

I read the updated version of the manuscript “Constraints on beta functions in field theories”. The authors made a significant modification by adding sections IIA and IIB which consist of two concrete examples illustrating their proposal. This is one of the things requested by me in the previous report and is a step forward towards making the paper understandable. Nevertheless, I am left with the impression that my main concern is left unaddressed. In fact, after the examples of section II are added, it makes it even more pressing. My main concern is that the procedure proposed by the authors is a mere rewriting of the Wilsonian path integral which, unless the theory has a meaningful gravitational dual, does not lead to any improvement of understanding of RG flows. Let me make the point more precise.

In section IIA the $O(N)$ vector model is considered. It is regularized on a lattice and a certain scheme is chosen to implement the Wilsonian RG. Now, due to the regularization, all the steps of the procedure appear well-defined mathematically. The RG is implemented using the method of the authors, which is dubbed “Quantum RG”. In this method one first solves the RG equations in the subspace of single-trace couplings and then uses additional path-integration to obtain the RG equation for the generic set of couplings. Now, in the $O(N)$ model all single-trace operators are quadratic in the fields, and, consequently, the single-trace RG is that of a free theory and is trivial. This makes it very explicit that the entire complexity of the RG flow is contained in the step which is dubbed as “obtaining the full RG flow from the single-trace one”. While I agree that the full RG can be “contained” in the single-trace one, it is also contained in the UV action of the theory, which can be a very simple functional of the fields. The trick is of course to take the path integral (or its regularized version), and I do not see why the order of integration proposed by the authors leads to any simplification. In fact, the choice of single trace fields seems rather arbitrary to me at finite N . In their reply to my first report, authors seem to say that any choice would work. Then why not choosing just the field bi-linears as a basis also in the example of section IIB ($O(N) \times O(N)$ matrix model)? It is still true that all multi-trace operators in the matrix model can be built as a product of field bi-linears, just with open indices. Since the procedure is supposed to work at a finite N , I do not see why one basis is worse than the other. But then it appears that Gaussian RG teaches us something non-trivial about pretty much any strongly coupled theory. I find this highly unlikely to be true.

I suggest that, instead of a very abstract and practically useless statement “the β -functions defined in a measure zero subspace of couplings completely fix the β -functions in the entire space of couplings”, the authors make a very concrete statement as what their procedure can be useful for. In the conclusions, they make a claim that anomalous dimensions in the IR fixed point, as well as the OPE coefficients can be calculated using their method. This is illustrated in the toy-model examples of section V, but seems completely disconnected from the concrete physical models of section II. Are authors going to claim that their method provides a new, more efficient compared to the known ones, way to calculate critical exponents and OPE coefficients in the critical 3D $O(2)$ model, for example? This is a very big claim, to say the least, are the authors ready to stand by it?

Given that the second report from the second referee is positive, I realize that asking the authors to complete the calculation of some critical exponents in a realistic model may be a too demanding task. As I said, I have hard time believing, that their method will be useful

for it. Neither the method seems to give any new general insights in RG flows, in the spirit of ref. [6]. However, in order to avoid killing the new ideas that are too deep to be understood at present, I would suggest the following compromise. In order to accept the paper, I would ask the authors to make a very concrete list of calculable physical quantities or properties of realistic QFTs that can be computed using their method, as well as an estimate of how hard the computation is. When one is simply left with a task of computing Eigenvalues of some QFT Hamiltonian, it is of course unclear whether such procedure is implementable in practice in any efficient way. This discussion should be placed somewhere in the beginning of the paper, so that anybody who opens it sees the list of concrete claims. It will then be up to the authors, or whoever else is interested in developing the method, to continue this line of research and check whether the claims are correct.