Hyperuniformity at the Absorbing State Transition: Perturbative RG for Random Organization

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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:

(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?

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

(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?

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 dangerously 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.

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} [17]
$d = 2$	0.44	0.49 {0.66} [17]
$d = 3$ (Numerical)	0.24 ± 0.02 [50]	0.26 ± 0.02 [50]
$d = 2$ (Numerical)	0.45 ± 0.03 [8, 9]	≈ 0.45 [26, 50]

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.)

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:

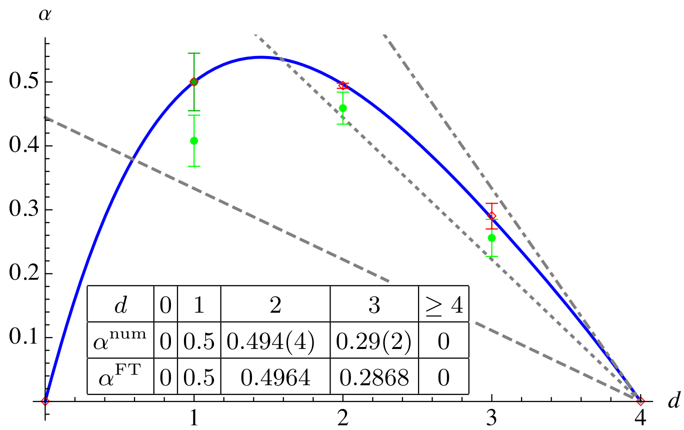

FIG. 1. The exponent α of the structure factor $S(q) \sim |q|^\alpha$ as a function of dimension d for the Manna model. The blue solid line is from the ϵ-expansion of [13], the red dots (with error bars) from simulations at depinning [14,15]. Simulations in green are from [16]. The dark green data point 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.

[MPPC] can substantially strengthen their case by

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

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

(6) repeat the calculation 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.

(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.

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.

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

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

(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.

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

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

I recommend publication in SciPost Core once the points above are addressed.

References

[1] J.A. Bonachela, M. Alava and M.A. Muñoz, *Cusps in systems with (many) absorbing states*, Phys. Rev. E **79** (2009) 050106(R), arXiv:0810.4395.

[2] P. Le Doussal and K.J. Wiese, *An exact mapping of the stochastic field theory for Manna sandpiles to interfaces in random media*, Phys. Rev. Lett. **114** (2014) 110601, arXiv:1410.1930.

[3] K.J. Wiese, *Hyperuniformity in the Manna model, conserved directed percolation and depinning*, Phys. Rev. Lett. **133** (2024) 067103, arXiv:2401.09123.