Dear Editors of the *Annals:*

I would like to thank the *Annals* editorial board and the referee for giving some consideration to some of the problems with [IP4] and [IP5]. I also appreciate the authors putting their thoughts related to the S-matrix in writing and acknowledging that pretty much all of their gluing argument is completely wrong (even if in different words). While others in the field would have found a video recording of my previous discussions with the authors regarding their reasoning at the top of page 1003 in [IP5] more entertaining, our written comments related to the S-matrix bring out far more serious problems with [IP5] that others in the field previously did not even imagine were there (having never read the 90-page [IP5]).

IP's response touches on some of the issues with [IP4] and [IP5] raised in [FZ0], but some very important ones are not addressed at all. These issues were selected by another person who wrote to the *Annals* last summer. While I am disappointed by this person's request for anonymity, I will honor it. This note contains a detailed response to IP's cover letter and Sections 1 and 2; the corresponding sections in this note are labeled in the same way. I have also included roundups on [IP4] and [IP5] as Sections A and B, respectively. There is simply very little in these papers which is correct and new (as of the time of their appearance on arXiv).

Issue 1 in IP's response concerns a relatively minor aspect of the symplectic sum formula, though it is the only aspect which distinguishes the version in [IP5] from the version in the much earlier [LR]. I feel that this is also one of only two contributions of [IP4, IP5] worth noting, but neither had been noticed in the field until [FZ1, FZ2]. IP's response focuses on a mistake in the attempt in the first version of [FZ0] to construct a topology that should have been constructed in [IP4, Section 5] to begin with. However, the substance of the original comment in [FZ0] remains unchanged: the required topology on the covers is not constructed in [IP4], other related topological constructions in [IP4, IP5] are not described, the stated group of deck transformations is wrong, and the described implications of the refinements suggested in [IP4, IP5] are also wrong, including in simple cases. A detailed list of problems appears at the beginning of Section 0.

Issue 2 concerns the S-matrix. It does not appear in the symplectic sum formulas of [LR, Lj2], and I am not aware of anyone else in GW-theory who believes it should. IP's response seems to suggest that the symplectic sum formula (and in particular the appearance of the S-matrix) should depend on how this formula is proved. The referee feels that its appearance in the symplectic sum formula is a minor issue because it does not effect the validity of the formula (as it acts as the identity in the symplectic sum formula according to [FZ0]). I believe it *very much* matters why the S-matrix appeared in the first place; IP clearly agree with me in this respect.

One of the two main steps in establishing the symplectic sum formula is a compactness theorem for maps into fibers of a symplectic sum fibration; this is (2) in Section 1 of IP's response (reproduced in Section 1 of this note). IP's response claims that such a result is established in Sections 3-5 and 9 of [IP5]. However, Sections 3-10 in [IP5] deal only with limits of maps that do not have components sinking into the divisor ("δ-flat" maps). The only refinement for such limits beyond the usual Gromov's Compactness is that the contacts with the divisor are the same from the two sides (Lemma 3.3).

The limiting behavior of other types of maps is considered only in Section 12, at the very top of page 1003, where a sequence of maps into the fibers Z_λ of the original fibration is dropped for a consideration of sequences of maps into a different fibration which have no apparent relation to the original sequence. This approach would have been going in the right direction in the more regular setting of [LR], because there are canonical, almost Kahler identifications of the fibers of the two fibrations. In the setting of [IP5], this approach has no substance at all.

IP's justification for the above switch between fibrations is that all those fibers are symplectically deformation equivalent and so have the same GW-invariants. By this reasoning, we could also replace the original sequence by a sequence of maps into a fixed fiber, which would obviously establish nothing. The conclusion of this reasoning is that each sequence of maps has a countable collection of positive-dimensional families of limits (countable because the number of $\mathbb{P}_V$ components can be arbitrarily large and positive-dimensional because there is no equivalence up to the $\mathbb{C}^*$-action on $\mathbb{P}_V$). This conclusion is not explicitly stated in [IP5], but the countable aspect shows up explicitly in the inclusion-exclusion argument above Theorem 12.3, while taking the $\mathbb{C}^*$-equivalence would have resulted in no S-matrix for dimensional reasons (as happens in [LR, Lj2]).

The absurdity of IP's reasoning at the top of page 1003 in [IP5] is similar to their attempt to reconcile the statement of Theorem 11.1 in [IP6] with Example 12.5 in [IP5] at an SCGP workshop in March 14. After two days of analyzing the situation, this attempt came down to trying for 5-10 minutes to convince the audience that the cap product operation on (co)homology was not natural with respect to continuous maps. This occurred during the discussion session of the mini-course devoted to [IP6]. Even though the intended purpose of this workshop was to investigate various virtual class constructions and to share the results widely, T. Parker blocked the posting of the video of this discussion session. As indicated below, [IP6] has direct connections with [IP4, IP5].

IP's response does not elaborate their reasoning at the top of page 1003. It instead consists of distorting the trivial, one-paragraph explanation at the bottom of page 75 in [FZ0] for why the S-matrix acts as the identity in the symplectic sum formula. This argument would not shock anyone else in GW-theory, as it is the exact same reasoning as for why the S-matrix does not appear in [LR] or [Lj2]. Most of IP's objections in fact revolve around "trivial fibers", maps into the singular fiber of a symplectic sum fibration that have infinite automorphism groups and do not even appear in [LR] or [Lj2]. By Lemma 11.2(a) in [IP5], they contribute the identity (i.e. "nothing") to the S-matrix. Thus, "trivial fibers" cannot be part of IP's actual objection. Furthermore, IP now seem to know themselves that the S-matrix should not have appeared in their symplectic sum formula, as they do not even mention it at the beginning of the "proof" of Theorem 11.1 in [IP6].

Thus, the purpose of Section 2 in IP's response appears to be to distract attention from more fundamental problems, such as that [IP5] did not establish an appropriate compactness theorem, did not even attempt to prove the symplectic sum formula in the generality claimed (without semipositivity assumptions), and did not get the gluing done even in the most basic case (as they now admit), i.e. that there is basically nothing correct in [IP5]. Nevertheless, I respond in detail to all of IP's comments regarding the S-matrix in Section 2; a few additional comments on this issue appear in the middle part of Section 0 (see *Issue 2* on page 7) and in Section 1.

Issue 3 concerns the entire gluing argument in [IP5], which deals only with maps without components into the divisor V (or more accurately the rubber $\mathbb{P}_V$) to begin with. IP acknowledge *three errors*; they invalidate the gluing analysis in [IP5] completely. According to the beginning of Section 8 in [IP5], eigenvalue estimates are *the key analysis step*. IP now propose replacing them with a completely different approach, more in line with the much earlier [LR] and other related literature. The proposed modifications still do not touch on all of the issues. Even if the draft of their new argument is completely correct (which I doubt), this leaves almost nothing of [IP5] and still would not establish the main statements in [IP4, IP5] in the claimed generality. A few additional comments on this issue appear in the last part of Section 0 (see *Issue 3* on page 8).

The most widely known issue with [IP4, IP5] is that these papers claim to construct relative GW-invariants and to prove a symplectic sum formula without any semi-positivity assumption. There is no mention of a semi-positive restriction in the 4-page summary of [IP4], the 7-page summary of [IP5], or any of the main theorems (bottom of p940, 10.6, 12.3) in [IP5]. Nevertheless, the rest of the papers considers only semi-positive cases whenever GW-invariants are concerned. IP justify this with Remark 1.9 in [IP4] stating that their methods apply to the general case because *in a separate paper [IP5]* they *describe* an alternative virtual class construction. This "paper" is listed as *in preparation* in the second (09/01) and third (01/04) arXiv versions of [IP4]. This remark replaced Remark 1.8 in the 1999 arXiv version, which claimed that the semi-positive restriction can be removed because of the virtual class construction of [LT]. However, applying this construction would have required gluing maps with components into the rubber $\mathbb{P}_V$, which is not done in [IP4].

The virtual class construction advertised in [IP4] is claimed in [IP6], which appeared on arXiv 11.5 years after the second arXiv version of [IP4]. This construction builds on [CM], which first appeared on arXiv almost 5.5 years after the second version of [IP4]. Furthermore, for two of the most crucial analytic points, [IP6, Lemma 7.4] and [IP6, (11.4)], which require gluing maps with components into $\mathbb{P}_V$, IP cite [IP4] and [IP5]; these two papers restrict to "semi-positive" cases precisely to avoid such gluing. Thus, it appears that someone (perhaps a referee for [IP4]) noticed the issue with Remark 1.8 in the first version of [IP4] described at the end of the previous paragraph, and IP in response replaced this remark with Remark 1.9 in the second version. More than a decade later, they then produced the promised *separate paper* [IP6] by referring the reader back to [IP4] and [IP5] precisely for the gluing statements which are claimed to be unnecessary because of the upcoming *separate paper*.

In addition to every possible major point being wrong in [IP5], there are very few completely correct statements. The statements of even the main theorems and the three applications (the last equations in Sections 15.1 and 15.2 and equation (15.4)) in [IP5] are wrong. For example, the absolute insertions in the equations (1.7) and (1.24) of [IP5] are encoded differently; so the main formula (0.2) cannot hold with any reasonable definition of the exponential in the equations following (1.7) and (1.24). While *each* of these misstatements is certainly minor, there are *hundreds* of them in [IP5] (the remarks in [FZ0] list some of them). This is far beyond the level at which the *Annals* should hold accountable the original handling editor for [IP5] and the referee who claimed to have read it finding it correct.

In summary, [IP4, IP5] contain fairly little which is both correct and new.

(A) Both papers claim the results without a semi-positivity restriction, but consider the main claims only in semi-positivity settings, relegating an automatic extension to *a separate paper [IP5]*; the latter appeared a decade later as [IP6] and refers the reader back to [IP4, IP5] precisely for the deferred issues.

(B) As the discussion regarding the S-matrix indicates, [IP5] does not establish an appropriate compactness theorem for maps into the fibers of a symplectic sum fibration.

(C) As IP's cover letter basically acknowledges, [IP5] completely fails to carry out the gluing needed even in the most basic case of the symplectic sum formula.

(D) There are hardly any correct statements in [IP5].

In my view, it is completely irrelevant whether IP can completely re-write [IP5] now. The correct statements of the main theorems had already appeared in [LR] and had been known even before then. The first version of [LR] appeared on arXiv almost 3 months before the first version of [IP3], which is a brief announcement of relative GW-"invariants" and a symplectic sum formula, almost 1.5 years before the first version of [IP4], and over 2.5 years before the first version of [IP5]. Furthermore, [IP3, Section 2] imposes only the obvious conditions of [IP4, Definition 3.2(a)] on (J,ν). As the conditions of [IP4, Definition 3.2(bc)] are not imposed, the advertised relative GW-"invariants" depend on (J,ν); see [IP3, Theorem 2.5] (this dependence is accidentally mentioned even in [IP5, Definition 11.3]). Thus, the purpose of the announcement [IP3] appears to have been to lay a claim to the symplectic sum formula as soon as possible after the appearance of [LR]. By the late 1990s, a proof of the symplectic sum formula was a purely technical, even if non-trivial, problem. All of the necessary tools were available and gathered in [LR]; [IP5] failed to even adapt properly the arguments in the much earlier [LR].

Because of the obvious issue (A) and the 2.5-year lag behind [LR], many people in the field, including those who cite [IP4] and [IP5] routinely, dismiss both papers completely. The difference is that I have *read* these papers (not just looked at them) *twice*. My view is more nuanced. Without actually reading [IP5], few people could have possibly suspected the issue (C), especially with the very basic gluing. While it is common knowledge in the field that there should be no S-matrix, I doubt many people had suspected that its appearance had something to do with the much deeper compactness issue (B). On the other hand, [IP4, IP5] contain some suggestions for topological improvements of the usual symplectic sum formulas and a sleek, alternative (almost) proof of the Bryan-Leung formula for counting curves on the rational elliptic surface. While I personally find these aspects of [IP4, IP5] intriguing, they are way too minor for an *Annals* paper.

Being in the same field, I certainly like the kinds of problems IP work on and their way of thinking about them (in particular as reflected in [IP7]). Unfortunately, they are almost the only people left in this field because of their own tactics, such as routinely claiming results prematurely (as happened with the symplectic sum formula, the Yau-Zaslow formula for curves on $K3$, the super-rigidty for curves in Calabi-Yau threefolds, GW-invariants relative to normal crossings divisors, and a new virtual class construction) and dismissing results of others in the field. Meanwhile, the most senior symplectic topologists preferred to look the other way over the past 15-20 years and in some cases effectively encouraged these tactics. IP's referring the reader of [IP6] to [IP4, IP5] for crucial statements they *knew* not to be in [IP4, IP5], submitting it to *JDG*, and presenting it as a mini-course at an SCGP workshop are reflections of how the field of symplectic topology has been functioning over the past two decades and direct results of how the *Annals* handled the original submissions of [IP4, IP5]. Even worse, [IP6] remains on arXiv about a year after the above issues were brought up in a discussion in front of an audience of 50-60, which included Ekholm, Fukaya, McDuff, Ono, Ruan, Salamon, and D. Sullivan.

The whole issue with [IP4, IP5] and [IP6] could have been completely resolved long ago for the benefit of what remains of the field of symplectic GW-theory, without causing any harm to IP and allowing everyone to move on. IP have known about (A) for 15 years, about (C) for at least a year, and have at least suspected something related to (B) for over two years ([IP6] appeared on arXiv in 02/13). All of these issues are fundamental to [IP4, IP5] and are what a proof of a symplectic sum formula is about.

In an e-mail to IP in April 14 and in a letter to the *the Annals* in January 15, I suggested a very dignified wording for a withdrawal statement for [IP4, IP5]. Instead of proceeding in the proposed constructive manner, IP have chosen to waste the time of the *Annals* editorial board, of the referee, and of other people in the field, including themselves and me, and have now done more damage to themselves. However, the most direct practical impact of their recent actions right now is ironically on the only one of Ionel's six former students whose PhD thesis ever made it to arXiv: I have now spent months dealing with [IP4, IP5] and [IP6] instead of finishing up some work with her student who needs to apply for jobs in a few months. Some senior people contribute to their field by cleaning up the work done to make it easier for others to make progress; a relative analogue of [MS] and [RT1, RT2] based on [LR] would be such a contribution in the present case. IP's response and [IP6] suggest that they instead prefer to add to the existing mess in symplectic GW-theory and to discourage others from working in this field.

The saga with [IP4, IP5] has been going on for about 15 years, just in a quieter way until recently, and has done huge damage to the field of symplectic GW-theory. If the present note is not sufficiently convincing to wrap up this situation, perhaps the most efficient way to proceed is to meet for a videotaped discussion in Simonyi 101 in presence of Tian, Eliashberg, Hofer, and McDuff. All participants should be required to sign irrevocable waivers allowing for unrestricted distribution of the resulting video. Unlike the virtual class papers discussed at the March'14 workshop at SCGP, [IP4, IP5] are in the *Annals*, so the journal's reputation is also at stake.

Aleksey Zinger, 03/12/15

Contents

0 IP's cover letter

IP: Issue 1. The referee's first point concerns the topology of the space $\mathcal{H}_X^V$ defined in [IP4]. We addressed this in previous correspondence, where we explained the error that Tehrani and Zinger made, and why our description of $\mathcal{H}_X^V$ is correct. A referee chosen by Annals subsequently agreed.

The first version of [FZ0] was indeed wrong to claim that a topology on $\mathcal{H}_X^V$ with the desired lifting property does not exist if V is disconnected. The entire issue is relatively minor compared to other problems with [IP4, IP5], though it concerns an intriguing suggestion which distinguishes these papers from the much earlier [LR]. Our incorrect statement is only a minor part of the issue itself, as indicated below.

(1) Section 5 in [IP4] gives two, nearly identical, *set*-theoretic descriptions of $\mathcal{H}_X^V$. The topology on $\mathcal{H}_X^V$ is *never* described and it is not shown that the relative evaluation map lifts to this cover, especially over the strata of maps from nodal domains. The topology on $\mathcal{H}_X^V$ is not obvious from the description in [IP4], especially when the marked points come together.

(2) The space $\mathcal{H}_X^V$ is the disjoint union of covers $\mathcal{H}_{V;\mathbf{s}}^X$ of V^ℓ, where ℓ is the length of the contact vector $\mathbf{s}$. As part of IP's *description of* $\mathcal{H}_X^V$ in [IP4, Section 5], the group of deck transformations of $\mathcal{H}_{X;\mathbf{s}}^V \longrightarrow V^\ell$ is specified incorrectly as the rim tori module $\mathcal{R}_V^X$, instead of $(\mathcal{R}_X^V/\mathcal{R}'^V_{X;\mathbf{s}})\times\mathcal{R}'^V_{X;\mathbf{s}}$ for a certain submodule $\mathcal{R}'^V_{X;\mathbf{s}}\subset\mathcal{R}_X^V$; see [FZ1, Section 1.1].

(3) While the covers $\mathcal{H}_X^V$ are determined by (X,V), lifts of the relative evaluation maps to these covers are not unique and the refined relative GW-"invariants" generally depend on the choices of these lifts; see [FZ1, Section 1.2]. This makes these "invariants" hardly computable outside of very rare cases, though it is possible to use them for some qualitative applications (as demonstrated in [FZ1, FZ2]).

(4) In the simple cases of [IP5, Lemmas 14.5,14.8], these "invariants" are described as being indexed by the rim tori (in addition to the standard indexing for the usual relative GW-invariants). This is wrong as the covers in these statements are $\mathbb{C}\longrightarrow\mathbb{T}^2$; see [FZ2, Remarks 6.5,6.8]. IP's "invariants" can be indexed by the rim tori only when the covers are completely disconnected (which happens for the curve classes with no contact with the divisor, for example).

(5) IP's proposed refined relative GW-"invariants" give rise to a refined relation between the GW-invariants of the symplectic sum and its degeneration $X\cup_V Y$; see [FZ2, Section 1.2]. Contrary to an explicit claim in the abstract of [IP5] and many statements throughout the rest of [IP5], it does not usually lead to a refined relation between the GW-invariants of $X\#_V Y$ and any kind of relative GW-invariants of (X,V) and (Y,V) because the cohomology of $\mathcal{H}_{V;\mathbf{s}}^X\times\mathcal{H}_{V;\mathbf{s}}^Y$ usually does not admit a Kunneth decomposition; see [FZ2, Example 3.7]. Even when the GW-invariants of $X\#_V Y$ split into refined relative GW-"invariants" of (X,V) and (Y,V), this is rarely computationally useful because of the dependence of the latter on the choice of the lift; see (3) above.

(6) The existence of the crucial refined degree gluing map [IP5, (3.10)] is never established. As indicated in [FZ2, Sections 3.1,4.1], this is a subtle issue that depends on consistent choices of certain coset representatives. This issue becomes even more delicate in the case of the convolution in [IP5, (10.8)] needed for a symplectic sum formula for IP's relative invariants.

In summary, IP's suggestions for topological refinements to the usual relative GW-invariants and the standard symplectic formula are very vague, contain incorrect statements in the general setup and in simple examples, and completely misdescribe the implications of these refinements in GW-theory. I am not aware of a single application of these suggested refinements in the 16 years since the first arXiv version of [IP4] and the 14 years since the first arXiv version of [IP5], with exception of the qualitative applications described just recently in [FZ1, FZ2]. The last two papers investigate the implications of the topological suggestions made in [IP4, IP5].

IP: Issue 2. The referee's second point is about the "S-matrix" that appears in the main formula in [IP5]. It is not clear if the referee is suggesting that there is an error, but we address this anyway in Section 1 below.

Based on the referee's comment at the beginning of Section 2, I believe that he/she feels that the appearance of the S-matrix in the symplectic sum formula is a pretty minor issue because it does not effect the validity of the formula (as it acts as the identity in the symplectic sum formula according to [FZ0]). While I agree that it does not effect the validity of the formula, I believe it *very much* matters why it appeared in the first place. According to IP's next comment, their second statement in Section 1, and the second paragraph in (a) in Section 2 of their response (see the paragraph beginning *It is important ...* on page 14 of this note), IP agree with me that it matters why the S-matrix appeared (but not that it clearly acts as the identity in the symplectic sum formula). As summarized below and discussed in detail in Section 2, its appearance and IP's response show that IP did not determine (and claim to still not realize) what the limiting maps into the central fiber Z_0 of a symplectic sum fibration $\pi\colon Z \longrightarrow D$ should be (as described in [Lj1]).

IP: The S-matrix (our terminology) is a term in the symplectic sum formula that arises quite naturally from the analysis.

The S-matrix arises from a completely nonsensical argument at the top of page 1003 in [IP5], which concludes with a completely unnatural implication for the limiting maps into the central fiber. As explained in detail in Section 2 below, a sequence of maps into the fibers $Z_\lambda \equiv \pi^{-1}(\lambda)$ with $\lambda \longrightarrow 0$ is dropped for a consideration of sequences of maps into a different fibration which have no apparent relation to the original sequence. This approach would have been going in the right direction in the more regular setting of [LR], because there are canonical, almost Kahler identifications of these fibers, but not in [IP5].

By the top of page 1003 in [IP5], sequences of maps into Z have countable collections of positive-dimensional families of limits (countable because k in Section 2 can be arbitrarily large and positive-dimensional because there is no equivalence modulo the $\mathbb{C}^*$-action on $\mathbb{P}_V$ in [IP5]). IP's count in the proof of Lemma 11.2(a) in [IP5] implicitly uses this action (otherwise, one would be counting elements of the positive-dimensional spaces $\mathcal{M}_{\mathbb{I}}$ of "trivial fibers" defined at the top of page 999 in [IP5]). If such an action were applied to all limiting maps with components into $\mathbb{P}_V$, the dimension of the space of such limits would be smaller than the dimension of the main stratum (limiting maps with no components sinking into V). These limits thus would not pass through the required constraints for purely dimensional reasons and would not contribute to the symplectic sum formula, just as happens in [LR] and [Lj2]. The last paragraph on page 75 in [FZ0] arrives at the last conclusion by making use of the forgotten $\mathbb{C}^*$-action in relation to the R-term in [IP5, (11.3)]. So, [IP5] implicitly takes the $\mathbb{C}^*$-action into account in order to even make sense of the S-matrix, but not for the non-"trivial fiber" maps that make up the non-identity part of the S-matrix. This does not seem very *natural* either.

IP: We proved that it is trivial for the genus 0 Gromov-Witten invariants (Proposition 14.10 of [IP5]). But were unable to show that it is trivial for higher genus, so it is a necessary part of the formula.

Nevertheless, IP drop the S-matrix without any comment at the beginning of the "proof" of [IP6, Theorem 11.1]. So do all other citations of [IP5] I am aware of.

IP: We have no example where the S-matrix is non-trivial, and have always hoped that someone would show that it is superfluous. Tehrani and Zinger claim to have proved this. Unfortunately, their proof seems to be wrong, and displays significant misunderstandings.

The S-matrix does not appear in [LR] or [Lj2]. The trivial, one-paragraph explanation at the bottom of page 75 in [FZ0] would not shock anyone else in GW-theory, as it is the exact same reasoning as for why the S-matrix does not appear in [LR] or [Lj2]. Most of IP's objections in fact revolve around "trivial fibers". By Lemma 11.2(a) in [IP5], they contribute the identity (i.e. "nothing") to the S-matrix.

IP's acknowledgment of the highly technical problems in *Issue 3* below and attempt to convince an SCGP workshop that the (co)homology cap product is not natural would suggest that it is not Tehrani and I who *display significant misunderstandings*. IP's focus on the "trivial fibers" in their comments and non-mention of the S-matrix at the beginning of the "proof" of [IP6, Theorem 11.1] suggest that they might be trying to deflect attention from far more fundamental problems with [IP4, IP5], including the related compactness issue indicated in (B) on page 4. Nevertheless, detailed responses to IP's comments appear in Section 2.

IP: Issue 3. Tehrani and Zinger correctly identify three errors in [IP2]:

- *(i) Parts of the analysis in Sections 6-8 addresses only the basic case of maps with intersection multiplicity $s=\mathbf{1}$.*
- *(ii) Formula (7.5) for the adjoint D_F^* is not correct.*
- *(iii) The sign of the curvature in (8.7) is wrong, invalidating the proof of Proposition 8.2.*

This already leaves almost nothing of the gluing part, i.e. IP's *half (1)* in Section 1, even in the case of maps without components into the rubber $\mathbb{P}_V$.

IP: Issue (i) is fixed by making the Sobolev norms (6.9) depend on s, and doing the necessary bookkeeping. Specifically, one must verify that Lemmas 6.9 and 7.1, Proposition 7.3 and Lemma 9.2 continue to hold; this is easily done using the pointwise estimates already appearing in the proofs. (Examples of this occur at the end of the proofs of Lemma 6.9 and 7.2.).

What does this leave of the published version of [IP5]? Sections 2-5 are basically setup, more or less standard; even they have problems (mostly minor, but lots of them). The changes for Section 9 of [IP5] outlined in Section 3 of IP's response still make no mention of properly bounding the quadratic error terms in the expansion of the $\bar{\partial}$-operator in Proposition 9.4 or of controlling the collapse of the injectivity radius of the target needed for the injectivity claim of Proposition 9.1. However, this is details relative to the bigger picture.

IP: Issues (ii) and (iii) can be bypassed by rewriting Section 8 using a different approach. Instead of establishing eigenvalue estimates for the linearized operator, one can prove Proposition 8.1 by transferring the partial right inverse P from the nodal curve C_0 to its smoothing C_μ. This is the approach to gluing used by Donaldson-Kronheimer for Yang-Mills instantons and later used by McDuff-Salamon to prove Ruan-Tian gluing of holomorphic maps. The required estimates are much easier, the adjoint D_F^* never appears, and again can be established using only pointwise bounds already appearing in our paper.

This is also the approach taken in the much earlier [LR], but with a different setup. The eigenvalue estimates are *the key analysis step* of [IP5], as stated at the beginning of Section 8 and highlighted in the second paragraph on page 939 (*a Bochner formula* was supposed to be part of this).

IP: We include a (draft) of a new Section 8 below.

I have not read Section 4 in IP's response, which contains this draft. Having read [IP5], IP's response, and IP's writings that involve heavy analysis, I am skeptical that it is correct. Even if it is perfect, this still means that [IP5] contained essentially nothing which is both new and correct. It would not even establish what they claim, i.e. a construction of relative GW-invariants and a proof of the symplectic sum formula without a semi-positivity assumption.

IP: The statements of theorems remain as given in [IP5]. Several proofs in Sections 6, 7 and 9 need minor adjustments, and most of Section 8 needs to be replaced. We will issue a corrigendum.

The correct versions of the *statements of theorems given in [IP5]* had already appeared in [LR] and had been known even before then. The first version of [LR] appeared on arXiv almost 3 months before the first version of [IP3], which is a brief announcement of relative GW-"invariants" and a symplectic sum formula, almost 1.5 years before the first version of [IP4], and over 2.5 years before the first version of [IP5]. Furthermore, [IP3, Section 2] imposes only the obvious conditions of [IP4, Definition 3.2(a)] on (J,ν). As the conditions of [IP4, Definition 3.2(bc)] are not imposed, the advertised relative GW-"invariants" depend on (J,ν); see [IP3, Theorem 2.5] (this dependence is accidentally mentioned even in [IP5, Definition 11.3]). Thus, the purpose of the announcement [IP3] appears to have been to lay a claim to the symplectic sum formula as soon as possible after the appearance of [LR].

In summary, this leaves pretty much nothing of [IP5]. It did not discover the correct versions of the main *statements of theorems given in [IP5]* (or in [IP3]). Its only purpose was to provide an alternative proof of these statements, which even IP now agree it failed to do. By the late 1990s, a proof of the symplectic sum formula was a purely technical, even if non-trivial, problem. All of the necessary tools were available and gathered in [LR]; [IP5] failed in even obtaining analogues of the arguments in the much earlier [LR]. In my view, it is irrelevant in regards to [IP5] whether IP can completely re-write it 15 years later.

1 Overview of [IP5]

IP: The basic object of study is the fibration $\pi : Z \longrightarrow D$ over the disk $D \subset \mathbb{C}$ constructed in Section 2. This is a deformation space: Z is a compact symplectic manifold with compact almost complex structure whose central fiber $Z_0 = \pi^{-1}(0)$ is the singular space $X \cup_V Y$ obtained by identifying symplectic manifolds X and Y along a codimension 2 submanifold V. All other fibers Z_λ, $\lambda \neq 0$, are smooth and symplectic; any one of these can be taken to be symplectic sum.

Section 2 in [IP5] reviews Gompf's symplectic sum construction and reformulates it in terms of a fibration $\pi\colon Z \longrightarrow D$. The space Z is not compact if D is an open disc and has boundary if D is a closed disk, i.e. Z is not a *compact manifold* in either case, but this is irrelevant. As summarized in [FZ0, Remark 3.2], there are multiple problems even with this section. They are all fixable, but illustrate my point that [IP5] contains hardly any correct statements: the most important statements are very wrong, dozens of other statements are wrong, and there are hundreds of typos (some of which are listed in [FZ0]).

IP: The bulk of the paper consists of relating the (J, ν)-holomorphic maps into Z_0 and into Z_λ. There are two halves of the argument:

(1) A gluing construction, done in Sections 6-9, that constructs maps into Z_λ from the "δ-flat" maps into Z.

(2) Limiting arguments, done in Sections 3-5 and 9, that describe the maps into Z_0 that are limits of maps into Z_λ as $\lambda \longrightarrow 0$.

Both parts are necessary to ensure that there is a one-to-one correspondence.

The reverse order would have been more logical, but that is okay.

Sections 3-10 in [IP5] deal only with "δ-flat" maps. The limiting behavior of other types of maps is considered only in Section 12, at the very top of page 1003, where a sequence of maps into Z_λ as $\lambda \longrightarrow 0$ is dropped for a consideration of sequences of maps into a different fibration which have no apparent relation to the original sequence. As explained in detail in Section 2 below, the claim of [IP5] resulting from this approach is that sequences of maps into Z have countable collections of positive-dimensional families of limits. In particular, there is no *one-to-one correspondence* between maps into Z_0 and nearby Z_λ. Furthermore, the possible limits of maps into Z that are not δ-flat are never determined correctly or even considered in a reasonable way.

IP: The referee, following Tehrani and Zinger, suggests that the theorem is more easily proven by replacing Z by a "stretching necks" model of the symplectic sum. We agree that Part (1) can be done this way. This is how gluing theorem is gauge theory are usually done, and we are very familiar with such arguments. However, we did not - and do not - see how Part (2) can be done. The limiting arguments repeatedly uses Gromov Compactness, which directly applies to sequences of maps $f_n : C_n \longrightarrow Z_{\lambda_n}$ with $\lambda_n \longrightarrow 0$. If one takes the approach of stretching the necks, the limiting object (corresponding to Z_0) is a disjoint union of non-compact components $V \times \mathbb{R} \times S^1$, and convergence proofs become much more difficult.

The limiting object $V \times \mathbb{R} \times S^1$ of [LR] corresponds to the limiting object $\mathbb{P}_V$ of [IP4, IP5] and [Lj1, Lj2] in the same way as a pair of infinite half-cylinders corresponds to a node of a Riemann surface. It fits very naturally with the limiting arguments (2), as described in [LR, Section 3.2]; any issues with this part of [LR] are pretty straightforward to fix. Unlike in [IP5], there is a canonical identification between fibers of the two fibration and it leads to the expected limits (unique k and up to the $\mathbb{C}^*$-action on $\mathbb{P}_V$). The top of page 1003 in [IP5] appears to be a completely botched attempt to adapt this part of [LR] and leads to an absurd conclusion.

2 The S-matrix

IP: Here are a few comments regarding the referee's second complain, which reads:

> *Referee: Another objection of a topological nature concerns the "S-matrix", which TZ claim is always the identity and hence unnecessary. However, this does not seem to be a mistake (but rather a consequence of the rather clumsy way that IP set up the problem), and so I think this objection is less worrisome.*

IP: We disagree. We believe the proof in [FZ0] is completely wrong, and we do not see why the S-matrix term can be dropped from the symplectic sum formula for genus $g \geq 1$.

We never claimed that the S-matrix *is* the identity, only that it *acts as* the identity in the symplectic sum formula. The reason for the difference is that the insertions into the S-matrix relevant to the symplectic sum formula do not offset the $\mathbb{C}^*$-action on $\mathbb{P}_V$. However, with other insertions, the S-matrix can produce non-zero output.

IP's comments reproduced below completely distort the trivial, one-paragraph explanation at the bottom of page 75 in [FZ0] for why the S-matrix can be dropped. I see nothing wrong with this argument. It is consistent with the understanding of morphisms into the central fiber $Z_0 = X \cup_V Y$ that everyone in GW-theory, apparently with the exception of IP, has.

However, even T. Parker made no objection to the standard description of such morphisms during B. Chen's talk at SCGP on 06/05/14. He only smirked at this notion being attributed to [CLSZ] and independently [FZ0]; I quickly pointed out that this notion had long been standard in GW-theory and was due to J. Li [Lj1]. This talk was videotaped.

IP: We proved in Theorem 14.10 in [IP5] that the S-matrix is not needed for the genus $g = 0$ relative invariants. To show this for higher genus, one would have to show that for any symplectic manifold V and any complex line bundle $\mathcal{N} \longrightarrow V$, certain relative invariants of the $\mathbb{P}^1$-bundle $\mathbb{P}_V = \mathbb{P}(\mathcal{N} \oplus \mathbb{C})$ over V vanish as in [IP5, (12.8)]. In the algebraic case, this has been shown by Pandharipande and Maulik using virtual equivariant localization. We did not - and do not - see how to do this in the symplectic case.

Nevertheless, IP drop the S-matrix without any comment at the beginning of the "proof" of [IP6, Theorem 11.1]. So do all other citations of [IP5] I am aware of.

The last subscript in [IP5, (12.8)] indicates that the insertions in these relative invariants come entirely from V. Since there is a $\mathbb{C}^*$-action on the constrained spaces of the resulting maps, of course these invariants vanish. This is the point of the last paragraph on page 75 in [FZ0], which would not shock anyone else in GW-theory. The vanishing of the relevant invariants is established in [LR] and [Lj2] for precisely this reason.

IP are not specific in their reference to Maulik-Pandharipande. In the middle of the "proof" of [IP6, Theorem 11.1], they "credit" Maulik-Pandharipande and Faber-Pandharipande with showing that similar kinds of invariants vanish if the divisor V contains no curves. These correspond to the R-term in [IP5, (11.3)]; its vanishing would imply that the S-matrix *is* the identity. It does not vanish completely and even the statement of [IP6, Theorem 11.1] is wrong, as discovered at an SCGP workshop in March 14. However, the R-term does vanish on the constraints pulled back from V, i.e. as in [IP5, (12.8)], because of the $\mathbb{C}^*$-action on $\mathbb{P}_V$.

IP: If Tehrani and Zinger can prove the needed result (equation (12.8) in our paper), they should write a self-contained paper doing this. This would be an important simplification to the symplectic sum formula, and we would welcome it.

By *the needed result*, IP mean the "vanishing" of the numbers in [IP5, (12.8)]. The reason for this is given just above and in the last paragraph on page 75 in [FZ0]. The *important simplification* has been part of the symplectic sum formulas in [LR] and [Lj2] from the start (and any Maulik-Pandharipande paper IP might be referring to above is an application of [Lj2]).

IP: From our reading of [FZ0], it looks like Tehrani and Zinger first guess what the symplectic sum formula should be, noted that their version was different from ours, and concluded that we are wrong.

It has long been known in GW-theory what the symplectic sum formula is (no S-matrix in particular), but this has nothing to do with our argument. The content of the last paragraph on page 75 in [FZ0] is that the contribution of the S-matrix is the cardinality of a finite set with a free $\mathbb{C}^*$-action; such a set is empty.

The implication of our argument is that the presence of the S-matrix does not effect the validity of the symplectic sum formula; this is part of the referee's comment above. However, the S-matrix should not have appeared in the first place. The only reason it appeared is because of the argument at the top of page 1003 in [IP5]. It is completely wrong in the setting of [IP5], but would have been heading in the right direction in the more regular setting of [LR]; see more below.

IP: (a) [FZ0] claim, without explanation, that there is no need to include "trivial fibers" in the approximate maps, that is, maps $f : \mathbb{P}^1 \longrightarrow \mathbb{P}_V$ whose image is a fiber of $\mathbb{P}_V$. But such maps can arise in the limit process done in Section 3 of [IP5] and therefore must be included in the approximate maps.

The limit process done in Section 3 of [IP5] is Gromov's convergence in Z. It cannot, by itself, give rise to "trivial fibers" or any other maps to $\mathbb{P}_V$. The purpose of this section is to describe limits of δ-flat maps, as done by Lemmas 3.2 and 3.3; Lemma 3.4 is also about such maps. According to Lemma 3.2, limits of δ-flat maps contain no components mapping into V and so do not give rise to components into $\mathbb{P}_V$. There is not even a mention of $\mathbb{P}_V$ in Section 3 in [IP5]; it first appears in Section 11 as an independent object and in Section 12 in a limit context.

By a "trivial fiber", IP mean a morphism into $\mathbb{P}_V$ from a union of trees of $\mathbb{P}^1$'s, without absolute marked points and with precisely two relative marked points on each tree, that represents a multiple of the fiber class. The set of these maps is denoted by $\mathcal{M}_{\mathbb{I}}$ at the top of page 999 in [IP5]. By Lemma 11.2(a) in [IP5], they contribute the identity (i.e. "nothing") to the S-matrix. Thus, the "trivial fibers" cannot be part of IP's objection to why the S-matrix acts as the identity. This statement is entirely about the R-term in [IP5, (11.3)]. This term does not need to be zero (and in fact it is not necessarily zero even if V contains no curves, contrary to what is claimed in the "proof" of [IP6, Theorem 11.1]). The point of the last paragraph on page 75 in [FZ0] is that the R-term sends the constraints coming entirely from V to zero because of the $\mathbb{C}^*$-action on $\mathbb{P}_V$.

However, there are major related geometric issues. The elements of $\mathcal{M}_{\mathbb{I}}$ are not taken up to the $\mathbb{C}^*$-action on $\mathbb{P}_V$ and so $\mathcal{M}_{\mathbb{I}}$ is positive-dimensional. IP's count in the proof of Lemma 11.2(a) in [IP5] implicitly uses this action. While one could say that IP just forgot to mention this $\mathbb{C}^*$-action, its non-use is a central part of IP's limit process for sequences of maps that are not δ-flat; see below. If such an action were applied to limits with components into $\mathbb{P}_V$, the dimension of the space of such limits would be smaller than the dimension of the main stratum (limits of δ-flat maps, i.e. no components into V or $\mathbb{P}_V$ after re-scaling). These limits thus would not pass through the required constraints for purely dimensional reasons and would not contribute to the symplectic sum formula, just as happens in [LR] and [Lj2]. The last paragraph on page 75 in [FZ0] arrives at the last conclusion by making use of the forgotten $\mathbb{C}^*$-action in relation to the R-term in [IP5, (11.3)]. So, [IP5] implicitly takes the $\mathbb{C}^*$-action into account in order to even make sense of the S-matrix, but not for the non-"trivial fiber" maps that make up the non-identity part of the S-matrix.

The limiting behavior of sequences of maps that are not δ-flat is supposedly addressed at the top of page 1003 in [IP5]. This is done by replacing the smoothing $Z \longrightarrow D$ of $X \cup_V Y$ by a smoothing $Z^{(k)} \longrightarrow D^{k+1}$ of

$$Z_0^{(k)} \equiv X \cup_V^k Y \equiv X \cup_V \mathbb{P}_V \cup_V \ldots \cup_V \mathbb{P}_V \cup Y$$

for all $k \in \mathbb{Z}^+$. The consideration of a sequence of J-holomorphic maps $u_r : \Sigma_r \longrightarrow Z_{\lambda_r}$ into the fibers of $Z \longrightarrow D$ with $\lambda_r \longrightarrow 0$ is replaced by the consideration of some sequence of maps into fibers

$$u_r^{(k)} : \Sigma_r \longrightarrow Z^{(k)}_{\mu_{r;1},\ldots,\mu_{r;k+1}}$$

of $Z^{(k)} \longrightarrow D^{k+1}$ with $\mu_{r;j} \longrightarrow 0$ for each j "while keeping the values of the other $\mu_{r;j'}$ fixed" (which already does not make much sense). There is no explanation of how $u_r^{(k)}$ is obtained from u_r. The fibers Z_λ and $Z_{\mu_1,\ldots,\mu_{k+1}}$ with $\lambda, \mu_1 \ldots \mu_{k+1} \neq 0$ are symplectically deformation equivalent, but have different almost complex structures from Lemma 2.3 in [IP5]. According to Ionel's explanation in McDuff's office on 03/26/14, this is not a problem because the GW-invariants of Z_λ and $Z_{\mu_1,\ldots,\mu_{k+1}}$ are the same and so we can easily go between sequences in the two spaces. By this reasoning, we could also replace each of the original fibers $\mathcal{Z}_{\lambda_r}$ with $\mathcal{Z}_1$. Thus, fundamentally it leaves the question of what the top of page 1003 in [IP5] has to do with any kind of limit of the original sequence u_r. While we can deform J_λ to $J_{\mu_1,\ldots,\mu_{k+1}}$, the original sequence could simply disappear along such a path.

The concluding claim of the argument at the top of page 1003 in [IP5] is that a sequence of maps into the fiber $Z_\lambda \subset Z$ limits to a map into $Z_0^{(k)}$ for *every* k sufficiently large (and without the $\mathbb{C}^*$-action). In particular, the compactification in [IP5] is highly non-Hausdorff and contains maps that are unstable (have infinite automorphism groups) in the sense of [LR] and [Lj1]. This does not fit with virtual class constructions. While there is no need for such a construction in the rare, semi-positive types of cases, the abstract, the summary, and the main theorems in [IP5] claim a symplectic sum formula without a semi-positivity restriction. As a consequence of ignoring the $\mathbb{C}^*$-action, IP believe that all maps into $Z_0^{(k)}$ need to be counted (not up to the $\mathbb{C}^*$-action on $\mathbb{P}_V$) for the purposes of the symplectic sum formula. On the other hand, they believe that the limiting process can be renormalized so that the maps converge to a slice of the $\mathbb{C}^*$-action on $\mathbb{P}_V$. These two statements, which were made during a long discussion between Ionel and me in McDuff's office on 03/26/14, are contradictory as the dimension of the slice is smaller than the dimension of all maps. Furthermore, the convergence to a slice is equivalent to the convergence to maps modulo the $\mathbb{C}^*$-action on $\mathbb{P}_V$.

In the more regular setting of [LR], the setup at the top of page 1003 in [IP5] would be going in the right direction and is basically a different way of phrasing a small part of the analogous argument in [LR]. In the setting of [LR], there is a canonical identification of the fibers

$$Z_{\mu_1\cdot\ldots\cdot\mu_{k+1}} \subset Z \qquad \text{and} \qquad Z_{\mu_1,\ldots,\mu_{k+1}} \subset Z^{(k)}$$

which preserves the almost complex structures of Section 3.0 in [LR]; see the end of Section 6.2 in [FZ0]. Thus, a sequence of maps into Z_{λ_r} gives rise to a sequence of maps into $Z_{\mu_{r;1},\ldots,\mu_{r;k+1}}$ whenever $\mu_{r;1}\cdot\ldots\cdot\mu_{r;k+1}=\lambda_r$. There is then a unique choice of k such that for suitable splittings of λ_r into $k+1$ factors the limit taken in $Z^{(k)}$ gives rise to a limit of δ-flat maps into $Z^{(k)}$ without "trivial fibers". Even for the correct k, there are lots of "suitable splittings", which leads to the limiting map being well-defined only up to the $\mathbb{C}^*$-action on each $\mathbb{P}_V$. Both of these aspects are analogous to Gromov's convergence for J-holomorphic maps into a fixed target. The top of page 1003 in [IP5] appears to be a completely botched attempt to quickly arrive at a conclusion similar to [LR]; the first version of [LR] appeared on arXiv more than 2.5 years before the first version of [IP5]. As noted below, the approach taken at the top of page 1003 in [IP5] is very different from that taken in [IP4, Section 6], even though the two situations are very similar.

IP: It is important to realize that any proof of a symplectic sum formula necessarily involves two parts: a limiting argument, like the one done in Section 3 of [IP5], that describes the set of all maps that are candidates for gluing, and a gluing theorem that shows that all of these candidates can be glued.

By the "candidates for gluing", IP mean all possible limits of maps $u_r\colon \Sigma_r \longrightarrow Z_{\lambda_r}$ into the fibers of $Z \longrightarrow D$ with $\lambda_r \longrightarrow 0$. As stated above, Section 3 of [IP5] is concerned only with δ-flat maps. As explained above, the top of page 1003, which is the only place in [IP5] where limits of arbitrary sequences are considered, does not describe the limit of a sequence of maps into fibers $Z_{\lambda_r} \subset Z$. It considers maps into the fibers $Z_{\mu_1,\ldots,\mu_{k+1}} \subset Z^{(k)}$ without any indication of how to pass from the first sequence to a sequence of the second type.

It would also be nice for a "convergent" sequence to have precisely one limit. If the limiting maps into $\mathbb{P}_V$ are not taken up to the $\mathbb{C}^*$-action on $\mathbb{P}_V$, every map in the whole orbit of a limit of a sequence of maps into $\mathcal{Z}_{\lambda_r}$ will also be a limit. This needs to be taken into account, which is not done in [IP5]; there is not even a mention of the relevant $\mathbb{C}^*$-action on the limiting maps. According to IP's response, they still believe there should be no $\mathbb{C}^*$-action on these maps.

IP: If one omits the trivial fibers maps, as [FZ0] want to, one has to refine the limit argument of Section 3 to prove that no limit map contains a component that is a trivial fiber map. We do not see why that is true, [FZ0] do not prove it, and without this statement any gluing theorem will not *be a correct count of curves in the symplectic sum.*

As noted above, the relevant limit process is the subject of Section 12, not 3, in [IP5] and is discussed at the top of page 1003. As explained above, it has no substance at all and its conclusion (non-Hausdorffness because of no $\mathbb{C}^*$-action on $\mathbb{P}_V$) is not dealt with in the gluing process of [IP5]. There are no "trivial fibers" appearing in [LR] or [Lj1]; it is well-known in GW-theory that they should not appear. The fact that IP attempt to consider limits with components into $\mathbb{P}_V$ from a slightly different viewpoint should not effect the final result.

As pointed out above, the "trivial fibers" cannot be part of IP's objection to a reason for why the S-matrix acts as the identity anyway because they do not effect the R-term in [IP5, (11.3)].

IP: (b) [FZ0] work with J-holomorphic maps, rather than (J,ν)-holomorphic maps. But the analysis for gluing can only be done for regular maps (those for which the cokernel of the linearized operator is zero). Ruan-Tian perturbations, or another method of regularizing curves, are essential. This is true even in the simple case of gluing done by Ruan and Tian [RT].

We work with (J,ν)-maps. A sequence of such maps into (X,V) or Z with (J,ν) as in [IP4] or [IP5] gives rise to $(J_{X,V},\nu_{X,V})$-maps into $\mathbb{P}_V$ with $J_{X,V}$ and $\nu_{X,V}$ described explicitly in Section 4.1 and above Remark 4.8 in [FZ0]. This pair is $\mathbb{C}^*$-equivariant. The regularity of this pair is equivalent to the transversality of the correct version of the bundle section in Lemma 6.3 of [IP4]. Without this transversality, there would be no relative invariants, in the semi-positive case or in general.

The (J,ν)-pair $(\widetilde{J},\pi^*\nu|_V)$ on $\mathbb{P}_V$ specified in the proof of Proposition 6.6 in [IP4] is wrong; it is also $\mathbb{C}^*$-equivariant, but does not suffice for achieving the transversality on $\mathbb{P}_V$.

IP: (c) [FZ0] ignore the δ-flat condition (Definition 3.1 in [IP5]), saying that it is "used only to rule out components mapped into V". But the δ-flat condition is essential for the decay estimates in Section 5, and for the uniformity of the pointwise bounds used throughout Sections 6-9. It is therefore essential to prove gluing. The S-matrix is needed precisely to circumvent this analytic difficulty.

As described above (as well as in [LR], [IP5], and [Lj1]), sequences of maps into Z_λ that are not δ-flat give rise to maps into $Z_0^{(k)}$ with $k\geq 1$. A key point of [LR] and [Lj1] is that such maps should be considered up to the $\mathbb{C}^*$-action on $\mathbb{P}_V$; this makes the dimensions of the strata of such maps smaller than that of the main stratum. If one is concerned only with a symplectic sum formula on the level of numbers (as is the case in [LR] and [IP5]), there are thus no such limits passing through the imposed constraints and therefore they do not need to be considered in the gluing. Because IP do not impose the $\mathbb{C}^*$-action on components into $\mathbb{P}_V$, the last paragraph in page 75 in [FZ0] arrives at the same conclusion in a slightly different way. As only the limits of δ-flat maps pass through the constraints, we do not ignore the relevance of the δ-flat condition to anything.

For the purposes of a symplectic sum formula on the level of virtual classes, one would have to consider limits of maps that are not δ-flat. However, such a formula is the subject of [Lj1, Lj2] only, not [IP5] or [LR].

IP: (d) In fact, [FZ0] have set up the gluing incorrectly. They have confused the limiting argument in [FZ0] with the limiting argument in Section 3 of [IP5], and to have confused the maps in the *boundary of the relative moduli space with the approximate maps needed in the gluing theorem. The maps in the boundary of the relative moduli space are discarded; they never enter [IP5].*

We did not set up any gluing. The limits arising from sequences of maps that are not δ-flat have components mapped into $\mathbb{P}_V$. This statement is made in [LR], [Lj1], [IP4], and [IP5]. The convergence in the first three papers is studied by re-scaling the maps in the normal direction to V, whether V is a divisor in X in the relative setting or the intersection of the divisors X and Y in the symplectic sum setting. The description of the convergence process in [IP5] outside of the δ-flat case of Section 3 is the subject of the top of page 1003. As explained above, this description has no substance and results in sequences of maps having countable collections of positive-dimensional families of limits (countable because k above can be arbitrarily large and positive-dimensional because there is no equivalence modulo the $\mathbb{C}^*$-action on $\mathbb{P}_V$ in [IP5]). Thus, IP do not establish a compactification for maps into Z and appear to suggest that they are not aware of the standard compactification (i.e. that of [Lj1]). In their terminology above, they did not even determine the correct *candidates for gluing*.

IP appear to claim that they do not see parallels between the limit process for relative maps to (X,V) and the limit process for maps to $Z \longrightarrow D$ that approach the central fiber $Z_0 = X \cup_V Y$. I am not aware of anyone else in GW-theory who does not view the two processes as being essentially the same and leading to essentially the same conclusions. This viewpoint is an important part of establishing the symplectic sum formula. During our discussion at SCGP on 03/21/14, Parker appeared to see some substance in the analogue I drew between the two limits, but it seems they have now chosen to stick by what they said in [IP5].

During the same discussion, it came out that they did not see why the $\mathbb{C}^*$-equivariant deformations are sufficient for transversality on $\mathbb{P}_V$. This is necessary to establish the existence of relative invariants, including in [IP4]. During our conversation in McDuff's office on 03/26/14, E. Ionel stated that they had not realized until recently that a pair (J,ν) in [IP4, Definition 3.2] induces a similar pair on $\mathbb{P}_V$. This is necessary to establish the compactness of relative moduli spaces in [IP4], as well as in the symplectic sum context. Such a pair in fact appears in the proof of [IP4, Proposition 6.6], but is described incorrectly, which causes problems with the transversality.

A Roundup on [IP4]

The abstract and the 4-page summary of [IP4] suggest that this paper defines relative GW-invariants for arbitrary (X,ω,V) and more generally than the relative GW-invariants of [LR]. While the relative moduli spaces in [IP4] are defined for a wider class of almost complex structures on (X,ω,V) than in [LR], relative GW-invariants for (X,ω,V) are defined in [IP4] only in a narrow range of "semi-positive" cases. According to the last paragraph of [IP4, Section 1], the main construction of relative GW-invariants in [IP4] applies to arbitrary (X,ω,V) because of a VFC construction *in a separate paper [IP5]*, listed as *in preparation* (not *work in progress*) in the references. This citation first appeared in the 2001 arXiv version; it replaced Remark 1.8 in the 1999 arXiv version, which claimed that the semi-positive restriction can be removed because of the VFC construction of [LT]. However, applying this construction would have required gluing maps with rubber components, which is not done in [IP4]. The VFC construction advertised in [IP4] is claimed in [IP6] by building on [CM]. However, [CM] first appeared on arXiv almost 5.5 years after the 2001 version of [IP4]. Furthermore, for two of the most crucial analytic points, [IP6, Lemma 7.4] and [IP6, (11.4)], which require gluing maps with rubber components, the authors cite [IP4] and [IP5]; these two papers restrict to "semi-positive" cases precisely to avoid such gluing.

In my view, the contributions of [IP4] consist of:

(IP4c1) the conditions on the (J,ν) pairs of [RT1, RT2] in [IP4, Definition 3.2] that should lead to a geometric definition of relative GW-invariants in some "semi-positive" cases. These cases, which are not specified correctly in [IP4], form a smaller portion of the overall cases than in the absolute setting of [RT1, RT2]. The introduction of (J,ν) pairs in [RT1, RT2] was a fundamental innovation that immediately led to some applications (associativity of quantum product for semi-positive symplectic manifolds and enumeration of rational curves in $\mathbb{P}^n$). It later formed the basis for the virtual class constructions in symplectic topology. In [IP4], it is simply an adaptation of this innovation in combination with a rescaling similar to the earlier [LR].

(IP4c2) a very vague suggestion that the usual relative evaluation maps can be lifted to some natural covers of the symplectic divisor and its products and that this leads to refined relative GW-invariants in some sense.

The problems in [IP4] include the following.

(IP4p1) The notion of relative map described by [IP4, Definitions 7.1,7.2] allows the contact marked points to lie in any layer (instead of just the last one). If the contact marked points are not required to lie on the last layer, the relative moduli space cannot be Hausdorff. Since [IP4, Section 6] does force the contact marked points to lie on the last layer, this is just an omission, but in the definition of the most important notion of [IP4].

(IP4p2) The relative maps are defined in [IP4] in terms of elements of the kernel of the linearized $\bar{\partial}$-operator on the normal bundle. In the proof of Proposition 6.6, they are described as $(\widetilde{J},\pi^*\nu|_V)$-holomorphic maps for an almost complex structure $\widetilde{J}$ on the $\mathbb{P}^1$-bundle

$$\pi\colon \mathbb{P}_X V \equiv (\mathcal{N}_X V \oplus \mathcal{O}_V) \longrightarrow V$$

induced from V via an arbitrary connection on $\mathcal{N}_X V$. This is incorrect because $\widetilde{J}$ should be induced by a connection arising from a torsion-free connection on TX and the correct induced ν-term on $\mathbb{P}_X V$ does not (generally) vanish in the normal direction to V; see the beginning of Section 4.1 and the equation above Remark 4.8 in [FZ0].

(IP4p3) The rescaling argument in [IP4, Section 6] does not check that the bubbles in the different layers connect (which is done in [LR]). As [LR], [IP4] does not check that the resulting moduli space is Hausdorff. Overall, [IP4, Section 6] is an imperfect adaptation of the rescaling argument of [LR, Section 3.2] for more general (J,ν)-pairs.

(IP4p4) The failure to carry out the gluing in the simplest possible case in [IP5] raises doubts about the compatibility of the (J,ν)-pairs of [IP4] with gluing as necessary for any virtual class construction. In contrast, the compatibility with gluing in [RT1] is illustrated in the proof of the associativity of quantum multiplication.

(IP4p5) The topology on the desired rim tori covers of [IP4, Section 5] is not specified. The description of this cover is wrong about the group of its deck transformations and about the resulting GW-invariants in the simple cases of [IP5, Lemmas 14.5,14.8]; see [FZ0, Section 4.3] and [FZ2, Remarks 6.5,6.8].

(IP4p6) The lifts of the relative evaluation maps to the above covers are not unique and the refined relative GW-"invariants" generally depend on the choice of such a lift; see [FZ0, Section 4.4] and [FZ1, Sections 1.1,1.2]. This makes these "invariants" not computable outside of very rare cases. It is possible to use them for some qualitative applications though, as demonstrated in [FZ1, FZ2].

B Roundup on [IP5]

According to the abstract, the long summary, and the main theorems in [IP5], i.e. *Symplectic Sum Theorem* and Theorems 10.6 and 12.3, the symplectic sum formulas in [IP5] are proved without any restrictions on X, Y, V, but most arguments are clearly restricted to "semi-positive" cases. According to the beginning of [IP5, Section 8], *the key analysis step* is *obtaining estimates on the linearization* of the $\bar{\partial}$-operator of an approximately J-holomorphic map. This part of the proof has 3 serious consecutive errors, i.e. with each sufficient to break it. The gluing argument has additional problems, leaving pretty much nothing correct in it (or the entire paper). I see no practical way of fixing it without using the SFT approach suggested by [LR] with the more regular almost complex structures of [LR].

The problems in [IP5] include the following.

(IP5p1) The operator in [IP5, (7.5)] is not the adjoint of the operator in [IP5, (7.4)] with respect to any inner-product, because the first component of its image does not satisfy the imposed average condition. This ruins the argument regarding the linearized operators being uniformly invertible at the start. The correction would have to be of L^2-type, which is not compatible with the required L^p_1-norms.

(IP5p2) Gauss's relation for curvatures, [IP5, (8.7)], is written in a peculiar way, resulting in a sign error. The sign error in [IP5, (8.7)] is crucial to establishing a uniform bound on the incorrect adjoint operator in [IP5, (7.5)].

(IP5p3) The argument at the bottom of page 984 in [IP5] implicitly presupposes that the limiting element η lies in the Sobolev space $L^{1,2}_{\mathbf{s}}$. This is the last step in establishing a uniform bound on the incorrect adjoint operator in [IP5, (7.5)].

(IP5p4) The justification for the uniform elliptic estimate in [IP5, Lemma 8.5] indicates why the degeneration of the domains does not cause a problem, but makes no comment about the degeneration of the target. It is unclear that it is in fact uniform with the chosen norms.

(IP5p5) The map Φ_λ in [IP5, Proposition 9.1] appears to be non-injective because the metrics on the target $\mathcal{Z}_\lambda$ collapse in the normal direction to the divisor V as $\lambda \longrightarrow 0$. The wording of the second-to-last paragraph on page 938 suggests that the norms are weighted to account for this collapse and the convergence estimate of [IP5, Lemma 5.4] could accommodate norms weighted heavier in the vertical direction, but the rather light weights in the norms of [IP5, Definition 6.5] appear far from sufficient.

(IP5p6) Neither the summary of [IP5] nor the proof of [IP5, Proposition 9.4] makes any mention of whether the quadratic error term in the expansion [IP5, (9.10)] of the $\bar{\partial}$-operator is uniformly bounded. The latter mentions only the need for the 0-th and 1-st order terms to be uniform (in (a) and (b) on page 939).

(IP5p7) As explained in the summary and in Section 12 in [IP5], the S-matrix appears in the main formulas (0.2) and (12.7) of [IP5] due to components of limiting maps sinking into V. Such components should correspond to maps into the rubber up to the $\mathbb{C}^*$-action on the target, just as happens in the relative maps setting of [IP4, Section 7]. This action, which is forgotten in the imprecise limiting argument of [IP5, Section 12], implies that such limits do not contribute to the GW-invariants of $X\#_V Y$ for dimensional reasons, and so the S-matrix should not appear in any symplectic sum formula of [IP5]. As shown in [FZ0, Section 6.5], the S-matrix does not matter anyway because it *acts as* the identity in all cases and not just in the cases considered in [IP5, Sections 14,15], when the S-matrix *is* the identity. I am not aware of anyone else who believes the S-matrix should have appeared in the first place.

In my view, the contributions of [IP5] consist of:

(IP5c1) a vague suggestion that refined relative GW-"invariants" give rise to a refined symplectic sum formula of some sort. This suggestion indeed leads to a refined relation between the GW-invariants of a smooth fiber $X\#_V Y$ and the singular fiber $X\cup_V Y$. Contrary to a claim in the abstract of [IP5], it does not usually lead to a refined relation between the GW-invariants of $X\#_V Y$ and any kind of relative GW-invariants of (X,V) and (Y,V) because the cohomology of products of the covers of [IP4, Section 5] usually does not admit a Kunneth decomposition; see [FZ0, Section 5.3] and [FZ2, Example 3.7]. Even when the GW-invariants of $X\#_V Y$ split into refined relative GW-"invariants" of (X,V) and (Y,V), this is rarely computationally useful because of the dependence of the latter on the choice of the lift; see Section A. I am not aware of a single application of this suggested refinement in the 14 years since the first arXiv version of [IP5]; some qualitative applications are obtained in [FZ2] though. Furthermore, the existence of the crucial refined degree gluing map [IP5, (3.10)] is never established; as indicated in [FZ2, Sections 3.1,4.1], this is a subtle issue that depends on consistent choices of certain coset representatives.

(IP5c2) fairly detailed, but not completely correct, alternative proofs of three formulas in enumerative geometry that had been previously by other methods. These are certainly nice illustrations of the power of the symplectic sum formula, especially once their exposition is properly cleaned up (the ideas behind the proofs are clear from [IP5]). However, these applications are not new results and the arguments still have gaps; none of the three main claims is even stated correctly. As noted in the middle of page 941 in [IP5], two of the applications are essentially symplectic sum re-formulations of the original proofs. The third application is fundamentally different from the original proof and also contains the most significant gap; see [FZ2, Remark 6.12]. This third application would have made for a nice IMRN level paper, but certainly not an *Annals* paper.

The general structure of the symplectic sum formula itself had been known well before the first arXiv version of [IP5] and even before [IP3]; the arXiv version of the latter predates the first version of [IP4] by 12.5 months and of [IP5] by 28.5 months. It describes relative GW-"invariants" that depend on the choice of (J,ν) because the last two conditions of [IP4, Definition 3.2] are not imposed in [IP3]; see [IP3, Theorem 2.5]. This dependence is accidentally mentioned even in [IP5, Definition 11.3]. The symplectic sum formula with cohomology insertions that do not come from the singular fiber in [IP5, Section 13] involves similar kinds of relative "invariants" that depend on auxiliary choices.

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