Report on JHEP_252P_0420

Date: May 17, 2020

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Title: `Examining Instabilities Due to Driven Scalars in AdS`

Received: 2020-04-25 00:02:09.0

Referee report

General comments: In the paper under review the author explores the weakly nonlinear dynamics of a scalar field subject to time-periodic boundary conditions in anti-de-Sitter spacetime. This model links with two topics, the instability of AdS and time-dependent holography. The paper is mainly based on the first of these scenarios, following the seminal work of Bizoń and Rostworowski [3] and the application of sophisticated perturbation methods (Two-Time-Formalism and Resummation methods) [14,17] that allowed constructing the renormalization flow equations to describe the dynamics at times $t \sim 1/\epsilon^2$ (where $\epsilon \ll 1$ is associated with the amplitude of the scalar field). These studies targeted the weakly nonlinear dynamics of a scalar field vanishing at the boundary ($\phi(t, \pi/2) = 0$) of AdS guaranteeing the conservation of the energy inside the space. The main goal of the current paper is the construction of the renormalization flow equations; but in this case, the author considers a time-periodic boundary condition of the form

$$\phi(t, \pi/2) = \sum A_n \cos(w_n t + \varphi_n). \tag{1}$$

This is the crucial difference between the paper and [14,17], because this kind of boundary conditions allows the exchange of energy through the boundary and turns on non-normalizable modes which were absent in the case of homogeneous conditions. It makes the construction of the new flow equations nontrivial (we mean that the application of [17] is not straightforward and extra work must be done).

In connection with time-dependent holography, time-dependent boundary conditions allow the exchange of energy through the boundary; however, this exchange is not fully determined by $\phi(t, \pi/2)$ and its time derivatives, but it also depends on the properties of the field in the bulk. In consequence, time-periodic boundary conditions do not guarantee the periodic exchange of energy, generically leading the system to a complicated evolution. This kind of excitation of holographic models is widely used, usually in the form of quenches [25,27] but also as a driving [26,28]. Therefore, this paper makes a connection with the current literature.

Opinion: We consider that the paper under review contains an interesting calculation; however, we also consider that the implementation and presentation are not appropriate for publication. We enumerate the main points that require a significant improvement below. We also recommend a new review after the author has made the revisions.

1. Clarification: we have some questions that must be clarified because they may challenge the validity of some calculations:
   (a) Equations (3.12-3.13) are the flow equations when only normalizable modes are present; however, these terms are absent in (4.8), (4.17), (4.18), (4.26), (4.27), the flow equations with normalizable and non-normalizable modes. This absence is not clear, we expect that setting $A_\omega = 0$ in the flow equations we recover (3.12-3.13). Additionally, the solution of (4.8) is described in the second paragraph in section 5 without taking into account the mentioned terms. Please, clarify the absence of the terms given in (3.12-3.13).
   (b) Focusing on the case $m^2 = 0$ to illustrate the following comment, the normalizable modes have frequency $w_i = 2i + d$ and the non-normalizable solution also associated with these frequencies diverge at the origin. In this case the decomposition (2.11) is not possible (for a regular scalar field); therefore, a natural question is whether the paper covers the case of boundary conditions when one of these frequencies is present. This observation also challenges the validity of appendix B where $\bar{w} = w_l$ is assumed.
   (c) The first paragraph in section 4 explains how the non-normalizable frequencies are involved in the resonance channels. The paper is focused on the study of couplings between two non-normalizable modes and two normalizable modes. This restriction is not clearly justified, finding situations where one or three non-normalizable modes are present, providing some counterexamples to this explanation.
      - One non-normalizable mode and three normalizable modes: The author says that the resonant condition (2.19) is not

satisfied when only one mode is non-normalizable; however, this claim is false. We provide a counterexample in the case of AdS_5 and $m^2 = 0$; then, normalizable frequencies are $w_i = 2i + 4$ with $i = 0, 1, ...$ Taking the non-normalizable mode $\bar{w} = 2$ (this mode is regular at the origin and boundary) and the normalizable frequencies $w_{i,j,l} = 4,\ 6,\ 8$ the resonance $w_l = w_i + w_j - w_k$ is satisfied: $8 = 4 + 6 - 2$. Even more, the frequencies that extend the series $w_i = 2i + d$ with $i = -1, -2, ..., -(d-2)/2$ (or $-(d-1)/2$) have a dense net of resonances with normalizable modes making them the most interesting boundary frequencies.

  - Three non-normalizable modes and one normalizable mode: When three modes are non-normalizable the author mentions that they can contribute to $S_l^{(3)}$, however these terms are not analyzed in the paper and there is no explanation about this decision.

(d) In relation to (4.11), this channel is available for even integer values of Δ_+ (not only for the massless scalar). For example, in the case of $d = 3$ and $m^2 = -2$ we have $\Delta_+ = 2$ and (4.11) is also satisfied. The author should carefully consider the validity of (4.15) in these new situations.

2. Obscure explanations: In general the paper suffers from obscure explanations or lack of them that hinder the understanding of the main statements. It assumes a significant knowledge of the techniques and conventions used in [17] and holographic driven systems.

   (a) Connections with holography are vague. For example, the last paragraph on page 4 links with AdS/CFT but does not provide useful information for the understanding of the paper. This paragraph should be substituted by the last paragraph in section 5 which provides a better explanation. Additionally, the paper mentions many times the variation of energy inside AdS due to non-normalizable modes (for example, the beginning in section 5); however, the author does not provide expressions of this fact, despite this is the crucial difference between the current paper and [14, 17]. The expression for the exact variation of the energy as well as its leading order in terms of the amplitudes of the non-normalizable modes should be given.

   (b) In the paper, we can find different restrictions on the values of the frequencies and the mass. After reading the paper, the range of applicability of these calculations or the full expression of the flow equations are not clear. To improve this point we suggest the addition of a paragraph explaining the range of the parameters covered in the paper and clarifying the exact terms involved in the flow equations and the associated boundary conditions.

(c) Explanation about (2.14). The crucial point of this paper is the fact that the projection of the RHS of (2.14) onto the basis of normalizable modes does not diverge and therefore the flow equations can be constructed. However, this idea is not well explained in the main text or in appendix A.

(d) The explanation about equation (2.16) and (2.17-2.18) is very obscure for a reader that is not completely familiarized with the technical details given in [17]. $c_l^{(3)}(t)$ was not previously defined. Additionally, above (2.16), in the sentence "... at $\mathcal{O}(\epsilon^3)$ can be absorbed into... at that order [6]", what order does it mean? The sentence seems to indicate that the absorption is made in field ϕ_3 but indeed this is in field ϕ_1.

(e) Section 3 should be shortened, it requires almost 3 pages and its content was already addressed in [17, 18, 32] in the interior time gauge for massless and massive scalars. As the author clarifies later, the presence of resonant channels does not depend on the gauge and therefore section 3 does not provide novel information. The important elements of this section are equations (3.1-3.3), (3.8) and (3.12-3.14) and the explanation that the resonances $+++$ and $+-$ are not present. Formulas (3.5), (3.7) and (3.9-3.11) should be moved to an appendix.

(f) One of the most important parts, but at the same time one that requires a significant reformulation, is the first paragraph in section 4.

   i. Please, clarify the boundary conditions considered in this section, it is not clear due to the lack of labels for $\bar{A}$ and $\bar{w}$.

   ii. The discussion about the resonance condition (2.19),

$$\pm w_l = w_I \pm w_J \pm w_K, \tag{2}$$

   when non-normalizable modes are present is not clear. Above (in 1. (c)) we provided examples where (2.9) is satisfied when one of the modes is non-normalizable, contradicting one of the statements of this paragraph.

iii. When three modes are non-normalizable the author mentions that they can contribute to $S_l^{(3)}$, however these terms are not analyzed in the paper and there is no explanation about this decision.

iv. Please, clarify whether the expressions (3.5), (3.7) and (3.9-3.11) can be applied when some modes are non-normalizable.

v. We strongly recommend that the introduction in section 4 contains the specification of the boundary conditions, the convention of indices for normalizable and non-normalizable frequencies and the range of masses and frequencies covered in this section (the restriction $m_{BF}^2 < m^2 \leq 0$ given on page 11, line -3, should be highlighted).

(g) Page 16, paragraph below (4.16), lines 6-7. Channels $(++)$ and $(+-)$ are respectively activated for $\forall l \geq n$ and $\forall n$; therefore, $|\sum S_l|$ can not clarify whether $R^{(++)}$ vanishes. This comment should be clarified.

(h) Last paragraph on page 18. The author says that the resonant channel $+++$ does not contribute; however, it is not clear either the coefficients of this channel vanish or the resonant condition

$$w_l = w_i + w_j + w_k \tag{3}$$

is not satisfied. Can you explain the case $\chi = \Delta^+/3$? This situation may satisfy the resonance condition.

3. Lack of or inaccurate citations: The list of references provided in the paper is enough to cover the related literature; however, there are inaccurate comments about these references or lack of them.

(a) Page 3: "... $d = 4$ ... to draw the most direct comparison to the existing literature". Some references should be added. Even more, there is no comparison with this literature in the paper.

(b) Paragraph below (2.19). The current content is inaccurate, the described calculation was done in [17] for a massless scalar field in the interior time gauge, in [18] in the boundary time gauge and in [32] for a massive scalar in the interior time gauge.

(c) We mentioned in 2. (e) that section 3 contains an exercise where its results are already known in the literature; therefore the appropriate references should be included.

(d) Paragraphs 2 and 3, section 5. The dynamics of a periodically excited scalar field was already addressed in [26] and [28]; however, the author does not make any connection with these works. Instead of it, there is a citation to an unpublished work where the boundary conditions are static.

4. Inaccurate comments: we have found some comments with minor inaccuracies.

   (a) Page 2: References [3, 8, 9] simulate a scalar field in AdS; namely, they solve Einstein equations. Therefore the denomination "holographic AdS" is not completely accurate, this is just AdS.

   (b) Page 2, third paragraph last sentence. Can you clarify the meaning that non-normalizable modes couple to the source?

   (c) Page 3: "We demonstrate the natural vanishing of two of the three resonances...". Page 8, last sentence "... we are able to show numerically ...". The second sentence reports numerical evidence that the coefficients of the resonances $+++$ and $+-$ vanish; therefore, the claim in the first sentence is too strong.

   (d) Page 3, first sentence section 2. The scalar field is also spherically symmetric.

   (e) The notation S_l describes the projection $\langle S, e_l \rangle$; however, (4.20) and (4.23) use S_l to denote the part of S_l restricted to a particular resonance channel.

5. Minor mistakes:

   (a) Page 4, below (2.8). We think that the author means that the functions $\Phi_i^+(x)$ (instead of $\Phi_i^\pm(x)$) are regular at the origin. We can find counterexamples to the regularity of $\Phi_i^-(0)$ in the case of $m^2 = 0,\ w_0 = \Delta^+$.

   (b) Under (2.12), "... (2.11) collapses into a single term...", it is the second term in (2.11).

   (c) Last sentence in section 3.1 "... resonances are NOT present...", following section 4, the word "not" should be removed.

   (d) The LHS of equation (4.2) is a limit where $\tilde{x} \to 0$ while the RHS contains $\tilde{x}$; therefore, the expression is not consistent. This equation should be written as an expansion in powers of $\tilde{x}$.

(e) Section 4.1, line 3, "... $\{w_I, w_I, w_k\}$..." Should we replace one w_I with w_J? The indices i, j, k should be replaced with I, J, K in (4.3-4.5) to reflect that they can be normalizable and non-normalizable modes. Additionally, equations (4.3-4.5) report the same information, the frequencies with the opposite sign to w_l are normalizable. If they do not have a deeper meaning two of them should be removed.

(f) Paragraph below (4.7), T_l should be replaced with $\bar{T}_l$.

(g) Section 4, paragraph 2, line 2, "source equations", this term was not previously defined and there is no reference to any equation.

(h) (4.9) and (4.10) should be also applicable to the case of $m^2 = 0$, in contrast with the upper bound $m^2 < 0$.

(i) The absence of the term $R_{il}^{(-+)}$ is not clear in figure 5. Can you specify d? There is an early comment that suggests that $d = 4$; however, in this situation condition (4.11) is only satisfied for the second scenario ($\bar{w}_1 + \bar{w}_2 = 8$, namely $n = 4$) for $l = 0$. If this is the case, figure 5 is not appropriate to show the difference between a source term with and without $R_{il}^{(-+)}$.