Report of Reviewer 1 and our responses

In their paper: "Hyperuniformity at the absorbing state transition..." the authors compute the critical exponents in an $\epsilon = 4 - d$ expansion of a class of models showing hyperuniformity of the total density of particles. This article deals with a very interesting subject, aims to be clear, and presents results that seem reasonable and that reproduce known results when they should. However, even though the authors strive to explain as clearly as possible the assumptions on which their calculations are based, there remain too many unexplained/unjustified points to recommend the publication of this article as it stands.

I will first list my main comments in order of importance, followed by the less important points.

Remark 1. The authors repeatedly mention the existence of non-renormalizable infrared (IR) divergences in their theory (introduction pages 5 et 6, Appendix B). But the very concept of renormalizable or non-renormalizable IR divergences is meaningless: Perturbative renormalization only deals with the recursive elimination of UV divergences by modification of the short-distance structure of the theory (under the form of the addition of local counter-terms). IR singularities should certainly not be eliminated and in any case it is impossible to do so (at least if we want to remain in the same universality class). These singularities can be regularized by slightly shifting the system away from criticality by modifying a control parameter (temperature, reaction rates, etc.). But the singularities are not eliminated: fine-tuning the control parameter to bring it back to its critical value causes the singularities to reappear. As this is the universally accepted vocabulary of perturbative renormalisation, I suggest that the authors adhere to it.

We thank the referee for this helpful comment. However it affects only the presentation and not the substance of our calculations. To address it, we have systematically modified all inaccurate wordings of 'non-renormalisable IR divergences' to 'algebraic (not logarithmic) IR divergences'.

Remark 1' (less important than 1). In somewhat the same vein, the authors mention several times that certain couplings can be generated at the fixed point. This is not how renormalization group (RG) works. When couplings that are absent in the bare action are generated, they are generically generated from the first RG step, not only at the fixed point (which implies that they are also important when the system is not critical).

This is also a matter of presentation only. The RG agenda can be presented in two different languages; 'the field theoretic' one in which performs RG at the fixed point and the 'Wilsonian' one which is framed as a flow towards the fixed point. Historically, the Doi-Peliti theory has generally been presented in field-theoretic terms. Therefore, throughout the manuscript, we have used the field-theoretic RG language, where instead of a 'RG flow', we describe the RG to be performed exactly at the fixed point. We clarify this potentially confusing point by adding an Appendix A. Please see **pages 33-34** of the diff file or the revised manuscript.

Remark 2. I know of two circumstances that prevent a coupling to be generated along the RG flow: (i) symmetries: they exclude non-symmetric terms to be generated under RG transformations and (ii) algebraic constraints such as for instance causality and Ito's prescription which imply that both the bare action and the effective action (the generating functional of 1PI vertex functions) are proportional to the response field (no term without a response field can be generated). Except for these two cases, all possible couplings are generated as soon as coarse-graining, that is, RG transformations, is performed. This implies that if a set of n relevant (in the RG sense) couplings is absent in the RG flow near a fixed point, n fine tunings of coupling constants have been implemented in the bare action. This is what happens for instance at the tricritical fixed point of the Ising model: it is twice unstable in the IR which means that two fine tunings of bare parameters are required to reach the tricritical hypersurface (which is therefore of codimension two in the infinite dimensional coupling constant space). I therefore agree with the authors when they state that a model may have fewer divergent terms than one might naively expect from symmetries and power counting, but I would like to emphasize a crucial point that in my opinion has been overlooked in the article: this comes at a price, namely the fine tuning of as many couplings as there are relevant directions at the fixed point. Thus, the analogy with tricriticality put forward by the authors does not look relevant to me because if this is the mechanism at work in their case, it means that they are not studying a critical phase transition but rather a multicritical one, a situation that does not seem to correspond to the microscopic system under study. This crucial point requires clarification.

Within a Doi-Peliti perspective, the RO fixed point does exhibit features of a multicritical point. In particular, the stability analysis of our RO fixed point reveals an additional relevant direction besides the usual mass term, and extra fine tuning ensuring total particle conservation is required to reach the RO fixed point. More generally, this implies that while there is an explicit tuning of the mass term(either of total density, or of shear amplitude in the experimental realisation of periodically sheared colloidal suspensions), there also exists a fine tuning as a result of the symmetries preserved in RG. This is precisely the scenario at a tricritical fixed point, whereby the additional tuning ensuring the absence of the quartic term is required to reach the tricritical, not critical ($d_c = 4$), fixed point. We clarify this accordingly on **page 26** of the revised manuscript, or **page 27** of the diff file: This again bears a high resemblance to the analysis of the tricritical Ising fixed point [45, 46]. In order to determine their critical properties, certain terms in the action are suppressed. Rather than fully characterising the cancellation mechanism at play, we make use of its presence to remove undesired algebraic (not logarithmic) divergences.

Remark 3. Symmetries play a considerable role in the definition of universality classes and the authors are well aware of this. However, they do not perform a systematic study of all the symmetries of the bare action coming from the master Eq.(1), that is, before the Doi shift. The interaction part of the action reads: $\kappa a^\dagger(a^\dagger - p^\dagger)ap + \mu(p^\dagger - a^\dagger)a$. This action has an obvious $U(1)$ symmetry consisting in multiplying the a and p fields by a phase factor $\exp(i\alpha)$ and the complex conjugate fields by $\exp(-i\alpha)$ with α a real number. The conserved charge associated with this symmetry is the total particle number and its conservation is therefore clearly related to a symmetry of the action. Notice that performing the Doi shift hides this symmetry (for this reason, this shift can be dangerous as emphasized by Cardy and Tauber in their article: "Field Theory of Branching and Annihilating Random Walks"). Of course, the translation of the annihilation fields by a0 and p0 also hides this symmetry.

Remark 3'. The authors repeatedly refer to "implicit symmetries" (in the introduction for instance) without defining what this means and what they consist of (I do not know what an implicit symmetry is).

We agree with the referee that the shifts we performed, including the Doi shifts in the creation fields and the a_0, p_0 shifts in the annihilation fields, may hide apparent symmetries that are related to total number conservation. These are what we referred to as 'implicit'. Nevertheless, in the first scenario (Doi shifts), we have found an alternative symmetry that is equivalently related to total number conservation: that $\tilde{a} - \tilde{p}$ must be a factor of the action, as proved in Section 4.3. In relation to the terms in (a_0, p_0), we explain our reasoning in response to the closely related Remark 6.

As suggested,we have added discussions of (1) the symmetries apparently present before the Doi shift into the manuscript, and (2) a very brief explanation of why Doi shift is performed after all. This can be found on **page 9** of the revised manuscript or the diff file: 'While the Doi shift disguises the $U(1)$ symmetry, a consequence which could potentially lead to symmetries being overlooked [40], we keep track of the associated particle conservation and use the Doi-shift to conveniently cancel undesired sink-terms, that appear as surface terms in the construction of the field theory or as additional terms in the master equation implementing spontaneous extinction.'We have also deleted the confusing wording 'implicit'.

Remark 4. The authors write in section 3.2 that the shift by a0 and p0 refer to "the mean densities of a set of Poisson-distributed active and passive particles initialised in the distant past". This seems weird because: (i) this contradicts what the authors do when they translate the $a(x,t)$ and $p(x,t)$ fields by their mean field values $a_0 = \rho_A = \rho - \mu/\kappa, p_0 = \mu/\kappa$. These values are independent of the initial distributions of the A and P particles (and they depend on μ and κ that are of course independent of the initial distributions), (ii) in the long time limit, the initial distributions of the particles play no longer any role (at least for universal quantities) and only the total density remains unchanged.

Remark 4'. Then the authors claim that at gaussian level these mean densities remain unchanged under time evolution (before Eq.(5)). This is not correct if we consider the bilinear terms in the creation and annihilation operators of the A and P particles that describe diffusion and transmutation of the A particles into P particles: Taking only these terms into account, all A particles are transmutated into P particles in the long time limit, so that the average density of A and P particles evolves due to the Gaussian terms.

We have revised our wording on **page 9** of the manuscript (or the diff file) when introducing the shift to clarify that the shift mathematically only represents the initialisation and not exactly a translation of fields by mean values: 'These shifts should not be simply interpreted as perturbations away from mean densities (particularly as annihilation fields in Doi-Peliti formalisms are *not* exactly mapped to actual particle densities); rather, a_0 and p_0 are the mean densities of a set of *Poisson-distributed* active and passive particles *initialised in the distant past* [32].' We have also revised wording about initialised (Poissonian) mean and large-time limit, in order to clarify that we *chose* a particular initialisation where the initialised mean densities do not evolve, on **pages 9-10** of the revised manuscript (or the diff file): 'At Gaussian (linear) level, the passive particle density converges to the critical (total) density $\rho_{c,g} = \mu/\kappa$, where $\rho_{c,g}$ carries the suffix g to indicate that it is the bare Gaussian value of the critical density, and the active particle density converges to $\rho - \rho_{c,g}$, together conserving total particle number. Consequently, if we initialise with Poisson-distributed passive density $p_0 = \rho_{c,g} = \mu/\kappa$ and $a_0 = \rho - \rho_{c,g}$, the large-time limits of active and passive densities remain unchanged from their initial mean values.'

Remark 5. The authors claim that the presence of a dangerously irrelevant term in their theory plays an important role and modifies the hyperuniformity exponent. Their claim relies on the presence of a diffusive noise in the active phase which is not taken into account in some other version of the model. However this noise term is only explicit in the Cole-Hopf formulation of the model, not in that of Doi-Peliti where it is automatically taken into account. Consequently, the authors cannot explicitly show a term that would be dangerously irrelevant in their perturbative calculation, and the meaning of all this in this formulation is unclear (see also Remark 12 below).

We fully agree with the referee's comment and this is an important subtlety in our work: in the Doi-Peliti formalism, the conserved diffusive noise in the Langevin equation for active particles is not introduced as a separate term but instead automatically implemented alongside the deterministic diffusion. Therefore, we cannot find a simple way of separating the noise or identifying a term associated solely with it. The 'dangerous irrelevant' nature of this noise is most easily seen in the active phase at the Gaussian level, where the Cole-Hopf transformation (at field level within the Gaussian approximation, see response to Remark 12) finds an explicit additive noise, modifying the hyperuniformity exponent from 2 to 0. Beyond Gaussian results, we cannot unambiguously isolate a dangerously irrelevant term within the Doi-Peliti formalism; to explicitly *prove* dangerous irrelevance, the calculation with the conserved noise has to be done in the response field formalism, and most likely via functional RG, which is well outside the scope of the current work. Nevertheless, we have presented circumstantial *evidence* of dangerous irrelevance and have further added a discussion of its formal definition via scaling functions. This is pasted below, and can be found on **page 30** of the revised manuscript or the diff file.

'The latter case of dangerous irrelevance has a formal description [50], where with respect to a physical observable $Y(g_0, g_1(u))$, the coupling u is defined as 'dangerously irrelevant' if despite u flowing to zero, $Y(g_0, g_1(u \to 0))$ is ill-defined, for instance because g_1 contain inverse powers of the irrelevant coupling u. The ϕ^4 coupling for $d \geq 4$ in the Ginzburg-Landau ϕ^4-theory is an example of this, as the magnetisation observable contains inverse powers of the quartic coupling. In the following paragraphs, we explore a possible formal mechanism of how the conserved noise enters the structure factor observable as a dangerous irrelevant operator.'

We have also replaced a section of text on **page 30** of the revised manuscript (or **page 31** of the diff file) with the following:

'Formally, the equality between the scaling dimensions $[\rho - \rho_c] = [\delta\rho]$ in [17] is equivalent to arguing that the local fluctuation amplitude inside a region of size ξ^d, $\delta\rho_\xi$, is comparable to the (global) distance to the critical point $\delta\rho_\infty := \rho - \rho_c$. Writing the structure factor $S(q)$ in terms of a general scaling function $S(q) = q^\varsigma F(g_0 := q\xi, g_1 := \delta\rho_\xi/\delta\rho_\infty)$, the absence of conserved noise thus reduces the general form to $S(q) = q^\varsigma F(g_0 := q\xi, g_1 = O(1))$. When conserved noise is present (which flows to zero under RG rather than being strictly absent) in the mapping between ρ and ρ_A, the previous equality between scaling dimensions $[\rho - \rho_c] = [\delta\rho]$ is no longer exact and $\delta\rho_\xi$ need not scale as $\delta\rho_\infty$. Notably, our hyperuniformity exponent $\varsigma = 2\epsilon/9$ indicates that local fluctuations prevail, leading to $g_1 = \delta\rho_\xi/\delta\rho_\infty \to \infty$. Therefore, our reasoning suggests that the difference between the **p**-conserving hyperuniformity exponent $\varsigma = \epsilon/3$ and our **p**-nonconserving one for RO, $\varsigma = 2\epsilon/9$ might fundamentally arise from the difference in $F(g_0, g_1 = O(1))$ and $F(g_0, g_1 \to \infty)$. We have also added a footnote: Such reasoning based on comparison of the exponents (and not the engineering dimensions) is analogous to the Harris criterion that determines whether a quenched disorder is relevant to a fixed point [53].'

Remark 6. The authors find that except for the hyperuniformity exponent ς, all other exponents are identical to those found in the other versions of the model (studied by functional RG). As said previously, the authors claim that these other versions of the model neglect the diffusive noise which the authors claim to be dangerously irrelevant. From the above statements, we can conclude that all exponents except ς are insensitive to the dangerously irrelevant term and therefore can be computed by ignoring this subtle aspect of the model. Therefore, we can conclude that they can be computed by usual means, that is, coming from the passive phase. Then, at least for the computation of these exponents, why not studying the model directly at criticality, that is, without performing any translation of the a and p fields? This is what is usually done in reaction-diffusion systems (and without performing the Doi shift that hides the symmetry). Doing this makes trivial to prove diagrammatically that the coupling constants in front of the $a^{\dagger 2}ap$ term and the $-a^\dagger p^\dagger ap$ term remain identical all along the RG flow if they are in the bare action (these two terms are renormalized by the same diagrams). Actually, it seems to me that working with this parameterization of the field theory makes it perturbatively trivial because the four-point 1PI functions corresponding to the two bare terms mentioned above are only renormalized by a chain of bubble diagrams that can probably be resummed. Moreover, the propagator of the a and p fields are not renormalized and neither the transmutation parameter μ. The whole theory looks extremely simple in this parameterization except for the strange propagator of the p field: Only κ runs. There may be a subtle problem here that renders the above argument irrelevant, but in that case the authors should explain it in detail.

A primary drawback of the method suggested by the referee, whereby no shifts are applied in order to preserve apparent symmetries between couplings, is that the passive propagator would be singular as it does not contain any diffusion or mass term. Mathematically this may be resolved by adding a regulator ϵ_0 which then gets sent to zero at the end of the loop expansions, but the lack of q-dependence may still often lead to loop diagrams with algebraic, not logarithmic, IR divergences.

From a physical point of view the shift also appears necessary, again already informed by the Gaussian calculations. If the shift a_0 had not been done and all calculations are instead performed directly at criticality, Eq. (19) would reduce to $S(q) = p_0$ and the cancellation mechanism we outlined below Eq.

(19) would not be manifest. Indeed, there exists a discrepancy in $S(0)$ between taking the limit $q \to 0$ first or $a_0 \to 0$ first.

Moreover, as also discussed beyond Gaussian level in the beginning paragraphs of Section 5.2, for the system to be hyperuniform despite the diverging passive fluctuations, there need to be active fluctuations similarly diverging even as the mean active particle density is decaying to zero. Thus, in our physical picture, the shift is not just a mathematical toolkit to regularise the field theory, but is in fact a crucial step to properly explain the cancellation mechanism that we believe to give rise to hyperuniformity. We have added a footnote on **page 10** of the revised manuscript (or the diff file) to clarify this point:

Notably, while in the original action (3) the passive propagator is singular since it does not contain any diffusion or mass term, the passive propagator in (6) after performing the shifts a_0 and p_0 attains a mass term. This regularises the infrared divergences.

Remark 7. The authors repeatedly mention the existence of a fixed point manifold but never explain what this manifold consists in. It seems crucial for a good understanding of the paper that they very precisely explain what this manifold is.

We have added a brief explanation and citation for the fixed point manifold on **page 5** of the revised manuscript (or the diff file): The FPM is also referred to as a 'class of equivalent fixed points' and leads to redundant operators that do not follow scaling relations nor enter the RG flow functions [35]. Therefore, choosing a judicious form of the fixed point action where many operators are 'redundant' can ease the perturbative RG calculations considerably and we exploit this in our approach below.

And now perhaps less important remaks.

Remark 8. The authors present a power counting argument in Eqs. (32-34) based on the assumption that the a and p fields together with their response fields have the same dimension $d/2$. This asumption is unjustified. It is well known for non-equilibrium systems that the quadratic terms of the action only determine the scaling dimension of the product (field . response field), which is d, and not of the two fields separately. In the absence of a symmetry exchanging the fields and the response fields, there is no reason for them to have the same scaling dimension. Such symmetry exists for directed percolation, but there are many other systems for which this is not the case. The only systematic method I am aware of for attributing a scaling dimension to the fields of a given model is to perform a one-loop calculation and then to give a scaling dimension to the fields and, as a consequence to the couplings, so as to reproduce the degree of UV divergence of each individual graphs (the superficial degree of divergence of a graph is entirely determined by dimensional analysis once the dimensions of the couplings are known). Once fixed at one loop, it is valid at all loop orders. I see no reason why the power counting given by the authors should be correct.

Note that physical arguments may sometimes require that two interaction terms in the bare action are equally relevant (otherwise, compared to the other, one term could be neglected and the physics would then be completely different). In this case, this gives an additional equation for the field dimensions, which is sufficient to fix them completely, thus avoiding a one-loop calculation (for a striking example, see 'Pair Contact Process with Diffusion: Failure of Master Equation Field Theory' by H.-K. Janssen et al.).

We thank the referee for this insightful comment and we fully agree that individual dimensions for the annihilation/creation fields cannot be trivially determined in general. While in the original manuscript we had deferred the discussion to Section 5.1, we have now added justifications on **page 16** of the revised manuscript (or **pages 16-17** of the diff file): 'These redundant degrees of freedom can sometimes be readily fixed either via symmetries between the annihilation and creation fields (such as rapidity-reversal symmetry present in directed percolation), or through physical arguments that enforce two or more interaction terms to be equally relevant [44]. In our calculations for Gaussian correlators in Subsection 3.2, all Gaussian correlators, including $\langle\breve{a}(q,t)\tilde{a}(-q,t)\rangle$, $\langle\breve{a}(q,t)\breve{a}(-q,t)\rangle$, $\langle\breve{a}(q,t)\breve{p}(-q,t)\rangle$ and $\langle\breve{p}(q,t)\tilde{p}(-q,t)\rangle$, scale as $q^0F(q\xi)$ at bare level. This indicates that creation and annihilation fields have the same scalings at bare level, in which case $[\breve{a}(x,t)] = [\tilde{a}(x,t)] = [\breve{p}(x,t)] = [\tilde{p}(x,t)] = d/2$. In particular, two points are worth noting here: one, that $[\breve{a}(x,t)] = [\breve{p}(x,t)]$ is expected as the total density must be conserved; and two, that similar considerations of dimension counting will also be useful at $O(\epsilon)$ for prediction of the hyperuniformity exponent, as will be explained in Subsection 5.1.'

Remark 9. As a consequence of Remark 8, the upper critical dimension of the model is not computed by the authors who assume that it is 4. This situation is clearly unsatisfactory and it would be interesting to see whether implementing the program outlined in Remark 6 makes it possible to determine the upper critical dimension (which, in principle, is not affacted by a dangerously irrelevant operator).

We believe the justification provided as response to Remark 8 could serve as evidence that the upper critical dimension is computed to be 4. Additionally, loop diagrams calculated in Section 4 exhibit logarithmic divergence near four dimensions as well. In any case, it is not necessarily 'unsatisfactory' to 'assume' that the upper critical dimension is 4, when this is known as an exact result from independent calculations done by another method.

Remark 10. The authors claim that the model (after the shifts by a_0 and p_0) presents UV non-renormalizable divergences that should cancel for the RO universality class broadly following the same scenario as the tricritical Ising model in $d = 3$ (see Appendix B, for instance and section 4.3.3). They claim that in this latter case, the ϕ^4 coupling is generically generated along the RG flow even it is absent in the bare action and it produces dangerous UV divergences. This statement looks weird to me: The ϕ^4 term is super-renormalizable in $d < 4$ and I doubt that it generates nasty UV divergences. It is true that being super-renormalizable in the UV, it produces strong IR divergences, but this is a different story. Here again, it seems to me that the authors mix UV and IR divergences (see for instance the sentence in B: "... non-renormalizable divergences in the form of negative powers of m.")

We thank the referee for pointing this out. While the tricritical Ising encounters algebraic (IR) divergences with a super-renormalisable quartic coupling in $d < 4$, here in RO we encounter algebraic (and not logarithmic) divergences in both IR and UV regimes. Hence, the cancellation mechanism that we *assume* to be broadly similar to the tricritical model is, in practice, somewhat more complicated.

However we still believe the evidence offered for a scenario of this kind to be very strong. To avoid confusing IR with UV divergences and also as a response to Remark 1, we have replaced all wordings of 'non-renormalisable' divergences with 'algebraic (not logarithmic)' divergences.

Remark 11. The authors introduce the notion of effective couplings (bottom of page 11 for instance) without defining them. I do not know what an effective coupling is. Is it a coupling appearing in the effective action?

We have added a definition of 'effective coupling' on **page 25** of the revised manuscript (or **page 26** of the diff file), consistent with its usage in Tauber's book: Importantly, each reaction produces several coupling constants, but only specific combinations of them, which we refer to as *effective couplings*, appear in the loop corrections and thus the RG flow functions.

Remark 12. The authors use the Cole-Hopf transformation in the 3.3 section. However, this transformation is unjustified at the operator level: if it were justified, then the annihilation operator would be invertible which it is certainly not. The Cole-Hopf transformation is heuristically interesting but should be used with great care. I suggest that the authors mention this point.

We thank the referee for this remark and fully agree that Cole-Hopf transformation should be taken with great care. In our manuscript, we have only applied Cole-Hopf at the Gaussian level, where it reproduces the same structure factors from tree-level calculations of the action. We do not pursue Cole-Hopf beyond providing heuristic Gaussian explanations. We have clarified this point by adding the following sentences on **page 14** of the revised manuscript (or the diff file): This agreement of results demonstrates field-level Cole-Hopf transformation as a good heuristic for deriving Langevin equations from Doi-Peliti actions. However, it is important to note that operator-level Cole-Hopf is invalid, as it would wrongly suggest that the annihilation operator is invertible, and that truncating the Cole-Hopf action at quadratic order also lacks clear justifications. Therefore, in this paper we restrict the use of Cole-Hopf transformation to Gaussian level.

Remark 13. Eq. (24) cannot be valid if the noise field η is real. This is not a detail at least beyond perturbation theory as can be seen in the supplemental material of the article: "Langevin equations for reaction-diffusion processes" by Benitez et al. where the deformations of contours are performed by taking care of the reality of the fields.

Convergence issues discussed in the paper by Benitez et al. are due to incorrectly interpreting the complex conjugate Doi-Peliti annihilation and creation fields as real and purely imaginary respectively. Our functional integral (25) (originally Eq. (24)) is derived via the Cole-Hopf transformation which avoids similar issues at least at Gaussian level. We thank the referee for this important comment and have added corresponding clarifications on **page 14** of the revised manuscript (or the diff file): We note that a rigorous derivation of the nonlinear Langevin equations from the Doi-Peliti fields (i.e. without using Cole-Hopf transformation) can be performed using contour deformations in the complex plane [31].

Remark 14. There are typos in Eqs. (10) (a $d\omega'$) and (23) (a square and a factor of 2).

We thank the referee for pointing out the typos in Eqs. (10) and (23), which have now been corrected.

Remark 15. The simplest dangerously irrelevant operator in equilibrium theory is the ϕ^4 coupling in $d \geq 4$. This term does not break the $O(N)$ symmetry and in general a dangerously irrelevant operator does not need to break any symmetry (although some of them do break symmetries) contrary to what is suggested in section 5.2: "A dangerously irrelevant operator typically (a) breaks some symmetry..."

We agree with the referee that the original sentence in the manuscript might be misleading as it indicates all dangerously irrelevant terms to be symmetry-breaking ones. We have now revised **page 29** of the revised manuscript (or **page 30** of the diff file) to avoid this confusion: A dangerously irrelevant operator typically (a) breaks some symmetry and/or (b) cannot be neglected during the calculation of certain observables. The subsequent paragraphs have also been revised to discuss these two scenarios separately. Please see revised manuscript (**page 30** of the revised manuscript of **page 31** of the diff file, also pasted in response to Remark 5).

Remark 16. In apppendix A, the authors mention a "fixed point action in which all the relevant coupling constants take their fixed point values". In my opinion, this sentence is a bit misleading. First, it is the effective action that can be at a fixed point, not a bare one (typically, a bare action involves a finite number of couplings while the effective action involves infinitely many couplings). Second, only dimensionless couplings can reach a fixed point value, not the dimensionful ones. Third, at a fixed point associated with a second order phase transition, there is only one relevant direction (loosely speaking, the mass direction) and all the other ones are irrelevant. They all have a fixed point value, not only the relevant one.

We have revised the sentence on **page 34** of the revised manuscript (or the diff file): 'This means that the RG fixed points are determined by where these coupling constants are scale-invariant under renormalization. Therefore, one can define an effective 'fixed point action' in which all the dimensionless renormalised coupling constants reach their fixed point values.'

For all the reasons listed above, I believe this article requires significant revisions before it can be published.