behind his co-author all the more inappropriate.

It is of course difficult for most people to judge where the truth in this situation lies. In conclusion, I want to point out that I wanted to present my views on the mathematics in [LR] in an open public discussion with them. A.-M. Li and Y. Ruan have declined to take part in such a discussion, even after dismissing me as ignorant in [LR'14].

P.S. All mentions of Li without indication of the first name in the following e-mails refer to An-Min Li of Sichuan University in China. He should not be confused with Jun Li of Stanford University, who has written a 160-page treatise [Lj1, Lj2] on the symplectic sum decomposition for the virtual classes of moduli spaces of stable maps in the algebraic category ([LR] and [IP4, IP5] claim much coarser results that concern only certain top intersection numbers on these spaces). While I have read LR's Inventiones paper and IP's two Annals papers carefully, my familiarity with Jun Li's two JDG papers is limited to their introductions (one of which contains two versions of the symplectic sum formula) and the definitions of relative morphism and morphism into the nodal fiber of the symplectic sum fibration in the algebraic setting. Both are now standard notions in GW-theory. The second one does not appear in either [LR] (where pairs of relative maps are used instead, as clearly stated in the top half of p18 of [LR'14]) or in [IP5] (where no \mathbb{C}^* -action on the rubber maps is taken, which leads to the infamous S -matrix at the top of p1003).

E-Mail 1

Date: September 16, 2015

From: A. Zinger

To: A.-M. Li

Cc: S. Donaldson, K. Fukaya, H. Hofer, G. Tian, D. McDuff, R. Pandharipande, J. Li, C.-C. Liu, J. Bryan, B. Chen, B.-L. Wang, X. X. Chen, S. Song, B. Lawson, R. Lazarsfeld, J. Starr, Y. Ruan

Dear Professor Li,

I am delighted you will be visiting SCGP for Donaldson's workshop on toric Kahler manifold October 5-9. I hope your visit will also make it possible for us to have a public, videotaped discussion on your 2001 Inventiones paper with Yongbin Ruan to clarify some issues regarding it and to help the field move forward.

Your arXiv posting, 1405.3821, makes it abundantly clear that you are certain that my views on your paper are wrong and that you would be delighted to demonstrate this publicly. While I obviously disagree with your views in 1405.3821, I do not doubt your sincerity. I am thus planning to come back to Stony Brook from Bonn for a few days to provide you with an opportunity to demonstrate that I am wrong about your paper.

I contacted Fukaya and Hofer last week suggesting that they organize a discussion on your paper at SCGP in the afternoon of Wednesday, October 7 (as SCGP workshops normally have no talks on Wednesday afternoons). As usual, Fukaya was very receptive to having a mathematical discussion and contacted Donaldson to confirm that Wednesday afternoon would be open. Donaldson was initially open to this proposal and still maintains that the Wednesday afternoon can be kept open. Unfortunately, X.X. Chen seems less certain of your sincerity in 1405.3821 and appears to have convinced Donaldson that you would not want to have a discussion on a paper you are so proud of.*

I hope you will confirm your sincerity in 1405.3821 by e-mailing Donaldson and me this week that you are willing to have a discussion on your 2001 paper during your upcoming visit to Stony Brook. I also hope he would then not object to this discussion being held under the auspices of SCGP in accordance with its mission to further the mathematical knowledge and understanding, especially in geometry related to physics.

Sincerely,
Aleksey

P.S. I am cc'ing this e-mail to a number of other people to leave no doubt that I have been trying to find ways to clear up the present unfortunate situation in a transparent and efficient manner, in spite of the reluctance/resistance by some other people.

*Please see E-mail 4 for a retraction/correction of this statement.

E-Mail 2

Date: September 18, 2015

From: A.-M. Li

To: A. Zinger

Cc: S. Donaldson, K. Fukaya, H. Hofer, G. Tian, D. McDuff, R. Pandharipande, J. Li, C.-C. Liu, J. Bryan, B. Chen, B.-L. Wang, X. X. Chen, S. Song, B. Lawson, R. Lazarsfeld, J. Starr, Y. Ruan

Dear Professor Zinger,

I like to thank you for your email, but I will not engage with you in any public discussion about my paper with Yongbin. This decision has nothing to do with my sincerity in my last year reply. As I don't think, the public (video-taped) discussion would result in any meaningful progress in our disagreement on our paper. Our paper has been in public domain for more than 17 years (since March 1998). After the first version appeared in arXiv, many people including Ionel-Parker and Jun Li have written research articles on the same subject. We are preparing an expository book in details to explain our work on relative GW invariants. At the moment, I have no time to satisfy your personal requirement during my short visit to SCGP.

That visit is to attend the workshop on Toric Kahler Geometry. Our whole focus is to present our work on the existence of extremal metrics in dimension 2 using new techniques from affine differential geometry. It would not be an easy task amid our jet lag to make our two long and technical papers understandable by the workshop participants, though we will try our best to achieve our goal. I certainly don't have any energy and time to discuss with you.

Best regards,
An-Min Li

PS: I know it is very inappropriate to cc this email to all of you who are on the distribution list of Zinger's email. I apologize for this mass mailing as it makes sense for those who like to hear my side of story.

E-Mail 3

Date: September 18, 2015

From: A. Zinger

To: A.-M. Li

Cc: S. Donaldson, K. Fukaya, H. Hofer, G. Tian, D. McDuff, R. Pandharipande, J. Li, C.-C. Liu, J. Bryan, B. Chen, B.-L. Wang, X. X. Chen, S. Song, B. Lawson, R. Lazarsfeld, J. Starr, Y. Ruan

Dear Professor Li,

Thank you very much for your response.

As you are probably aware from Bohui Chen, the “correctness” of Ionel-Parker’s VFC paper was sorted out in 1 hour at the SCGP workshop in March’14. So, it is quite possible we would have made good progress with your paper as well in a fairly short amount of time. All the issues concern the 30-35 lightly written pages by you, not the other half by Ruan. But of course, it is far easier to issue contemptuous arXiv responses than to stand by them in face-to-face meeting when such a convenient opportunity arises.

In contrast, Fukaya has been willing to engage in a discussion with anyone and in any format on a paper from the same time which is not even in the Inventiones (or any other paper of his for that matter). He (and McDuff) even organized a whole semester program and two workshops in large part to bring anyone interested to SCGP to raise questions about his work, in public or in private. In contrast, SCGP is now bringing you all the way from central China. While it is primarily for a workshop on a different topic, a minor discussion of relevance to the other area at SCGP would not have fundamentally interfered with your participation in this workshop and would have helped to clarify what you claim is your fundamental contribution to this area.

I do not know what the correctness of your paper has to do Ionel-Parker’s and Jun Li’s papers. While the 1st version of your paper has been on arXiv since March 1998, I am not aware of anyone willing to defend your paper in an open mathematical discussion. Unfortunately, few people in this field read papers; those who do and find problems prefer to keep quiet. Since you are a member of the Chinese Academy of Sciences, raising questions about your papers could potentially have dire consequences for anyone in China.

While you refer to the 1st version in your e-mail, even the 3rd one (from 09/21/98) makes no mention of the \mathbb{C}^* -action (p63), which is crucial to get codimension 2 boundary (as you highlight in your arXiv response); there is only \mathbb{R} -action as usual in the convergence to a periodic orbit. The additional S^1 -action appears only in the 4-th version, from 06/29/00; Ionel-Parker’s first version, from 07/21/99, already contains it.

Your own arXiv response says just about everything is standard and needs no proof (or even mention of a correct statement). The irony is that it delivers the **clearest** argument of all against your paper, in the top half of p18. You claim that the integral of something over a compactified moduli space, after taking a virtual class with your “integration over the top stratum method”, depends only on the virtually main stratum. There are plenty of examples when this is not the case (e.g. the stable maps and stable quotients invariants of the quintic) and in general this statement

does not even make sense. This is related to you not taking Jun Li's maps into the singular fiber, but something which does not compactify the space of maps into the smooth fibers. You state so clearly at the top of p18 and implicitly claim to have invented Jun Li's compactification in 1110.6803. This implicit claim was repeated by your co-author, Bohui Chen, in a videotaped talk at the SCGP workshop in June 2014 and corrected by me during the talk. Without having Jun Li's compactification or something even more elaborate, there can be no proof of the symplectic sum formula.

At Bohui Chen's suggestion to discuss "the mathematics in LR", we had a discussion about LR in my office in June 2014. It came down to him constantly moving away from what was actually done in LR to what could be done (which I knew without him). Even from this perspective, he was obviously not successful in explaining how invariants arising from a VFC would not depend on the compactification. He did try though, which makes me suspicious about the technical aspects of whatever appears in your (and his) VFC paper. The whole discussion took about half an hour. There was not much he could say in the defense of your paper itself, even though he is a co-author on 1110.6803 which heavily relies on it. I am sure this discussion took place at your request and was reported back to you, with no appropriate actions taken. While I had agreed to keep a discussion on "the mathematics in LR" private, that was on the condition that he would not be acting as your advocate or agent. Since he did not abide by this condition and it was not a discussion of "the mathematics in LR", I see no reason to be bound by keeping it private either.

It is a surprise to me that a mathematics member of the Chinese Academy of Sciences feels there is much "mathematics" in your arXiv response, as you claim at the top of p6. There is also 1501.01094, where you claim constructing open GWs without any topological restrictions on the Lagrangian (even the physicists do not believe this is possible). In light of no mention of the crucial \mathbb{C}^* -action in the first 3 versions of LR and the apparent belief that "compactifications" do not effect invariants with your "integration over the top stratum" VFC approach, the appearance of 1501.01094 is perhaps not too surprising. There is then 1507.05416, which you quickly withdrew after Siefring apparently pointed out to you that your main "theorem" was well-known to be false in a field in which you claim to be an expert (in your arXiv response). Both of these pre-prints are spinning the same types of arguments as LR. Whatever may appear in your book 15-20 years after your published paper will not change what is in your paper.

Your unwillingness to discuss your paper and present it to the whole world via video, when such a convenient opportunity arises, makes your confidence in your paper clear to me though.

Sincerely,
Aleksey

E-Mail 4

Date: September 22, 2015

From: A. Zinger

To: A.-M. Li, X. X. Chen

Cc: S. Donaldson, K. Fukaya, H. Hofer, G. Tian, D. McDuff, R. Pandharipande, J. Li, C.-C. Liu, J. Bryan, B. Chen, B.-L. Wang, S. Song, B. Lawson, R. Lazarsfeld, J. Starr, Y. Ruan

Dear all,

The purpose of this e-mail is to correct the statement I made that mentioned Xiuxiong Chen, specifically

Unfortunately, X.X. Chen seems less certain of your sincerity in 1405.3821 and appears to have convinced Donaldson that you would not want to have a discussion on a paper you are so proud of.

After further consideration, I realize that I did not have the information on the communications between him and Donaldson that would have implied that he had anything to do with Donaldson's view regarding the potential negative side effect of the discussion I had suggested on the SCGP workshop on Toric Kahler Manifolds. While Xiuxiong is the other organizer for this workshop, my statement should have been based on the principle the buck stops with the president (without implications on the overall merits of the relevant decision).

Therefore, I am retracting any mentions of Xiuxiong in this e-mail and sincerely apologize to him for bringing his name into this situation.

Sincerely,
Aleksey

E-Mail 5

The e-mail below is in response to an e-mail inquiry for specific statements in [LR'14] I have found insulting. Most of the omitted information below would have identified the recipient; the remainder (2 words) is omitted due to possible misunderstanding. None of the omitted information is material to the Li-Ruan discussion. The original recipient was not Donaldson, but I e-mailed the text below to him as an attachment on September 21 (without his request). The text in *italics* is directly from [LR'14].

Date: September 20, 2015

From: A. Zinger

To: [one person]

Dear [name omitted],

Li's arXiv response contains no words like moron or idiot. I would have ignored even some of the harshest individual phrases, such as the ones on p4, 5 and 8, but taken together they become a different story. Even Bohui did not disagree that Li's wording was insulting, but tried to defend him in June 2014 by saying his English is not perfect. It seemed fine to me in his e-mail. He wrote that e-mail in less than 2 days; the arXiv response was written in 2 months (contrary to his outright lie, he saw that 100-page manuscript one month before it was posted on arXiv; Ruan forwarded it to him).

The ending of the abstract is very mild compared to what is to come, but being in the abstract is indicative:

In [TZ], the authors made an effort in comparing the methods and ideas in [LR] vs [IP-1] [IP-2], but their criticisms on [LR] are based on their own lack of sufficient understanding of [LR].

Middle of p2:

When the article [TZ] was first circulated in a large mailing list, we informed the author our long version. They refused to consider it! Since the issue of enough detail is precisely the center of dispute, we question the authors fairness in treating our work.

According to LR, the "40 pages they were forced to remove" were about the contact setting. The 3rd version did not even have the \mathbb{C}^* -action without which there is no relative invariants (it was focusing on the contact case, which had already been done by Hofer, and there is only \mathbb{R} -action in that case).

Bottom of p3:

However, we feel that it is the challengers responsibility to actually be familiar with the technique we applied (a standard technique in 90s) and to understand our proof before making the judgment.

A fair statement, but not its implication. The part most related to "standard technique" is what he copied directly from Hofer and managed to mess up in the process. It could be cut by two thirds.

Middle of p4:

In fact, in this note we shall demonstrate that most of their complaints and criticisms of [LR] are resulted precisely from the authors lack of basic understanding of our approach. It will certainly take a while for the authors to really understand the paper [LR]. We sincerely hope that when they finally understand the main techniques in [LR]

He was invited to back his statement in a face-to-face meeting. I do not know about the Chinese norms, but his declining to do so (and not retracting his claim) would be an absolute ignominy under the European norms and clearly under the Japanese norms.

Middle of p5:

it is clear to us that T&Z either misunderstood or did not understand at all of these mathematical techniques. They often made ridiculous comments on the mathematics in [LR], even on some of materials that are already well known nowadays. For example, it is clear that T&Z are not familiar at all about the Fredholm analysis and the compact properties of the L^2 -moduli spaces when there are certain Bott-Morse type equations involved (cf. (LR4) and (LR5)).

The last part refers to statements that in principle required references or justifications. In practice, none of this was necessary if one does not copy blindly from the (more difficult) contact case in Hofer's papers. I wrote a 1.5-page proof of what he establishes in 5.5 pages. His response to this (bottom of p12) is that they did something more general, applicable to the contact case (but there is no contact case here).

Entire bottom half of p5 (too long to reproduce), but includes

they simple made their wishful and often ignorant judgments based on their self-claimed righteous mathematical viewpoint.

How does this sound? Regarding his comments on the expertise on p5, he makes it clear at the top of p7 and in the middle of p18 that he was not aware of the now standard notions introduced by Jun Li in 2000/01.

p8, middle:

We are shocked of this kind of naive viewpoint. This explained why they either did not understand or dont respect others work though it is clear that T&Z has learnt a lot from [LR].

p11, middle:

We think that T&Z totally misunderstood or could not follow our proof:

He then reproduces most of the relevant proof in the paper, starting exactly after the second of the two issues I explicitly pointed out in a remark.

p13, top:

It seems that T&Z didnt know what we needed later and didn't understand very well the standard elliptic estimates, even didnt understand well the results in [HWZ].

His response there explicitly agrees with the stated objection, except the latter is not reproduced.

p14, top:

It seems that T&Z didnt understand the standard bubbling construction in the literature very well. Rather strangely, T&Z commented that this is impossibly true.

The issue here that he does not get the difference with the contact case he copied from Hofer, where the vertical distance is indeed bounded by the horizontal energy. There is no connection between vertical and horizontal part in his case (this could be just a product).

p18, bottom:

T&Z seem always try to ignore any significant point in [LR] but focus on some non-essential points instead. Their attitude to the research paper under discussion is certainly unprofessional, considering that they got some key ideas from [LR].

He now contradicts his own abstract. So does 3.4.5 on p19 (but in the opposite way).

Li visited Beijing* last summer. There were many people there who realize that 1501.01094 cannot be right, and no one [2 words skipped] dared to sort this out with him. I know plenty of people who have not been afraid to ask Tian questions and argue with him [4 words skipped], but apparently people are completely intimidated by Li. [2 short sentences skipped] With my field, he has done huge damage (in part inspiring the later Ionel-Parker stuff) and is continuing to do so.

The structure of the symplectic sum formula was known by then (conjecturally); the only question was the proof. As you can see from the first two pages (his words and my response), he immediately deviates from the issues in question. At the top of p6, he says

We let mathematics itself in this note to speak for itself.

Many people can judge this “mathematics”, as there is very little of actual mathematics; most of it is complaints and derogatory statements. This is written by a mathematics member of the Chinese Academy of Sciences. Is this what constitutes mathematics to be proud of in the Chinese mathematical community? I know of a number of great Chinese mathematicians and lots of very good ones. I just hope Li decides to retire one day, stop bringing disgrace on the Chinese mathematical community (with things like his response, 1501.01094, and 1507.05416), and stop damaging my field further.

Best regards,
Aleksey

*BICMR

E-Mail 6

The excerpts below are from my e-mail in response to someone else's e-mail. The text *in italics* is what I am responding to.

Date: September 22, 2015

From: A. Zinger

To: [one person]

All I can say is that it is really unfortunate that he chose those wordings on p4, p5 and p8.

I think Li chose these words with the intent to discredit what we had written about LR, because there is nothing he can say of substance in its defense (and in fact says something more damaging to LR than what we had said without him realizing this). I hope someone reading his response will realize this and that in fact he discredits LR far more than we did. Perhaps he actually realizes the situation after talking with Bohui and knows what happened at the IP discussion at the VFC workshop and this is why he declined to have a discussion with me.

I had the impression that you are much more critical of IP than LR.

I think it is important to distinguish specific papers of authors from the authors. The long manuscript is more positive toward the mathematical approach in LR because it is earlier and more importantly I believe their idea of adapting Hofer's papers from contact geometry can be made to work. IP's approach works fine as far as the construction of relative invariants in semi-positive settings goes, but not for more general manifolds (when one has to do gluing) or the symplectic sum formula.

IP's VFC paper was the promised pre-print in their first Annals paper (a decade earlier) that was supposed to extend everything from the semi-positive case to the general case. However, it referred back to the Annals papers for the gluing they claimed would not be necessary because of their upcoming pre-print. They had of course known this and in fact had received my referee report 2 weeks before their mini-course at Fukaya's VFC workshop (I had been hoping they would withdraw their mini-course which I was "moderating" and save all three of us from what happened at the discussion session). Since they acted intentionally, this was fraud, no longer mistakes, however serious mistakes could be. They have since engaged into filibustering with the Annals, as the letter on my website points out.

The pre-arXiv version of the long manuscript contained no personal comments; parts of it contained comments about the LR and IP papers (the rest was discussion and proofs of some minor statements). The first arXiv version contains some comments on IP's behavior; these were added after their reaction to the pre-arXiv version. It appears Li also quickly realized that he could not fight that 100-page manuscript on mathematics and decided to turn this into acrimony. While Parker can undermine other people somewhat through journal/grant reviews (which he had already been doing anyway), he is not in a position to intimidate people like Li. With all their claims around 15 years ago, there was a lot of excitement about them, especially Ionel, but it has started to die down over the past 10 years (as Ionel's arXiv postings after 2003 might suggest). IP really did not have the standing to intimidate anyone, blatantly (like Li tried to do in his response) or quietly, even before the issues with their papers were made public. People do not speak of them with nearly the same care/fear they speak of Li.

My two cents is that both sides should excise maximum restraint in using emotional words and give the other the benefit of doubt.

I agree with this sentiment, but clearly not Li. The 100-page manuscript contains no emotions. Its Section 2 lists problems with LR and IP very specifically and points out to specific later remarks for even more details. Li's response is mostly emotions and no specifics, except for the item at the very end where he admits that the crucial multiplicity was not justified.

One can have doubts as to what the authors meant if there is something stated. LR does not contain even many crucial statements, never mind proofs. Li responds to such comments that "their" proofs are standard (but "they", i.e. the statements, are not even mentioned). The top half of p18 in his own response should leave no more doubt that LR did not contain even a setup for a proof of the symplectic sum formula.

It is my sincere hope that your relationship with [LR] has not reached the point of no return.

I am actually sorry for Ruan's situation in this regard. He has contributed enormously to this field by bringing in ideas from physics (LR is not an example of this, as it arises from Tian's earlier suggestion and Caporaso-Harris). However, technical things are not his thing, which he is open about, and he has had to reply on others for this. His papers with Tian from 20 years are one of the foundations of the field, but Li is clearly not on the level of Tian technically (whatever mistakes Tian may have made in the huge number of papers he has written). I do not know how solid that FJRW stuff is analytically. Fan did not manage to present even the background properly in Spring 2014. Ruan did not respond when I abstractly raised this concern about FJRW. With LR, he at least said that everything was fine, though with not much confidence, when I told him 2 years ago that I would read it after finishing comments on IP.

When he first saw that manuscript 1.5 years ago, Ruan was angry at me for a few hours, but then suggested an idea for solving a completely unrelated problem I had been occasionally thinking about. His proposal made complete sense, but it was too algebraic for my tastes, and I told him I was not going to do this and that he should suggest this to certain other people. I do not know if he did, but 1.5 years later some people did write up this kind of argument.

So, I feel Ruan has a broad vision, but is not into details, and he has had a huge positive influence on the field. Nevertheless, Ruan is an author on this paper; he cannot stay out of this and simply point at Li. I just hope someone can talk sense into Li so that he stops creating problems between Ruan and me; we are actually academic brothers. I would have also liked to be on collegial terms with IP and be able to discuss mathematics with them, but it is them who are making this impossible for now.

In symplectic geometry, [LR] is well established . . .

It is well established in the sense that people routinely cite LR along with IP and Jun's paper, without being specific and often for formulas that are not even claimed in LR or IP. Jun Li proves the symplectic sum formula on the level of homology classes; IP and LR claim it only on the level of numbers resulting from integrating certain cohomology classes over these homology classes. IP also contains S -matrix, which they do not compute outside of very special cases. Some people told me they realize this, but cite all three to be "nice".

Update 10/18/15

The reason A.-M. Li gave for declining my offer to come back from Bonn to Stony Brook during his visit to give him the opportunity to stand by [LR] and its scornful defense [LR'14] is that he would not *have any energy and time to discuss* [LR, LR'14] with me (see his e-mail on p7). This workshop had 3 one-hour talks on each of the five days. These included Donaldson's overview talk for the workshop and his talk in the Geometry and Topology seminar (which is supposed to be our *second colloquium*), A.-M. Li's talk overviewing his papers with B. Chen and L. Sheng and his colloquium talk, and the SCGP general audience talk by someone else. B. Chen and L. Sheng delivered the technical lectures on their joint papers; these papers are precisely in A.-M. Li's area of expertise (in contrast to [LR] and [LS1]), and not B. Chen's or L. Sheng's.

On 10/05/2015, Fukaya e-mailed A.-M. Li very respectfully inviting him for another visit to SCGP (all expenses paid) to discuss the mathematics in [LR] and [LS1]. The latter, which would have been an absolutely revolutionary development if true, claims to construct open Gromov-Witten invariants without any restrictions on the Lagrangian (not even in terms of the dimension); even the physicists do not expect such invariants above dimension 3 or in dimension 3 without severe restrictions on the topology of the Lagrangian. Fukaya may have sensed a trivial error in [LS1] based on a correct statement in the first page of the introduction. While both authors of [LS1] attended the SCGP workshop on *Toric Kahler Manifolds*, it appears that neither was willing to discuss [LS1] with him.

Fukaya's invitation to A.-M. Li was cc'ed to Y. Ruan, S. Donaldson, G. Tian, H. Hofer, D. McDuff, Jun Li, B.-L. Wang, and me. A.-M. Li declined this invitation. I do not know to whom he cc'ed his response, but this did not include me. Since Fukaya then responded to his own e-mail, rather than A.-M. Li's, I suspect that the latter had explicitly asked Fukaya not to share his e-mail with me so that I would not object to any statements in it. As far as I could tell from Fukaya's response, A.-M. Li said he would not discuss the mathematics in [LR] because my writings had been disrespectful and that he had no obligation to discuss a published paper. B. Chen, A.-M. Li's colleague who attempted to defend [LR] in June 2014, could not point out anything disrespectful in [FZ] and agreed that A.-M. Li's arXiv response [LR'14] was insulting (but attributed this to his English not being perfect); illustrations of the derogatory nature of [LR'14] appear in the e-mail beginning on p11.

People can make mistakes in their papers, even grievous and/or fundamental ones, while actually believing in their papers. A systematic pattern of mistakes can be damaging to others in the field and can indicate negligence on the part of the the authors. Papers of Y.-G. Oh and Fukaya-Oh-Ohta-Ono are well known in the field to fall into this category. However, to the best of my knowledge, their authors have always been willing to discuss them. Examples of this include the following.

- (1) In early February 2012, Y.-G. Oh implored me to read the n -th version of his attempt [Oh1] to prove the existence of super-rigid almost complex structures for Calabi-Yau threefolds ($n = 4$ as far as the arXiv postings go). Having seen some of the previous versions of [Oh1], I had finally agreed to do so only on the condition that he would withdraw it from arXiv and never post on this topic again if I found anything substantially wrong with it. Just like the previous versions, it contained nothing of substance and revolved around writing the linearization of a bundle section along the vanishing locus in different ways. Within 3 hours of the end of our discussion, he not only honored his end of the agreement, but also e-mailed to several people in the field honestly acknowledging the situation.

- (2) In February 2012, Y.-G. Oh agreed to a public discussion with D. McDuff on his n -th attempt [Oh2] to establish the non-simplicity of the group of area-preserving diffeomorphisms of D^2 (because the revolving format of these attempts, the number n is harder to determine in this case, but is somewhere close to 10). Similarly to the situation with [Oh1], this n -th attempt contained nothing of substance in terms of addressing the fundamental issue and revolved around a trivial error concerning measures. This discussion took place at IAS during the special year on symplectic dynamics and was attended by 30-40 people. Y.-G. Oh made no attempt to filibuster it.
- (3) There have been many concerns raised about [FO] since the late 1990s, generally behind the authors' backs and without specifics. H. Hofer and K. Wehrheim had been particularly vehement in dismissing [FO] and all other available approaches to constructing a virtual class for the moduli spaces of pseudoholomorphic maps and insisting on Hofer's polyfold approach as being the only feasible one. In order to ameliorate this situation K. Fukaya and D. McDuff organized a semester program at SCGP and two workshops in large part to bring anyone interested to SCGP to raise questions about [FO], in public or in private. Four hours at the first workshop were dedicated to [FO] and Fukaya-Ono did not try to avoid questions. K. Wehrheim declined the invitation to come for the whole semester and canceled her brief visit scheduled for the first workshop. H. Hofer had nothing to say about [FO], beyond the misstatements Fukaya-Ono had already acknowledged.
- (4) Many papers of Fukaya-Oh-Ohta-Ono contain (or contained in earlier versions) problems of varying significance. These have ranged from the claim in [F, Section 8.2] to construct real genus 0 GW-invariants without a topological assumption ruling out sphere bubbling, which was later retracted, to the statement of [FOOO, Theorem 1.1] which corrects a significant flow in the version from the late 1990s.

While the outcomes in the four cases above were different, the authors had genuinely believed in their work, were willing to discuss it, and retract or correct any portions. Thus, they cannot be accused of engaging in fraud by insisting on statements they knew to be wrong.

A.-M. Li and Y. Ruan's refusal to discuss the mathematics in their papers suggests to me that they are engaged in mathematics primarily for the sake of playing politics and advancing themselves, not mathematics. Meanwhile, they have continued to produce new papers, including [CLSZ] and [LS1], that are based on [LR]. For the reasons described in the paragraph on page 3 of these notes concerning the top half of p18 in [LR'14], it is no longer a possibility that they do not realize that [LR] contained little toward a proof of the symplectic sum formula. Thus, they insist on statements in their papers that they know to be wrong. This is particularly disturbing in the case of Y. Ruan, the William Fulton Collegiate Professor at the University of Michigan, i.e. a holder of a professorship named in honor of someone known for his impeccable integrality.

I find it sad that the supposed leaders in this field (such as Y. Eliashberg, H. Hofer, D. McDuff, D. Salamon, C. Taubes, G. Tian) had been aware of problems with [LR] and [IP4, IP5] for a decade before [FZ], appear to be perfectly fine with the present situations with these papers and the tactics of the authors to dealing with them, and seem to want me to shut up.

E-Mail 7

Date: October 28, 2015
From: A. Zinger
To: Y. Ruan
Cc: G. Tian, H. Hofer

Dear Yongbin,

It has now been over a month since A.-M. Li publicly declined to back up your scornful arXiv response regarding your 2001 Inventiones paper in a face-to-face meeting and I still have not heard anything from you regarding this. Li's schedule at the SCGP was pretty light, with 3 talks per day in the 5-day workshop, including several overview talks. He gave only an overview talk on Tuesday and a colloquium on Thursday, both using slides. His two co-authors gave more technical followup talks. As you probably know, he has also declined Fukaya's very respectful invitation to have a discussion on your paper and his open GWs pre-print in the future, apparently claiming he had no obligation to engage in a discussion on a published paper and that my wordings had been rude. Bohui was unable to tell me what was offensive in the long arXiv manuscript, but did agree that your arXiv response was offensive (using Li's "poor English" as an excuse).

As you might recall, you declined to discuss your paper with me more than 2 months before that arXiv response, even privately, and kept on pressuring me to delay posting that manuscript so that your co-author could prepare a "response". I find it sad that you feel that your response has any mathematical content in the defense of your paper. Until recently, I had been separating your and your co-author's contributions to your paper. However, it is a joint paper and the part in question is by far the substance of the paper. I will no longer separate you and from A.-M. Li in regards to this paper. It is really none of my business which of you typed which words in that paper (or did not type words that should have been typed).

I respect the enormous contributions you have made to the development of GW-theory and related fields, like FJRW, by bringing in ideas from physics. I also appreciate that you suggested to me an approach to computing the genus 1 stable quotients at that SCGP workshop even while being angry about that manuscript. As I told you then, I was not going to work on it, but Bumsig Kim did so in 1506.03196 (I do not know if he heard this from you; there is no acknowledgment). However, all of this has no connection as to whether your 2001 paper contains much of substance. I feel your approach to handling the present situation is unethical and does not reflect the integrity that might be expected of someone holding the William Fulton Collegiate Professorship.

Contrary to the misrepresentation at the top of p2 of your paper, Tian had suggested such a formula precisely in the setting considered in your paper. For non-transverse contacts, the multiplicity coefficients had been provided by Caporaso-Harris well before your paper (as IP explained quite well). The purpose of your paper was to prove a version of the symplectic sum formula. You never really cared about its proof, as you basically told me, only about applications, but I am sure you realize that the two statements made so clearly in the top half of p18 of your arXiv response (attached) imply that your paper fundamentally contained no proof of the symplectic sum formula.

So, you **know** what the situation is, but are refusing to admit it. I suggested 1.5-years ago that

you check with Melissa regarding your paper. I suspect you did not do so because you were worried to hear her response and had never really trusted Li's competence. People make mistakes; the willingness to acknowledge them is not a sign of weakness, but of confidence. Holding on so heavily to something so questionable as your 2001 paper and 2014 arXiv response might start raising doubts about the significance and solidness of your other contributions. Why do you need this?

I tried to sort out the mathematics in LR with each of you (including almost a month before posting anything to arXiv). You refused to do so, even after your indignant arXiv response. With your approach to this situation, you are damaging not only the field, but yourself. Had you acknowledged the situation honestly 1.5 years ago, it would have looked like an extremely gracious act demonstrating your confidence in your other work (and would have saved other people lots of time). You instead had Li demonstrate what you consider to be "great mathematics" in that arXiv response. Your acknowledging the situation now would still look reasonably gracious and confident of your other work, but the time for this may be running out. Your 2001 paper drove some people from this field and encouraged IP to produce their mostly wrong and partly fraudulent Annals papers and almost completely fraudulent VFC preprint; I hope you will now take this opportunity to lead them by example in the opposite direction. I will soon start taking steps to bring more attention to this situation unless this situation is resolved. I am making this last attempt to try to move forward as amicably as possible because I respect your contributions to the development of this field.

Best regards,
Aleksey

E-Mails 8,9

Date: October 28, 2015
From: Y. Ruan
To: A. Zinger
Cc: G. Tian, H. Hofer

Dear Aleksey,

I found your message offensive. As a general policy, I will not respond to any personal attack since it will only invite further personal attack. Therefore, I will not respond to your current message. In fact, I will refuse to read any message containing personal attack!

In the future, if you have any mathematical issue you want to talk to me, you are welcome to do so. However, I have several rule of engagements: (1) No personal attack such as accusation on "unethical"; (2) No wild speculation such as "I suspect you did not do so because you were worried to hear her response and had never really trusted Li's competence". Namely, I want to keep it completely mathematical. If you feel that you can do so, you are welcome to write to me. To avoid the misunderstanding, please put "No personal attack" on the subject. Otherwise, I will delete your message.

By the way, I have had an extended career in mathematics. There is a lot of stuff in public domain for people to exam my character and my mathematics. If University of Michigan someday feel that I am unfitted to be William Fulton Collegiate Professorship and decide to have a committee to exam my case, you will have my strong recommendation on such a committee, where it seems to be a more proper place to air your concern.

Best, Yongbin

Date: October 28, 2015
From: Y. Ruan
To: A. Zinger
Cc: G. Tian, H. Hofer

To add to my previous message, please send your future email communication to me only, no mailing list. I found that the discussion on mailing list is more about scoring point in public opinion than real mathematics.

Best, Yongbin

E-Mail 10

Date: October 29, 2015
From: A. Zinger
To: Y. Ruan
Cc: G. Tian, H. Hofer

Dear Yongbin,

Thank you for your response, though I had not hoped for something more honest.*

- (1) You completely refused to discuss your 2001 paper with me during the SCGP workshop in March 2014 and delegated the response to A.-M. Li, in particular authorizing him to act on your behalf regarding this paper.
- (2) You kept on asking me to delay posting 1404.1898 to arXiv until your response was ready. I did so by 3 weeks primarily to give you time to consider the situation. If not for you, I would not have done so.
- (3) Your response, 1405.3825, contains no mathematics in defense of your paper, consists primarily of personal insults, and explicitly claims (on p2) that you had not been given the opportunity to discuss your paper before 1404.1898 was posted. Examples of the insults begin on p10 of the LiEmails file attached to my previous e-mail.
- (4) I doubt you followed my suggestion to check with Melissa in Spring 2014. You seem to implicitly dispute this assumption in your e-mail, but do not state so explicitly. You yourself admitted that you were no analyst and did not care about the proof.
- (5) Your co-author refused to discuss your paper with me during his visit to SCGP a month ago, making up some pathetic excuses, even after your scornful response and even though I had offered to come back from Bonn exclusively for this purpose. I know you were aware of this.
- (6) Your co-author refused to engage in any discussion with me in the future and in particular declined Fukaya's very respectful invitation to come back to discuss your 2001 paper and his recent arXiv pre-print in the same spirit (in which Fukaya himself is interested). I suspect you were aware of this as well.
- (7) A personal attack is what you are doing in 1405.3825. I did not use such language, before or since then. You are welcome to bring up any specific insults and derogatory statements in my last e-mail.

Which of the above statements are false?

Regarding some of the mathematics in your paper and response, the top half of p18 in the latter clearly states that

- (1) you do not use a compactification of the space of maps into the symplectic sum fibration.
- (2) with your "integration along the top stratum" VFC approach, the invariants do not depend on the virtually codim 2 boundary strata.

*This phrase had been intended to be read *though I had hoped*, but I doubt this had an impact on Ruan's response.

You are no analyst, but I am sure you realize that there can be no proof of the symplectic sum formula without (1) and that (2) is simply nonsense. Which statements in this paragraph do you disagree with?

Most people cannot read most of your mathematical papers too easily, but they can read 1405.3825 and see for themselves what you consider to be great “mathematics” (as you suggest at the top of p6).

Regarding your second e-mail, I am not trying to score any points, but have simply been trying to sort out the present situation over the 1.5 years. It is you, your co-author, and IP who have scored lots of points by being good at mathematical politics and have driven out pretty much everyone else from symplectic GW theory as a result. I want open mathematical discussions, not backroom dealings, at which all four of you have been so successful at the cost to the field overall. I tried to communicate with you personally in Spring 2014, during and after that workshop. You used this opportunity to delay me from posting 1404.1898, then claimed in 1405.3825 that you had not been offered the opportunity to discuss your paper before it was posted on arXiv, and engaged in personal attacks.

I would be happy to discuss the mathematics in your paper and arXiv response in a public, video-taped discussion, just like Dusa did with Oh at IAS in February 2012, during the symplectic dynamics program there. However, I will not engage into any more private discussions with you, at least until this issue is settled. The previous e-mail was my last attempt to resolve this situation reasonably quietly; I had cc’ed that e-mail to only two people (both very relevant). You immediately brushed off my attempt. It would have been easier for me to not even try, but I had been hoping for some honesty from you in regards to this particular situation; it could have been beneficial to all involved and the field more generally.

Sincerely,
Aleksey

E-Mail 11 and Response

The text in small print below is my response to Ruan's e-mail. Since I told Ruan that I would no longer engage in personal communications with him until the present situation is resolved and he claims he would ignore any e-mail from me cc'ed to someone else, I see no reason to respond to the statements made in his e-mail directly and instead comment on them below. November 10, 2015

Date: October 30, 2015

From: Y. Ruan

To: A. Zinger

Dear Aleksey,

I saw your last email and deleted it because you cc to someone else. As the rule of engagement I set in my previous email, I will not read any of your email sent on a mailing list because it is more about scoring points in public opinion than real mathematics. If you want to talk to me, send it to me alone. I have always enjoyed to talk to you.

I doubt that someone as politically savvy as Ruan would not have wanted to know what someone else in the field is saying about him, especially to people like Tian and Hofer. I also have no reason to believe that he did not bcc his response to them (or some other people), especially since my own e-mail had been cc'ed to them. I have likewise always enjoyed discussing mathematics with him, but unfortunately he has refused to discuss the mathematics in [LR] with me since March 2014 and has not made a single statement about [LR] since then that could be considered *real mathematics*. Li-Ruan's scornful response [LR'14] to the purely mathematical statements in [FZ] contains almost no mathematics and is precisely an attempt to *score points*.

I understand your frustration. Everyone does. But this is not the reason to insult or threaten people. I am annoyed by your threat to attack me. I am sorry that I am not responding to any threat.

I appreciate Ruan's understanding, but would have much preferred more honesty from him in handling this situation, at least after March 2014. My communications with him contained no *insults* or *threats to attack* him, but only an advisement that *I will soon start taking steps to bring more attention to this situation unless this situation is resolved* (see the e-mail on p18). If there is nothing for him to hide about this situation, Ruan should welcome any additional attention.

Mathematics is better discussed in private. Going public created so much hostility which will make situation so much more difficult to resolve. It should be reserved as last mean.

Mathematical *ideas* might sometimes be best discussed in private. The substance of mathematical *papers*, especially those published in the very top journals, should be sorted out in open public discussions, not through backroom dealings. I certainly feel that it is appropriate to advise the authors of any concerns privately first and I had done so well before posting [FZ]. Unfortunately, the only response from Li-Ruan was a flood of insults in [LR'14] with a small supplement in Ruan's recent e-mails. Their tactics are now forcing me to start down the path of *last means*.

This was the reason that I tried to get you to delay your posting to give ample time to resolve the difference in private. After all, this is an old paper and you spent a year to read it.

This is a clear indication that Ruan never had a clue as to what the half of the paper written by A.-M. Li actually contained or paid any attention to my comments about it over the past 2 years. It is 30-35 very lightly written journal pages which purport to contain a complete construction of relative GW-invariants and a proof of a symplectic sum formula for GW-invariants; J. Li's two papers are over 160 pages and build on a vast amount of literature in algebraic geometry. It should not take anyone in the field more than a few full days to read through this part of [LR] completely and to realize that it contains little towards these aims. I advised Ruan in Toronto in mid-October 2013 that this was Tehrani's view as well as my initial impression after a preliminary look, but I also told him that I would read [LR] carefully later on. I did so in January 2014, compiling notes along the way. It did take me about a year to read [IP4, IP5] as they do contain a lot of technical statements, which just happened to be pretty much all wrong.

Just out of respect, you should have given Anmin Li an equal amount of time to prepare and respond and ideally in many rounds. I even enlisted Rahul to help me. You didn't want to listen (one month is not "ample"). Your refusal to give Anmin Li even a chance to respond as well as considering our long version made Anmin Li very angry.

I am responsible for my own arXiv postings and have no obligation to have someone else preview them (except for my coauthors) and prepare responses to them. In my view, [LR] is such a disgrace from the mathematical standpoint that it would have been ethically right for me to post [FZ] without even giving the authors time to consider the situation. The time I did give them, about a month since the pre-arXiv version of [FZ] and 6 months since my discussion with Ruan in Toronto, was certainly plenty to decide how they felt about their paper and the merits of the objections to it raised in [FZ]. Their arXiv response [LR'14] expresses unconditional confidence in [LR], completely dismisses (pretty much) all issues raised in [FZ] regarding [LR], and thus makes it abundantly clear that they had plenty of time for this decision. I had no more obligation to provide Li-Ruan with unlimited time for a response to [FZ] than anyone else posting anything on arXiv has an obligation to provide someone else with time for a response.

I don't think that he has any trust that he will get a fair treatment from you (as well as in Simons center).

Even A.-M. Li's own excuse for declining to back [LR'14] in a face-to-face meeting was not that pathetic. He would not have gotten any *treatment* from me. His only *treatment* by SCGP would have consisted of an all-expenses-paid trip to the US from central China in order to promote his work. He already got one such trip in October 2015 from the SCGP workshop budget. Fukaya generously and respectfully offered another from his own research budget, including to discuss A.-M. Li's recent preprint [LS1]. The discussions would have been videotaped and posted online unedited by SCGP staff with no mathematical understanding of the situation. They would have provided him with an opportunity to demonstrate that the objections about [LR] raised in [FZ] were unfounded and that his construction of open GW-invariants in [LS1] was correct; it seemed he would have jumped on this opportunity if he sincerely believed in his work. Oh accepted an opportunity for such a discussion on [Oh2] with McDuff at IAS in February 2012, while Fukaya (along with McDuff) organized a whole semester and two workshops at SCGP so that anyone interested could raise questions about [FO]. While the outcomes in the two cases were opposite (see p16), both sincerely believed in their work; this does not appear to be the case with Li-Ruan.

Your arrogance and disrespect to people created all these messes which we now have to dig out.

Ruan's arrogance assertion contradicts his stated belief that I spent a whole year studying [LR] and the fact I had taken the time to produce detailed mathematical comments on it. No one has provided me with a single example of disrespectful statement in either [FZ] or in any of my e-mails. In contrast, [LR'14] contains hardly any mathematical statement and primarily consists of complaints and insults; illustrations of these are provided in the e-mail beginning on p11. Finally, this *mess* was clearly created by [LR] in the first place and further exacerbated by Li-Ruan's dishonest response to the issues raised

in [FZ].

I left symplectic geometry almost twenty year ago and viewed myself only as a bystander in the current fight. Now you dragged me into these messy.

A *bystander*? Ruan is one of the two authors of the paper in question. If [LR] had been actually refereed, he might have been just another professor at Wisconsin now, instead of the William Fulton Collegiate Professor at Michigan. As he himself admits, he actually did not have much to do with the part of [LR] purporting to establish its main claims and has little idea of what it contains. He is now blaming me for actually reading his paper, instead of his coauthor for doing little of what should have been done or himself for failing to familiarize himself with the main part of [LR] or finding a more competent coauthor in the first place. If Ruan does not want to deal with this situation, he could simply retract [LR, LR'14] and be done with it, instead of hiding behind his coauthor with whom he seems to be on fraternal terms.

I had hoped Ruan would lead Ionel-Parker by example by honestly acknowledging the situation with [LR]. He has instead decided to adopt their tactics by engaging in outright lies with his twenty years statement. The paper [LR] was published 14 years ago; its earlier versions did not contain the crucial \mathbb{C}^* -action on the rubber maps. Ruan's most recent work is clearly related to GW-theory, which is fundamentally part of symplectic topology. According to *MathSciNet*, Ruan had two publications [CoR, PaRY] in orbifold GW-theory and five [HLR, HR, tLR1, tLR2, RoRS] in *symplectic* GW-theory since 2008 (i.e. in the past 7.5 years, perhaps 10 years since their initial completion); [CoR] and [HR, tLR2] were published in 2013 (i.e. less than 3 years ago). The papers [HLR, HR, tLR1, tLR2] are closely related to [LR]. Is Ruan suggesting that he did not actually work on any of these 7 papers, but perhaps just made a vague related suggestion and his more junior colleagues felt obliged to include him as a coauthor? Does this really rise to the level of integrity that could be expected of the William Fulton Collegiate Professor?

Nevertheless, we do have to find a solution that everyone is comfortable about it. I believe that there is one. But it will take time for people to get their emotion down and start to reasoning.

The Li-Ruan paper has greatly contributed to the destruction of the field of symplectic GW-theory since its posting on arXiv 17.5 years ago. It has been over 1.5 years since fundamental problems with [LR] have been brought to Ruan's attention in a carefully written manuscript containing no emotions. This may be a short time for someone in a comfortable permanent position, but is a good chunk of the time a postdoc or a graduate student has to complete work before applying for jobs. With junior people basically unable to stay in this field, it is not surprising it has almost died out. Ruan did not contact me in the month since his coauthor (to whom he had referred me for any problems with [LR]) refused to speak with me during his upcoming visit to Stony Brook. As far as I can tell, Ruan is simply hoping that this issue would disappear and does not care about the enormous damage [LR, LR'14] have already caused and are continuing to cause to the field.

In particular, I hope that you will get your emotion down. Name calling is not going to solve the problem.

As a start, it seems to me that Li-Ruan need to realize that it is them who need to *get emotions down* and stop engaging in *name calling*. They have not provided a single example of *emotions* or *name calling* in [FZ]; I think I managed quite well not to indicate my complete shock that someone could feel that [LR] contains reasonably good mathematics, let alone is suitable for *Inventiones*. Li-Ruan's arXiv response [LR'14] is mostly *emotions* and *name calling* (examples appear in the e-mail beginning on p11), and Ruan's own e-mails contain these also.

I believe that Anmin did not shut down Fukaya proposal completely. He is just not comfortable about the setting in Simons Center. One possibility is to find a neutral place.

A.-M. Li response to me (p7) made it clear that he would not engage in a discussion with me, either at SCGP during his October visit or anywhere else at a later time. I do not know for certain what his e-mail declining Fukaya's invitation said. As far as I could tell from Fukaya's response, A.-M. Li said he would not discuss the mathematics in [LR] and that he had no obligation to discuss a published paper. I suspect that he had explicitly asked Fukaya not to share his e-mail with me so that I would not object to any statements in it. Li-Ruan could dispel any uncertainty about the content of this e-mail by A.-M. Li's asking Fukaya to make it public.

Why cannot Ruan himself discuss [LR] with me though? As an author, he is 100% responsible for every statement in this paper.

I am organizing a workshop a week from now in Beijing to understand Gukov's recent exciting proposal for a new invariant on 4-manifold and will be very busy in next two weeks.

It looks like Ruan will have another *I left this field almost twenty year ago* excuse if in 5-10 years someone actually reads his foundational work on FJRW theory and realizes it contains little solid mathematics beyond translations of physics.

Afterwards, I will go back to US for a week. Let's talk then. We can even talk over the phone.

It looks like Ruan has not even noticed that I am not currently in the US, as I have mentioned several times over the past two 1.5 months, and has not yet realized that I would no longer engage in contact with him without additional people involved (at least until the present situation is resolved). I no longer have trust in his integrity, as his e-mail clearly puts the present situation on its head.

Best, Yongbin

More on Ruan's work and first e-mail

Date: November 10, 2015

In his e-mail on p20, Ruan says

I have had an extended career in mathematics. There is a lot of stuff in public domain for people to examine my character and my mathematics.

He has certainly had a lot of publications since his PhD in 1991, 48 according to *MathSciNet* with a large number in the very top journals. The question is how many of them were actually *read*, as opposed to just *looked at*, and how much of the mathematics in them he contributed and can explain in a discussion, as opposed to having just contributed some vague ideas inspired by physics.

I believe [RT1, RT2] have been well studied over the past two decades. A quick look at random parts of these papers and [LR] would indicate a sharp contrast in how these papers are written. The former do not look like the first paper written by a graduate student with no prior technical writing experience. They also contain fairly technical analysis. It thus seems pretty clear that just about anything of mathematical substance, as opposed to general discussion, in these papers was written by Tian and that Ruan has little understanding of much of these papers, even though he might have gotten Tian interested in this subject in the first place.

During graduate school, I read [KQR] in detail. This paper caused me major stress and took several months of my time just before I needed to apply for postdoctoral jobs. It applied the approach of [P] for counting genus 1 curves in $\mathbb{C}P^2$ by determining some degenerate contributions to counting genus 2 curves in $\mathbb{C}P^2$. An approach to counting genus 1 curves by determining some other degenerate contributions is suggested in [I]. I used the principles behind the latter approach in genus 2, but the numbers I obtained for $\mathbb{C}P^2$ differed from those in [KQR]. Fortunately, my computer program generated these curve counts for $\mathbb{C}P^2$ and $\mathbb{C}P^3$ and the first two nonzero numbers turned out to be the same, as the case had to be for geometric reasons. This suggested to me that something was wrong with [KQR] after all. After reading it carefully, I realized that the authors determine the contributions from some of the boundary strata and then simply claim the remaining strata do not contribute, without even describing these strata. As shown in [Z], one remaining stratum does in fact contribute and is the only contributing stratum fundamentally different from the genus 1 case of [P]. Even though [KQR] contained no new idea beyond [P] and carried out no fundamentally new computations, it appeared in a far better journal. When I brought the problem with [KQR] to Ruan's attention, he immediately directed me to one of his coauthors (who agreed with my correction). As [KQR] did not go far beyond [P] in genus 2, it appears Ruan's only contribution to this paper was to suggest applying the approach of [P] and he cared little about how this was actually done.

At the March 2014 workshop at SCGP, Bohui Chen (M.S. student of A.-M. Li and PhD student of Ruan) claimed that he had read [LR] and felt it was fine. When we actually started to discuss [LR], there was nothing he could say in its defense and *read* became that *he was not an author of this paper and did not read it carefully*. However, he is an author of [CLSZ], which is an orbifold version of [LR] and relies heavily on the arguments (not just statements) in [LR]. None of the other 60-70 workshop attendees, including a significant number closely associated with A.-M. Li and/or Ruan, claimed familiarity with this paper.

I am also somewhat familiar with [HLR]. Z. Tian, then a student at Stony Brook with whom I talked regularly, found a mistake in this paper. When he contacted Ruan about this, the latter immediately directed him to T.-J. Li (who agreed with the mistake and quickly fixed it). Given the referee (whom I happen to know), this paper might well be essentially fine. However, it is a fairly straightforward application of the triangular transforms principles of [MP]. The referee actually sounded somewhat hesitant about its suitability for the *Inventiones*; I suspect that an unbiased algebraic geometer familiar with [MP] would have been even more hesitant in this regard.

The pattern with [RT1, RT2], [KQR], [LR], and [HLR] has been the same: Ruan seems to provide the inspiration and then relies on others to carry it out mathematically. Sometimes the others do so, sometimes not; Ruan seems to care little about this part. Whenever any mathematical questions arise about his papers, he seems to have little idea of what they are about and refers them to his coauthors. I hope someone will actually read the foundations of the FJRW and GLSM theories carefully in not too distant a future to avoid problems later. Whatever the situation is with his other papers, Ruan has refused to discuss the mathematics in [LR] with me and is hiding behind the actions of his coauthor.

In spite of how negative the above sounds, I actually feel that people with Ruan's intuition about which ideas from physics can and should be adopted in mathematics can bring immeasurable benefits to mathematics in general and my field in particular. However, they can become destructive when they rely on others to realize their intuition and brush off any objections raised without understanding what the others did on their behalf. People coming from other fields, claiming to be experts in a given field, and using this to dismiss objections to their work can be even more destructive (A.-M. Li had not done any work with *stretching the neck* techniques or in GW-theory before [LR] and he is obviously no universal geometer like Donaldson, Tian, or Yau).

Instead of engaging in verbal altercations, Ruan could confirm his integrity and mathematical competence by simply participating in a public videotaped discussion with me on the mathematics in [LR, LR'14], of which he is so proud. He has refused to do so and is continuing to hide behind A.-M. Li.

E-Mail 12

Date: November 12, 2015
From: A. Zinger
To: W. Fulton

Dear Professor Fulton,

As you might have heard, there is an unpleasant situation between me and the holder of the professorship which is supposed to honor you. It is detailed in the attached file LiRuan111115. My view, detailed in math/1404.1898, is that Ruan's paper on the symplectic sum formula for Gromov-Witten invariants contains nothing resembling a proof of this formula. This paper is under 70 journal pages, very lightly written, and about half of it is fairly minor applications (Jun Li's two papers are 160+ fairly dense pages).

I e-mailed a pre-arXiv version of 1404.1898 to Ruan just before an SCGP workshop in March 2014. While at SCGP, Ruan completely refused to discuss any mathematics in the main part of his paper with me and said he would have his coauthor respond to me instead. He also made it clear that he did not write this part and has little understanding of it. I had realized this and at that point simply wanted to explain to him what is going on in there (without expecting any answers on the spot from him). None of the other 60-70 participants in this SCGP workshop, which included many associates of Ruan and/or his coauthor, was willing to discuss the mathematics in this paper with me either.

I delayed posting 1404.1898 to arXiv for 3 weeks after this workshop to let Ruan consider the situation and suggested that he also check with Melissa about his paper. He wanted to delay me from posting it until his co-author could prepare a response on their behalf. That response, 1405.3825 (LiRuanResp attached with my comments), came a month afterwards. It contains almost no mathematics and consists primarily of complaints and insults (examples on pp11-13 of LiRuan111115). The little mathematics it does contain, the top half of p18, makes it abundantly clear that the original paper could not possibly contain a proof of the symplectic sum formula (for the reasons explained in the middle paragraph on p3 of LiRuan111115). A lot of people can actually read this response and see what Ruan and his coauthor consider to be good mathematics.

In early October, A.-M. Li attended an unrelated SCGP workshop. I e-mailed him about a month in advance offering to come back from Bonn so that we could have a public videotaped discussion on the mathematics in his paper with Ruan (after their dishonest and insulting arXiv response I had no intention of engaging in private discussions with either of them). He declined, making up a pathetic excuse (these e-mails were cc'ed to a number of other very senior people). He also declined Fukaya's later invitation to come back at a future time, including to discuss his recent preprint on open GW-invariants. As far as I could tell from Fukaya's response, Li claimed he had no obligation to discuss a published paper (but I cannot tell what he really said, as he did not cc the response to me).

Ruan was cc'ed on all these e-mails, but still did not contact me himself to try to make other arrangements to resolve this situation. Instead I e-mailed him advising that I intended to start bringing more public attention to the present situation unless it is resolved. He brushed this off.

While he mentioned that something needs to be done, this was rather vague and is based on his expectation that A.-M. Li would have a discussion with me. His e-mail also put the whole situation upside down, claiming that I need to calm down and set the emotions aside. He claimed he would delete any e-mail from me cc'ed to someone else, after I told him that I would not engage in personal contact with him until this situation is resolved (i.e. without cc'ing my e-mail to someone else). After what happened over the past 1.5 years with this paper, I simply do not trust him anymore.

This situation was originally only about the mathematics in one of Ruan's many papers and I was willing to discuss it privately at first. I still want to discuss the mathematics in this paper, but now only in a public, videotaped format. However, I also feel that Ruan's hiding behind the actions of coauthor is disgraceful. In my first recent e-mail to Ruan, I said

I feel your approach to handling the present situation is unethical and does not reflect the integrity that might be expected of someone holding the William Fulton Collegiate Professorship.

His response to this was

If University of Michigan someday feel that I am unfitted to be William Fulton Collegiate Professorship and decide to have a committee to exam my case, you will have my strong recommendation on such a committee, where it seems to be a more proper place to air your concern.

My interpretation of this is that he would be happy for the UMich Math Department to review whether his integrity rises to the level expected of the holder of this position at any time of its choosing and he would recommend that my input be considered. For as long as this is not done, he feels he has the full backing of the UMich Math Department. I have now let you know about this situation and earlier Mustata (in response to his referee request).

This is of course a complicated situation. In my view, the most efficient, transparent, and fair way to resolve it would be a public videotaped discussion between Ruan and me on the mathematics in a paper Ruan is so proud of; he and his coauthor have refused to do so and are blaming me for essentially reading their paper. I hope you will take a look at his last e-mail and my response to it on p23 of LiRuan111115 (attached) and will encourage Ruan to take responsibility for his paper.

Thank you very much,
Aleksey

E-Mails 13,14

Date: November 16, 2015
From: Y. Ruan
To: A. Zinger

Dear Aleksey,

I am back to US now. Do you have a phone number so that we can talk over the phone? When will it be a good time for you?

Best, Yongbin

Date: November 17, 2015
From: A. Zinger
To: Y. Ruan
Cc: G. Tian, H. Hofer, W. Fulton

Dear Yongbin,

As I mentioned in my previous e-mails a few times, I am in Bonn now.

I tried to sort this out with you privately, over 1.5 years ago, but all I got in response was 1405.3825 and your recent e-mails. You call me arrogant and disrespectful without any indication why and immediately say that I need to get my emotions down and that name calling is not going to help. I feel your e-mails put the situation completely on its head and blame me for actually reading your paper. My response to your last e-mail begins on p23 of the attached file.

For these reasons, I no longer feel comfortable discussing anything with you without involvement of at least two more senior people. This can be done over skype as a conference call with such people, but I would record it (if I can figure out how). Otherwise, we can discuss it over e-mail (which I would prefer), but I would cc all my e-mail to people of my choosing. The purpose of this is not to score points (as you say), but to keep a record, including possibly of their unwillingness to do anything in regards to this situation.

Sincerely,
Aleksey

E-Mail 15

Date: November 17, 2015
From: Y. Ruan
To: A. Zinger, K. Fukaya
Cc: G. Tian, H. Hofer, W. Fulton, W. Fulton

Dear Aleksey,

I am disappointed that you don't want to discuss with me in private to try to find a solution comfortable with everyone. Mathematics is done by human. As human being, it is important that we have a friendly research environment. This includes to try to resolve the difference as much as one could in private. This was why I tried to get you delay the posting of your long paper in order to give Anmin Li a chance to respond in private. I also asked Rahul to help because you are close to him and may listen better from him. Now we end up with this mess.

I still believe that the best route to get out this is to take it private again. If you change your mind, I am always available to talk.

Best, Yongbin

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