History of the paper "Towards a More Well-Founded Cosmology"

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Abstract
First, this paper broaches the definition of science and the epistemic yield of tenets and approaches: phenomenological (descriptive only), well-founded (solid first principles, conducive to deep understanding), provisional (falsifiable if universal, verifiable if existential), and imaginary (fictitious entities or processes, conducive to empirically unsupported beliefs). The Big-Bang pardigm and the ΛCDM "concordance model" involve such beliefs: the emanation of the universe out of a non-physical stage, cosmic inflation (invented ad hoc), Λ (fictitious energy), and exotic dark matter. They fail in the confidence check that is required in empirical science. They also face a problem in delimiting what expands from what does not. In the more well-founded cosmology that emerges, energy is conserved, the universe is persistent (not transient) and the 'perfect cosmological principle' holds. Waves and other perturbations that propagate at c (the escape velocity from the universe) expand exponentially with distance. This dilatation results from gravitation. The cosmic web of galaxies does not expand. Potential Φ varies as -H/(cz) instead of -1/r. Inertial forces arise from gravitational interaction with the rest of the universe (not with space). They are increased where the universe appears blueshifted and decreased more than proportionately at very low accelerations. A cut-off acceleration a0 = 0.168 cH is deduced. This explains the successful description of galaxy rotation curves by MoND. A fully elaborated physical theory is still pending. The recycling of energy via a cosmic ocean filled with photons (the CMB), neutrinos and gravitons, and wider implications for science, are briefly discussed.

This paper was written in response to an invitation (in October 2015) by *Foundations of Physics* to contribute to a special issue on the Foundations of Astrophysics and Cosmology. The invitation was motivated by my previous paper "From magnitudes and redshifts of supernovae, their light-curves, and angular sizes of galaxies to a tenable cosmology" in *Astrophysics and Space Science 350* 755-767 (2014). When I was invited, I had already begun to consider a follow-up. I felt a need to be more explicit about the fundamental scientific problems of the tradition that has led to the currently accepted doctrine. I wanted also to further elaborate a more well-founded alternative.

I submitted a first version of the paper and received comments by two referees. The comments and my responses to them are appended here below. Then, I submitted a revised version that took the comments into account. After that, one of the referees voiced remaining objections on three points, and my paper was rejected on this basis:

1) "GR stands falsified": There was no objection to my basic reasoning in the introductory sections, from which this follows logically. The referee had trouble in following me here because of a prior commitment to GR, which most potential referees are likely to have.

2) The "delimitation problem" (between what expands and what does not): The referee saw a necessity of using a more refined metric, while the problem I address is the necessity to specify under which conditions this metric, no matter how refined, must not be used. I have modified the text in order to make it still clearer wherein the problem consists.

3) About the "gravitational potential": This point concerns a misunderstanding that I feel guilty for myself. It was, however, easy to amend this by orienting and labeling a figure more adequately. This section has now also been extended by calculations of the inertial force (equations 16-18), which are relevant to galactic dynamics and to motion in deep gravitational wells. These now lend considerable substance to the alternative that emerged.

After its rejection by *Foundations of Physics*, I submitted a new version to a less well-known journal, *Universe*, where it was quickly and more superficially reviewed by two referees. Both of them rejected the paper – essentially because it is an insult to those active in the field (I regret that some may feel so) and at variance with current doctrine (there can be no doubt about this).

My next submission of the paper, again to *Astrophysics and Space Science*, was met with silence for seven months. In

January 2018, the editor suggested to transfer my submission to *Foundations of Physics*, not knowing that it had been submitted to this journal to begin with. There are reasons to believe in this:

Scientific journals do not normally publish any paper that directly discredits the currently accepted doctrine within their field, no matter how wrong it is.

This is because referees can easily identify deviations from current doctrine and established practice, while it requires a higher intellectual effort to assimilate a new point of view and to follow a non-traditional path of reasoning. Referees naturally tend to avoid this extra effort. Students, researchers and editors tend to be aware of this and are likely to choose a mode of action that does not threaten their own carrier. Thus, they prefer to follow the mainstream. These circumstances are apt to preclude fundamental progress in science for long periods of time.

A journal with the explicit aims and scope of *Foundations of Physics* promises to be more committed to the advancement of science than to the defense of ill-founded doctrines. In the present case, this was a false promise, since only one of the two referees was able to reason accordingly, and the editor relied on the other one. Nevertheless, the comments from both referees contributed to the improvement of the paper.

Subsequently, I decided to put the details of the submission history, the anonymous referees' comments and my responses to these on the Web together with the paper. This will facilitate for those interested in my paper to decide for themselves what to accept and what to reject. It will also be for the benefit of those interested in the history of science and in the advancement of science despite this deep rooted impediment to its progress.

Comments to the author

Author's responses and measures taken

Manuscript submitted to *Foundations of Physics*, 2016-04-29
Reviews received 2016-10-14. A revised version of the paper was demanded by *Foundations of Physics*.
The revised version can be viewed here, where text that had been modified appears in green. (The line numbers and, in many cases, the page numbers indicated by the Reviewers are no longer the same in the revised version.)

Reviewer #1:
Recommendations to Author
The paper is interesting as it is aimed at a clarification of the epistemological status of the Big Bang model; a question seldom treated in the general literature.

The paper is reasonably well documented. However, it is apparently not fully up to date with the most recent publications. This is already clear from a brief glimpse of the reference list where most papers are dated before 2010 with only a few from 2010-2015. Though this deficiency probably does not heavily alter the conclusions of the paper, I think some updates are probably needed as indicated below in the detailed comments.

Sorry. My paper is not focused on "recent research" but on "long standing deficiencies" in cosmology. Among recent investigations, I mention [52], which makes a slight difference to my reasoning.

I have noted in different sections a considerable overlap between the proposed text and a previous publication by the author in Traunmüller (Astr. Sp. Sci, 350, 755, 2014). This is particularly true for the content of Section 3 and to some extent Section 5. Though some repetition is tolerable for the general understanding, the author should make clear what complementary information is brought by the present text or otherwise refer to this publication.

Please see my response below, under Section 3. My previous paper contained, as an essential part, an analysis of SNe data. In this case, the most recent results were important, and I take pride in having included them. This part is not repeated in the present text, but only referred to.

Some sections of the paper need probably to be shortened.

Section 2. Though there is an effort to somewhat "quantify" the degree of confidence of a particular theoretical

approach by way of a confidence parameter, this part is not particularly convincing. It is probably too long and need to be shortened, focusing mainly on examples fully relevant to the subject. The main topic that could be treated here could be the difference between the Perfect Cosmological Principe (basis of the Steady State Cosmology) versus the Restricted Cosmological Principe (basis of the Big Bang model).

I have made this section more strict and informative without increasing its length. I mention a few examples, but without going into details. Section 3 contains a wealth of specific examples, and section 2 is meant to prepare the reader for the evaluation of these.

Section 3. This part is generally clear and correctly presented. It gives a concise review of the key elements constituting the present and generally accepted Big Bang model. A large part is already treated in Traunmüller (2014) and the relation to this paper should be made clear. In this part devoted to a review of standard cosmology, the author should also make some references to the latest results from the PLANCK satellite (Ade et al. 2014, 2016) and to recent related discussions (see Ijjas et al. 2014 and references therein).
Planck 2015 results. XIII. Cosmological parameters Ade et al., A&A in press (2016)
http://www.aanda.org/articles/aa/abs/2014/11/aa21591-13/aa21591-13.html
Inflationary schism after Planck 2013 Ijjas, Steinhardt, Loeb (2014) Physics Letters B, Volume 736, p. 142-146.
http://www.sciencedirect.com/science/article/pii/S0370269314004985

Section 3 was not meant as a review of the standard model, but as a critical presentation of selected key features and their historical background. Since the CMB is so prominent among the Planck results but not so in my paper, I find it preferable not to refer to these. (I do not either wish to evoke the impression that I would be familiar with all of them.)
I have inserted the proposed ref. to Ijjas, Steinhardt, Loeb (2014) together with Guth, Kaiser, Nomura (2014) instead of a ref. to Steinhardt in Sci. Am.

The present paper was written upon an invitation that I interpreted as a request to elaborate the reasoning in my previous paper, ref. [37]. I refer to it only where it contains important information that is not repeated here. I do not wish to give it more prominence. Instead, I feel obliged to mention papers which contain a reasoning similar to mine, even if they are not top ranked [39, 40 41, 65].

Section 4. This section is not sufficiently informative and does not bring elements essential to the paper. Particularly disturbing is the different references quoted at the end of this section [28,29,30] that mixes works of very different impact (alternative cosmology, specific test of MOND with galaxy clusters, basic inflation model). The author should make clear the logical relation with the rest of the paper or delete this part

The ref. numbers were in error. This is now corrected to [27,28,29]. Special thanks for your close attention. I have added a statement “Let us now search for where this wrong choice appears to have been made” at the end of section 3 and made it explicit in the first passage of section 4 what appears to have been this wrong choice. I have also added “This will be approached in section 5.3” at the end of section 4. Since section 5.3 has become more conclusive after this revision, I judge section 4 now to be well motivated.

Appendix

The content of the Appendix is very secondary for the understanding. It merely duplicates one figure included in the reference [72]. I therefore suggest deleting it overall and referring to the publication in the text.

I have deleted the figure and inserted the verbal information slightly compressed into the main text on p. 15.

Detailed comments

Few sentences are too long and hardly understandable in standard English. They should be simplified, shortened or split. Examples are :

p. 2, line 36-38 :
It may not always be clear what can be taken as a first principle, but many actual theories build on an assumption of which it is clear beyond any doubt that it does not qualify as a first principle in our sense, and this paper is mainly concerned with such cases.

> It may not always be clear what can be taken as a first principle, but many theories build on an assumption that can easily be called into question. In such cases, it is clear beyond any doubt that the assumption does not qualify as a first

principle in our sense.

+ deletion of less relevant statements and insertion of one that introduces the following passage.

p. 2, line 59-60 + p. 3 line 1 :
It is, e.g., evident that questionable assumptions on which previously established theories had been based tend to be retained not only as long as they remain compatible with the empirical evidence but even as long as they can be made compatible with it by ad hoc means.

> The history of science shows us that questionable assumptions on which previously established theories had been based tend to be retained not only as long as they remain compatible with the empirical evidence but as long as they can be made compatible with it by ad hoc means.

(I did not really see a problem here.)

p. 4, line 40-45 :
In these cases, a numerical rating of confidence that would be generally valid is not obvious, except at the level of rank order: it is, e.g., justified to be more confident in reasoning based on a generalizing assumption that has not been falsified compared with an alternative in which essentially the same assumption is made under a certain condition that needs to be specified.

> In these cases, a numerical rating of confidence that would be generally valid is not obvious, except at the level of rank order. It is, e.g., justified to attach more confidence to a reasoning based on a generalizing assumption that has not been falsified than to an alternative that can be said to involve the same assumption under a restrictive condition that needs to be specified.

p. 10 line 42-47
When a theory persists in standing falsified, one should realize that a wrong choice is likely to have been made at a branching of the path that has led to it and preferably search for the right path, instead of proceeding on the once chosen path and dreaming up imaginary environmental conditions (Λ, CDM, etc.) under which this would be the right one.

> When a theory persists in standing falsified, it is likely that a wrong choice has been made at a branching of the path that has led to it. In such cases, one should preferably search for the right path, instead of proceeding on the once chosen path and dreaming up an imaginary environment (WIMPs, dark energy, etc.) in which this would be the right path. Let us now find out where this wrong choice appears to have been made.

p. 3 line 7-8 :
the difference between "paradigm" and "theory" should be explained

Here, I have chosen to be explicit about the relevant properties instead of referring to paradigms in general. Since it is a paradigm, not a specific theory, it is flexible

> Due to its free parameters and liberal allowance for evolution, it is flexible

p. 3 line 24 :
the "concordance model" should be briefly characterized here

> standard model of BB cosmology, the ΛCDM concordance model,

p. 6 line 35 :
The cosmological constant was formally re-introduced in 1998 from the deviation of the SNIa magnitude-redshift relation but it was already strongly required much earlier to make the age of Universe compatible with a H_0=75 Hubble constant.

I inserted “formally” and added:

A non-zero Λ had already been required earlier in order to make the age of the universe indicated by the Hubble constant compatible with the estimated ages of the oldest star clusters.

p. 7 line 3 :
More precisely, the Steady State theory did not make a prediction of the CMB but can provide an CMB explanation through starlight diffusion through specific dust.

OK, but the suggested explanation was not widely accepted. Since it may be considered as reasonable, I substituted “no convincing explanation” for “no reasonable explanation”.

p. 7 line 33 :

"since these theories (GR or CM) stand falsified already at scale of galaxies"
This argument is repeated at several instances in the text (see also p. 10 line 35 and p. 12 line 13). As it is formulated, it seems a too strong statement with respect to the reality and it should be somewhat attenuated. At the galactic scale, the discrepancy may still be explained by various hypotheses (hidden baryonic matter as cold gas or MOND) and MOND does not falsify GR.

This is a very important point to which both Reviewers objected.
I now clarify this more carefully in the rewritten first passage of the subsection on “Dark matter” (with a footnote). I maintain that GR stands falsified ‘at the present state of our knowledge’, with stress placed on this condition. I also mention the perihelion advance of Mercury which, similarly, brought CM to stand falsified.
My argument appears first already in the Introduction, on p. 3:
“In such cases [when a theory is confronted with incompatible evidence], a theory stands falsified until a convincing explanation of the discrepancy is presented.” I continue now:

“Although this is clear enough, it is not very rare in scientific practice that falsifications are brushed aside by advancing excuses in the form of ad hoc assumptions and constructs, also purely imaginary ones, that can only be believed in.”

I have also modified (and shortened) what is now the second passage on p. 5 on this point.

p. 9 line 32-35 :
About the dependency of standard candle magnitudes with redshift, some more recent works have been produced that the author may quote and discuss. This includes for instance :
Cosmological test with the QSO Hubble diagram López-Corredoira, M.; Melia, F.; Lusso, E.; Risaliti, G. (2016) International Journal of Modern Physics D, Volume 25, Issue 5, id. 1650060 http://adsabs.harvard.edu/abs/2016IJMPD..2550060L
Cosmological tests using gamma-ray bursts, the star formation rate and possible abundance evolution Wei, Jun-Jie; Wu, Xue-Feng; Melia, Fulvio; Wei, Da-Ming; Feng, Long-Long (2014) Monthly Notices of the Royal Astronomical Society, Volume 439, Issue 4, p.3329-3341 http://adsabs.harvard.edu/abs/2014MNRAS.439.3329W with a recent update
Gamma-ray burst cosmology: Hubble diagram and star formation history Jun-Jie Wei, Xue-Feng Wu in Proceedings of 14th Marcel-Grossmann Meeting, to appear in IJMPD http://arxiv.org/abs/1607.01550

While the redshifts observable in QSOs and GRBs are substantially larger than those of SNe, which is an advantage, I am not equally confident in an evaluation of their data. The redshifts of QSOs and GRBs may be affected substantially by intricate source-specific factors in addition to the cosmic redshift. I refer to one GRB investigation [42] because it had been used in [41], where a model in accord with the PCP was considered. This was not done in the papers mentioned by the Reviewer. Since I consider the analysis of the magnitude-redshift relation in my previous paper [37] as sufficiently conclusive, I have chosen to elaborate other aspects in the present paper. If I were to extend the analysis in [37], I would include QSOs and GRBs, but I have no such plan. I am aware of Melia’s Rh=ct model. Since this expansion model is not as special as SEC and the old de Sitter model, I have nothing to say about it separately.

p. 10 line 56
"and more recently, Mach" I suppose the author means "and later, Mach"

> and later, Mach [Deleted comment on deleted exponent.]

p. 17 line 39
"is related to distance D" : D should be defined here as definition of distance in cosmology is not straightforward

This part has been deleted in the revised version. D (without an index) occurrs in several equations before this place, and my use of it was consistent also in the first version. In the revised version, the new Figure 1 will be of help. Where this figure is mentioned, it is also told how D, actually 2D, can be measured.

p. 18 Figure 2 line 30
What means "Original" in the title "Original contributions"?

This is now the modified Figure 3. Original contributions

> Naive contributions [as if eq. (11) was valid]
[Deleted comments on former colors of lines no longer relevant.]

Section 5.3 Gravitation and Inertia
This section is interesting but relies on a major assumption that cannot be verified, namely that gravitation is mediated by gravitons with redshift dependency like photons. From this, a specific redshift dependency $(1+z)^{-1/2}$ is derived for the gravitational potential, leading to the distance dependency shown in Figure 2. This assumption is somewhat arbitrary and still highly speculative.

It was even worse than that. I made an error when invoking gravitons together with a simplicity assumption. This became clear, soon after submitting the manuscript, when I tried to treat the problem without invoking quanta. The expanded distances cannot be described by a factor of $(1+z)^x$, wich appeared in several equations and in the Figure.

In the revised section 5.2, I inserted two numbered equations for the expanded distance D_{exp} (9, 10), associated text and the new Figure 1.

The consequences derived in the last part of this section are therefore very weak.

I agree, but now this is different. The revised section 5.3 contains substantial changes after equation (11). The Figure inserted there is also new. A strong consequence is now made evident in a deduction of the phenomenology described by MoND. This includes a calculation of the value of Milgrom's a_0. This has caused a few revisions already in several places prior to section 5.3 and it is now also mentioned in the Abstract.

According to the criteria derived in Section 2, the confidence level is more likely to be close to 0. The author should comment on this and add some caution to his conclusions.

This is now commented in the last three passages of the paper.

Reviewer #2:
The author discusses a steady state cosmology in the light of modern observations, emphasizing the perfect cosmological principle and criticizing the inflationary paradigm. The actual calculations presented to support the claims are not very detailed and are restricted to simple Newtonian relationships.

The discussion in the text is rather clear, but there seem to be several incorrect statements at crucial places.
For instance, the author repeatedly claims that "GR stood falsified" by observations of galaxy rotation curves. This statement is incorrect because GR does not fix the matter content. GR is perfectly compatible with observations if there is non-visible (or dark) matter in addition to the visible one.

This is a very important point to which both Reviewers objected.
I now clarify it more carefully in the rewritten first passage of the subsection on "Dark matter" (with a footnote). I maintain that GR stands falsified 'at the present state of our knowledge', with stress placed on this condition. I also mention the perihelion advance of Mercury which, similarly, brought CM to stand falsified.

Furthermore, the author incorrectly comments that cosmological redshift is incompatible with energy conservation (page 10). Redshift is just an effect of relative time measurements made by different observers in the universe.

What I actually say is "The energy that radiation loses due to the cosmic redshift disappears without being transformed into any other form. This violates the most basic first principle of physics." In a theoretical discussion, it can be claimed that it is more appropriate to consider the situation in comoving coordinates. In this perspective, there is no energy loss – but there is no redshift either. Therefore it is true that the redshift is incompatible with energy conservation. I have inserted a footnote and the quantification, a fraction of $1-(1+z)^{-1}$.

The discussion in point (1) of section 5.2 ignores constructions using so-called swiss-cheese models, which show how a gravitationally bound system can be embedded in a surrounding expanding space-time.

I have revised the first passage of section 5.2, so that the nature of the inconsistency becomes clear. At the end of the passage, I mention "Swiss cheese models" (two new refs.) – and that these do not remove the inconsistency. They only quantify it. The crucial distinction is that between 1) standards of comparison and anything that behaves like these and 2) anything that expands. In the passage after point (1), I have made it explicit that standards of comparison are assumed not to expand in void regions either. This remains so in Swiss-cheese models.

The distinction between coherent and incoherent obsects seems to be meaningless. Instead, it is the distribution of matter and curvature (nearly spherically symmetric versus nearly homogeneous) which is relevant for whether

subsystems expand.

This distinction reflects the actual practice in BB cosmology, which is inconsistent. GR does not draw a distinction between coherent and incoherent objects but between gravitation and other forces. This leads to a non-standard cosmology according to my criterion (2), in which even planetary systems expand. If coherence (by other forces) was irrelevant, your second statement would imply that, within a cosmic void, an astronomical spectrometer would expand almost like the void itself, so that no cosmic redshift would be observable. I think that this is not what you wanted to say, but that you tacitly assume standards of measurement never to expand. About this I say now in the third passage of section 6:

"The routine assumption that standards of measurement do not expand in BB cosmologies can also be understood as an instance of path dependence. It reflects the idea of the rigid ruler of CM, which continues to be tacitly relied on in the frame of theories in which such rulers no longer exist."

The calculations on page 17 are very unclear: The author seems to confuse the matter distribution at one time (equation (11)) with the distribution on the past light cone of an observer (equation (12)). If (12) is used for the distribution at one time, which is the relevant notion in Newtonian gravity where one has a single potential, then the distribution is not homogeneous due to the dependence on redshift.

I think the revised text on p. 16 makes this clear now. The distribution is homogeneous in frames of reference in which the distances D are valid, but for gravitation and other interactions that propagate at c, the expanded distances D_{exp} are valid.

In connection with equation (12) I made an error when invoking gravitons together with a simplicity assumption. This became clear, soon after submitting the manuscript, when I tried to treat the problem without invoking quanta. The expanded distances cannot be described by a factor of $(1+z)^x$, which appeared in several equations and in the Figure.

In the revised section 5.2, I inserted two equations, associated text and a Figure for the expanded distance D_{exp} (9, 10). Section 5.3 contains substantial changes after equation (11). The Figure inserted there is also new.

These are just a few examples of problematic statements.

When it comes to the new viewpoint proposed by the author, the language becomes revealingly vague ("would likely prevent black holes from forming" on page 11, "it is not far fetched to imagine" on page 15). The new developments presented in this paper are therefore insubstantial.

It was not my ambition to present a fully developed deductive theory, but only to suggest a path that leads to such a theory. However, the former text in section 5 did not reach my aim. The revised text over-reaches it slightly, since it contains, in section 5.3, a deduction of the phenomenology described by MoND, also of the value of Milgrom's a_0. This is now also mentioned in the Abstract.

In what is now the second passage on p. 8, I mentioned "explanatory approaches", although I finally denied their explanatory power indirectly. To be consistent, I substituted "deductive" for "explanatory".

This version contains some additional minor stylistic revisions.

Revised text submitted to *Foundations of Physics*, 2016-12-14

Based on the following comments by Reviewer #2, this manuscript was rejected by *Foundations of Physics*, 2017-01-17. My responses to these comments show how the points they concern are treated in subsequent and the present version of the paper.

Reviewer #2:

The quality of the manuscript has not been improved by the changes made. In this second report, I focus on the most important shortcomings: "GR stands falsified": The author does not give strong enough reasons for this claim.

My reasons are stated in the following passage on p. 8, where "with C = 0" means "in which it is not scientifically justified to be confident at all":

"As long as the required amount of dark matter is neither predicted on independent grounds nor empirically confirmed to be present, its supposed presence remains an excuse with C = 0. This means in fact that, at the present state of our knowledge, GR and CM stand falsified already at the scale of galaxies. Therefore, we cannot be confident in models of the whole universe based on these theories. CM actually stood falsified already when faced with the anomalous perihelion advance of Mercury and the search for the supposedly responsible planet Vulcan had failed. The problem with this single case was solved by GR. The present, more widespread and more substantial one is still awaiting its solution – which will be attempted in Section 5.3."

Both Reviewers had accepted my reasoning in Section 2, where this is explained. I have only, by a few words, made it clear that the evidence is more overwhelming in the case of galaxy dynamics than in the now uncontroversial case of the orbit of Mercury.

He mentions the example of Pauli's proposal of the neutrino, which could have been considered "purely imaginary" at the time when it was made. Before neutrinos were discovered, energy conservation did not "stand falsified" by beta decay.

In the following passage on p. 9, I stress that Pauli's proposal was epistemologically very different from the proposal of "dark matter" and "dark energy".

"Assuming the existence of non-baryonic dark matter and dark energy has sometimes (e.g. Lahav & Massimi, 2014) been compared to Pauli's hesitant prediction of the neutrino, whose existence was verified only 25 years later (Mößbauer, 1998). These cases had in common that the existence of an entity that had not been known previously was suggested by abductive reasoning. However, the foundations on which these suggestions rested were epistemologically very different. The nuclear mechanism known as ß-decay appeared to violate a first principle: conservation of energy. Given that this is a principle of the kind in which we can be confident even when faced with evidence that appears to contradict it, the existence of a new particle, which was later named the neutrino, was the simplest conclusion that could be drawn. This was not a fictitious assumption but a well-founded prediction. In contrast, the magnitude–redshift relation of a type of supernovae appeared to violate just the BB paradigm, in which it was not justified to be confident, and which rests on a theory (GR) that in fact stood falsified already in view of the dynamics of galaxies."

The wording is modified in order to make this clear even for those who have not read Sections 1 and 2.

If this were the case, we would have the meaningless situation that a statement "stands falsified" for some time and is later unfalsified by observations.

In the light of new knowledge, a statement that stood falsified may well become tenable again. This is now said in a new passage on p. 5, into which I have also lifted a former footnote about the notions of "standing falsified" and "being tenable":

"It is well known that empirical falsifications are not firmly conclusive. This is part of the Duhem-Quine problem. Falsifications are only valid within the frame of the knowledge we have. A statement that stood falsified may even become tenable again in the light of new knowledge. Strictly speaking, universal statements can only be claimed "to be tenable" or "to stand falsified", unless it follows from definitions and logic alone that they are "true" or "false". The classification of a tenet as fictitious (C = 0) might also change in the light of new knowledge, but as long as we lack this knowledge, our confidence in it must remain at zero if we wish to remain within science."

The language used by the author is highly misleading and does not reflect his own discussion of a continuous scale of confidence levels.

My discussion is concerned with making it clear in which cases we are at one of the end points of this scale, where we can be either fully confident (C=1, 2a in Table 1) or not at all (C=0, 2c in Table 1), given the knowledge we have. In the present version, I return to this also in the discussion section, on p. 25:

"The confidence check suggested there is called for in the definition of empirical science as the pursuit of reliable statements about nature. It requires, above all, to single out ad hoc assumptions (2c in Section 2), but it requires also to distinguish between merely tentative assumptions (2b) and those which appear reliable at the state of our knowledge (2a). Scientists often accept the tenets of established theories without reflecting about their reliability at all, while philosophers of science rather image all assumptions as fallible without distinction. None of these attitudes is suited to promote science fundamentally. Some scholars even reject the pursuit of objective observations, claiming that all observation is necessarily prejudiced, since it depends on assumptions. However, assumptions can be well-founded, and these must not be dismissed as 'prejudices'."

Some of the current proposals for dark-matter or dark-energy candidates (including the cosmological constant) are

predictive, and there is no reason to believe that cosmology could not go the way beta decay eventually did.

I had already made it clear enough (in Section 2) that the epistemological value of predictions based on ad hoc assumptions is zero and (in the quoted passage on p. 9) that there was never such a problem in the case of beta decay.

Delimitation problem: The author confuses properties of the idealized FLRW metric with the expansion behavior in a universe with structure. It is true that "the FLRW metric and various GR-based alternatives [...] do not leave anything more extended than a point unaffected [by expansion]." But it is incorrect to say that "without a delimitation criterion, which the metric does not provide, it is clearly inconsistent to exempt anything, such as standards of comparison" (page 14). One would just have to use a more refined metric which takes into account the effects of matter of a measurement device, a solar system, or a galaxy. Swiss cheese models, for instance, do not "predict the cosmic redshift to be unobservable from inside cosmic voids" because light rays crossing different underdense and overdense regions would be subject to redshift.

... and so would standards of comparison, unless there is an exemption for a set of things that includes these. I have now modified the text in the Section 5.2 (p. 15), in order to make it more evident that a more refined metric does not help in solving the delimitation problem: In common practice, the proper spacetime defined by local standards of comparison is not identified with this metric but with the one that is valid in the absence of gravitation and expansion.

"A solution of the delimitation problem does not require a more refined metric but a rule for when the metric that is valid in the absence of gravitation and expansion is to be used instead – if this should be logically defensible at all."

It is also incorrect to say that "these [standards of comparison] are, of course, kept together by other forces, but this is not captured by the metric." Non-gravitational forces imply energy distributions, which back-react on space-time geometry and are therefore captured by "the metric." Such effects may not be included in simplified metric models, but would be so in a complete solution to Einstein's equation. Dealing with a complete solution is certainly complicated, but this fact does not imply that there is a conceptual problem with cosmic expansion. The author's discussion of coherent and incoherent matter is based on an oversimplified understanding of metric solutions. It remains too vague to justify his criticism. The big-bang model is based on the FLRW metric and perturbative inhomogeneity, but this description is used only to derive the expansion behavior on large scales relevant for cosmology. By extending the range of this approximation to smaller scales where significant inhomogeneity can no longer be ignored, the author introduces artificial problems into the framework.

The quoted clause is no longer in my text. The marginal effects that are considered in this comment remain irrelevant in the actual practice of exempting everything up to the size of the largest galaxy clusters. This remains a problem. I have inserted [after criterion (1) on p. 15] a reference to a paper that exemplifies what the criterion implies:

Giulini, D. (2014). Does cosmological expansion affect local physics? *Studies in History and Philosophy of Modern Physics 46*, 24–37.

It substantiates the conclusion I draw in the same passage: Criterion (1) makes BB models fail at the very largest scale, while models in which only waves expand remain tenable.

Gravitational potential: The relation between Fig. 3 and equations (11)-(13) is unclear. It seems that the "radius in Hubble length units" is an upper bound on the integrations in (11)-(13), but these integrals are supposed to be done over all of space in order to include all masses. What, then, is the relevance of showing only the results of integrations over finite regions? Moreover, the curves that supposedly correspond to equations (13) and (14) do not seem right. The integrands in (13) and (14) are positive. It is then impossible to obtain non-monotonic curves as shown in the figure if one integrates with increasing upper bounds.

I feel guilty for having caused this misunderstanding. The new Figure 3 is turned upside down (as it should be), the numbers are negative and labelled “Potential per unit radius”. I have also modified the caption accordingly. The potential calculated by, e.g., equation (15) corresponds to the area delimited by the dashed line and the abscissa.

The discussions in this part remain exceedingly vague and unclear. The claims made by the author are unjustified.

In Section 5.3, I have inserted equation (14), some words in its context, and modified the passages before and after equation (15) in order to make the reasoning in calculating the equivalent Φ and a_0 more explicit and easier to follow.

The text that follows after equation (16) in the present version of the paper is largely new or rearranged. Equations

(17) and (18), which imply that "black holes" cannot form, were not included in previous versions. The Abstract has also been rephrased.

Text submitted to *Universe*
Date of manuscript submission 15 Mar 2017 14:55:41

This manuscript has been rejected by *Universe* on the basis of the comments by Reviewers #3 and #4.

Reviewer #3 made me add Fig. 1, but did not display much interest in the paper and suggested rejection because it is offensive towards the scientific community.

Reviewer #4 advanced comments that are concerned with the traditional doctrine, i.e., with GR and Big Bang cosmology - not with the reasoning followed in my paper from the premises in Sections 1 and 2 to the conclusions that follow and to the more well-founded cosmology that emerges in Sections 4 and 5.

Reviewer #3:
The paper is devoted to question the so-called concordance model in cosmology in a more philosophical and epistemological ways rather than in a scientific one. Although the discussion is an open and dynamical issue within the scientific community, the paper does not provide any new insight or proofs about his claims.

For instance, the author assumes (starting -presumably- from a generalization of the Cosmological principle) different distance relations that leads to a different expression for the apparent magnitude. Nevertheless, this is not supported anywhere, nor theoretically neither by testing the model with observational data. Even the references on which this claim is supported do not provide any proof.

I refer to my previous paper (Astrophys. Space Sci. 2014, 350, 755), in which I showed, using data on magnitude and redshift from 892 type Ia supernovae, that the redshift factor (1+z) follows an exponential function of distance and that, for “standard candles”, magnitude m=5log[(1+z)ln(1+z)]+const. It would be convenient to show this in a figure also in the present paper. Since my text in Section 3 is already to some extent repetitive, I have not taken one from the previous paper, but inserted a new one, Figure 1, which shows that equation (4) is tenable.

The rest of the paper is written in the same line.

Has it been read? The most 'substantial' part of my paper concerns galactic dynamics, for which I present a deductive approach that explains inertia, the velocity c, and the phenomenology described by MOND. The literature on MOND that I refer to, in particular the extensive review article by Famaey & McGaugh, contains the relevant background.

In addition, the paper is full of some offensive sentences towards the scientific community, more typical of a soccer magazine than of a serious scientific journal, for instance "It appears that the whole community engaged in concordance model is blinded by preconceptions".

I substitute for it:
“These impose preconceptions that prevent mainstream researchers from noticing even the most obvious alternatives.”

My reflections on path dependence and confidence (Section 2) are central in this paper. I regret that the conclusions from these can be perceived as an affront by adherents of Concordance Cosmology and even of GR and Newtonian mechanics, irrespective of my wording.

Hence, I think the paper should be rejected for its publication in Universe.

Date of this review: 28 Mar 2017 19:03:27

Reviewer #4:
The manuscript presents an epistemological approach to the scientific pillars of the Standard Cosmological Model. Although I understand that this paper does not pretend to analyse the Standard Cosmological Model in such a way to be scientifically rigorous, there are a number of inaccuracies and errors that make the speculations present in this

work far enough to be true. The author tries to falsify the Big Bang model using a series of well known shortcomings but ignoring the successes of the model. The model makes fundamentals predictions that have been verified up to the precision allowed by the observations.

The precision of the predicitions of the Big Bang model is often lauded in the introduction of papers on cosmology, but this merely expresses that the authors belong to the community of believers in the “Concordance Model” (aptly so named). Verification of a prediction only confirms that the model remains within the set of tenable alternatives. It does not single out any specific one among these, and it cannot be asserted to be valid outside the range of data covered.

One can not consider General Relativity to be wrong because it is not able to explain everything in the Universe. It is true that one need to include Dark Matter and Dark Energy, but it is also true that General Relativity predicts Gravitational Waves that were observed one years ago.

The observation of gravitational waves does not single out GR in particular. It speaks in favour of field theories of gravitation.

The author should take in mind that: any relativistic theory of gravity should include the well established successes of General Relativity in its weak field limit, as well as General Relativity includes Newtonian gravity.

Sorry, I think I have made it clear enough that General Relativity and Newtonian gravity are both to the same extent dramatically *un*successful in the weak field limit, i.e., where $g < cH$. As for the relations between theories, see also p. 24:

"The non-speculative cosmology that emerges represents a Machian alternative to GR whose predictions deviate from those of GR both where gravitation is very weak and where it is very strong, as can be seen in (18). GR will remain a limiting case of such a more comprehensive theory, to which it points out the way, and Einstein (1917b) considered this to be the fairest destiny a physical theory can have. However, there is a limit to such developments: the most well-founded theories can no longer be topped in this way, since they will themselves be the most comprehensive ones."

Here I report few general issues that can be found in the paper.

- Line 14: "Big Bang cosmology further faces conceptual and pragmatic problems in delimiting what expands from what does not." This is not a problem since, in General Relativity, the space and time are intended as dynamical entities.

The problem that I describe in the first four passages of Section 5.2 and which I summarise in the quoted sentence from the Abstract (now reworded) involves, in addition to a "dynamical entity", a static spacetime whose range of validity needs to be specified.

- Line 101: The Kepler laws are not phenomenological. They can be recovered with a rigorous mathematical approach based on the classical mechanics

Classical mechanics is due to Newton (1687). Kepler published the last one of his phenomenological laws in 1619 and died before Newton was born.

- Line 186: ";....has emanated under conditions to which physics does not apply." This sentence is false. The fact that classical mechanics and General Relativity can not describe the status of the Universe at the BB is because one need a quantum theory of gravity to do it.

I substitute “physics, as we know it,” for “physics”. This is more precise.

- About Dark Matter: the author is simply ignoring all successes of GR+DM.

I do not really ignore them. On p. 8, I make it clear that it is not justified to attach significance to "exotic" entities. Dark matter is introduced as a fudge factor. It does what it is meant to do - to compensate for the failure of the model. See also p. 4-5:

"2c) Assumptions that, in addition to not being rooted outside the theory in question, also lack independent empirical support. Any reasoning based on these remains within the domain of imagination. Such assumptions are ‘fictitious’ and lead to epistemically void beliefs. Modern theoretical physics offers a range of “fairy tale physics” (Baggott, 2013) in which fictitious assumptions are either primary, as in string theory, or secondary, as in the “dark sector” of BB cosmology, discussed in Section 3."

It is true that we do not know what DM is at particle level, but we have many candidates for it and we are trying to detect them. Each candidate is well motivated at theoretical level in particle physics, and quantum field theory. Finally, there are also many prescription to modify the gravity to account also for the dark matter.

I refer to Modified Newtonian Dynamics in many places.

- About inflation: the problem of inflation is actually unsolved (as the author said). However, there are indications that it happened (like tensor mode in the CMB power spectrum) that can not be ignored.

- About Dark Energy: the need of introducing Dark Energy is not related to having an expanding Universe but to the fact that the expansion is accelerated.

In my reasoning, this is not a fact but an ad hoc excuse, and Dark Energy would be needed even to keep the expansion constant.

- About conservation of Energy: the point is the following. The energy is conserved if time-translation invariance holds. But in general relativity space and time are dynamical, and they can evolve with time. When the space through which particles move is changing, the total energy of those particles is not conserved. However, general relativity gave us a single important equation, namely “energy-momentum conservation.” The meaning of this equation is straightforward: energy and momentum evolve in a precisely specified way in response to the behaviour of space-time around them.

“Energy-momentum conservation” is specific to GR. Conservation of energy is the most fundamental idea - and it is a weakness that it is not necessarily respected in GR.

Date of this review 12 Apr 2017 15:24:16

Towards a more well-founded cosmology (pdf) | **Summary of the model** (3 p.)