In addition to the in text replies, a summary is attached to the end.

REMARK ON SYMPLECTIC RELATIVE GROMOV-WITTEN INVARIANTS AND DEGENERATION FORMULA

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ABSTRACT. In this note, we give item-by-item responses to the criticisms raised in [TZ] by Tehrani ad Zinger on our paper [LR]. We illuminate the main ideas and contributions in [LR] in section 2, itemize the responses to issues raised in [TZ] and conclude that we have provided a complete proof of the degeneration formula in our published paper [LR] and its more detailed versions in arXiv. 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].

There is not much content in [LR] to understand or not.

The conclusion seems to be that everything "is standard" and no proof (or even mention of the key statements) is needed.

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1. Introduction

More than fifteen years ago, Yongbin Ruan and I developed a theory of relative Gromov-Witten invariants and degeneration formula (see [IP-1] and [IP-2] for a different approach and readers may refer to Remark 2.3([TZ]) for a detailed documentation). Since then, our formula has been applied and tested many times, for example, an algebraic treatment of our theory was developed by Jun Li [L]. Recently, there was an article [TZ] casting certain doubt of our results. As far as we understood, the author did not question the correctness of our result as well as the effectiveness of our approach. The dispute is if we provided enough detail which qualifies it as a complete proof. Incidently, we do have a much longer version of paper available online which is predated the published version and contains much more details. For the published version, the referee forced us to cut off 40 pages of material which he considered to be standard. Namely, our long version was considered to contain too much details. Ironically, fifteen years later the readers of different generation complains that our shorter published version has too few details. 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 author's fairness in treating our work.

Nevertheless, we feel that it is our responsibility to answer any questions anyone may have for our work. This is the purpose of this article. Originally, we hope to result the difference through a private discussion. Unfortunately, we were not given such an opportunity. We regret that we have to response to the article [TZ] in public. When the paper [LR] was in preparation, Yongbin Ruan was in the process of moving to other areas of mathematics. Since then, he has invented several important areas such as Chen-Ruan cohomology and FJRW-theory. The theory of relative Gromov-Witten theory and degeneration formula was written up by myself which I will take full responsibility for its correctness and completeness. Instead of dragging him back from his current important works, it is more appropriate for me to respond to all the criticisms in [TZ].

[LR] is about GW theory from symplectic side, for which the standard background was (and still is) McDuff-Salamon and Ruan-Tian.

1.1. Background. Our paper was written more than fifteen years ago for a different generation of mathematicians. Every paper assumes reader's familiarity of certain basic or standard material. Our paper is no exception. To help current younger generation to understand our paper and its production, it is very important to discuss the background of our paper and what was considered to be standard material then. Our approach was an adaption of the so-called *neck stretching technique.* This technique has been developed in gauge theory in the late 80 and early 90's under the name of analysis on manifolds with cylindric end or L^2-moduli space theory. Floer homology is such an example. When our paper was prepared, this technique was already quite standard in the gauge theory community. There are several books on this technique, for example, two books on L^2-moduli space theory by Mrowka-Morgan-Ruberman([MMR]) and by Taubes ([Taubes]). Donaldson's beautiful manuscript on Floer homology was available to the public (the actual book [D] was published in 2004) . Our paper was prepared between 1996 and 1998. We certainly assumed readers' familiarity in some basic or standard knowledge of this neck stretching technique. Nevertheless, in our first version of the paper, we did provide a rather complete version on this technique in Gromov-Witten theory.(cf. [LR] Version 1.) *This is irrelevant as far as (the published version of) [LR] goes.*

After our paper was submitted to Inventiones mathematicae, it went through a long refereeing process and several revisions were produced. One of disputes with referee is exactly that we want to assume less background on the neck stretching technique while he/she wants to assume more. There is no way we can foresee that next generation of mathematicians does not know so much about this standard technique. We welcome any effort to rewrite our theory in the new language which young people are more familiar with it. However, the article [TZ] is different. Namely, they challenged the completeness of our proof. However, we feel that it is the challengers' responsibility to actually be familiar with the technique we applied (a standard technique in 90's) and to understand our proof before making the judgement. In this response, we discuss this issue in details in §2.4 and §3.1.

This is no reason for not even mentioning key intermediate statements with appropriate citations (never mind their proofs).

We are not really concerned with the virtual stuff regarding this paper. From our point of view, this is a completely separate issue.

Another crucial technical issue in [LR] involved is to define invariants using the virtual neighbourhood technique. Unlike the L^2-moduli spaces in the Yang-Mills theory, which had been well estabilished by that time, the developement of virtual techniques for Gromov-Witten theory was just at the early stage, and even after 15 years' today. That is why SCGP (Stony Brook) has a half-year program on the foundation of Gromov-Witten theory. As we know, there had been several different approaches at that time, such as Fukaya-Ono([FO]), Li-Tian([LT]), Liu-Tian([LiuT]), Ruan([R]), Siebert([S]), just to name a few. In [LR], we provide a completely new approach to this issue: we show that the invariants can be defined via the integration on the *top* stratum in the sense of virtual neighbourhood. It turns out that this new approach is very effective in many later applications. Here again, we feel that the authors of [TZ] are not very familiar with this new viewpoint and hence have no idea of the efficiency and hence the correctness of our approach in [LR]. We will discuss this issue in more details in §2.2.

The article [TZ] posed 16 specific questions. After studying their questions carefully, we concluded that our proof is COMPLETE. In fact, in this note we shall demonstrate that most of their complaints and criticisms of [LR] are resulted precisely from the author's 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] and will be able to rewrite in their own language, they won't claim that they provide a complete proof of those theorems in [LR].

In spite of agreeing with the rather important point (LR15) on p19.

If we (or others) produce a complete proof, then it is a complete proof

2. Response

These were statements.

In this section, we will answer explicitly the 16 questions posed by the authors of [TZ]. We remark that when we were preparing [LR], as a standard practice, we write it as a research paper rather than a textbook. Hence, we illuminate the proofs in [LR] that we believe is enough for experts to understand, not to provide a training wheel for all those not familiar with the knowledge accumulated over the last decade. Any detail that can

[LR] does not even contain key intermediate statements, never mind proofs.

Most of [LR] is a hodgepodge of random statements; a lot like the first paper by a graduate student without technical writing training.

be routinely filled would not be presented here. Here we focus mainly on the new ideas in [LR].

We like to point out that comments of Tehrani and Zinger (T & Z for short), (LR1)-(LR16) (P15 in [TZ]), may be classified as the following three types.

(1) comments that are related to standard materials to the subjects, e.g (LR4), (LR6), (LR9), (LR11), (LR12);

Definition of relative map, which has no substance as written.

(2) comments that are on some minor typo or overlooks that can be easily fixed by diligent readers, e.g, (LR1), (LR2), (LR5), (LR15);

The key multiplicity issue.

(3) comments that are on the mathematical techniques developed in [LR]. Reading [TZ], 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)). Other similar comments include (LR3), LR(7), LR(8), (LR10), (LR13), (LR14) and (LR16).

There are no such techniques developed in [LR]. It is all "standard" according to this note.

Since T & Z make many incorrect comments even on what we believe had already been quite standard materials by the time when the paper [LR] was written, we can't help to question their expertise on this topic to judge the correctness of the paper [LR]. Moreover, on the main contributions of [LR] to the development of the degenerate formulae and their application to symplectic topology and birational symplectic geometry, we remake that

There is no proof of anything major to understand; this note says it is all standard.

(1) T & Z understand neither the approach in [LR] nor the essence of proofs therein;

(2) they simple made their wishful and often ignorant judgements based on their self-claimed righteous mathematical viewpoint.

We made no objection regarding the content of the applications to birational geometry of their proofs, all of which are due to Ruan.

There is NO mathematics in this note. People should read [LR]; it is easy!

Due to these, we will begin with an outline of the approach in [LR] to the symplectic sum formula and highlight the new points of that paper in §2.2,§2.3 and §2.4. We let mathematics itself in this note to speak for itself.

2.1. **Outline of approach to symplectic sum formula ([LR]).** We use the same notations as in [LR]. Let (M, ω) be a compact symplectic manifold of dimension $2n + 2$, and $\widetilde{M} = H^{-1}(0)$ for a local Hamiltonian function as in the beginning of Section 3 in [LR].

free

Under the assumption that the Hamiltonian vector field X_H generates a circle action on a neighborhood of $\widetilde{M}$, there is a circle bundle $\pi : \widetilde{M} \to Z = \widetilde{M}/S^1$ with a natural symplectic form τ_0 on Z. We assume that $\widetilde{M}$ separates M into two parts to produce two cylindrical end symplectic manifold M^+ and M^-. Collapsing the S^1-action at the infinity, we obtain the symplectic cuts $\overline{M}^+$ and $\overline{M}^-$, both contain Z as a codimension two symplectic submanifold. We also consider the limiting manifold M_∞ as we stretch the neck along $\widetilde{M}$.

To obtain and prove the symplectic sum formula, we began with the following strategies.

(A) we relate the Gromov-Witten invariants of M with that of M_∞(cf. Theorem 5.6 in [LR]), *To define these, one should in principle start with (\ov{M}^+,Z), not a symplectic cut.*

(B) then we relate the Gromov-Witten invariants of M_∞ with the combination of relative invariants of $(\bar{M}^\pm, Z)$ (cf. Theorem 5.7 in [LR]).

Note that (A) and (B) yields the symplectic sum formula. For this purpose,

(C) We introduce the relative moduli spaces for symplectic pairs $(\overline{M}^\pm, Z)$ (cf. Definition 3.14 in [LR]) and the moduli spaces on M_∞ (cf. Definition 3.18 in [LR]);

(D) Then we define the invariants for these moduli spaces, in particular, including the relative GW invariants of $(\overline{M}^\pm, Z)$.

We will recall the main ideas to get (C) and (D) in §2.3 and §2.2.

Remark 2.1.1. We would like to mention our work on relative orbifold Gromov-Witten theory([CLSZ]). In [CLSZ], we employ a different approach to get the symplectic sum formula for orbifold Gromov-Witten invariants. Instead, for a degeneration family of

symplectic orbifolds, we construct a degeneration family of moduli spaces of pseudo-holomorphic curves. In [CLSZ] we then adapt an integration argument to conclude the symplectic sum formula easily. On the moduli space level, T & Z's approach in [TZ] seems very similar to the approach in [CLSZ].

2.2. **The approach of defining invariants.** In [LR], we introduce a *new* approach for the definition of Gromov-Witten invariants, which is not same as those virtual fundamental class approach in the existing literature. By refinement of estimates of gluing maps, we showed that these invariants can be defined via the *integration* on *top stratum* of the moduli space. As we know, the common way to define invariants is by using the intersection theory. Of course, one envisages that one can define the invariants by considering certain *virtual* intersection theory for the top stratum. A dual approach is to define invariants via the integration, for example, see P. 227 in [MS-2]. However, in order to make sense of the integration theory, one needs to establish the smoothness of compact moduli space.We know that the smoothness of the space has only been achieved very recently by several groups (including my recent joint work with Bohui Chen and Bai-Ling Wang). However, in this paper, we shall *avoid* the smoothness at lower strata, and prove that "the invariants defined by integration can be obtained by integrating virtually on the top stratum". We believe that this is a highly nontrivial and very useful statement (see the following Remark). *If so, what is the point of the joint work mentioned?*

Remark 2.2.1. When we compare invariants mentioned in (A) and (B) (in §2.1), we only need to compare the top strata of moduli spaces in the virtual sense. *If they are obtained consistently.*

We now outline this key idea in [LR]. Let $\overline{\mathcal{M}}$ be a compactified moduli space of the top stratum $\mathcal{M}$. Set $\partial\mathcal{M} = \overline{\mathcal{M}} \setminus \mathcal{M}$. For simplicity, we first assume the regularity holds for $\overline{\mathcal{M}}$ and suppose that $\partial\mathcal{M}$ is of codimension ≥ 2. By gluing maps, we may give local coordinate charts for neighborhoods of any point $x \in \partial\mathcal{M}$. At the bottom of Page 204 in [LR], we explained that the integrand, which is *well defined* on $\mathcal{M}$, behaviors well near ∂M with respect to the local coordinate charts mentioned above. Hence, the invariants

can be defined via the integrations on $\mathcal{M}$. In fact, such a strategy is commonly used for singular spaces. In order to achieve this goal, we provided much more *refined* estimates for differential $\partial/\partial r$ of gluing maps, for example, see Lemma 4.7, 4.8, 4.9 and 4.13 in [LR]. All these estimates were not appeared in any literature. We would like to point out that in recent work of FOOO on the smoothness of moduli space, they also consider the estimates of the similar type.

> According to [LR, Proposition 4.10(2)], the evaluation map is a pseudo-cycle. Thus, the invariants can be defined by intersection. The integration argument is based on similar principles.

Remark 2.2.2. We remark that if $\overline{\mathcal{M}}$ is not regular, we should apply the virtual neighborhood technique, and then apply the above argument to the virtual neighborhood. T & Z commented (LR16 in [TZ])) that the above approach is not necessary since one can take the approach of intersections etc. We are shocked of this kind of naive viewpoint. This explained why they either did not understand or don't respect others' work though it is clear that T & Z has learnt a lot from [LR].

> Thanks for bringing [H] and [HWZ1] to our attention.

2.3. **Relative moduli spaces and relative Gromov-Witten invariants.** In [LR], we introduced the relative moduli space of stable maps and define the relative GW invariants.

Let (M, Z) be a symplectic pair as in [LR] and let $\mathcal{M}$ be a relative moduli space. By the time the paper was written, it is well known that the compactification argument from contact geometry would yield a codimension one boundary. This issue is first resolved *in* [LR] by introducing the $\mathbb{C}^*$ action on the space of maps to rubber components. For example, one might compare with [IP-1] which is the first version of IP's series. In [IP-1], they did not acturally provide the precise construction of relative moduli spaces. However, from their statements, their invariants are only well defined up to chambers. This is exactly due to the boundary of codimension one issue.

Once we have this key observation, it remains to build up the moduli space $\overline{\mathcal{M}}$ following the following standard steps:

(E) compactification (§3([LR])): we adapt the standard L^2-moduli space theory that has been intensively developed for Chern-Simons theory, for example, we follow

> The C^*-action is also key to Hausdorffness, as shown shown in J. Li's work, but its role in Hausdorffness is not even mentioned in [LR].

> True. However, in the first 3 versions of [LR], equivalences on relative maps do not involve C^*-action either (only R); see p63 in the 3rd version, for example. The 4th version appeared after the first version of IP's relative paper, which has the \C^*-action.

closely with <u>Donaldson' book</u> ([D]). More details will be discussed in §2.4 and §3.1; *Mentioned in passing only once in the paper (top of p198).*

(F) regularization(§4.1([LR])): we use Ruan's argument to build up a global regularization;

(G) gluing theory(§4.1([LR])): the standard package of gluing theory consists of the following steps

(G1) the construction of gluing maps,

(G2) the injectivity and surjectivity of the gluing maps. *Never explicitly mentioned.*

Not in the relative case, where the gluing parameters cannot be chosen arbitrarily.

In [LR], we explained (G1) mainly for one *relative* nodal case, and we think that the generalisation to lower strata are already standard. (cf. §3.2.3). With understood, we focused instead on *Nothing new; correct statements are from [H] and [HWZ1].*

(G3) the refined and <u>new estimates</u> for gluing maps, which was of importance to our approach of defining the relative invariants (cf. (H) below and §2.2);

(H) to define the invariants(§4.2([LR])): this already explained in §2.2; and again we emphasize that this depends heavily <u>on our refined and new estimates for gluing maps (G3).</u> *Not much of this.*

2.4. L^2 **moduli space theory and Bott-Morse type Morse theory.** The L^2 moduli space theory is perfectly suitable for moduli spaces modeled on cylinder end domain. It relates to study gradient flows of certain infinite dimensional Bott-Morse function. In our case, this function is the smooth functional $\mathcal{A}$ on the smooth Banach manifold $W_r^2(S^1, \tilde{M})$ (cf. §3.1in [LR] for the definition of $\mathcal{A}$).

Remark 2.4.1. In the first version of the paper (see [arXiv 9803036v1],P52), $\mathcal{A}$ is defined up to constants $\tau_0(T^2)$. Due to the opinion of referees, we cut the paper into this new shorter version, in this version we only use a local version of $\mathcal{A}$. In fact, mathematically, the global one is more convenient for applications. *Then, why did you switch?*

Using the standard arguments, the crucial part of the L^2 moduli space theory is to develop the Morse theory of $\mathcal{A}$ in the sense of Floer. In [HWZ], authors studied the case

The only purpose of 6.5-page Section 3.1 of [LR] is Theorem 3.7 . The correct and applicable version of this theorem can be established in 1.5-2 pages by adopting, instead of copying, the reasoning in [H] and [HWZ1]; see Proposition 6.8 and its proof in [TZ].

that the critical points are isolated. While in our case, we show that $\mathcal{A}$ is of Bott-Morse type. Hence the main goal of §3.1 in [LR] is to generalize the theory of HWZ to the case of Bott-Morse type. The key ingredient is Proposition 3.4 in [LR] which enabled us to prove Theorem 3.7 in [LR].

Remark 2.4.2. T & Z questioned about Proposition 3.4 and Theorem 3.7 in [LR]. All of their questions can be solved by standard infinite dimensional analysis for the Bott-Morse type functional. We will explain in more details in §3.1.

3. RESPONSE TO TZ'S COMMENTS

In this section, we respond to the comments (LR1-16) of T & Z on [LR] according the topics discussed in the the previous section.

3.1. **Compactification.** We comment on (LR4-8) in this subsection.

As explained in §2.4, the compactification of the relative moduli space depends heavily on the study of gradient flow equation of the functional $\mathcal{A}$, which is of Bott-Morse type. T & Z questioned this soundness of this technical part, in particular, their comments (LR4) and (LR5) directed at Proposition 3.4 and Theorem 3.7 respectively. We suspect that T& Z are not familiar with those standard analysis involved with the study of L^2 moduli spaces.

3.1.1. On (LR4): T& Z complained that (1) we use the L^2 inner product to define a Riemann metric on the Banach manifold $W_r^2(S^1, SV)$; and (2) the infinite dimensional version of Morse lemma is used.

Response: *Nothing wrong in principle, but a justification (or citation) should have been included in [LR].*

(1) we have no idea what is wrong with L^2 inner product. In fact, this is *widely* used, and, for example, cf. the equation in Page 32 of [D], or Lemma 2.1.1 in [MMR].

(2) the argument using Morse Lemma is also standard. For example, one may be referred to Page 29 in [D] to how this is applied for Yang-Mills Floer homology. We

thought and still think that this part of the infinite dimensional Morse theory is well known. There is no need to explain further here about T& Z's complaints.

3.1.2. *(LR5).* On (LR5): T & Z commented that Theorem 3.7 in [LR] (1) incorrectly stated; (2) the proof including circular reasoning; and (3) the theorem can be justified in a few lines. They further claimed that the statement after the proof of [LR, Theorem 3.7] does not make sense, because the constants there depend on the map $C \to \mathbb{R} \times \tilde{M}$. In Proposition 5.8 of [TZ] the constant is C_u which is depending on u.

Response:

Author's typo? in a rather important place.

(1) Lemma 3.7 is a local theorem concerning with the behaviours of u near ∞. We note that $\mathbb{C}$ in the statement of this Lemma should be $\mathbb{C} \setminus \mathbb{D}$. From the context of our paper it is easy to see that this is clearly a editorial typo (in fact, we had $\mathbb{C} \setminus \mathbb{D}$ in the first version of our paper [arXiv 9803036v1], Page 29 Theorem 3.7 in [LR]).

But not in the third.

(2) We think that T& Z totally misunderstood or could not follow our proof: (cf. Remark 5.7 in [TZ]) T& Z said that we presupposed "the flow stays within a small neighborhood $O_{x,\epsilon}$ when time large". We certainly did not assume this, instead, this is exactly the main goal of the theorem. The statement was stated right after (3.29) in [LR] and was proved by the contradiction argument based on (3.22) in Proposition 3.4([LR]) (we copy this part below). In fact, this is the main technical issue for Bott-Morse type gradient flows, though the argument was already standard for not-Morse type functions in gauge theory (cf. [D],[MMR]).

We recite our proof in [LR] from line 17 Page 175 to line 8 Page 176 below:

> *We show that for any C^{∞}-neighbourhood U of $\{x_k(t+d),\ 0 \leq d \leq 1\}$ there is a $N > 0$ such that if $s > N$ then $\tilde{u}(s,.) \in U$. If not, we could find a neighbourhood $U \subset O$ and a subsequence of s_i (still denoted by s_i) and a*

This skips the top of p175, where the functional A first appears in this "proof".
By the middle of p171, A is defined only locally. This also skips (3.25), where we could not see the second equality, but that is details. Remark 6.7 (previously 5.7) explicitly refers to the skipped part of the "proof".

sequence s_i' such that

(3.30) $\tilde{u}(s,.) \in O \ \ for \ s_i \leq s \leq s_i',$

(3.31) $\tilde{u}(s_i',.) \notin U.$

By Lemma (3.6) and by choosing a subsequence we may assume that

$$\tilde{u}(s_i', t) \to x'(k't) \ \ in \ C^\infty(S^1, \tilde{M})$$

for some k'-periodic solution $x'(t) \in O$. We may assume that O is so small that there is no k'-periodic solution in O with $k' \neq k$. Then, we have $k' = k$, $x' \in S_k$. We assume that $\tilde{E}(s) \neq 0$. From (3.29) we have

$$\int_{S^1} \tilde{d}(\tilde{u}(s,t), \tilde{u}(s_i,t)) dt \leq \frac{1}{C} \left(\tilde{E}(s_i) \right)^{\frac{1}{2}},$$

where $\tilde{d}$ denotes the distance function defined by the metric $g_{\bar{j}}$ on Z. Taking the limit $i \to \infty$, we get

$$\int_{S^1} \tilde{d}(x'(kt), x(kt)) dt = 0.$$

It follows that

$$x'(kt) = x(kt + \theta_0)$$

for some constant θ_0. This contradicts (3.31). If there is some s_0 such that $\tilde{E}(s_0) = 0$, then $|\Pi\tilde{u}_t|^2 = |\Pi\tilde{u}_s|^2 = 0 \ \ \forall s \geq s_0$. We still have a contradiction.

(3) Then the argument for Theorem 3.7 can be applied to the case that $\mathcal{A}$ is Bott-Morse type, which certainly *generalized* the result in [HWZ]. If the case that the contact manifold $\tilde{M}$ is a circle bundle of a line bundle, the similar theorem can be proved in a rather easy way, for example, this point of view was employed again in [CLSZ] and was

In [LR], \tilde{M} is a circle bundle and a short simple argument should have been given, instead of copying from [H] and [HWZ1].

also used by T& Z. We remark again, the way we adapt is suitable for more general cases for contact geometry.

(4) It seems that T& Z didn't know what we needed later and didn't understand very well the standard elliptic estimates, even didn't understand well the results in [HWZ]. In fact, let $C - D_1$ be a neighborhood of ∞ and $u : C - D_1 \to \mathbb{R} \times \widetilde{M}$ be a J-holomorphic map, put $z = e^{s+2\pi it}$, such that

It is stated below (3.41) in [LR] that C_r are constants (no dependence of them is mentioned) .

(i) $E_\phi(u) \le b$;

(ii) $|a(s, t) - ks - \ell_0| \to 0$, $|\theta(s, t) - kt - \theta_0| \to 0$, $|y(s, t)| \to 0$, as $s \to +\infty$;

(iii) $u(s_0, t)$ lies in a compact set $K \subset R \times \tilde{M}$ where s_0 is a fixed point in $[1, +\infty)$.

Then the constants C_r in our paper depend only on b and K. This follows from the standard elliptic estimates and the estimates in [HWZ]. This uniform bound is certainly necessary for the gluing theory in [LR].

3.1.3. On (LR6) and (LR8): T& Z commented that (1) the compactness argument in [LR] is vague on the targets; (2) a special type of maps, "contracted rubber map", is ignored.

Response: *This has different presentations too; see Section 3.0 in [LR], especially the top of p169.*

(1) <u>From the context we actually assume that the target of the sequence is M^+</u>; the general cases can be dealt with similarly (for example, similar situations in Floer theory are well known); *No mention in the paper though (or here).*

(2) "the contracted rubber map" is *never* appeared for the cases studied in [LR]. As we always considered the map to $\mathbb{R} \times \widetilde{M}$, which can never be a contracted rubber map with only one puncture/node at one of the divisors, even in the compactification.

3.1.4. On (LR7): T& Z questioned (3) of Lemma 3.11 in [LR]. They commented that "...in contrast to the setting in [H, HWZ1], the horizontal and vertical directions in the setting of [LR] are not tied together"(cf LR7) "... the claim of [LR, Lemma 3.11(3)] in fact cannot be possibly true"(cf. Remark 4.5([TZ])).

Then the compactification is wrong, because such maps are part of the relative moduli space; see figure.
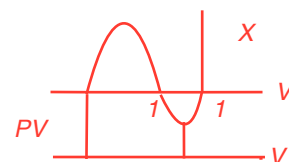
The validity of Lemma 3.11(3) clearly depends on choice of the rotation phase t_0 in (3.53); it cannot hold for all t_0. (3.55) bounds vertical distance by horizontal energy. This bound holds only for the horizontal distance.

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Response:

Our proof of (3) in Lemma 3.11([LR]) is correct. This is a standard geometry consequence from "no energy lost" in bubble tree argument and is proved by studying the energy on the "connecting neck" between two bubbles (for example, Lemma 4.5.1 and Page 57 in [MS-1], §6 in [MMR]). It seems that T&Z didn't understand the standard bubbling construction in the literature very well. Rather strangely, T&Z commented that this is impossibly true. We sketch a simple argument below.

By our construction (see (3.53) in [LR])

$$v_i(r, t) = (b_i(r, t), \tilde{v}_i(r, t)) = (a_i(log\delta_i + r, t) - a_i(log\delta_i, t_0), \tilde{u}_i(log\delta_i + r, t)).$$

This means that $\{u_i(-m, t), \ t \in S^1\}$ and $\{v_i(m, t), \ t \in S^1\}$ is connected by a tube. As $i \to \infty$, the connecting tube gives a gradient flow from one critical point $\lim_{s \to -\infty} \tilde{u}(s, t)$ to another critical point $\lim_{r \to \infty} \tilde{v}(r, t)$. We have proved that the energy of the flow is 0, hence these two critical points are identical (in $W_r^2(S^1, \widetilde{M})$).

3.2. **Gluing theory.** We answer (LR10-12) in this subsection. Before we start with individual questions, we would like to comment on two general concerns raised by T&Z. The first concern is the gluing theory for stable maps in rubber components. We understand, as we introduce an $\mathbb{C}^*$ action on moduli space on rubber components, it is fair to ask for a treatment accordingly. In fact, this is done by taking slices with respect to the action (cf. **2.** in Page 188 ([LR])). Probably, T&Z did not spot this?! The second concern by T&Z is how to generalize the gluing construction from one nodal case to general cases, which they think might be nontrivial. We will explain this point in §3.2.3.

3.2.1. (LR10) comments on the gluing when the rubber components involved: (1) the gluing is not up to $\mathbb{C}^*$ action; (2) Remark 5.11([TZ]) concerns the target spaces with respect to the gluing parameters, in particular, when more than two gluing parameters are involved.

are involved.

Response:

(1) For this case, the slice is constructed to deal with the $\mathbb{C}^*$ quotient.

In Page188 of [LR], from Line 26 to Line 31 we wrote: *Nothing about slices here.*

> **2:** $N = R \times \tilde{M}$. *We must modulo the group C^*-action generated by the S^1-action and the translation along R. We fix a point $y_0 \in \sum$ different from the marked points and puncture points. Fix a local coordinate system a, θ, w on $R \times \tilde{M}$ such that $u(y_0) = (0,0,0)$. We use $\mathcal{C}'(\sum; u^*TN) = \{h \in \mathcal{C}(\sum; u^*TN)|h(y_0) = (0,0,*)\}$ instead of $\mathcal{C}(\sum; u^*TN)$, then the construction of the neighborhood $\mathfrak{u}_b$ of b is the same as for M^+.*

This means that we constructed a slice, this equivalent to work on the quotient of the C^*-action. *This is not what is written in the box.*

(2) The construction of gluing map itself manifests the parameter (the length of the connecting cylinder in the middle) for the target space explicitly. Though we did not spell out this parameter, it is fairly easy to get it. For example, if only glue two components $M^\pm$ with gluing parameter (r, θ), the length of connecting cylinder is r; if we glue three components M^+, N and M^-(where N is a rubber component between $M^\pm$) with two gluing parameters $(r^\pm, \theta^\pm)$, the length of connecting cylinder is $r^+ + r^-$, and etc.

3.2.2. *(LR11) and (LR12).* In (LR11,12), T& Z complained some routine issues in gluing theory such as (1) the injectivity and surjectivity of the gluing maps were not explained, *mentioned* and (2) we applied the implicit functional theorem without necessary bounds on the Taylor expansion of $\bar{\partial}$. *mentioning*

(r,\theta_0) are gluing parameters for the target on p192, not domains. With 2+ of these, there needs to be an explicit identification of the resulting domains, as at the end of Section 6.2 (previously 5.2) in [TZ].

Response:

(1) All arguments on injectivity and surjectivity are intensively developed by Taubes, Mrowka, Donaldson and etc in gauge theory and already became standard in the gluing theory. In the case of stable maps, this, for example, was discussed by Fukaya-Ono (see [FO], Chapter 3). Essentially, this does not require extra hard estimates. In this

But these are relative stable maps. These properties depend on the proper equivalence relation on the maps, which is very different from the absolute case or the contact case. In any case, there is no mention of these properties in [LR] whatsoever.

paper, we decided to mainly focus on the new issue that we concerned, for example, the estimates of $\frac{\partial}{\partial r}$ for gluing maps, which is certainly a *harder* issue. This refined estimate was certainly new at that time.

[NO!]

Remark 3.2.1. We would like to remark that in [LR] the estimates for $\frac{\partial}{\partial r}$ is of order r^{-2} when $r \to \infty$, in fact, this can be achieved to be of exponential decay order $\exp(-Cr)$.

[They are not mentioned in the proof of Proposition 4.10.]

(2) This is a standard issue. Nevertheless, the necessary bounds for applying the implicit functional theorem were given in Section 4.1. The 0-th and 1-th order were given by (4.16) and (4.26) in ([LR], P193, P195). The quadratic estimate was given by (4.3) in ([LR], P187). [(4.3) is a decay estimate; there is no estimate on the quadratic error term anywhere in [LR].]

[The only case considered in [LR] w/o any further comment.]

3.2.3. *From one nodal case to general cases.* We <u>first</u> consider the case with one gluing parameter, namely, we glue two components of targets, say $M^{\pm}$, with gluing parameter $\rho = (r, \theta)$. Suppose we have a nodal surface consisting of $\Sigma^{\pm}$ with relative nodal points $(p_1, \ldots, p_v)$ and a pair of relative maps $u^{\pm} : \Sigma^{\pm} \to M^{\pm}$. We want to glue a map $u_{\rho} : \Sigma_{\rho} \to M_{\rho}$. One of the main issue is to construct Σ_{ρ}. The gluing parameter ρ determines the gluing parameters $(\lambda_1(\rho), \ldots, \lambda_v(\rho))$ at nodal points. Here $\lambda_i(\rho)$ is given by the local behaviour of u at p_i. We already gave this for $i = 1$ (cf. (4.14),(4.15) in [LR]), and of course, <u>it can be generalized</u> to multiple nodal points in a straightforward way.

If the gluing parameters are more than one, this is already explained in §3.2.1.

[But not done, even though it would have taken no space.]   [With no indication of what would constitute good slices.]

3.3. **Invariants.** We answer the comments (LR13,14,16) in this subsection.

3.3.1. *(LR13).* In (LR13), T& Z commented: VFC approach is based on a global regularization of the moduli space. The 3-4 pages dedicated to this could be avoided by using the local VFC approach of Fukaya-Ono or Li-Tian.

Response:

Obviously, the global regularization if exists, would be much better than the local regularization. The pay-off is 3-4 pages, while without the local regularization, a further

argument would be of more than 30-40 pages. So the logical comment is "we use global regularization to avoid the local VFC approach of FO and LT".

3.3.2. *(LR14).* In (LR14) T& Z commented that the construction for the rubber components needs to respect with the $\mathbb{C}^*$-action.

Response:

We always take slices for this group action. See top of p17.

3.3.3. *(LR16).* In (LR16), T& Z complained that it was not necessary to define the invariants using integrations and, therefore, the estimates of $\frac{\partial}{\partial r}$ of gluing maps are not necessary .

Response: This is explained in §2.2. We believe that T&Z did not understand the main idea and the contribution in [LR].

3.4. **Others.** Comments (LR1), (LR2), (LR3), (LR9) and (LR15) are on some miscellaneous points. We collect them at here.

3.4.1. *(LR1).* **Response to (LR1):** in the formula of Theorem 5.8 ([LR]), we agree with T&Z that we missed an obvious term $\mathrm{Aut}(\mathbf{k})$. This can be easily fixed by readers when they apply our formula.

3.4.2. *(LR2).* **Response to (LR2):** the way we formulate the relative stable maps followed [R] for example. It might not be treated as a standard way today though. The importance of this concept is that we first introduce the idea of "relative" and the $\mathbb{C}^*$ action. 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].

3.4.3. *(LR3).* T& Z wrote: "In addition to being imprecise, Definition 3.18 in [LR] of the key notion of stable map of $X \cup_V Y$ ($\bar{M}^+ \cup_D \bar{M}^-$ in the notation of [LR]) is incorrect, as it seperates the rubber components into X and Y-parts".

This is the most important notion in this paper (definition of relative map) and it is not even close to being a definition.

(J) is known to be the correct choice (J. Li's work) and is needed for the gluing to work properly. How can you possibly prove a symplectic sum formula without working with limits into the singular fibers?!

Response:

By definition $M_\infty = M^+ \bigcup M^-$ (corresponding to $\bar{M}^+ \cup_D \bar{M}^-$ mentioned above). There are two points of view of moduli spaces of maps in M_∞.

(**I**) Considering the relative stable maps into M_∞ with matching condition at infinity, and then take the compactification. To be precise, we have the moduli spaces $\mathcal{M}_{A+}(M^+, g^+, T_{m+})$, $\mathcal{M}_{A-}(M^-, g^-, T_{m-})$ and their compactification $\overline{\mathcal{M}}_{A+}(M^+, g^+, T_{m+})$, $\overline{\mathcal{M}}_{A-}(M^-, g^-, T_{m-})$. We can define the moduli space $\overline{\mathcal{M}}_A(M_\infty, g, T_m)$ as a triple $(\Gamma^-, \Gamma^+, \rho)$ where $\Gamma^\pm \in \overline{\mathcal{M}}_{A\pm}(M^\pm, g^\pm, T_{m\pm})$ and $\rho : \{p_1^+, ..., p_v^+\} \to \{p_1^-, ..., p_v^-\}$ is a one-to-one map satisfying the conditions described in Definition 3.18 in [LR];

(**J**) consider the relative stable maps into M_r and take the limit as $r \to \infty$.

We know that T&Z actually took the approach (**J**). The disadvantage of this is that this space is not virtually smooth. On the other hand, we know that (**J**) fits into the degeneration framework well, for example, this has been already used in [CLSZ] for orbifold cases. The very important observation is, these two different choices have same top stratum, and thanks to our definition of invariants, they actually provide same invariants.

Having the same (virtually) main stratum does not in general imply that the invariants are the same (e.g genus 0 stable maps, stable quotients, and linear sigma-model)

Remark 3.4.1. Moreover, the second condition (2) in Definition 3.18 makes it slightly different from the moduli spaces in the usual sense. Let us suppose that $v = 1$. We have evaluation maps at $p_1^\pm$ to be $ev^\pm : \overline{\mathcal{M}}_{A\pm} \to D$. Define

$$\mathcal{M}' = \overline{\mathcal{M}}_{A+}(M^+, g^+, T_{m+}) \times_D \overline{\mathcal{M}}_{A-}(M^-, g^-, T_{m-})$$

be the fiber product with respect to $ev^\pm$; however, Condition (2) uses the "evaluation map" to space of closed orbits, it turns out that the space $\overline{\mathcal{M}}_A(M_\infty, g, T_m)$ becomes the k copies of $\mathcal{M}'$, where k is the multiplicity of the closed orbits at infinity.

3.4.4. *(LR9).* **Response to (LR9):**

The Hausdorffness issue was already standard in the subject even it may be nontrivial (see [R], Theorem 3.13 and [FO], Chapter 2). We do not think it is necessary to repeat standard materials, in particular for papers published in Invent. and Annuals.

3.4.5. *(LR15).* **Response to (LR15):** It is a very important point.

T&Z asked the explanation of multiplicity k. We agree that this needs more explanation. This k factor is due to Remark 3.4.1 and it leads to a natural map Q of degree k used in P. 210([LR]).

It is not even stated that the relative moduli spaces are Hausdorff. It is a rather delicate issue in the absolute case and much more so in the relative case. It depends on choosing the equivalence relation correctly.

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On Symplectic Sum Formulas in Gromov-Witten Theory:
Additional Comments on A.-M. Li's Response
Aleksey Zinger   Updated: 12/27/14

The red italicized comments on A.-M. Li's note are mine. Below is a summary of my thoughts on it.

As can be seen from the e-mails posted on my website, the first version of [TZ] was e-mailed to several people, including Ruan, on 03/14/14, in the hopes of having discussions regarding this during the workshop at the Simons Center the following week. I posted it on arXiv on 04/07/14, once the relevant parties' views toward this manuscript had become clear (see my remarks in Section 2 of [TZ]). Furthermore, I told Ruan in October 2013 that I was looking at the Ionel-Parker work and would look at his paper next. I also told him I was skeptical that Sections 3-5 of [LR] could possibly contain reasonably complete construction of relative invariants and proof of the symplectic sum formula; Ruan assured me that *everything was there*, which seems to characterize quite well the application parts written by him. In particular, the authors of the relevant papers had the opportunity to review my manuscript before it was posted on arXiv (contrary to A.-M Li's complaint). On the other hand, A.-M. Li did not provide me with a copy of his response before posting it on arXiv, either via Ruan or directly (my e-mail is not hard to find), even though this concerned an arXiv manuscript, not a published paper.

There is no question that the symplectic sum (or degeneration) formula has played an important role in GW theory over the past decade. However, most of the applications I am aware of have been in the algebraic category (the applications in Hu-Li-Ruan and McDuff's uniruled paper are not, but are in the algebraic spirit). They rely on the clearly stated formulas in J. Li's 165-page work on the subject. The corresponding portion of [LR], Sections 3-5, which claims to establish this formula on the level of numbers (but not cycles as in J. Li's work) is only 43 pages and is generally fairly lightly or inefficiently written. For example, A.-M. Li agrees that the 6-page argument of Section 3.1 could have been completely done in 1.5-2 pages (see (3) on page 12); his reason for not doing so is supposedly that his argument could extend to the contact case, which *is irrelevant to* [LR] and *has been dealt with already in [HWZ1]* (the italicized text is from the middle of page 159 in [LR]). While there are plenty of citations of [LR] (as well of the Ionel-Parker work) in the literature, I am not aware of any citations of a specific formula in [LR] (or IP); [LR] (and IP) is often cited along with J. Li's work by people who have never looked at [LR] (or IP) for essentially political reasons (as some of them have admitted to me). Furthermore, according to A.-M. Li, the relative stable maps of [LR] and maps to the singular fiber are not the same as J. Li's versions (see (A) and (B) below); thus, I am not aware of any uses of the notions of these maps introduced in [LR].

As far as I can tell, the substance of A.-M. Li's note consists of the following.

(1) A.-M. Li claims that Mohammad and I do not understand (essentially) anything. In particular, the most important properties of the relative moduli spaces and of the gluing construction (see (LR9) and (LR11) below) are supposedly so standard that they are not worth even mentioning. If so, it is unclear to me what his contribution to [LR] was supposed to be. The applications to

birational geometry are well-known to be due to Ruan. The target-stretching idea behind the notion of relative stable map in [LR] is really nice, but a proof is required to show that it actually works. The refined gluing estimates A.-M. Li highlights so much are pretty straightforward, stated imprecisely, and not essential (absolute GW-invariants had been previously defined without them).

(2) The standard notions of relative stable map and stable map to the singular fiber are not due to [LR] (and so are due to J. Li [L]); see (A) and (B) below.

(3) In light of (A) and (B) below, I was too generous to assume that the most important properties of the relative moduli spaces and of the gluing construction (see (LR9) and (LR11) below) at least hold in the setup of [LR]. They depend heavily on the definition of the moduli spaces. As it turns out the relevant moduli spaces are different in [LR] from J. Li's work [L], there is no way all of the most important properties hold; see more below. Thus, [LR] does not simply lack a proof (or even an outline of one), but its moduli setup is wrong.

(4) A.-M. Li appears to be completely out of touch with what has been going on in GW theory for the past one to two decades. He seems to believe that basic, completely standard, statements or arguments appearing in [TZ] must have been taken from [CLSZ] (top of page 7, top of page 13, and middle of page 18 in his note). I had not even looked at [CLSZ] until B. Chen forwarded it to me during the March workshop as it supposedly elaborated [LR]; this does not appear to me to be the case.

A.-M. Li's response to our view that [LR] simply does not contain a proof of the most important statements (complete or incomplete, right or wrong), even on the level of key intermediate statements comes down to the following.

(LR6) Our comment in [TZ] that the compactness argument in [LR] involves one node at a time is simply ignored; see 3.1.3. on page 13. This leads to the problems indicated in (A) below.

(LR9) According to the top of page 19, there is no reason to give an indication for why the relative moduli spaces are Hausdorff because this is completely standard for the absolute moduli spaces. It is in fact a pretty delicate issue even in the absolute case and even more so in the relative case, when the target of the maps changes. This property depends heavily on choosing the equivalence relation properly and on the role played by the $\mathbb{C}^*$-action on the rubber. A.-M. Li highlights the role of the $\mathbb{C}^*$-action only in the boundary strata having virtual codimension at least 2 (middle of page 8).

(LR10) The text quoted in A.-M. Li's note (top of page 15) does not say a word about choosing slices, but he claims that this is what it means. There is certainly no indication of how such slices should be chosen to ensure the injectivity and surjectivity of the gluing map; this becomes more of a delicate issue if multiple smoothing parameters are involved.

(LR11) According to the bottom of page 15, there is no reason to even mention either the injectivity nor surjectivity of the gluing construction in [LR, Section 4.1] because it is completely standard in gauge theory (A.-M. Li replaced *mentioned* in [TZ] with *explained* in his note). These two properties clearly depend heavily on the equivalence relations chosen and cannot

be just standard. Points (A) and (B) below cause problems with both. According to the middle of page 18 in A.-M. Li's note, standard (or more standard) notions of maps to the singular fiber are used in [CLSZ], but somehow a crucial difference between the moduli setups in [LR] and in [CLSZ] does not prevent the important properties from holding in both cases (according to A.-M. Li).

According to A.-M. Li, the statements of Lemma 3.12 and Definition 3.18 in [LR] are as intended; see the bottom of page 13 and the top of page 18.

(A) According to Lemma 3.12, there can be no maps with fiber components into the rubber (the $\mathbb{P}^1$-bundle of J. Li) that have only one or two special (marked or nodal) points. Relatedly, the compactification argument in Section 3.2 of [LR] results only in components of genus 0 going into the rubber and they can attach to the main target space at only one point. In each of these respects, the moduli spaces of [LR] are different from J. Li's work and cannot possibly work as needed.

(B) According to Definition 3.18, a stable map into the singular fiber ($M^+ \cup_Z M^-$ in the notation of [LR]) is a pair of relative maps (into (M^+, Z) and (M^-, Z)). This is also different from J. Li's work. These pairs are not limits of maps into smooth fibers (according to A.-M. Li himself), except for the top strata of the relative moduli spaces. The Gromov-Witten invariants entering into the symplectic sum formula can be viewed as arising only from the top strata, but only after a suitable regularization. Such a regularization needs to extend from the singular fiber to the smooth ones. In order for this to happen, one needs to describe maps into the smooth fibers that are close to maps into the singular fiber, including those not in the top strata. The moduli space of maps into the singular fiber in [LR] is thus not too suitable for establishing the symplectic formula.

Prior to A.-M. Li's note, I felt somewhat ambivalent about [LR]. The symplectic sum formula in the transverse case and without relative invariants appears in Tian's 1995 expository article. The relevant multiplicities in the general case are immediate from basic algebraic considerations (well explained at the top of page 938 in IP's symplectic sum paper). The full symplectic sum formula (with primary insertions as in [LR]) for $(\mathbb{P}^2, L)$ is established in Caporaso-Harris (as can be seen from Section 15.1 of IP's paper). The purpose of [LR] was to establish such a formula in the symplectic setting. While [LR] contains no proof (right or wrong) of such a formula or of the key properties of the relative moduli spaces, I felt that at least [LR] introduced the proper notions of such moduli spaces and suggested an adaption of the target stretching approach from the contact setting that could establish the formula efficiently. According to A.-M. Li's note, the former is not the case (see (A) and (B) above), which leaves very little of [LR]. While the idea of the relative map in [LR] may well have led to the correct notions in J. Li's work, it is hard for me to see how this can suffice for being in the Inventiones.

The aim of [TZ] was not to start a personal conflict with any of the four authors, but to encourage other people in the wider symplectic community to read the symplectic sum and other papers. As we said at the beginning of Section 2 in [TZ],

We hope that the detailed list of specific points below will make it easier for others to gain some mathematical understanding of the issues involved, instead of judging this manuscript or the related papers based on feelings and hearsay.

I hope A. M. Li's note, including *We let mathematics itself in this note to speak for itself*, will provide further encouragement in this regard (the note itself contains basically no mathematics).

Post Scriptum

I realize that people would rather stay out of any discussions concerning other people's papers and focus on their work. However, this can be very damaging to the development of the field, as appears to have been the case in symplectic topology (its geometric/topological side) over the past couple of decades. In particular, this appears to have driven a lot of strong young people into neighboring fields (such as GW theory from the AG side and homological mirror symmetry) and perhaps some out of mathematics. I am aware of at least one person who had a spectacular thesis in GW theory from the symplectic side, could not see a proof in [LR], but could understand J. Li's monumental work, and very successfully moved on to GW theory on the AG side.

I believe the leaders in the field have a moral obligation to ensure that important papers are properly sorted out. D. McDuff has dedicated the past two or three years trying to move forward discussions concerning virtual cycle constructions in symplectic topology which had been happening mostly through back-stabbing for the previous 15 years. In the case of the symplectic sum papers, it would take H. Hofer and G. Tian very little time to familiarize themselves with the paper in their respective journal. In particular, I cannot see it taking H. Hofer more than a few hours to read through Sections 3-5 in [LR]. I hope H. Hofer and G. Tian will contribute a little bit of their valuable time to the present discussion. I have no hope that the referees for the three papers in question will step forward to state their views publicly.