Report of Reviewer 2 and our responses

The work under review is arXiv:2507.07793, and I will abbreviate it as [MPPC]. It is a modified version of the prior submission arXiv:2310.17391 by three of the four authors.

Content. The work aims at providing an analytic prediction for the hyperuniformity exponent in the Random Organization Model (ROM). It is claimed to be distinct from the one in the Biased Random Organization Model (BROM). The reason for this difference is attributed to an additional conserved noise term. Technically, the work uses the Doi-Peliti formalism for a model of active and passive particles, which is in the same universality class. The authors then perform an RG analysis of the latter.

Context. The authors' model, the (Abelian) Manna sandpile model, ROM and BROM all belong to the Conserved Directed Percolation (CDP) class. It was conjectured long ago that the CDP class can be mapped onto the depinning of disordered elastic manifolds. That this mapping goes beyond scaling exponents was shown numerically by measuring the renormalized force correlator [1]. Later, an explicit mapping was found [2], which equates the activity ρ in sandpile models to the driving velocity $\dot{u}$ at depinning, $\rho = \dot{u}$. This mapping was completed [3] by an identification of the particle density n as $n = \nabla^2 u$. What one learns from the mapping is that sandpile models need a functional RG for their field theoretical treatment.

Appreciation. [MPPC] claims that a standard multiplicative renormalization of their model is sufficient to access the critical exponents in the Manna class to (at least) 1-loop order, which via the mapping gives exponents at depinning for disordered elastic manifolds. If true, this is a major breakthrough since the calculations avoid the complications of functional RG.

The referee has serious doubts about this claim:

Regarding these serious doubts, we will first provide a point-by-point response, then discuss our edits and revisions to the manuscript corresponding to these doubts collectively in responses to Comments 7 and 8.

(1) the roughness exponent ζ at 2-loop order can be written analytically; this expression involves an integral over the 1-loop fixed-point function $\Delta(w)$; this non-trivial number is unrelated to any momentum integral. How can it appear in the scheme the authors propose?

We thank the referee for this informative and intriguing comment. The function Δ in the disordered manifolds context is defined as the force-force correlator quenched and written in terms of height u and space x. As the force is mapped to the passive densities and the height to the time integral of the active densities, the physical meaning of Δ is unclear in the RO context. Beyond difficulty in interpreting Δ in RO, a main concern for us is that 2-loop calculations in RO may not be completely justified. We elaborate this in response to Comment 4.

(2) there are many technical assumptions, either marked via "assume" or "argue", that to a large extent determine the outcome.

While our work is indeed based on a set of assumptions (universality, renormalisability, hyperuniformity), we do not think these affect the significance of our results, as we have reproduced all exponents to $O(\epsilon)$, except for the hyperuniformity exponent, without recourse to Functional RG analysis.

For clarity, we have decided to present the assumptions explicitly before proceeding with any calculations. We think some of these would be uncontroversial in other settings; nonetheless we mention them upfront in order to maximize transparency in relation to the specific case of RO.

In particular, although the assumptions are unproven, we have strong reasons to believe that they are justified, as we summarise below.

Universality: our one-loop results for exponents $\nu_\perp, z, \beta$ match those found by independent calculations using the q-EW mapping. *Renormalisability*: We assume the cancellation of algebraic divergences and the vanishing of couplings that could generate them, so that the resultingng theory is renormalisable. Crucially, we systematically account for all remaining logarithmic divergences. *Hyperuniformity*: the assumption that the system does exhibit hyperuniformity is strongly evidenced by several independent numerical simulations.

(3) an assumption not stated is the multiplicative renormalisability of the active and passive particle densities. This assumption contradicts the mapping discussed above, which identifies active particles ρ and total number of particles n as scaling fields. This assumption is not innocent, since $n = \nabla^2 u$ and $\rho = \partial_t u$. Thus at the upper critical dimension both operators have the same dimension, while below they do not. This is easily missed in a 1-loop calculation. Can the authors push their calculation to 2-loop order, or at least check what happens if n and ρ are used as scaling fields?

We thank the referee for this very interesting comment. It is also intriguing to us that while connected via an exact mapping as found in [2], there remains a mismatch of the two methods already at the level of the scaling fields. For example, in the RO context, while total particle density is a real physical variable, the Doi-Peliti action is explicitly constructed via only active and passive operators, and it is not immediately clear how a transformation to the total density should be performed. In particular, we note that the Doi-Peliti annihilation fields are *not* real density fields, and thus the intuitive transformation $\rho(x,t) = a(x,t) + p(x,t)$ (where $a(x,t)$ and $p(x,t)$ are the Doi-Peliti annihilation fields) does not necessarily make sense. We thus believe that n and ρ cannot be used as scaling fields in the Doi-Peliti formalism. On the other hand, it is also not immediately clear (at least to us) in the disordered elastic manifolds how passive-passive spatial correlation should be written, because via the mapping, passive particle fluctuations are quenched. Therefore, the 'natural' implementation on two sides of the mapping seems to be significantly different.

Beyond 1-loop calculations, it may be possible that a 2-loop calculation could potentially further elucidate the points above. However, this additional calculation would take at least a year and may very likely encounter other technical issues. We elaborate on the technical difficulties in response to Comment 4.

This part of the calculation evaluates the roughness exponent ζ and the dynamical exponent z, and

[MPPC] seemingly reproduce the established results. (The same holds true for β, but it is not an independent exponent.)

What is troubling is that [MPPC] find a different exponent for hyperuniformity. They attribute this to an additional dangerously irrelevant conserved noise. The referee could not find any substantiation that this additional conserved noise, which by power counting is irrelevant, is actually dangerous irrelevant. This scenario was at lengths analyzed in [3]. There it was concluded that under the simplest assumptions the additional noise is the gradient of an ABBM-type noise, and that this destroys hyperuniformity in dimension $d = 1$, in contradiction to simulations. Thus the evolution equation for the time-integrated noise term must have a feedback term, also expected on physical grounds, rendering it irrelevant in all dimensions. As a result, there is only one universality class, encompassing ROM and BROM.

We thank the referee for this interesting comment about [3]. While we accept that there is an exact mapping between BRO and disordered elastic manifolds, we have not seen evidence for an exact mapping between RO and any similar manifold model. Therefore the claim that BRO and RO share an hyperuniformity exponent is a claim that the irrelevant conserved noise in RO is not dangerous.

We do not think that [3] establishes that claim convincingly. Specifically, [3] expands this discussion by giving two potential possibilities for including this additional noise. While the obvious choice, the gradient of an ABBM-noise, destroys hyperuniformity in $d = 1$ and hence is invalid, the authors in [3] commented that a natural modification of ABBM-noise remains irrelevant by dimension counting. While the referee is not persuaded by our arguments that the conserved noise is dangerous, we are equally unpersuaded by the argument of [3] that it is not dangerous. The latter is only possible if there is either (i) a false assumption or (ii) a calculation error in our calculations, yet the referee has not identified any convincing candidate for either (i) or (ii). We don't think this is an adequate grounds for rejecting the paper from SciPost Physics.

As a final note, we would like to comment that despite having different hyperuniformity exponents, we do retrieve the same roughness exponent ζ, which is related to the scaling of field ρ_A. Where our results differ lies in adoption of the mapping used in [3] but not in our work, $\partial_t \rho = \nabla^2 \rho_A$ in the absence of conserved noise, that links from ζ to the hyperuniformity exponent ς associated with the scaling of field ρ. In particular, we argue that for the RO universality class, the presence of an additional dangerous irrelevant conserved noise breaks this link between ζ and ς.

Let us ask what numerics says about whether ROM and BROM have a different hyperuniformity exponent. [MPPC] show the following table

Dimension	RO/**p**-non-conserving	C-DP/Manna/**p**-conserving
$d = 3$	0.22	0.29 {0.33}
$d = 2$	0.44	0.49 {0.66}
$d = 3$ (Numerical)	0.24 ± 0.02	0.26 ± 0.02
$d = 2$ (Numerical)	0.45 ± 0.03	≈ 0.45

The last two lines state that within very small error bars, there is no detectable difference between the two classes. (Error bars should be given for CDP in $d = 2$ as well, and results for $d = 1$ should be included.)

We have now expanded our table on **page 30** of the revised manuscript (or **page 31** of the diff file) to include one-dimensional results.

Dimension	RO/**p**-non-conserving	C-DP/Manna/**p**-conserving
$d = 3$	0.22	0.29 {0.33} [17]
$d = 2$	0.44	0.49 {0.66} [17]
$d = 1$	0.66	0.50 {1.00} [17]
$d = 3$ (Numerical)	0.24 ± 0.02 [55]	0.26 ± 0.02 [55]
$d = 2$ (Numerical)	0.45 ± 0.03 [8, 9]	≈ 0.45 [26, 55]
$d = 1$ (Numerical)	0.425 ± 0.025 [8]	0.50 [17]

Then the question arizes, are the results at least consistent at 1-loop order? To better assess this, we can look at Fig. 1 of [3], reproduced here:

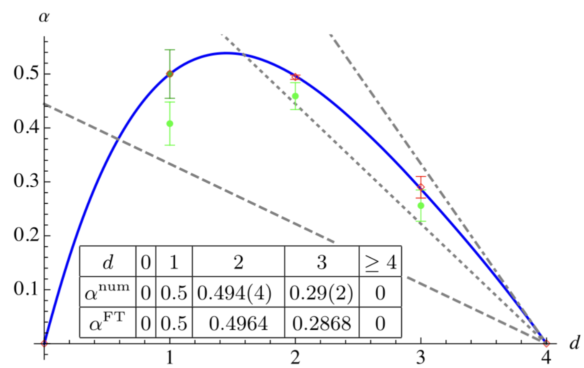

FIG. 1. The exponent α of the structure factor $S(q) \sim |q|^\alpha$ as a function of dimension d for the Manna model. The blue solid line is from the ϵ-expansion of [13], the red dots (with error bars) from simulations at depinning [14,15]. Simulations in green are from [16]. The dark green data point is from Fig. 2. In gray are the different ϵ-expansion results, $\alpha = \epsilon/9$ (dashed) [5], $\alpha = 2\epsilon/9$ (dotted) [17], and $\alpha = \epsilon/3$ (dot-dashed) [leading term of Eq. (25)].

The result of [MPPC] corresponds to the dotted line. The numerical predictions in dimensions 2 and 3 look decent. However, knowing that the hyperuniformity exponent α is smaller in dimension $d = 1$, the curve needs to bend down, which would destroy the agreement at least in dimensions 2. So in order to see wether the corrections of [MPPC] are small or not, one can simply look at the relative 1-loop deviation, which is 1/3. This is much bigger than any of the deviations in the above table. The referee's conclusion is that the results are incompatible with the numerics in all dimensions.

We fully agree that existing numerics are insufficient to conclude if there may be any discrepancy between the two hyperuniformity exponents. However we disagree that the results are 'incompatible' with the numerics in all dimensions. In particular, it is not logically possible to *disprove* an analytic calculation of an exponent to first order in small $\epsilon = 4 - d$ by appeal to numerical results with $\epsilon = 2$ or 3.

[MPPC] can substantially strengthen their case by

(4) calculating ζ and z at 2-loop order

We agree that it would be helpful to existing discussions if 2-loop calculations could be performed in the Doi-Peliti formalism and if thereafter a comparison of the $O(\epsilon^2)$ can be made with the results in disordered elastic manifolds. A major technical difficulty lies in that at 1-loop order already, we had to deploy the renormalisability assumption to discard (with strong reasons) the algebraic divergences. Beyond 1-loop order, however, this poses even more serious technical difficulties that we deem unlikely to be overcome. Therefore, we similarly believe that Functional RG might be a more established method to perform RG calculations beyond 1-loop. However, as already discussed, we do not know a way to address the case with conservative noise via Functional RG since the established path, via the q-EW mapping, is blocked. We thus believe that the suggestion of a two-loop study is a substantial new stud, which falls outside the scope that we can address.

(5) calculate $\Delta(w)$ following the protocol in [1]

We agree that calculating Δ, and repeating the calculation without the conserved noise, might in principle help clarify how our hyperuniformity exponent could be reproduced in a response-field formalism. However, $\Delta(w)$ does not appear to have clear counterpart in the Doi-Peliti formalism.

In particular, we do not think it is fair of the referee to say, in effect, that the Doi-Peliti approach can only be published in SciPost Physics if it can be extended to reproduce *all* the results of the q-EW mapping. If that was fair, we could likewise argue that the q-EW mapping results in [3] should only be trusted once the mapping has been extended to include the RO noise exactly, allowing confirmation of whether it is indeed dangerous or not.

(6) repeat the calculation without the additional conserved noise.

We thank the referee for this suggestion. However, Doi-Peliti fundamentally incorporates deterministic diffusion with conserved noise; in the action, both are implemented in a single, diffusion-like term. It is thus unlikely that the Doi-Peliti calculation can be repeated without the additional conserved noise.

If these demands can be fulfilled, publication in SciPost may be merited. If this is not the case, my recommendation is that

(7) the article be rephrased stating that this is an interesting calculation, potentially an alternative to functional RG, but that work remains to be done to confirm this.

The assumptions underlying our work are already stated with unusual clarity in our paper. However, to further reflect the referee's concerns we have added paragraphs to the Conclusions section that reiterates the assumptions made earlier. These are pasted below, can be found on **pages 32-33** of the revised manuscript of **page 33** of the diff file.

Our calculations rely on several technical assumptions. First, we assume that the critical fixed point for RO universality class can be accessed perturbatively by expanding around the Gaussian fixed point to $O(\epsilon)$, with $\epsilon = 4 - d$. While this assumption is standard in field-theoretic renormalisation group analysis, its validity is far from guaranteed here: locating the RO fixed point requires fine-tuning of parameters to enforce the underlying symmetries and conservation laws characteristic of this universality class. An immediate question is whether this calculation, which effectively probes along the stable direction toward a saddle fixed point, holds to higher orders in perturbative RG, or if functional RG methods are required to capture potential non-perturbative features. It is equally intriguing to ask how an absence of diffusive conserved noise could be treated in the Doi-Peliti framework. At present, it is not clear how this can be done, as the stochastic conserved noise and the deterministic diffusion are both implemented in a single term in the Doi-Peliti action. In either case, overcoming these obstacles could greatly extend existing knowledge of the analytical structure of the RO fixed point.

The second crucial assumption is that the spatial correlations in total density are hyperuniform at the critical fixed point. This allowed us to derive the hyperuniformity exponent without a rigorous proof of the cancellation of diverging amplitudes. Future work could focus on an analytical justification; this would not only strengthen the theoretical foundation of the work presented in this paper, but may reveal intriguing mechanisms driving the emergence of hyperuniformity as well.

(8) since there is not enough evidence to sustain the claim that there are different hyperuniformity exponents for ROM and BROM, or that there is an additional "dangerously irrelevant noise", these statements should be withdrawn.

We reject this recommendation. Unless one or more of our (clearly stated) assumptions is incorrect, or there is a technical error in our calculations, there are indeed different hyperuniformity exponents for RO and BRO. This is fully understandable if the RO noise is dangerously irrelevant, a scenario which is strongly indicated by the behaviour above d = 4 where the hyperuniformity exponent is altered throughout the entire active phase.

We insist that we are justified in making all these statements since the referee gives no specific or substantiated evidence for either an incorrect assumption, or an incorrect calculation.

Concerning the issue of dangerous irrelevance, we have added a paragraph, on **page 30** of the revised manuscript or the diff file, linking this to a formal statement about scaling functions which we already quoted above in response to Remark 5 of Referee 1. This paragraph addresses the behaviour of scaling functions for the correlators, supporting the viewpoint that the BRO calculation of [3] and the RO calculation of this paper can be both correct.

'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 the text **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].'

More points which need to be addressed:

(9) about the "cancellation pattern" (already in the abstract, and again later on): In Ising, the critical dimension moving from $d = 4$ to $d = 6$ has to do with a cubic term and a symmetry breaking $\phi \to -\phi$. The tricritical point in dimension $d < 3$ is different.

The tricritical Ising model concerns the absence of ϕ^4 which effectively brings the upper critical dimension from $d_c = 4$ to $d_c = 3$. We have used this reference to draw an analogy of RO ($d_c = 4$) embedded in theories with $d_c = 6$; to remain in the RO universality class, there has to exist a cancellation mechanism of algebraically diverent terms like the cancellation mechanism of the quartic coupling in tricritical Ising. The referee's comment about the critical dimension moving from 4 to 6 within an Ising setting appears misplaced. The tricritical Ising model does not have a cubic term; it also does not have a quartic term (after renormalization). The leading order nonlinearity is ϕ^6 which is why the upper critical dimension is 3.

(10) is the "RG fixed-point manifold" related to the redundant mode $\Delta(w) \to \kappa^2\Delta(w/\kappa)$ present in FRG?

We thank the referee for this important insight and believe the referee is noting the redundant perturbation $\Delta(w) \to \kappa^2\Delta(w/\kappa)$ in Wegner's definition. We have added this interesting comparison into **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].

(11) clearly mark all statements on "dangerously irrelevant operator" as a conjecture, or provide evidence.

We have updated following statements of 'dangerously irrelevant conserved noise':

On **page 4** of the revised manuscript (or the diff file), we have changed 'show that conservative noise is required to fully understand the RO universality class whose hyperuniformity exponent is governed by this dangerously irrelevant term in the action' to 'strongly suggest that conservative noise is required to fully understand the RO universality class whose hyperuniformity exponent is then governed by this (dangerously) irrelevant term in the action.'

On **page 30** of the revised manuscript (or the diff file), we have changed 'Hence it is to be expected that the critical RO fixed point (non-conserved $\mathbf{p}$) likewise differs from the C-DP one (conserved $\mathbf{p}$) via the action of the dangerously irrelevant diffusive noise' to 'Hence it is to be expected that the critical RO fixed point (non-conserved $\mathbf{p}$) likewise differs from the C-DP one (conserved $\mathbf{p}$) via the action of the irrelevant diffusive noise if this is indeed dangerous as we have argued.'

On **page 32** of the revised manuscript (or the diff file), we have changed 'Diffusive noise splits the C-DP/RO/q-EW universality class into two subclasses by breaking a conservation law on the centre of mass of the total particle density. The RO subclass, in which the conservation law is broken by the diffusive noise (which is potentially dangerously irrelevant) gives a more singular hyperuniformity exponent than one found via a mapping to q-EW (quenched Edwards-Wilkinson) in which the conservation law is sustained.' to 'According to our calculations, diffusive noise splits the C-DP/RO/q-EW universality class into two subclasses. We have argued that it does so by breaking a conservation law on the centre of mass of the total particle density, and that the RO subclass, in which the conservation law is broken by the diffusive noise (which is then dangerously irrelevant) gives a more singular hyperuniformity exponent than one found via a mapping to q-EW (quenched Edwards-Wilkinson) in which the conservation law is sustained.'

We have also changed the title of Subsection 5.2 to 'The Physics of Hyperuniformity and the Potentially Dangerous Role of Irrelevant Noise'.

(12) correct Eq. (26): the term a_0 there is probably ρ_A. As written this is a time dependent noise which prevents an inactive state.

The Langevin equations in Eq. (26) are only at Gaussian level, where multiplicative noise can be approximated to be additive, hence giving variance proportional to the mean active density a_0 and not ρ_A. We clarify this on **page 14** of the revised manuscript (or the diff file): The variance for the noises are proportional to a_0 and not ρ_A since multiplicative noises become additive at the Gaussian level. In particular, we also would like to note that our derivation of the Langevin equations are not part of the RG calculations; instead, they are only intended to give a heuristic explanation of hyperuniformity emergence at the Gaussian level.

(13) explain regularization of Eq. (40). Does this respect all the symmetries of the problem?

Here we have used dimensional regularisation. Tauber's book lists a characteristic example for this regularisation technique, which we paste below:

$$\int \frac{d^d k}{(2\pi)^d} \frac{k^{2\sigma}}{(\tau + k^2)^s} = \frac{\Gamma(\sigma + d/2)\Gamma(s - \sigma - d/2)}{2^d \pi^{d/2} \Gamma(d/2)\Gamma(s)} \tau^{\sigma - s + d/2} \tag{1}$$

This is different from applying a UV cutoff, and we now explain these two RG methods in Appendix A on **pages 33-34** of the revised manuscript or the diff file. This does not break any existing symmetries of the action.

(14) Finally, I ask the authors to re-read [3], and be more careful to accurately represent its contents.

Following the referee's suggestions, we have re-read [3] and corrected several inaccuracies in our original statements about [3]. These include identifying the extensive arguments [3] made about potential implementations of the conserved noise in the Manna class, an emphasis of the 'exact' nature of the mapping onto disordered elastic manifolds, and noting that RG results in the disordered elastic manifolds context have been calculated to two- and three- loop orders. These changes can be seen in **pages 1, 3, 4, 29, 30** and **31** of the diff file.