First of all I would like to apologize in the delay in the submission of this report. Time management during the pandemic gets very complicated, and I had multiple other obligations. What I am able to submit now still probably cannot be called a traditional referee report. The reason is that I started reading the manuscript "Constraints on beta functions in field theories" multiple times, and every time time I could not get to understand the main claims made by the authors, as I was getting stuck in various details and definitions. I hope that what I write below will stimulate the discussion that will hopefully be helpful in understanding the issues brought up in the paper, also to a broader audience. At the moment, I am tempted to say that the results of the paper are either incorrect or largely overstated, but I would be happy to be wrong. At least, I have a hope that the idea, the authors are elaborating on, has potentially interesting implications.

Let me first try to summarize the claims of the paper, the way I understand them, then state what I am confused about and then ask several questions. The authors claim that there is a "significant simplification" in the structure of renormalization group flows present in general quantum field theories. The intuition for this simplification is coming from the previous work of one of the authors, refs [15], [16], in the context of AdS/CFT. For a QFT that has a dual gravitational description there is a notion of holographic RG, see e.g. ref. [25], which roughly states that the Wilsonian renormalization group procedure in such a QFT can be represented by choosing a surface in AdS parallel to the boundary and taking the bulk path integral in two steps by first integrating over fields on one side of the surface. For truly holographic QFTs the bulk path integral is semiclassical, so some additional intuition about decoupling of UV and IR degrees of freedom can be gained from this holographic RG procedure. The distance from the boundary to the surface is related to the Wilsonian cutoff, however, the relation is not fully understood. Namely, it is not known which cutoff corresponds to which definition of the distance. Since the bulk theory is gravitational, the latter is rather subtle to define. Thus even for holographic QFTs, the holographic RG procedure does not lead to many concrete quantitative predictions. In the present manuscript the authors claim that the logic of holographic RG can be used for an arbitrary QFT, without requiring even any large-N properties. This is achieved by performing a set of Hubbard-Stratonovich-like transformations, spelled out around eq. (40). Due to this rewriting of an action, some of the interaction terms are characterized as multi-trace and some as single-trace. Single-trace are those that do not involve any HS fields. The main claim of the paper is that doing path integral in this order leads to some interesting non-perturbative constraints on RG flow of the theory. One can first study a truncated version of the theory that involves only single-trace operators, and if RG flow in the truncated space of operators is solved, the full RG flow can be reconstructed. The major constraining power comes form the fact that application of an RG step can be thought of as evolving the radial bulk wave function with the bulk radial Shroedinger equation, which is a linear procedure. The authors then study a couple of toy-model examples of RG flows in order to illustrate the procedure.

I find the above-described procedure rather confusing for the following reasons. First, I do not see why for a generic strongly coupled QFT there should be a preferred set of single-trace fields, and even more so why it should be the same at different energy scales. Related to this, the path integral over HS fields will generically get strongly coupled and I expect the book keeping of operators used throughout the paper to fail. This is of course a manifestation of the fact that a generic QFT does not have any approximately local bulk dual. The authors argue that some the will remain due to linearity of the radial Shroedinger equation which is true non-perturbatively. However, to me it appears that linearity simply follows from the fact that path integration is a linear procedure ($\int D\phi(\Psi_1 + \Psi_2) = \int D\phi\Psi_1 + \int D\phi\Psi_2$), so I cannot see how any non-trivial constraint on RG can follow from this, and what is described as a Hamiltonian evolution is simply a rewriting of the path integral over HS fields. More on this point, if we go back to the holographic case, at the non-perturbative level there isn't even a bulk Shroedinger equation, instead there is a Hamiltonian constraint $H\Psi = 0$, and Shroedinger equation only appears in a semiclassical expansion of this constraint. Thus, if we can think about a generic QFT as a theory with a very non-local and non-semiclassical bulk dual, I do not see why Shroedinger equation would be relevant.

Another concern, is that even if some sort of truncation to the single trace operators was possible, in a real QFT this would still be infinitely many operators, so "solving" the RG flow in a truncated theory would still require further truncation, as long as some concrete results are to be obtained. So it is not clear to me how "significant" even the claimed simplification is. Another related concern is that the suggested procedure seems to require the use of "local RG", that is the coupling constants of the theory are promoted to functions of space. This could be a major complication, as opposed to a simplification. For many important goals it usually suffices to consider RG with space-independent coupling constants.

It would help if authors could explain what kind of concrete constraints on the RG flows one can hope to obtain due to the structure they found. For example, non-perturbative RG techniques are used to analyze the IR behavior of some theories, prove an existence of a gap or vice-versa an existence of a non-trivial fixed point in some theories, or either numerically or analytically compute the energies of low-lying states in the IR theory. Can any of this be achieved using the authors' method in any interesting QFTs? A good example could be, say the $\lambda\phi^4$ theory in two dimensions, for which there has been lots of progress in the few recent years (see e.g. 1901.05023 and references therein). What kind of verifiable predictions can authors make for this theory, which seems to fall into the class of theories discussed.

The authors do study two examples in sections V and VI, but they are not realistic QFTs. Unfortunately these examples did not clarify the picture for me. Section V is about a zero-dimensional theory, in which some of the issues I brought up above do not arise, for example here one can unambiguously identify what is a "single-trace" operator in the present context. However, it seems that RG flows are also rather arbitrary. Below eq. (93) the authors claim that " Remarkably, $\beta_2(j, j_2)$ at general values of j_2 is already encoded in Eq. (93), which determines the fate of the RG flow in the space of all couplings." I do not understand why this should be the case, what prevents one from considering an RG flow with an arbitrary $\beta_2(j, j_2)$, without modifying the beta functions at zero j_2? To me it appears that the constraint follows from the postulates that the authors make, but not from any fundamental principles.

Section VI studies a D-dimensional example, however it is not formulated as a conventional local QFT. At least there is no local Lagrangian presented. Moreover, the set of beta-functions does not seem very generic for a theory without a large-N counting parameter. Nevertheless, in this example the authors make claims about interesting physical observables, like operator spectrum and OPE. Could any predictions of this sort be made in a realistic theory that we can analyze by other methods as well?

To summarize, above, I presented a description of the results which is rather skeptical, however, I am reluctant to dismiss the idea of this paper whatsoever. It could be that there is a reinterpretation of the results which is eventually useful and teaches us something new about the crucially important ingredient in most of the physics - the RG. For example, maybe the large-N assumption should be kept explicit. It would still be very interesting, generic large-N theories are clearly much more general that holographic theories. I wonder if it could be useful to think about the results of the paper in the context of Constructive RG (see e.g. 2008.04361 for recent developments). I wonder if what authors' claim could help to identify a preferred truncated basis for operators that is efficient at least in some classes of theories. Something similar is attempted by means of so-called functional RG, although such methods are not always successful, again see e.g. 2008.04361 for a review. As I said above, concrete proposals for observables in any realistic QFT that we can verify by other means would be very useful, however, it seems that all the suggestions I have would require a significant rewriting of the manuscript. In its present form it attempts to make a claim that is simply "too big", thus risking to fail to deliver any message to the community whatsoever.