Referee queries on the second version of *Donaldson–Thomas invariants of local elliptic surfaces...* **by authors Jim Bryan and Martijn Kool**

Many thanks to the authors for responding to my comments and for producing a new version of this paper. I am afraid however that I have to continue to ask questions concerning my original Question 1, in relation to the core of the argument which relies on the use of the fpqc topology. In preparing this review, I have also consulted a colleague with more expertise in the formal aspects of algebraic geometry.

1. Definition 4, where an open subset is allowed to be "possibly in the fpqc topology" is already quite problematic; we wonder whether the authors really mean what they seem to write there ("Cohen-Macaulay subscheme of dimension 1" might be too broad in the context of fpqc maps). On the other hand, this falseness might be cancelled by the imprecise use of "restriction" when one is dealing with fpqc maps. (And it is misleading to use the phrase "open set" in this situation.)

2. In Section 5.1, they introduce the "open cover" they will use and then make a very confusing statement about "no embedded points or zero dimensional components". We are still confused about how these "small balls" intersect. (A lower-tech example: if one takes two points x and y in an integral curve, the spectra of the complete local rings both have the generic point in their support, so the "intersection" of these balls has support over the generic point.)

3. Definition 10 and the discussion of what it actually means (that takes place in the following paragraph) is extremely vague. Affine formal schemes and (usual) spectra of adic rings appear to be conflated. We are particularly confused about items (5), (6), and (7) on the list. There are some clues about how the authors are thinking about these things in the following text, but we are not convinced it is correct.

   Here is a concrete situation: take a conic curve $Y \subset \mathbf{P}^2$ and remove a point $c \in Y$ to yield an open subscheme C of Y. The authors seem to be claiming that there is a complete ring A and a map flat Spec $A \to \mathbf{P}^2$ that induces an isomorphism beween C and Spec A/I (where I is an appropriate ideal of definition). The standard way one would try to do this (which the authors hint at) is to take an open affine of $\mathbf{P}^2$ containing C as a closed subscheme and complete with respect to the ideal of C. But there is no such affine in this toy example. What could one do instead? One could take the inverse limit of the rings $A_n = \Gamma(O_X/I_C^{n+1})$, where $X = \mathbf{P}^2 - c$ and I_C is the ideal sheaf of C in X. Perhaps $A_{n+1} \to A_n$ is always surjective, but not we don't understand why $\mathrm{Spec}(lim A_n) \to X$ is flat. Is it?

   The authors should explain why this not a fatal problem for their construction. Even if this is OK, this part of the paper should be rewritten with an explanation of what they are actually talking about. If they can make this work, it would be a service to the profession to write it more carefully, since we suspect others would be interested in how it works.

4. As the authors themselves point out, they aren't really using fpqc descent. We are not convinced by what's written in Lemma 11, since they don't explain what they really mean by the Hilbert schemes they are looking at. (For example, since completion does not commute with base change, we are already not sure if the punctual Hilbert scheme can really be written as a Hilbert scheme of the complete local ring.) The arguments seem to be made pointwise, but (notwithstanding their one-line argument) it is not clear to us why this is sufficient to determine the algebraic structure. Perhaps the objects that the authors are discussing may each be supported on some infinitesimal thickening of the curve. Perhaps they can get away with working on the formal schemes (or thinking about the ind-Hilbert schemes associated to the systems of thickenings). They almost describe a proof of the requisite decomposition in the ind-context. But all of this should be spelled out in much greater detail.

In conclusion, I continue to think that this key reduction argument in the paper is problematic, and at the very least badly explained; possibly incomplete. I cannot make a recommendation on the paper until these points are adequately addressed.

The referee