Reports and discussion on preprint arXiv:gr-qc/0511113
On the geometry of cosmological model building

4 referees (anonymous) and author E. Scholz

The manuscript *On the geometry of cosmological model building* submitted to *Foundations of Physics* 21 November 2005, was refereed by 4 members of the community. Two of them were negative, one positive, one mixed with clear negative tendency. The referee reports were transmitted to the author and could be answered by him. After an extended phase of indecision, the paper was withdrawn on 15 January 2007 by the author. The referees' points of criticism may be characteristic or illuminating for the view of the community. Thus the critical discussion may be of interest for a wider audience than just the participants. It is documented in this appendix. Referee reports and author's answers have not been changed. The order of the reports is identical with the one of the communication with the journal editor.[1]

Referee Report 1

1. General Remarks

The standard model of cosmology supplemented by general ideas of inflation, is highly succcessful. It describes in terms of a few adjustable parameters a large number of increasingly accurate observations. (For a recent joint analysis, I refer to the important paper: U. Seljak, et al., Phys. Rev. **71**, 103515 (2005).) Any alternative cosmological model has to face this level of success. In my opinion, the author generally underestimates the weight of the existing body of observations.

This shows up already in the lengthy *Introduction* of the paper, that is in my opinion too negative. Examples: a) While I consider with many colleagues the Dark Energy as a great discovery pointing to new physics, with an enormous impact both on fundamental physics and cosmology, the author says that this is "even more problematic". For present day fundamental physics, the Dark Energy is a mystery, but that makes it very interesting (Planck's radiation law was also mystery at the time). b) Exotic dark matter is not a "postulate (. . .) that turned into an anomaly", but an established fact, because we observe its gravitational action on many different scales. The elucidation of its nature is one of the most fascinating research goals. c) I don't think that one can talk of an "inconsistency arising from the hypothesis of primordial nucleosynthesis". While the agreement between theory and abundance determinations — with substantial systematic uncertainties — is so far not perfect, one can justly regard the present situation as a great success. (For a recent review I refer to G. Steigman, astro-ph/0501591.) Moreover, the excellent agreement between big bang nucleosynthesis and CMB — determined by baryon densities provides impressive confirmation of the standard model of cosmology.

[1] Date of appendix: 15 January 2007.

2. Structural aspects of the proposed theory

The geometrical framework of the proposed alternative cosmology is the *integrable Weyl geometry* (IWG). In contrast to Weyl's original (physically unsuccessful) proposal the 1-form φ, describing the length or scale connection, is no more a dynamical but an *absolute* element of the theory, which can be gauged away. The theory is thus from the beginning not gauge invariant in the sense of Weyl, but only gauge covariant. E. Scholz attributes a physical meaning to some gauge choices, when he talks of *matter gauge*, *Riemann/Einstein gauge*, and *Hubble gauge*. In particular, he interprets the redshift as a Weylian length transfer $exp(\int \varphi)$. For me this is very disturbing. Why should an absolute element of the theory become physically relevant in this sense. (The following analogy might illustrate my worry: One can in an obvious way write the standard diffusion equation in a Lorentz covariant form by introducing a vector field n^μ, satisfying the Lorentz covariant equation $g_{\mu\nu}n^\mu n^\nu = -1, \quad n^\mu_{,\nu} = 0$, but nobody would consider different choices of the absolute element n^μ as physically meaningful.)

At least as disturbing for me is the authors generalization of Einstein's field equation. Since he wants to have them gauge covariant (actually he speaks of gauge *invariance*), he has to introduce an **external** field β of weigth -2, with no dynamics. This destroys the diffeomorphism invariance of general relativity, really a backward step in my opinion. As a consequence, his field equation (3) is in contradiction with $\nabla \cdot T = 0$, and hence with a diffeomorphism invariant matter Lagrangian. Unfortunately, the latter is never specified.

3. Cosmological application and observation

In Sects 3 and 4 Scholz considers what he calls Weyl-geometric Robertson-Walker models. In the Hubble gauge the metric takes a *static* form and all the redshift information is contained in the length connection φ. The author maintains that the metric in this gauge "as a serious candidate for defining the physical metric". So the expansion of the universe would be an illusion. The special class of "Weyl universes" is even time homogeneous in every respect. In Sect. 6 these models are compared with some empirical data.

First the luminosity-redshift relation of type Ia supernovae data is addressed. Here, only old data from 1999 are used, which have substantial systematic uncertainties. In the meantime much better data extending also to much higher redshifts have become available. Perhaps the best high-z SN Ia compilation to date are the results from the Supernova Legacy Survey (SNLS) of the first year (astro-ph/0510447). The other main research group has also published new data at about the same time (astro-ph/0510155). The author of the present paper should — as other authors who have proposed modified cosmology's — perform a (joint) likelyhood analysis of these data. (There are standard packages for doing this.)

A crucial drawback of the proposed cosmological models is that nothing is offered, that would be comparable to the detailed explanation of the observed anisotropies of the CMB radiation (/separation and relative heights of the peaks, anti-correlation with polarization data, damping scale, etc.) within the standard model. Foreground effects, as suggested by the author can not give all this.

The "Weyl geometric version of the Einstein universe", which are time homogeneous in the large, whence the evolution is regarded as a "local or better a regional feature", are in dramatic conflict with recent observations far in time. These have established that the universe at hight redshifts ($z > 6$) is *fundamentally different* from the predominantly ionized state today. Already a few years ago, observations of $z > 6.2$ QSOs show near- complete absorption of flux at wavelengths short-ward of Lyman α (Gunn- Peterson effect), etablishing that the neutral fraction of hydrogen is much greater than at low redshifts. This is evidence that there was a ionization 'phase transition' at a redshift somewhere above the presently reached value. Moroever, the volume averaged (comoving) star formation rate in galaxies is about 6 times less at $z \simeq 6$ than at $z \simeq 3$. The bright end of the luminosity function has evolved greatly in this redshift interval. All this was established on the bases of the HST/ACS GOODS survey, and the Hubble Ultra Deep Field (UDF). (See, e.g., A. Bunker, et al., astro-ph/0508271, and the references therein.)

In summary, the paper of E. Scholz has so many conceptual and observational drawbacks, that — in my opinion — it should not be published in Foundations of Physics.

—————- * —————-

Author's comments on first referee report

The referee is a staunch defender of the standard model of cosmology. That is challenging for a scientific discussion, but for a referee's report his position is, in my view, too strongly biased by this ex-ante position. Let me go through his objections.

"1. General Remarks"

a) Dark energy is, as the referee states, a "mystery …that makes it very interesting". No disagreement on this point, only different ways of dealing with this "mystery". The article makes a contribution to it. The Einstein-Weyl geometric approach gives a *constant* dark energy contribution, fitting well with a theoretical calculation of C.Castro (pp. 21, 28). The standard model gives a *time-dependence* of the dark energy contribution which raises strong conceptual and physical problems, which not only I consider as an anomaly. Defenders of the standard approach usually deal with it by soft and ad-hoc arguments (different kinds of "anthropic principles"). It is not the aim of this article to counterpose these different views and to discuss them in detail. But a careful and unbiased reader should at least be able to notice that a constant dark energy density poses much less problems for a physical explanation and may be a valuable contribution to solving the "mystery's" riddle.

b) In the submitted article *dark matter* (without further specification) *is accepted* as an "established fact" observed by its gravitational action, in contrast to the referee's reproach. Recent estimations given by J. Peebles are used for the determination of the main free parameter of the model (pp. 17, 26). The cosmological corrections used in the evaluation of the observational data by

J. Peebles agree even better with the Weyl geometric approach than with the standard one (pp. 17f., and app. III). The referee does not seem to have noticed this argument at all; at least he does not mention it.

c) In the criticized passage the qualification of dark matter as *exotic*, i.e. non- baryonic, is being discussed. While the hypothesis of primordial nucleosynthesis gives *good* values for the relative abundances of the light elements (discussed by the referee), if *fails completely* (ignored by the referee) with respect to the main parameter of maximally allowed density of known, although "dark" matter. Therefore "exotic", i.e. unknown types of matter has to be postulated in the standard approach. Exoticity of the matter gap is not "an established fact", as claimed by the referee. Exoticity of matter *cannot* be observed; it is a *theoretical construct*. High energy physicists have not (yet?) found convincing experimental indications of it. Detailed astronomical arguments (Overduin/Wesson, Physics Report 2004) referred to in the paper seem to show that exotic matter would even not be able to solve the problem, if one assumes its high energy physical existence. In this sense the postulate (of exoticity of the gravitating matter gap) has "...turned into an anomaly".

"2. Structural aspects of the proposed theory"

The Einstein equation in integrable Weyl geometry is scale gauge invariant; the whole theory is gauge covariant, as remarked by the referee. Matter behaviour (atomic clocks) defines a physically specified time and length scale like in classical (semi-Riemannian) relativity. Also classical relativity introduces an "absolute element" into the theory (its importance was Einstein's first argument against Weyl's original theory, as the referee knows very well). This choice is, however, usually hidden by an unquestioned default specification of Riemann/Einstein gauge. The proposed modification opens the eyes to the possibility of different specifications without affecting the field theoretic structure of Einstein's gravitation theory. I am sorry if this is perceived as "disturbing", but I cannot change it.

The referee is right that it would be lethal for the IWG approach, if the field equation (3) were in fact "in contradiction with $\nabla \cdot T = 0$", as he claims. If one reads $\nabla\cdot$ as covariant divergence with respect to the Weyl-Levi-Civita connection of IWG this would be the case.[2] In the approach of eGRT as developed in the article *Dirac's extended IWG* with the *scale covariant derivative* is used (first paragraph of section 2). It is clearly stated that this step is physically important, and reasons are given why it is so (compatibility with QM). In the sense of *scale covariant divergence* of IWG the contracted Bianchi identity holds, $div\,(g^{-1}(Ric - \frac{\bar{R}}{2} g) = 0$ and thus also $div\, T = 0$ (here T in its co-contra form). That follows from the validity of the equation in Riemann-Einstein gauge (then the Weyl geometric connection is identical to the Levi-Civita connection and the length transfer function is 0; therefore Dirac's scale- covariant derivation is identical to the one of Riemannian geometry). As Dirac's scale covariant differentiation of a scale covariant tensor leads to a scale covariant tensor, the equation holds in any gauge.

[2] This was the view of Straumann/Gähler/Audretsch, Ehlers/Pirani/Schild and others.

An explicit calculation for a gauge transformation

$$(g,0) \mapsto \tilde{g} = \Omega^2 g \, , \quad \tilde{\varphi} = -d \log \Omega = -\frac{d\Omega}{\Omega}$$

gives

$$\begin{aligned} div\, (\Omega^{-2}(g^{-1}(Ric - \frac{\bar{R}}{2}g)) &= -2\frac{d\Omega}{\Omega^3}(g^{-1}(Ric - \frac{\bar{R}}{2}g) + \Omega^{-2} div\, (g^{-1}(Ric - \frac{\bar{R}}{2}g) \\ &+ (-2)\, \varphi \otimes \Omega^{-2}(g^{-1}(Ric - \frac{\bar{R}}{2}g)). \end{aligned}$$

The second term is 0, the first and the last one cancel. Thus $\ldots = 0$. This explicit calculation makes apparent, how the terms affecting the (Levi- Civita covariant) $\nabla \cdot T$ of the referee's argument are absorbed in Dirac's scale covariant calculus on manifolds. It gives another reason, why Dirac's scale covariant derivative *has* to be used here.

Therefore for a "good" matter Lagrangian diffeomorphism invariance will hold (while its scale invariance will be broken), just like in the classical relativistic approach. In the article no explicit matter Lagrangian is given, because this is a different building site, open to other authors with different expertise.

Put on its feet, the referee's argument may be read as an obliquely formulated hint that energy conservation in the extended version of GRT should be discussed in the article. It can be implemented in the final form without much effort (≈ 1 page, see above).

"3. Cosmological applications and observations"

The more recent data on *supernovae Ia* are empirically very valuable improvements of the results from the late 1990s, but they cannot tell us anything new with respect to the model alternative. One can easily judge that essential, i.e. observable, differences to the standard model cannot arise *here*. Differences will become apparent only if other data are taken into account, like quasar frequencies, stellar formation rate, observations close to redshift $z \approx 10$ or $z \approx 12$ etc. If proposed by the editor of the journal, the data evaluation on p. 29 of the article can make use of the more recent data brought into the play by the referee.

The remarks on the *ionization* phenomena at high redshifts $z > 6.2$ are very interesting. Here we come closer to regions where finally a decision between the two geometric models will come into sight. Unfortunately for the referee (fortunately for the author) the empirical informations on the "ionization phase transition" is not at all as clearcut as claimed in the report. The data release 2004 of WMAP led to a measurement of the Thomson scattering optical depth to electrons and indicates a "reionization" at about $z \approx 17$ rather than at $z \approx 6$. Experts agree that the time of reionization as seen by the Gunn-Peterson effect (indicated by the referee) *conflicts* strongly with the estimates from electron column density measured by WMAP. We have thus to await, at least, more definite empirical evidence, before we try to judge whether *large scale* (overall) reionization is more than a theoretical expectation of the standard picture, fed by observations of *local* regions of higher concentration of neutral hydrogen than average.

The hint at the data on the star formation rate is very interesting. In the standard approach the data *seem to indicate* a reduction of star formation per volume from $z \approx 2$ or $z \approx 3$ to $z \approx 6$. In the Einstein-Weyl model this is not the case. The volume scanned by the backward lightcone over equal z-intervals decreases after passing the equator of the 3-sphere. A very critical communication of a senior colleague of the field from Zürich made me aware of the *Madau plot* of star formation rate with redshift. Contrary to what this colleague and the referee apparently think, the Madau plot is *not at all a proof* of global cosmic evolution; it may even turn out to be the other way round. The up and down of star formation fits well to the Weyl geometric approach. The Madau plot behaves qualititatively quite like the quasar frequency plot. From the point of view of the Einstein-Weyl models, the change of the star formation rate with redshift looks very much like a purely geometric effect of the light cone geometry. If such a correlation turns out to be valid, we have arrived at a strong argument *in favour* of the Einstein-Weyl model.

Star formation rates at *very high redshifts* may even become crucial for a discrimination between the two geometries: Close to $z \approx 11$ the star formation rate should go to zero (IWG approach, conjugate point of observer); it should increase again for still higher redshifts (beyond the conjugate point), if these become observable. The standard model would have to invent very strong ad-hoc hypotheses, in order to "explain" such a hypothetical behaviour, if observations would support this expectation. If, the other way round, something like a monotonous decrease of star formation up to $z \approx 17$ would be observed (the present range of the expected reionization phase in the standard approach), we had a clear empirical refutation of the Einstein-Weyl cosmic geometry.

By two statements near the beginning and close to the end of his report, the referee emphasizes that he wants to see an equivalent to the model explanation of the CMB anisotropies in the standard approach, before he might, perhaps, take an alternative approach into consideration more closely. It is true that such is not given in the submitted article, and it would be premature to do so at present. If the IWG approach touches ground empirically, the CMB anisotropies will correlate with super cluster inhomogeneities in a wide band around the equator of the spatial 3-sphere. The data analysis will have to be done in much closer relation with empirical data on observed matter structures than in the recieved view. The present results on higher order peaks of the spherical harmonic decomposition of the CMB anisotropies may turn out to give valuable information on different scale levels of matter structures in the universe; but these are open questions at the moment.

Moreover, there is an ongoing discussion on how well the number of free (adaptable) parameters of the inflationary and concordance model compares with the number of independent parameters of observational data taken into account. It is not the aim of the article or of these comments to put this comparison under closer scrutiny. Surely the model is a subtle mathematical superstructure buffering the problems of standard cosmology, but it is not at all clear, whether its coherence is more than an impressive proof of the inventiveness of the community.[3] In any case, it is a rather bad (methodologically

[3] The Weyl geometric approach lets us expect that anisotropy data and the geometrical

circular) argument to use the compound inflationary and concordance model and its coherence with different classes of empirical data as an *exclusion criterion* for other approaches. We should bring in more patience to see where the "weight of the existing body of observations" (referee report, p.1), and its additions in the near future I want to add, leans to.

Wuppertal, Jan 14, 2006, E.S.

Referee Report 2

TITLE : ON THE GEOMETRY OF COSMOLOGICAL MODEL BUILDING
AUTHOR : E. S c h o l z

This is a long and interesting paper. Instead of a Riemannian Geometry the author proposes the use of a Weyl geometry with integrable length connection as a framework for cosmological model building. The redshift - distance relation of galaxies (Hubble's Law) will in this context, for example, not be attributed to a global expansion of the universe during the course of its evolution but to a property of a Weyl geometry in a particular gauge (called the Hubble gauge) i.e. to the gauging of the unit of length in a particular (i.e. integrable) Weyl geometry. This may lead to a description of the universe without a singularity at the beginning. Several other topics in cosmology are also investigated within the proposed Weyl geometrical approach and found to be in agreement with observational data. The paper will probably trigger new discussions in the field of cosmology. It views various cosmological problems (anisotropy of the CMB, vacuum energy, anomalies of the standard approach etc. etc.) from a different and promising angle. The paper is well written and certainly merits publication in 'Foundations of Physics'. There are some minor points which I would suggest to the author to change. 1) The dramatic warning on p. 60 (Appendix I) and on p. 9 and p.12 in the text is, I think, not really justified: whether one consistently uses the Weyl vector field as in this paper (with a minus sign in the second equation of (50)), or with the opposite sign for these fields, is purely a matter of convention even if an "anti-intuitive sign convention" results in subsequent formulae. 2) Certain references involving citations from archives are not complete, i.e. contain some small "v" 's. (Compare, for example, refs. 8, 16 etc.) Very often there also appears a date, i.e. "visited November 10, 2003" etc., which, I think, is rather unusual and should be suppressed in the published version of the paper.

———————- * ———————-

Author's comments on second referee's report

This is a very positive report, of course I agree with it.

With respect to proposed changes: 1) I can easily de-dramatize the "warning", which the referee thinks much too strong. I would not like to delete it, as

---

structure of matter distribution in the universe are closely related. Then the number of independent parameters of observational data modelled by the inflationary model is even less than assumed at the moment. The balance between free model parameters and independent data parameters may well turn out to be neutral or even *negative*.

the sign change has already led to misunderstandings of the geometrical meaning of scale gauge transformations in the litaratur. 2) Thank you. I accept the advice.

Referee report 3

[See appendix "Referee report of FP-060112-Scholz" with separate pagination, 1–5]

— * —

Author's comments on third referee's report

This report reads quite surprising to me. Its main thrive seems to be that I have not written the paper the referee would have expected, if he had used a Weyl geometric approach himself. But why should I — surely it would be his job. I shortly react to his points of objection.

ad **1** : The referee seems to "misconstrue" (to take up a formulation of his own) the central argument of the paper. In his view its content consists of presenting a mere "shift" of the scalar curvature by a Weyl geometric modification of classical cosmological models. A mere "shift" of curvature values would, however, be a meaningless enterprise, if it were not related to a change of the model class which has potentially physical meaning. It would deserve at most one page, not half the number of pages I used.

ad **2** : I take the detailed criticism with respect to an "incorrect description of work by C. Castro" very serious, although my intention has not been to give a "description" of C. Castro's paper. Surely that would be a too difficult task for me. I was struck by the observation that C.C.'s *result* in the calculation of vacuum energy density, fits completely well with the model parameters obtained from the Weyl geometric approach, if one considers the upper and lower cut-off lengths scales introduced in his argument on face value (theory "positivistically", if one wants to criticize my argument). I am still struck by this agreement. Perhaps I should cancel the reference to Castro's work from the paper's abstract and deemphasize it in the discussion of section 5. I would not like to cancel it completely, even though the good agreement may turn out to be mere coincidence.

ad **3** : Here the referee starts complaining that I do not write *his* paper. The articles of E. Santamato mentioned are beautiful and imaginative explorations of an attempt for a fruitful usage of integrable Weyl geometry for a semi-classical geometric representation of relativistic quantum theory of spin zero particles by a stochastic ensemble of path trajectories. Santamato's proof that Weyl's scale connection can be used for this enterprise may very well indicate that there is more symbolic potentiality in this research direction. But nobody can say so at the moment (as far as I can judge). So it is expected a bit too much that I base my use of Weyl geometry on the principles of this research program.

ad **4** and **5**: It is well known since the 1920s that the usage of Weyl invariant actions quadratic in the curvature lead to a strict modification of the Einstein equations. There are good reasons not to expect too much from such an approach (although we never can be quite sure). While the referee seems to want me following this trajectory in the first half of this point, he makes another prescription to continue with Brans-Dicke theories of gravitation in his next paragraph. He argues that "one must use" the respective Brans-Dicke action, because otherwise one finds a "time dependent Newtonian 'constant', which is unphysical " (his point **5**). The choice of the Brans- Dicke action is not the only possibility, however. In his last clause the referee hits upon the reason why the defining property for the gauge which indicates the results of measuring devices directly (called *matter gauge* in the paper) has been chosen such that the Newton constant is constant over cosmic time. This choice has been explicitly stated, and its reason has been given. The referee's calculations on bottom of p. 3 and top of p. 4 show that he has started to convince himself of the fact that such a choice is consistent. In the last two paragraphs of point **5** he jumps back again, to the erroneous claim that "Hubble gauge" and "matter gauge" in the mentioned sense cannot agree. The models presented in the paper prove the contrary. They explore the situation in which Hubble gauge is equal to matter gauge. Maybe the referee was too rash in his early conviction that the whole Weyl universe class just consists in "shifting the curvature" parameters (point **1**) and gave up the attempt to follow the argument?

ad **6** and **8** : It is *not true* that "the author never addessed the crucial question" whether the expansion of the universe is a gauge artefact or a physically real expansion. The whole discussion of Weyl universes deals with a model constellation which surely would indicate that the "expansion" is a pure gauge effect, *if it should turn out to be empirically superior* to the Friedmann-Lemaitre approach. This is stated clearly in sections 4 and 7. But in the Weyl universe approach this is *not due* to a temporal variation of the constants, but to what is being described as a "neostatic" equilibrium of physical space-time geometry plus a cause of cosmological redshift in a second order (quantum) interaction of photons with the comic "ether". The latter term has been used in the paper purely descriptively as the "combined system of gravitational and electromagnetic fields" plus the quantum vacuum. True, I do not claim to be able to explain "what the ether is made of" (point **8** of the objections). If I were, I had solved one of the main riddles of present (and future?) basic physics. I never claimed so.

Moreover, the Pioneer anomaly is referred to in the paper only as a case for comparison of orders of magnitude of corrections: The result is that the cosmological corrections for low velocity orbits of both, the Weyl geometric and the standard approach, are 4 orders of magnitude *below* the anomalous acceleration measured in the Pioneer experiments. This is used as a criterion for comparing the changes to be expected by the Weyl geometric modification with the presently highest precision measurements of low velocity orbits. No claim for "explanation" of the Pioneer anomaly has been made in the paper. The referee again wants to have a completely different paper, probably the one published already and quoted by him in the passage.

ad **7**: After all these objections that I have not done what the referee or C.

Castro have achieved (which is admirable, but truely a different thing), the referee comes to his “strongest” argument. The most “serious error” he finds is to treat the Ricci tensor and the metric tensor on the same footing, i.e. as symmetric differential two-forms. The referee accepts this characterization for the Ricci tensor, but rejects it for the metrical tensor. He might like to have a look at a very old paper: B. Riemann: “Über die Hypthesen, welche der Geometrie zu Grunde liegen” .... Sorry that I start joking here. The referee was probably so excited about the “provocation” for not reading a mirror of his own papers that he seems to have forgotten some basics of differential geometry.

Thanks, on the other hand, for the correction of p. 33. This and the references to the beautiful papers of E. Santamoto have been the most important hints of this report to me.

Wuppertal, Feb. 10, 2006, E.S.

Referee report 4

Title: On the geometry of cosmological model building
Author: Erhard Scholz
Journla: Foundations of Physics
Editor: Alwyn van der Merwe
Date: February 4th, 2005 (sic! meant 2006, E.S.)

General remarks

The paper investigates the possibility of accomodating current cosmological data within a special subclass (the integrable ones) of Weyl geometries. The motivation comes from various difficulties that modern cosmology faces and that, in the standard canon, are turned positive as indication of ‘new physics’ (e.g. the existence of exotic dark matter, the cosmological constant etc.). In contrast, the present author sees them as “anomalies” and “testimonies of broad empirical evidence” against the hypothesis of primordial synthesis. While in my opinion it is certainly desirable to scrutinize the hypotheses on which modern cosmology rests, I at the same time feel that the critique offered here is too harsh and partially unjustified.

The paper gives an introduction to intgrable Weyl geometry in cosmology. A crucial part is the assignment of physical meaning to various gauges, which is rather ad hoc. In particuar, the cosmological redshift is now due to a non-trivial length transport. To me the justification for these assignments seems to be a rather weak part of the theory, reflecting the fact that Weyl’s generalization of (semi-) Riemannian geometry does not rest on a physical principle (comparable e.g. to the equivalence principle) that would determine the couplings to matter in a universal and (almost) unique fashion.

Also, there is a rather contrived way (the author calls it ‘artifice’) in which the classical Einstein equations are rendered gauge covariant and hence possible in the context of integrable Weyl geometry. The author introduces a new ‘coefficient’ β into the Einstein-Hilbert Lagrangian, which does not seem to be considered as a dynamical entity, though it does have a non-trivial scale weight

(-2) and hence should be properly thought of as a field. If this is correct, i.e. if β is a non-dynamical background field, then β manifestly breaks diffeomorphism invariance, which seems unacceptable. There is no discussion offered by the author about this point, or how to understand β otherwise.

Suggestions

I think the question of whether modern cosmological data could be described in the alternative framework of Weyl geometry is an interesting one and deserves to be looked at, even if the present developments in cosmology are judged as a success for the standard model. In particular, there is no need to discredit the standard cosmological model for reasons which, in parts at least, seems dubious to me. Statements of the sort that "it may turn out that the whole anisotropy signal is due to such ' foreground' effects" without giving references and/or further explanations are not helpful in that respect.

I suggest a mor modest discussion in which the general theoretical point is put forward with great care and which also addresses the difficulties of the Weyl's approach in the same critical spirit as it scrutinizes the standard model. I suggest that the author revises his manuscript along these general lines and resubmit the paper. In the present form I do not recommend it for publication.

——————- * ——————-

Author's comments on fourth referee's report

The report poses two basic theoretical questions and criticizes the style of the paper.

(1) Theoretical point: The Weyl geometric generalization of Riemannian geometry "does not rest on a physical principle (comparable e.g. to the equivalence principle) that would determine the coupling to matter in a universal and (almost) unique fashion."

It is true that the proposed Weyl geometric (weak) extension of GRT does not determine the coupling to matter "in a universal and (almost) unique fashion". (In fact I even would not dare to propose an extension with such a claim for universal determination of basic matter structures.) But there is a weaker kind of equivalence which, in my view, gives a strong motivation for the Weyl geometric generalization (here restricted to the context of cosmological model building). An assumed "true" (i.e. physical) *expansion* of spatial sections in Robertson-Walker manifolds and a *scale gauge* between different values of the cosmic time parameter are *observationally equivalent*. (Integrable) Weyl geometry is beautifully adapted to represent this equivalence. In this sense the proposed generalization of geometry generalizes Einstein's (much stronger) equivalence principle (section 3, p. 13f. of the submitted manuscript). At the moment I cannot give more. If empirical evidence speaks in favor of the IWG approach to cosmology there will be reasons to work towards a deeper, dynamical understanding of the equivalence.

(2) Theoretical point: The scale covariant factor β of the Hilbert-Einstein ac-

tion " ...does have a non-trivial scale weight (-2) and hence should properly be thought of as a field. If this is correct, i.e. if β is a non-dynamical background field, then γ manifestly breaks diffeomorphism invariance, which seems unacceptable."

The objection of a presumed "manifest break of diffeomorphism" seems to rely on a misunderstanding of the difference between Weyl geometric structure and classical relativity. Let us consider a Weylian manifold $(M, [g, \varphi])$ with an underlying differentiable manifold M and a Weylian metric $[g, \varphi]$ given by an equivalence class of pairs of a (semi-)Riemannian metric g and a scale connection φ (with equivalence relation given by the corresponding gauge transformations) and a diffeomorphism

$$f : \tilde{M} \longrightarrow M \; .$$

Any of the gauges may be drawn back; then it gives the metric $\tilde{g} := f_* g \, , \tilde{\varphi} := f_* \varphi$ on $\tilde{M}$. Because of compatibility with gauge change, this defines an isomorphism of gauged Weylian manifolds

$$(\tilde{M}, \tilde{g}, \tilde{\varphi}) \stackrel{f}{\longrightarrow} (M, g, \varphi) \; .$$

Any scale covariant (scalar, vector or tensor) field A of weight α on M can be drawn back, $\tilde{A} := f_* A$, and leads to a scale covariant field on $\tilde{M}$ (of the same weight). For the scalar covariant field β the drawback is particularly simple. It is just the composition

$$\tilde{\beta} = \beta \circ f \; .$$

If (g, φ) is the gauge in which β is constant ("matter gauge" in the paper), also $\tilde{\beta}$ is constant. Thus the gauge $(\tilde{g}, \tilde{\varphi})$ drawn back to $\tilde{M}$ is the corresponding "matter gauge". Specification of the matter gauge is part of the physical structure (in classical relativity it is Riemann-Einstein gauge *by default*). Arguments involving diffeomeorphism invariance work with the drawback of metric structure also in the case of classical GRT. How can the referee conclude that " β manifestly breaks diffeomorphism invariance"?

(3) Style: The referee criticizes that there is "no need to discredit the standard cosmological model" and proposes a "more modest discussion". I accept the point: Modesty is completely appropriate and I ought to go through the paper checking for formulations which read to self-assuring for the Weyl geometric approach. Of course I can state clearer that it is *my opinion* when I expect that it still "may turn out that the whole anisotropy signal" may be due to "foreground' effects" (criticized on p. 2 of the report). Or that it is *my view* to wonder why there is no broader discussion about the role of the hypothesis of primordial nucleosynthesis in the conclusion of the existence of exotic dark matter (p.1). A complete deletion of such critical remarks would, on the other hand, obscure the reasons why it seems justified to consider approaches like the Weyl geometric one.

Perhaps it is in the sense of the referee to include a critical (in both directions) discussion of evidence which seems to speak against the Weyl geometric approach. This could be the empirical evidence for "reionization" close to

$z \approx 6$, brought into the discussion by another referee (the first one), metallicity observations, and/or the available data on star formation rates over z. The paper will get some pages longer, without coming to definitive conclusions in one or the other direction. But it may contribute to a more reflective overall view with respect to the new model, demanded by the referee.

Wuppertal, Feb. 13, 2006, E.S.

Referee Report of FP-060112-Scholz

" On the Geometry of Cosmological Model Building " by Erhard Scholz

I deeply regret to inform the author that I cannot recommend this manuscript for Foundations of Physics. Hereby in these 5 pages I explain why I cannot support this manuscript and provide some suggestions for the author to improve his work

1 *Size of the paper could have been reduced by half*

All in all the author is saying the following : Given

$$R_{Weyl} = R_{Riemann} - 6\varphi_\mu \varphi^\mu + 6\nabla_\mu \varphi^\mu$$

in the very special case when $H = H_0 = constant$ and when the Hubble gauge is chosen $\varphi_\mu = (H, 0, 0, 0)$, it leads to $R_{Weyl} \sim \kappa + H^2$. Where $\kappa = k/(Radius\ of\ Curvature)^2$ (the definition of κ was never given, the author assumed that the reader knows this definition).

Because the Weyl scalar curvature R_{Weyl} is basically equal to the Riemannian scalar curvature R that is *shifted* by a factor of H^2, this fact can be translated as a mere *shift* in the values of the Ω_Λ parameter, from 0.75 to $1.25, 1.23, ..., 1.20$ described in the Table 1 of page-[45]. This is the crux of the paper which could have been explained in a few pages instead of 78 pages.

However, there are many problems with the paper :

2 *Incorrect description of the work by C. Castro*

First of all, BF does *not* stand for Background-Field as indicated by the author in the text. B and F are antisymmetric tensor fields related to what it has been coined as Topological BF theories.

Secondly, the description of the article C. Castro, Mod. Phys. Letts A 17, (2002) 2095 "Anti de Sitter Spaces from a BF-Chern-Simons-Higs Theory" was not properly made by the author. It is unfortunate that the author misconstrued the work of C. Castro.

It was shown by C. Castro why the MacDowell-Mansouri-Chamseddine-West formulation of Gravity, with a cosmological constant and a topological Gauss-Bonnet invariant term, can be obtained from an action inspired from a Topological BF-Chern-Simons-Higgs theory based on the conformal $SO(3,2)$ group. The Higgs field was essential and breaks the $SO(3,2)$ symmetry down to the $SO(3,1)$ Lorentz symmetry. .

The 4D anti de Sitter space AdS_4 space is a natural vacuum of the theory. The vacuum energy density was *derived* from first principles (and *not* postulated as argued by the author) to be precisely the geometric-mean between the Planck scale L_P and the throat size of Anti de Sitter space . Since the throat size of Anti de Sitter space coincides also with the throat size of de Sitter Space, upon setting the throat size to agree with the future horizon scale (of an accelerated de Sitter Universe) given by the Hubble scale (today) R_H, the geometric mean relationship yields the observed value of the vacuum energy density :

$$\rho_{vacuum} \sim (L_P)^{-2}(R_H)^{-2} = (L_P)^{-4}(\frac{L_P}{R_H})^2 \sim 10^{-120}(Planck\ Mass)^4 \sim$$

$$(electron\ neutrino\ mass)^4$$

Many people have *speculated* about this geometric-mean relationship but C. Castro *derived* it from first principles. He did not postulated it as the author erroneously argued.

Because de Sitter and Anti de Sitter spaces are conformally flat spaces (the Weyl tensor vanishes) their metrics belong to the same conformal *class* as the flat space metric, hence it is not surprising that the author has found results compatible with C. Castro's results, despite the fact that the author's starting action and procedure was incorrect (see why in the arguments below).

3 *The origins behind the Weyl Integrability Conditions*

The equations governing both the behaviour of the Weyl gauge field of dilatations φ_μ and the metric $g_{\mu\nu}$ must be derived *both* from an initial action. Namely, the integrability condition $f_{\mu\nu} = \partial_\nu\varphi_\mu - \partial_\mu\varphi_\nu = 0$ is required so that different clocks will tick at the same rate (upon arrival at the same point) irrespective of their path history. However, this condition should be obtained *directly* as a result of the equations of motion of the action w.r.t the Weyl field φ_μ, rather than imposing the Weyl integrability constraint by hand. E. Santamato was able to derive the condition $f_{\mu\nu} = \partial_\nu\varphi_\mu - \partial_\mu\varphi_\nu = 0$ directly from an action. see the articles E. Santamato, Phys. Rev **D** , vol. 29 (1984) 216; and vol. 32 (1985) 2615.

Let us forgo for the moment this subtlety and focus on other more serious problems. Let us assume that we agree for the moment to accept the Weyl integrability criteria without having to derive it from an action (as it was done by E. Santamato) . We still have some serious problems.

4 *The crucial starting action of page-[13] and all the ensuing equations of motion throughout the whole paper are not the correct ones to use* .

The equations of motion yield a conformal class of metrics (a family of conformally related metrics) due to the Weyl symmetry. Typical Weyl invariant actions that are *quadratic* in the curvature are for example

$$S = \int d^4x\ \sqrt{-g}\ [\ aR^{Weyl}_{\mu\nu}\ R^{\mu\nu}_{Weyl}\ +\ b(R_{Weyl})^2]$$

where the Weyl weights are :

$$w(R_{Weyl}) = -2; \quad w(R_{\mu\nu}R^{\mu\nu}) = -4; \quad w(\sqrt{-g}) = +4; ...$$

such that the action has overall zero Weyl weight.

For quadratic curvature actions involving the Lanczos-Lovelock Lagrangians (whose equations of motion for the metric are at most of second order) see for instance M. H. Dehghani. " Accelerated Expansion of the Universe in Gauss-Bonnet Gravity" hep-th/0404118

If one wishes to use actions that are *linear* in the scalar curvature, one must use the Weyl invariant action associated with the Jordan-Brans-Dicke theory (with the inclusion of matter and Maxwell EM fields)

$$S = \int d^4x \sqrt{-g} \, [-\phi^2 R_{Weyl} + g^{\mu\nu}(D_\mu\phi)(D_\nu\phi) - V(\phi) + L_{matter} + L_{Maxwell}].$$

where the Weyl weight of $\phi = -1$ so there exists the gauge choice

$$\phi^2 = \frac{1}{16\pi G} = constant. \quad \varphi_\mu = 0. \quad R_{Weyl} = R_{Riemann}$$

which will reproduce the Einstein-Hilbert action (plus matter terms...) and where the effect of the cosmological constant is encoded in the potential term $V(\phi = (16\pi G)^{-1/2})$. See the arguments below explaining why one can choose such gauge conditions.

5 *Why one must choose the Jordan-Brans-Dicke action*

If one does not use the Jordan-Brans-Dicke action but instead we use the action proposed by the author

$$S = -\frac{1}{16\pi G} \int d^4x \sqrt{-g} \, R_{Weyl}$$

by *promoting* the Newtonian "constant" G to be a scalar-like-parameter that behaves under Weyl scale transformations as $G \rightarrow G' = e^{2\xi} G$, one will then encounter a *time dependent* Newtonian "constant" which is unphysical.

The integrability condition

$$f_{\mu\nu} = \partial_\nu \varphi_\mu - \partial_\mu \varphi_\nu = 0$$

means that *locally* the Weyl field φ_μ is a total derivative $\varphi_\mu = \partial_\mu \xi$; i.e. the field φ_μ is *pure gauge* and, consequently, it is *devoid* of dynamical degrees of freedom. This is not necessarily true *globally* since there might be topological obstructions in writing φ_μ as a total derivative. Lets set aside topological obstructions at this moment since we know that Einstein's equations do not determine the global topology of the Universe.

Since φ_μ is a total derivative one can always find a gauge such that

$$\varphi'_\mu = 0 \quad and \quad G' = G_0 = G_N = The\ observed\ Newtonian\ constant$$

such gauge choice can be attained after performing a gauge transformation from the Hubble gauge $\varphi_\mu = (H, 0, 0, 0)$ to the new gauge $\varphi'_\mu = 0$ given by

$$\varphi'_\mu = \varphi_\mu - \partial_\mu \xi = 0 \Rightarrow \varphi_\mu = \partial_\mu \xi = (\frac{d\xi}{dt}, 0, 0, 0) = (H, 0, 0, 0)$$

and

$$G' = G_0 = e^{2\xi}\ G\ \Rightarrow G = e^{-2\xi}\ G_0; \quad where \quad \xi = \int H(t)dt$$

Such gauge transformation is indeed consistent with the meaning of a "constant" in Weyl space, given by $D_\mu G = 0$, when the Weyl weight of G is $w(G) = 2$, so that the vanishing Weyl covariant derivative of G reads

$$D_\mu G = (\partial_\mu + w(G)\varphi_\mu)\ G = (\partial_\mu + 2\varphi_\mu)G = 0 \Rightarrow \varphi_\mu = -\frac{1}{2}\partial_\mu lnG$$

upon setting $G = e^{-2\xi}G_0$ into the last equation, it yields once again (as expected) the pure gauge condition $\varphi_\mu = \partial_\mu\xi$ which naturally obeys the integrability condition $f_{\mu\nu} = \partial_\nu\varphi_\mu - \partial_\mu\varphi_\nu = 0$

To conclude, the Hubble gauge choice $\varphi_\mu = (H, 0, 0, 0)$ is intricately associated with a *time dependent* Newtonian "constant" G given by

$$G = e^{-2\xi(t)}G_0 = e^{-2\int H(t)dt}G_0$$

A variable Newtonian "constant" will *mess* up all of the results in this paper.

This *is* the reason why one must use the Jordan-Brans-Dicke action and/ or actions that are *quadratic* in the Weyl curvature. One may use actions involving Lagrangians of the type $F[\sqrt{-g}\ \phi^2 R_{Weyl}]$ where F is an arbitrary scalar function of the Weyl invariant combination $\sqrt{-g}\ \phi^2 R_{Weyl}$ of overall zero Weyl weight. These latter type of actions are becoming very popular today.

Concluding, the author should start with a Weyl invariant Jordan-Brans-Dicke action and study the equations of motion for the both the fields $g_{\mu\nu}$ and the Jordan-Brans-Dicke field ϕ, as well as the equations of motion for the matter and Maxwell fields. This would be the correct procedure to follow. More general, the author should include actions containing *quadratic* curvature terms as well and involving true dynamical degrees of freedom of the Weyl field φ_μ.

6 *Is the expansion of the Universe real or a gauge artifact due to Weyl invariance ?*

Related to the issues behind a time-variation of the Newtonian "constant" (and other physical constants) the author never addressed the crucial question :

Is the expansion of the Universe *a gauge artifact* resuting from the Weyl scaling symmetry or is it a truly physical fact ? Perhaps the Universe is not really expanding but it is an illusion resulting from the temporal variation of the fundamental constants ??? For example, the definition of the Hubble parameter in terms of the scaling factor $a(t)$ of the Friedmann-Roberson-Walker models is

$$H = \frac{dln\ a}{dt} = \frac{d\xi}{dt} \Rightarrow a = a_0\ e^{\xi(t)} = a_0\ e^{\int H(t)dt}$$

and it yields a *constraint* between the temporal evolution of the Newtonian "constant" and the scale size of the FRW universe

$$\frac{G}{G_0} = (\frac{a_0}{a(t)})^2$$

For recent references dealing precisely with the Conformal transformations, where the *dilaton* field is identified with the conformal scaling Weyl parameter $\xi = \xi(x^\mu)$, and a Weyl-scaled Gravity, dark energy and dilaton Cosmology see

H.Q. Lu, Z.G. Huang, W. fang and K.F. Zhang, " Dark Energy and Dilaton Cosmology" hep-th/0409309.

J. L. Crooks and P. H. Frampton, " Conformal Transformations and Accelerated Cosmologies" astro-phy/0601051.

7 *There are Serious Errors in the Text*

At the bottom of pages-[35, 36] there are *serious* errors. The curvature is a 2-form, a differential 2-form, therefore it is *incorrect* to write the Ricci tensor as if it were a metric tensor !

There are minor errors. For example, at the bottom of page-[33] t has to be replaced by τ.

All Equations should be *numbered*. There are numerous equations without a number.

The reasons behind the definitions of "ex-ante", "hyle" should be given in the text.

8 *About the Pioneer Anomaly and Energy Loss of Photons*

The entire section devoted to the Pioneer Anomaly and Tired Light Effect, ... should be the subject of entire different papers. For a thorough explanation of the Pioneer Anomaly based on the accelerated expansion of the Universe see the papers by P. Rosales in the arXiv.org and for references on the Pioneer Anomaly that are related to the Rindler-Unruh-Hawking thermal radiation of photons resulting from the effects of acceleration in QFT (and an accelerated Universe) see C. Castro, Progress in Physics vol.1 (2005) 20-30. Int. J. Mod. Phys. **A 18** (2003) 5445. It is desirable to establish the connection between Irving Segal's work and the Rindler-Unruh-Hawking effect.

There is a great controversy about the existence of the "cosmic ether " and whether or not it can be modelled in terms of the quantum spacetime foam, the fractality of spacetime, self-referential noise, D-branes, ... that I am not going to rekindle. This could be the subject of an entire Conference. However, the author did not explain how the Weyl scaling symmetry principle of calibration of scales will help us to understand *what* is the cosmic ether made of, assuming that it *exists* in the first place.

Finally, pertaining the anisotropy of spacetime, the author may benefit from the textbooks on Weyl-Finsler Geometry, etc...