Report

This article demonstrates the phenomenon of walking RG flows, which was proposed in the authors' previous article, in the example of the 2d Potts model. Technically, this draws on results from three areas: the 2d Potts model, general CFT in 2d, and conformal perturbation theory. My knowledge of the first and third areas is limited, and it would be good to have the article reviewed by experts such as the authors of refs. [4] and [32] respectively.

The main reason why I am confident that the article's picture of walking RG flows is correct, is that it provides predictions for relations between conformal dimensions and three-point structure constants: the Im-flip condition of Section 4.2. The authors check this condition in a few nontrivial cases, and I believe that it holds more generally, based on rough calculations.

Other reasons why the article is interesting are a good (but perfectible) review of the 2d Potts model, and the foray into the oft-neglected CFTs with complex central charges.

I believe that the text's clarity could and should be improved in many places. Although the general ideas are clear enough, the technical implementation is sometimes hard to follow. In my suggestions I will first focus on three areas that deserve particular attention, before giving miscellaneous comments on the rest of the text.

Suggested changes

Sections 2.4, 2.5 and 2.6

These Sections review the spectrum of the Potts model. While only some features of this spectrum will be needed later, this review is valuable in itself. I believe that its clarity and presentation should be improved along the following lines:

1. We have no less than 7 different notations for what is essentially the same parameter: c, Q, e_0, g, u, t, m. Using several notations is probably inevitable, as this parameter appears in various contexts and approaches. But 7 notations are too many, maybe Q, c, g or Q, c, t could suffice? If many notations are kept, at least there should be a synthetic table of their relations.

2. Similarly, we have two notations (e, m) and (r, s) for the same parameters. The discussion of Section 2.6 on how degenerate characters emerge from various terms in the partition function would be greatly helped by using (r, s) throughout, as degenerate representations correspond to r, s being nonzero integers of the same sign. Or at least, the partition function should be rewritten in terms of (r, s) in Section 2.6.

3. The formulas (2.15) and (2.16) are standard formulas in free bosonic CFTs in the presence of a background charge. The present text gives the misleading impression that these formulas are specific to the Potts model, and gives derivations that are not very enlightening and probably unnecessary. Once we know that we should compute the functional integral (2.13), these formulas can be immediately accepted, with possibly a reference to the textbook by di Francesco et al, Chapter 9.

4. If $e \in 2\mathbb{Z}$ for electric operators (Section 2.4), why is $e \in \mathbb{Z}$ allowed in $Z_c[g, 1]$ (2.23)-(2.19)?

5. It is strange that the partition function (2.23) is only partially described in Section 2.5, with one piece being postponed to eq. (2.31). Some restructuring would be welcome.

6. 1 is not prime: is (2.32) valid for $M = 1$?

7. The notation $\mathcal{O}_{e_0+2P,0}$ is confusing, and obscures the fact that this operator is degenerate if $P \geq 0$. A notation of the type $V_{1,P+1}$ would be much better. (I used the letter V in order to avoid the confusion with the non-diagonal operators $\mathcal{O}_{e,m}$.) Similarly, in (2.31) the notation $x_{e_0+2P,0}$ is confusing.

8. When discussing the operator content, in other words when grouping the terms in the partition function into Virasoro characters, it would be good to systematically indicate which contributions come from which terms with which multiplicities. Presenting all the results as a table might be appropriate.

9. Statements like 'does not exist as a primary operator' or 'a scalar operator of such dimension is present, but it's not a primary' or 'do not exist in the spectrum' should be made more precise. In the presence of negative multiplicities, it is hard to say anything definite about the spectrum: having statements about characters would be more accurate. The appearance of degenerate characters suggests, but does not prove, that degenerate operators exist. And null vectors are both primary and descendent.

10. In Figure 7, at $Q = 2$ there seem to be two degenerate operators of type $(1, 2)$ with different dimensions.

11. The operators ϵ, ϵ' are actually degenerate at all Q, whereas Figure 7 suggests that they are degenerate at $Q = 2, 3$ only. The indices (r, s) are not just positions in the Kac table: they make sense at all central charges.

Section 4 before 4.1

The authors could be more cautious in declaring that the light fields belong to degenerate conformal families: the characters suggest it, but do not prove it. Character-wise, a degenerate representation coincides with two non-degenerate representations with multiplicities 1 and -1, and we are in a context where negative multiplicities are allowed.

Moreover, fixing three-point functions of degenerate fields at arbitrary central charges using crossing symmetry is not really complicated. The authors' extrapolation from minimal models is a circuitous route, which gives correct results because the correlators are uniquely determined by differential equations and crossing relations, as the authors correctly state. But this holds for all central charges.

Therefore, I recommend the following:

12. Remove the discussion of minimal models, with its odd focus on the unitary models with integer m, whereas more general minimal models have two integer parameters. (The mention of minimal models in the article's Abstract could also be removed.)

13. Give the explicit formulas for the structure constants, as this would allow readers to check their subsequent expansions around $c = 1$. (See footnote 24.) The natural parameter for writing these structure constants is not m, but rather t or g. Maybe these formulas could be given in an appendix, which would include all needed structure constants, including those of the spin operator.

Section 4.5

This Section gives arguments for the existence of the real flow, based on the assumption that there exists a pair of complex conjugate CFTs that are moreover related by a RG flow. There are two arguments:

- A first-order argument argument, plus a heuristic argument that higher order corrections should deform the picture without changing it qualitatively.
- An all-orders argument, which is not rigorous because regularization issues are neglected, and which essentially shows the equivalence between the existence of the real flow, and the fixed point coupling constant g_{FP} being pure imaginary.

The all-orders argument does not really establish the existence of the real flow, but it does constitute a nontrivial consistency check. Moreover, it illustrates the properties of correlation functions in complex CFTs.

The logic of the argument, and the technical details, are not clear enough. Moreover, while interesting, this Section is not crucial to the main results of Section 4. I recommend that Section 4.5 be either deleted, or clarified. Suggested clarifications include:

14. Removing the misleading sentence 'a formal perturbative argument for the existence of the real theory', and clarifying the sentence after (4.49) as establishing '$X \Leftrightarrow Y$' rather than 'X and Y'.

15. Explaining the precise meaning of the two relations between the CFTs $\mathcal{C}$ and $\bar{\mathcal{C}}$, at the level of correlation functions: the relation that they are complex conjugates, and the relation via the RG flow.

16. 'regulate' $\to$ 'regularize'.

17. Typo in (4.47): g_2^k. Same problem in (4.48).

18. Better explaining the crucial step of going from the first to the second line of (4.47).

19. Clarifying that at each point on the RG flow, there exists only one operator Φ or Φ^*, which however changes name from Φ to Φ^* at some point, and which becomes real in a real CFT.

Miscellaneous suggestions

20. Does eq. (2.4) for the partition function at arbitrary Q reduces to (2.2) when Q is integer?

21. In Subsection 2.2, clarify the statement that 'we will take an intuitive approach to symmetry for non-integer Q...'. Does this symmetry for non-integer Q play any role in the subsequent analysis?

22. Typo: 'complex plain'.

23. In Section 3, the statement that 'multiplicities stay real for any Q' could be amended to 'multiplicities stay real for any real Q'. Thinking about complex Q is indeed quite natural in this context.

24. There is a dual use of the notation g in Section 4.1: the original g from eq. (2.6), which appears again in eq. (4.7), differs from the g in eq. (4.9).

25. In Section 4.1, the description of the Im-flip is confusing. Maybe an equation would be clearer.

26. In the Conclusion, state explicitly what it means to 'make sense' for a Euclidean CFT, and for a Minkowskian CFT.