**On the Mirror Conjecture**

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This note is to correct some of the misperceptions and misinformation by some about the so-called mirror conjecture controversy.

In section 1, we provide a historical and mathematical background of the conjecture, including many of the ideas leading up its final proofs. We outline some of the gaps in the paper [1], which claimed a proof of the conjecture. We then highlight original contributions in our proof [2] of the conjecture. In section 2, we address point-by-point the issues raised in [5].

## 1. The dispute on the quintic mirror conjecture

### *1.1. historical and mathematical background*

In 1989, B. Greene, a postdoc of Yau at the time, and R. Plesser constructed an important example of mirror manifolds. Using physics argument, they proved the first case of mirror symmetry for Calabi-Yau threefolds. Their result is considered fundamental, though its proof lacks the precision of a mathematical proof, hence the result does not constitute a mathematical theorem. Two years later, Candelas-de la Ossa-Green-Parkes built on the result, and with even greater mathematical precision, produced a spectacular solution to a long-standing problem in enumerative geometry. Since the solution still contains important mathematical gaps, the result is considered a conjecture, now known as *the mirror conjecture.*

The conjecture asserts that the prepotential function $\mathcal{F}_{cplx}$, a section of a certain line bundle on the complex moduli space of mirror quintics, coincides with the corresponding prepotential function $\mathcal{F}_{kah}$ on the complexified Kahler moduli space of quintics in $CP^4$, after a suitable change of variables or mirror transformation. The conjectural equality (up to an irrelevant quadratic polynomial)

$$\mathcal{F}_{cplx} = \mathcal{F}_{kah} \tag{1.1}$$

became mathematically precise when an algebraic geometric definition of $\mathcal{F}_{kah}$ was given by Kontsevich [7] in 1994; among other things, gravitational descendants were also studied, and the Atiyah-Bott equivariant localization on stable map moduli was carried out to give a complete determination of $\mathcal{F}_{kah}$ in principle, and his method was able to check the conjecture numerically.

Since 1991, a group in Boston led by Yau had begun a program to study the mirror conjecture and its generalizations. A long series of papers, both in physics and mathematics, by Yau and his collaborators have appeared, and a number of important new ideas have been developed toward understanding the conjecture over the next several years. Some of those ideas have later proved crucial to the final proof of the mirror conjecture. For example, in 1993-95, Yau et al published a series of papers identifying the fundamental role of the so-called Frobenius parameter. In particular, an explicit cohomology valued formula for the period of toric CY hypersurfaces, including the quintics, was found in terms of the Frobenius parameter, in the framework of the GKZ theory. The $\mathcal{F}_{cplx}$ can be recovered from the period through the *special geometry* of the complex structure moduli space. This relation can be stated as an integral relation for the period and $\mathcal{F}_{cplx}$. This suggests that there must be an object analogous to the period that bears a similar relation to $\mathcal{F}_{kah}$ on the Kahler side. Thus, the next step in our program was to find this object. A crucial break came in 1995 when we found that the gravitational descendant does precisely that. (See Theorems 3.2ii and Theorem 3.4 [2].) Thus, the remaining piece of the puzzle (1.1) for us was to connect the period with the gravitational descendant.

Around the same time, Morrison-Plesser studied the so-called linear sigma model (after Witten), in which $N_d = CP^{(n+1)d+n}$ was explicitly used as a compactification of an algebraic analogue of loop space consisting of degree $d$ maps $CP^1 \to CP^n$, which is equipped with the standard $S^1$ action. As a simple exercise, we computed the Euler class of the normal bundle at a particular fixed point component $CP^n$ in $N_d$ and found $\prod_{k=1}^{d}(H - k\alpha)^{n+1}$, precisely the denominator in our period series formula, convincing evidence that localization on $N_d$ must be an important ingredient (an observation made by others as well.) Furthermore, there was an obvious (at least set theoretic) correspondence $\varphi$ between the graph space $M_d$ and $N_d$ that tied all of the essential structures together in a diagram. (See p730 [2].)

Earlier, Kontsevich has shown that two types of $T$ fixed points are sufficient to determine *all* of $\mathcal{F}_{kah}$: the singular ones obtained by gluing two 1-pointed stable maps, and the smooth ones given by $d$-fold covers of $CP^1$ onto a $T$ invariant line in $CP^n$ ($n = 4$.) This led us immediately to the idea that we should consider how a vector bundle near the singular fixed points would behave. We found a short exact sequence which results in a simple quadratic identity involving Euler classes (see p730 [2].) This *was* the geometrical insight that led to our notion of Euler data. By a (functorial) localization argument at the same singular fixed points, we found a similar quadratic identity on $N_d$ involving those

Euler classes. (See Theorem 2.8 [2].) But it wasn't enough to determine $\mathcal{F}_{kah}$. Since the smooth fixed points were the only information *left*, localization there must be needed to determine it all. It is obvious that the correspondence $\varphi$ is a local isomorphism at these smooth fixed points.

In March 1996, [1] claimed a proof of the mirror conjecture. There were a number of ingredients common to both the approach in [1] and ours. (See section 2 of this note.) But some were not. The main interesting idea in [1] was to use *recursions* to connect the period to the gravitational descendant. Unfortunately, many of the arguments had no details and some of them were simply wrong. Furthermore, there was no proof of the main theorem in [1], and neither was there a proof of the main conjecture (1.1).

We spent the ensuing months trying to reconstruct the proof in [1], often consulting with other experts, but without success. We eventually gave up and decided to continue with our own approach to the mirror conjecture. By mid 1997, we had completed our proof, the genesis of which was also spelled out in the introduction [2]. Prior to its publication, we had numerous e-mail communications with Givental asking him to explain details of a number of specific points in his proof. All communications with him were cc'ed to a number of other experts. Despite many attempts, we were unable to get the details necessary to ascertain the completeness of [1]. As we have documented, many independent experts were consulted on [1], and they had reached the same conclusion we did. We had, therefore, decided to publish [2], in which we cited [1] and the accompanying expert opinions we'd documented.

Two papers [3][4] appeared later in 1998; [3] partially filled the mathematical gaps in [1], and [4], using crucial arguments of [2], completed the rest of the proof in [1]. Despite the fact that results of [2] were used, we had stated publicly that [1][2] should be considered independent proofs. Most workers in the field have since credited both papers [1][2] for the solution to the mirror conjecture. Evidently, the mathematical community has spoken on this issue overwhelmingly.

*1.2. gaps in [1]*

We now point out places in [1] where arguments were wrong, missing, or incomprehensible (to us and others.) Some of them might be technical, but others were crucial. The list is not meant to be exhaustive. The reader is strongly advised to examine [1] and make an informed judgment for himself.

p9: The paper asserted that the correlator "$(t,..,t)_{n,d}$" *are* correlators of the GW theory on $X'$. We could not find a proof of this assertion anywhere in the paper or any of the references cited there. It turned out that a proof did not exist at least until around 1999.

pp13-14: The proof of Corollary 6.2. There was a 1-line assertion, without proof, of the Euler class of the normal bundle of fixed points in the graph space. But the argument there was wrong because it did not take into account several important special cases. The correct answer turned out to be crucial also for proving the "convolution" property (second assertion in "main lemma" on p35) later. A correct proof was given later in [3] pp22-26 and p78.

p42: Remarks 1)-3). This was the only other place in [1] where the *main theorem* in its introduction was mentioned. No proof was given. An assertion of this theorem, say, in the case of quintic, was a statement that a certain vector-function $I$ coincided with another vector-function $J$, after a change of variable. $I$ was an explicit power series, with the hyperplane section of $CP^4$ being one of the expansion parameters. $J$ was a certain generating function for certain virtual numbers $n_d$ of degree $d$ curves in quintics.

Remark 1) was two sentences about $S^*$ ($= I$ when $\hbar = 1$) and the mirror transformation in the 1991 paper of Candelas et al. The first was about expanding $S^*$ using the hyperplane section as a parameter; the second said that the change of variable coincided with the mirror transformation of Candelas et al.

Remark 2) first referred to a 1993 paper of Batyrev about mirror CY manifolds constructed as complete intersections in toric manifolds. This was irrelevant to the quintic case. It then asserted that *Corollary 11.8 proved the mirror conjecture described in detail in a 1993 paper of Batyrev et al.* But this 1993 paper generalized only the period computation of Candelas et al to projective complete intersections. It did not have an algebraic geometric formulation of the mirror conjecture because it *predated* Kontsevich's theory of stable maps. The 1991 *mirror conjecture* of Candelas et al states that

$$\mathcal{F}_{cplx} = \mathcal{F}_{kah}$$

where the left side is the prepotential of special geometry on the complex structure moduli space, and the right side is the prepotential on the Kahler side. This formula became a precise conjecture once the right side was mathematically defined. An algebraic geometric definition of $\mathcal{F}_{kah}$ was *not* available until stable maps were invented in 1994. Thus the references made in Remark 2) to Batyrev's 1993 papers, as important as they were, were irrelevant to the mirror conjecture for quintics.

Remark 3) read *"The description '[14]' of the quantum cohomology algebra of a Calabi-Yau 3-fold in terms of the numbers $n_d$ of rational curves of all degrees $d$ (see for instance '[9]' for the description of the corresponding class $S$ in these terms) has been rigorously* *justified in '[13]'. Combining these results with Corollary 11.8 we arrive to the theorem formulated in the introduction."*

Giving argument for the *main* theorem of a paper in a subremark was inappropriate. But more importantly, the argument was wrong. Corollary 11.8 in [1]asserted that after a change of variable, $S^*$ coincides with the $S$ series, the gravitational descendant, a generating function for certain cohomology classes on $CP^4$. '[14]' was a paper of Aspinwall-Morrison in which they derived a version of the so-called multi-cover formula. '[13]' was a paper of Manin in which the same formula was derived, but using the stable map compactification. It was a statement purely about $CP^1$, not about quintics or $CP^4$.

'[9]' was a paper of Givental in which he studied the mirror formula from the point of view of "D-modules" and ODEs. This paper made no use and made no reference to stable map theory, and it *predated* stable map theory. So, Remark 3) failed, as did 1)-2), to connect Corollary 11.8 to, let alone proving, the mirror conjecture. This was an important mathematical gap.

There were numerous other assertions in [1] that had little or no mathematical arguments, and, sometimes, no definitions.

p18: Proposition 7.1. There was just one sentence in the proof. "It can be obtained by a straightforward calculation quite analogous to that in '[2]'." Here '[2]' was a 228-page long paper of Dubrovin.

p27: Proposition 9.6. In the middle of its proof, a sentence read "It is a half of the geometrical argument mentioned above." It's not clear what this was referring to (above where? which half?)

p28: Apparently continuing with the proof of Proposition 9.6, a paragraph began with "In the Borel localization formula for $\int e_0^*(\phi)\cdots$ the fixed point will have zero contribution unless the marked point $x_0$ is mapped to the $i$-th fixed point in $CP^n$ (since $\phi_i$ has zero localizations at all other fixed points.)" It's unclear what "$\int e_0^*(\phi)\cdots$" meant and where this assertion was proved. In Lemma 9.7 (apparently crucial later), there was a similar sentence involving the notation "$\int c^{(n+1-l)d-1}\cdots$", which we couldn't follow either.

p29: Proposition 9.8. This was about certain recursion relation involving $Z_i$. The proof was again one sentence. The second and third sentences read "This is how the recursion for the correlators $z_i$ beomes possible. The rest is straightforward." Thus the second sentence said the first sentence showed how a recursion was possible, and the third said the rest was easy. We couldn't see why something being possible implies its truth. It's unclear what "the rest" meant, so it's hard to say if it was easy or not.

p30: Proposition 9.9. This was about certain uniqueness property of the recursion relations. The proof was half a sentence "Now it is easy to check" But, again since we couldn't check, it's hard to tell if it was easy or not.

p30: Lemma 10.1. Again this is about Borel localization formulas for something denoted by "$\int_{Y_{2,d}} c^{d-1} \cdots$" It's unclear what this meant.

p31: Apparently stating certain analogues of propositions in Section 9, Propositions 10.2,10.4, Corollary 10.3 were stated without proof. Prior to stating them, there was one sentence that said "Let us modify the results of Section 9 accordingly." We didn't know what "accordingly" meant here. After this was Proposition 10.5 and a corollary of it. The next page began with "We have proved the following Theorem 10.7." It's unclear how the theorem followed from those propositions or where their proofs were.

p37: Proposition 11.4. This had multiple parts. One of the key parts seemed to be (3), a recursion relation for the $Z_i$ in the Calabi-Yau case. It asserts existence and uniqueness of solution to this relation. In its proof, Lemma 9.7 and Lemma 10.1 were quoted, though we couldn't follow how there were being used. Apparently referring to two integrals appearing in $Z_i$, a sentence said "Thus these integrals have a recursive expressions identical to those of Sections 9 and 10." We could not find any recursive expression for those integrals in Section 9 or 10.

### *1.3. what's new in [2]*

We now highlight some of what's new in our approach.

In [2], the idea of Euler data was introduced. As we pointed out, the geometrical insight that led to this was a short exact sequence and the gluing identity at singular fixed points. This notion turns out to allow one to completely sidestep the recursions in [1] (which required a number complicated fixed point computations, as done in [3] later.) This idea was adopted in numerous subsequent papers.

As we pointed out earlier, one of the key ingredients in both [1][2] was the use of the Frobenius parameter to write down a cohomology valued period. The Frobenius method is of course classical in ODE theory. But the fundamental role of the Frobenius parameter in the mirror conjecture was first noticed in the 1993 paper of Hosono-Klemm-Theisen-Yau, and its generalizations in 1995 by Hosono-Lian-Yau, where the mirror conjecture was studied and generalized. Our period formula was given in terms of Gamma function earlier, but it's easy to translate them into product formula. The same idea also proved crucial in later generalizations. For example, in [12] there is an essentially the same period formula, but our papers are never cited there.

A proof of the regularity of the collapsing map (lemma 2.6 in [2]) was given. A definition of the correspondence is elementary, and was certainly well known to experts at the time. Thus no originality was claimed for this correspondence, and it's explicitly mentioned that [1] also contained the statement of the lemma. J. Li was asked to help write down a rigorous proof. The proof uses an algebraic geometric argument: essentially representing the correspondence as a natural transformation of functors realized by a universal determinant line bundle. This proof was cited in [4]. The ideas in this proof were also cited in a number of later papers. For example, these ideas, in contrast with the argument in [1], were used extensively in [15]. After [2], two proofs were given by [3]. The first one was new. The second one apparently provided details for the argument in [1], with a number of differences. The second proof in [3] constructs the line bundle differently on a "prequotient" to realize the same correspondence.

The collapsing map serves two purposes in our approach. One, to set up the so-called functorial localization: an equivariant commutative diagram involving the linear sigma model, the graph space, a singular fixed point component on the graph space and its counterpart in the linear sigma model. It was proved in [2] using the Atiyah-Bott formula. The commutative diagram gave us a simple way to compare a class on one space (restricted to a fixed point component) and its pushforward. This functorial localization method, as far as we know, was new. This was a key method used in a number of subsequent papers (e.g. Theorem 4.6 [15].) The collapsing map was also used to compare Euler data at certain smooth fixed points in [2]. Here a trivial but important observation was that the map is a local isomorphism at those smooth fixed points.

We derived the Euler class of the normal bundle of fixed points in the graph space $M_d$ in Theorem 2.8 [2]. This was a crucial result needed to carry out the functorial localization. The very same result was used *later* in [3] in order to correct and complete at least two crucial proofs in [1]: Corollary 6.2 and the "convolution" property.

We introduced the notion concavex bundles. For the quintic mirror conjecture, this is not strictly necessary. But the point is that our approach applied naturally and immediately to this general setting. In particular, it gave a very easy but different proof of the multiple cover formula. There were two earlier approaches to the same formula using different methods. Our approach also applies immediately to general multiplicative classes, of which the Euler class is the simplest example.

The proof (Theorem 3.4 in [2]) that the prepotential $\mathcal{F}_{kah}$, generating Euler numbers on stable map moduli spaces, coincides with the prepotential $\mathcal{F}_{cplx}$, was new. A crucial step in this proof was the *special geometry* relation Theorem 3.2ii [2], which remains the only way we know to connect $\mathcal{F}_{kah}$ to the Euler data arising from the gravitational descendant, and to see how it is the prepotential of the special geometry on the Kahler moduli space. The same relation has been quoted and used by others in recent papers.

Note: B. Kim, a fresh PhD student of A. Givental at the time, posted a paper (alg-geom/9712008v1) right before our preprint of [2] appeared (alg-geom/9712011.) Several weeks later, he posted a new version (alg-geom/9712008v3) in which substantial revisions had been made. In particular, a key integral formula, virtually identical to that in Theorem 3.2ii in [2], was added in the new version to connect the gravitational descendant to Euler numbers on stable map moduli spaces. In the following year in [4], the very same integral formula was used to fill a mathematical gap in [1], and Kim's paper was cited as a reference for this result. Another substantial revision was the inclusion of a proof of the "double construction" using a crucial result on the normal bundles of fixed points to carry out localization on the graph space. Again, that result first appeared and proved in Theorem 2.8 [2]. Kim also used the notion of *concavex* bundles, introduced in [2]. Our preprint was not cited anywhere in Kim's.

## 2. The footnote [5]

We shall address point-by-point (our response labelled "R" below) the issues raised in [5], where the two papers [1][2] on the mirror conjecture are compared. First, note that what is actually compared is aspects of the overall strategies adopted by the two papers, *not* the proofs; what's pointed out in [5] is a parallel of the two sets of languages (and notations), *not* the underlying mathematical structures of the two proofs. Second, since so many pre-existing sources have influenced both papers, it would be surprising if there were no parallel, especially given the narrow scope of the mathematical problem at hand. Third, the comparison in [5] is inaccurate in that it often overstates results in [1], understates those in [2] or ignore altogether the mathematical insights leading to them, as we shall point out. For a more comprehensive comparison of the two papers [1][2], see [16].

**1.** *The genus* 0 *mirror conjecture for complete intersections in the projective space* $X = CP^n$ *has now five proofs — the four variations of the same proof (in [1] , in [12], the one outlined above but applied to convex bundles over* $X$ *instead of concave bundles, and the one in Section* 5 *of this paper based on nonlinear Serre duality), and the proof recently given in [2] . Here we compare the methods in [2] with our approach.*

**R1.** The first statement in **1** understates the generality of [2] . Not only does it apply to general concavex bundles, a notion introduced in [2], but also does it apply to general multiplicative classes, of which Euler class is the simplest example, and it is certainly more general then [1]. Second, it is more accurate to say that [5] compares merely aspects of the overall strategies of [1] [2], and not the proofs.

**2.** *The key idea (see Step* 2 *above) — to study GW-invariants of the product* $X \times CP^1$ *equivariant with respect to the* $S^1$*-action on* $CP^1$ *instead of GW-invariants on* $X$ *— is borrowed in [2] from our paper [1] , Sections* 6 *and* 11*. In fact this idea is profoundly rooted in the heuristic interpretation [13]of GW-invariants of* $X$ *in terms of Floer cohomology theory on the loop space* $LX$ *where the* $S^1$*-action is given by rotation of loops. The generator in the cohomology algebra of* $BS^1$ *denoted* $\hbar$ *in our papers corresponds to* $\alpha$ *in [2].*

**R2.** The first statement in **2** is factually wrong. [2] did *not* borrow this idea from [1]. First, there is an obvious identification (parameterized vs. unparameterized curve)

$$\{CP^1 \xrightarrow{f} CP^n\} \equiv \{CP^1 \xrightarrow{(f,id)} CP^n \times CP^1\} \tag{2.1}$$

(This was also pointed out in hep-th/9401130.) For degree $d$ maps, what [1] called *the toric compactification* of the left side is what [2] called *a linear sigma model*, following [6] which introduced it explicitly in 1994 after [8] to do just that: to give a “naive” compactification of the algebraic analogue of loop space of $CP^n$. This *is* $CP^{(n+1)d+n}$. In the same year the stable map theory [7] gave a compactification of the right side. This is the graph space $M_d$. Second, the idea of circle action on loop spaces is ancient. The heuristic interpretation of the relationship between Floer cohomology and quantum cohomology mentioned in **2** goes back to at least [9] of 1993, (which entitled *“On equivalence of Floer’s and quantum cohomology,”*) which predated [13] by a year. Thus the idea of $S^1$ localization on loop spaces and the background heuristic behind the Floer/quantum cohomology relation seem to be known well before [1] or [13]. The idea was used in [1] as well, and we have also cited this in [2]. For a description of how the $S^1$ localization arose and how the earlier papers influenced [2] on this point, see subsection 1.1. Third, the issue here is *full proofs*, since heuristic arguments were already available from physics.

**3.** *Another idea, which is used in all known proofs and is due to M. Kontsevich [1994], is to replace the virtual fundamental cycles of spaces of curves in a complete intersection by the Euler cycles of suitable vector bundles over spaces of curves in the ambient space. Both papers [1] and [2] are based on computing the push forward of such cycles* *to simpler spaces. Namely, the cycles are $S^1$-equivariant Euler classes of suitable bundles over stable map compactifications of spaces of bi-degree $(d, 1)$ rational curves in $X \times CP^1$, the simpler spaces are toric compactifications of spaces of degree d maps $CP^1 \to X = CP^n$, and the push-forwards are denoted $E_d$ in [1] and $\varphi_!(\chi_d)$ in [2].*

**R3.** There seems to be no real mathematical issue raised here, except that the comparison of notations is wrong, because $\varphi_!(\chi_d)$ was never used in [2], and the notation $E_d$ apparently denoted more than one different objects in Propositions 11.4 and 11.6 [1]. The correct comparison should be that the notation $\varphi_! e_T(V_d)$ in [2] corresponds to a certain 0-pointed analogue of the notation $\mu_*(\mathcal{E}_d)$ in [1].

**4.** *The toric compactification is just the projective space $CP^{(n+1)d+n}$ of $(n+1)$-tuples of degree $\leq d$ polynomials in one variable $z$, which genericly describe degree $d$ maps $CP^1 \to X = CP^n$; the space is provided with the $S^1$-action $z \mapsto z\exp(it)$ (as in the loop space!) Thus both papers depend on continuity of certain natural map (denoted $\mu$ in [1] and $\varphi$ in [2] ) between the two compactifications. The continuity is stated in [2] as Lemma 2.6. It coincides with our Main Lemma in [1] , Section 11. The proof of Lemma 2.6 attributed in [2] to J. Li coincides with our proof of the Main Lemma. The difference occurs in the proof of a key step formulated as Claim in [1] : our proof of the Claim by bare hand inductive computation in the spirit of G. Segal's representation of vector bundles over curves via loop groups is replaced (and this is the contribution of J. Li) by a more standard algebraic-geometrical argument based on the proof of Theorem 9.9 in Hartshorne's book. It is worth repeating here the remark from [1] that a different proof of the lemma was provided to me by M. Kontsevich, with whom we first discussed the map between the two compactifications in Fall 1994.*

**R4.** By collapsing all but one component of a rational curve, (2.1) extends immediately to a set theoretic correspondence $M_d \to CP^{(n+1)d+n}$. This correspondence (and a number of variants thereof) was, of course, known to experts before [1], including Kontsevich. The regularity proof in [2] was written with J. Li's help. One should not confuse Lemma 2.6 [2] with the Main Lemma [1]. First, the former statement is (a 0-pointed version of) one of two assertions in the latter (which is a 2-pointed version.) Second, the proofs are *not* the same. The contrast is explained in subsection 1.3. The main point in the regularity proof in [2] is the construction of the universal determinant line bundle, not Theorem 9.9 in Hartshorne's book. Third, the proof of the second assertion, the convolution property, in [1] is wrong. It turns out that a correct proof requires a crucial result on normal bundles obtained first in [2]. In fact, [3] gives a correct proof later using essentially this result (see pp22-26 and p78 there.)

**5.** *The new concept introduced in [1] — the eulerity property of the classes $E_d$ (Definition 2.3 in [2] ) — is to replace both the recursion relation (Step 1 above) and the polynomiality property (Step 2) of the gravitational GW-invariant (J in the above outline). Eulerity is actually equivalent to recursion + polynomiality. Theorem 2.5 in [2] asserting the eulerity property of the classes $\{E_d\}$ coincides with Proposition 11.4(2) in [1] deduced there from the recursion + polynomiality. The proof of Theorem 2.5 in [2] is based on the same localization to fixed points of $S^1$-action on spaces of curves as in our proof of Corollary 6.2 in [1] which guarantees the polynomiality. The recursion is derived in [1] by further fixed point localization with respect to the torus acting on $X = CP^n$. Thus the proof in [2] shows that the latter localization argument is unnecessary.*

**R5.** We are, of course, glad that [5] acknowledged our contributions. However, statement **5** is inaccurate and ignoring our mathematical insight leading to Euler data in our approach. First, the new notion of Euler data was not introduced in [2] for the purpose of *replacing* recursions+polynomiality. As spelled out on p2 [2], Euler data is a notion built on the linear sigma model $CP^{(n+1)d+n}$ to *capture* the behavior of vector bundles at singular fixed points, with respect to the multiplicative nature of the Euler class. These singular points arise from gluing two 1-pointed stable maps, as described in [7] in 1994 and did *not* come from [1]. Second, there may well be a language dictionary to compare *statements* about Euler data to those about recursions solutions, *after the fact.* But this is merely a linguistic comparison. Furthermore, it is misleading. For in order to compare the mathematical contents, one must first *derive* the recursions with full proofs, which the Euler data approach completely sidesteps, not to mention that the two notions are defined on two different spaces. Note that a lot of the missing details in [1] (cf. [3]) were in fact directly related to the recursions. Third, there is *no way* Theorem 2.5 [2] can coincide with Proposition 11.4(2) [1]. The former is a statement that the gravitational descendant gives rise to an Euler data. The latter is a completely different statement about certain polynomials being determined by their values at certain fixed points. Fourth, the argument in [1] is the sentence: "(2) follows from the definition of $\Phi$ in terms of $Z_i$." The proof of Theorem 2.5 of [2], on the other hand, is an application of functorial localization at the singular fixed points, and the short exact sequence mentioned above, plus a crucial result on normal bundles, as in pp740-744 [2]. A more plausible comparison may be the *statement* of Theorem 2.5 [2] and some version of the second assertion of "Main Lemma" [1]. But once again, it is important to keep in mind that at issue here is *full proofs*, not languages or notations.

**6.** *The relationship among the two solutions to the recursion relation — the gravitational GW-invariant and the explicitly defined hypergeometric series (I in the above outline)*

*— is based on some uniqueness result (Proposition* 11.5 *in [1] ) for solutions to the recursion relation satisfying the polynomiality property. The corresponding result in [2] is Theorem* 2.11 *about linked Euler data. Linked there translates to our terminology as the recursion coefficients in the recursion relations for $I$ and $J$ being the same. The proof of the uniqueness result in [2] is the same as in [1] or [12]. The difference is that the uniqueness property is formulated in [2] solely in terms of the Euler data $\{E_d\}$ and not in terms of gravitational GW-invariant the data generate.*

**R6.** Again, the notion of linking in [2] was *not* introduced for the purpose of *replacing or translating to* the recursions coefficients in [1]. But rather, it was to *capture* the structure of the *smooth* fixed points, given by maps $CP^1 \to CP^n$ which are $d$-fold covers of a $T$-invariant line in $CP^n$. Why these smooth fixed points? First, [7] has already shown that precisely two types of fixed points: the singular ones and the smooth ones just described are sufficient to determine *all* virtual numbers $n_d$ for quintics in $CP^4$. Thus, any proof of the mirror conjecture by localization should at least include a reformulation of this result. The question is *how*? In [2], we saw that the singular fixed points enter through an exact sequence and Euler data. Thus the remaining question is how the smooth fixed points enter. Since our only task at hand is to compare two Euler data via a mirror transformation, and the smooth fixed points are, by default, the only information left, one must expect that they enter the comparison of the two Euler data, which as we saw has nothing to do with recursions. In our approach, the *geometrical origin* of linking is precisely that $\varphi$ is a local isomorphism at the smooth fixed points, an easy fact, and it is not about and does *not* require deriving recursions or computing their coefficients.

Obviously, having a dictionary for two notions *after the fact* sometimes allows you to restate a result about one notion in the *language* of the other. But it is *not* true that theorems, let alone their proofs, about Euler data and their mathematical origin must be *coming from* their recursions-solutions counterparts, or vice versa.

**7.** *The uniqueness result allows to identify the gravitational and hypergeometric solutions to the recursion by some changes of variables (the mirror transformations). This is deduced in [1] from Proposition* 11.6 *which states that both the recursion relation and the polynomiality property are preserved by the mirror transformation (see Step* 4 *above). The corresponding result in [2] is Lemma* 2.15 *which says that the (equivalent!) eulerity property is invariant under mirror transformations. It turns out however that while it is straightforward to check the invariance of recursion and polynomiality (Proposition* 11.6 *in [1] ), it is technically harder to give a direct proof of the invariance of eulerity, which requires the notion of lagrangian lifts introduced in [2] . The use of lagrangian lifts is therefore unnecessary.*

**R7.** Our proof of Lemma 2.15 [2] is two pages of pure manipulations. We, however, would wager that if we leave out details the way it was routinely done in [1], it could have been done in five lines. It is not appropriate to point to a technical lemma to suggest that [2] is inferior to [1] in terms of degree of technicality. This suggestion is, of course, false. To actually fill gaps in [1], the paper [3], which does not even prove the main theorem, already exceeds 80 pages. On the other hand, the full proof of the mirror conjecture in our approach [2] takes less than 20 pages, not to mention the greater conceptual generality of [2] and its instant universal acceptance. That, we believe, is a more appropriate way to compare the degree of technicality of the two approaches.

**8.** *The last part of the proof in [2] (see Section* 3 *there) addresses the following issue: while the previous results allow to compute some GW-invariants in terms of hypergeometric functions, what do these GW-invariants have to do with the structural constants of quantum cohomology algebra involved in the formulation of the mirror conjecture?*

*The computational approach to the issue in [2] is also not free of overlaps with [1]. However it remains unclear to us why the authors of [2] ignore the fundamental relationship between the gravitational GW-invariant and quantum cohomology which resolves the issue momentarily. The relationship was described by R. Dijkgraaf and B. Dubrovin [10] in the axiomatic context of* 2*-dimensional field theories and adjusted to the setting of equivariant GW-theory in Section* 6 *of [1]. According to these results the structural constants of quantum cohomology algebra (such as Yukawa coupling in the case of quintic* 3*-folds) are coefficients of the linear differential equations satisfied by the gravitational GW-invariants in question. In fact such a relationship was the initial point of the whole project started by [11],[13] and completed in [1][12].*

**R8.** First, **8** has misstated the issue. The mirror conjecture of Candelas et al *is* the statement that (1.1)

$$\mathcal{F}_{cplx} = \mathcal{F}_{kah}.$$

The prepotential $\mathcal{F}_{kah}$ may, *in principle*, be determined by determining the quantum differential equation in question. But that is *not* the issue. The issue is *proving* the conjecture (1.1). After all, it has already been known since 1994 [7], that $\mathcal{F}_{kah}$ can be determined in principle by an *explicit* recursion relation of GW invariants.

Second, section 3 of [2] is *not* a computational approach. Instead it is a series of applications of the *Mirror Principle*, chief among them is the proof of the mirror conjecture (1.1). For this, a key conceptual result Theorem 3.2ii [2] is crucial, which says that $\mathcal{F}_{kah}$ bears the same *special geometry* relation to the gravitational descendant, as $\mathcal{F}_{cplx}$ does to the period. Special geometry is a conceptual underpinning of mirror symmetry for Calabi-Yau threefolds. Thus, calling our result "computational" is a gross mischaracterization. A year after [2], the paper [4] uses the special geometry relation to fill a mathematical gap in [1], precisely on this point. In fact, it is shown that our result can be reproved using three equations of GW invariants, of which [4](p5) sketches a proof. In turn, the proof relies at least on a key comparison result obtained in alg-geom/9608011, which appeared five months *after* [1]. Neither the special geometry relation in [2] nor those three equations of GW invariants in [4] are contained or cited in section 6 of [1]. Third, **8** falsely claims that (1.1) follows immediately ("resolves momentarily") from section 6 of [1]. Contrary to the claim, the purported argument for (1.1) (remarks 1-3 on p42 of [1]) does not even mention section 6 anywhere, nor is there a statement of our result or its equivalent, let alone its proof. (See 'Note' in subsection 1.3 above.)

The last sentence in **8** falsely claims that [1] already contains a full proof of the mirror conjecture, for we have already given ample reasons to show otherwise. The paper [12] contains an argument for a toric version of the mirror conjecture which was based on eight explicitly stated axioms, *not* proved in [12]. Thus it is inappropriate to call the project "completed." A complete proof of the general toric mirror conjecture appears in [14] without assuming any axioms.

**9.** *Thus the two proofs of the same theorem appear to be variants of the same proof rather than two different ones, except that our reference to the general theory of equivariant quantum cohomology, developed in [1] , Sections* $1-6$*, for concave and convex vector bundles over convex manifolds, is replaced in [2] by a computation.*

**R9.** This false conclusion in **9** is based, in large part, on a number inaccurate or factually wrong assumptions, which we have addressed above. Again, the main theorem we have proved in [2] is the mirror conjecture (1.1). It is *not* proved in [1], neither is our proof a variant of [1]. What is indicated in [5] is merely a parallel between some aspects of the respective overall strategies and languages adopted in the two approaches. Their underlying mathematical structures, not to mention the proofs, are different, as we have explained.

**10.** *It is worth straightening some inaccuracy of [2] in quotation. As it is commonly known, "Givental's idea of studying equivariant Euler classes" (see p.* 1 *in [2] ) is due to M. Kontsevich [1994] who proposed a fixed point computation of such classes via summation over trees. The idea of the equivariant version of quantum cohomology* *listed on p.* 6 *of [2] among "a number of beautiful ideas introduced by Givental in [1][11]" was actually suggested two years earlier in [GK] by a different group of authors. The statement in the abstract that the paper [2] "is completing the program started by Candelas et al, Kontsevich, Manin and Givental, to compute rigorously the instanton prepotential function for the quintic in* $P^4$*" is also misleading: the paper is more likely to confirm that the program has been complete for two years.*

**R10.** It seems that [5] has mistakenly relied on an old draft of [2]. The statements quoted might have been inaccurate, but for reasons different than those asserted in **10**. In fact, we had decided to revise a number of statements, including the above-mentioned, in an old draft long before [5] appeared. For example, since we were unable to ascertain the completeness of [1] even after repeated communications with the author, we felt that perhaps calling some of the ideas there "beautiful" might have been premature at that point.

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