Report on the Ph.D. thesis entitled "Aspects of Lorentz and CPT Violation in Cosmology" by Nils Albin Nilsson (M. Sc.)

The topic faced by the candidate is very interesting: the problems to unify General Relativity and Quantum Gravity are very well known, as well as the humongous multitude of paths to follow to achieve this goal. One of the main flaws with most of the proposals is their testability. This thesis focus on a well defined issue and group of theories based on the violation of Lorentz invariance, one of the main cornerstones of modern Physics, but also one of the most active and promising fields in which to search for hints to an effective Quantum Gravity theory.

The implications of a detected and detectable departure from such an invariance would be of capital importance, and any study in this direction is not only necessary but obligatory to more deeply understand and tune our understanding of Nature. Given the very high precision and the extreme scales required to perform experiments which could confirm or confute such scenarios, the quest is highly difficult. For that, most of the work presented here is based on a phenomenological approach. Nevertheless, any possible result, even negative, is necessary to advance. I have no doubt that the topic is one of the main hot topics in Cosmology (and beyond it) in the very next future, and the candidate seem to be able to participate in the debate with nice results.

The thesis is 127 pages long and consists of: a short Introduction, introducing the aims and scopes of the thesis; Chapter 2, which presents the state-of-the art and the scientific background of the main topic; four main Chapters showing the main results of his research activity; a conclusion section; and an appendix where observational data are presented. The bibliography is very rich and contains 247 entries.

It is based on three journal papers and one conference proceedings, in which the candidate was co-author together with his Ph.D. supervisor, prof. dr. hab. Mariusz P. Dabrowski, with Ewa Czuchry, and with Kellie O'Neal-Ault and Quentin G. Bailey both from Embry-Riddle Aeronautical University (USA). Most of the papers are relatively new and so perhaps not yet cited as they would deserve, with a total number of citations of 12 and corresponding Hirsch index $h = 2$ (data from INSPIRE).

For Chapter 3, *"Energy Scale of Lorentz Violation in Rainbow Gravity*, Phys. Dark Univ. 18 (2017); for Chapter 4, *Horava-Lifshitz cosmology in light of new data"*, Phys. Dark Univ. 23 (2019); for Chapter 5, *"Preferred-frame Effects, the H_0 Tension, and Probes of Horava-Lifshitz Gravity"*, Eur. Phys. J. Plus 135 (2020), which I see in the meantime has been accepted and published, but the candidate refers also to a paper which is not available because in preparation, ref. [231]; for Chapter 6, *"A 3+1 Decomposition of the Minimal Standard-Model Extension Gravitational Sector"*, presented at the 8th Meeting on CPT and Lorentz Symmetry (CPT19) Bloomington, Indiana, USA, May 12-16, 2019. All the journals are well known and high profile in the field. Other three works seems to be in progress, as stated by the candidate. All the works were published in the period [2017, 2020].

In the following, I will explicitly discuss some *major criticisms*, which are needed to be addressed by the candidate in order to cut down on some doubts, and will provide a list of *minor comments.*

1 Major comments:

1. pag. 10: the list of probes confirming the accelerated expansion of our Universe is a bit short and not exhaustive. The cited ref. [45] focuses more on forecast; *Planck* results are missing, as well as a more exhaustive list of BAO observations. And it must be stressed that the seminal work from Perlmutter et al. (1999), which opened the road to dark energy together with Riess et al. (1998) (here cited as ref. [44]) is missing;

2. pagg. 30 - 34: interpretation of the main results from Chapter 2. I have not clearly understood which parameters have been used to fit the data. This is not stated clearly anywhere in the text and it is important to understand how the candidate has obtained constraints on the limiting energy E reported in Table 3.1. Comparing the Friedmann equations Eqs. 3.14 and 3.17, the Lorentz-violating model considered here is totally indistinguishable from a standard ΛCDM. Unfortunately, a table summarizing constraints on all parameters is missing. Moreover, the parameters which we would measure *directly* are those called Ω'_X here. Thus, how the constraint shown in Table 3.1 have been obtained: using the left side of Eq. 3.20, or the ratios of Eqs. 3.18? Maybe the results would be the same, but I would have a confirmation by the candidate;

3. pagg. 30 - 34: in order to obtain all the constraints, the candidate had to provide an analytical form to the functions h and f, which are *suggested* in literature. Why there is no estimation from f and only from h? How much general are the obtained results?

4. on pagg. 45 and 48: in Table 4.1, could the candidate clearly state which are the *primary* fitting parameters and which ones are *derived*? For the case of detailed balance, by looking at Eqs. 4.18 and 4.19, they could be $\{\Omega_m, H_0, \Omega_k\}$, being Ω_r function of H_0, and expressing, for example, ΔN_ν in terms of remaining parameters. In light of this comment, I do not understand the statement on pag. 45 for which the parameters Ω_k and ΔN_ν *"have been left as free parameters in our analysis"*. A similar clarification for the case of beyond detailed balance would be appreciated;

5. on pag. 53: the statement *"the errors on ω_3 and ω_4 are very large, so only order is relevant here"* is disputable. One cannot *simply* disregard a parameter just because of its distribution or because of large errors. In fact, the large errors might be telling that these parameters have a negligible role on the fit, that they do not contribute enough to have cosmological evidence, and thus the model is statistically weak. Thus, my question is: what is the relative contribution of ω_3

w.r.t. Ω_r, and of ω_4 w.r.t. ω_1? Are they really not too much relevant?

6. Referring to both Chapters 3 and 4: the candidate has run some MCMC and compared results with ΛCDM. But I found no mention about convergence tests of the MCMC, which would assure about the goodness of the obtained statistical inference. Has the convergence been checked? Moreover, a proper comparison with ΛCDM could be provided only by the appropriate statistical probes, like Bayesian Evidence. In such a way, the very general and qualitative statements which the author provides in the text would have a solid quantitative ground. Have these statistical tests been used?

7. on pag. 58: what is the most correct definition of *local frame*? Although the candidate states that the redshift range up to $z \approx 8$ is local when compared to CMB redshift ($z \sim 1100$), this range is much wider than $[0.0233, 0.15]$, which is used to derive the *local determination of the Hubble constant by Cepheids and SNeIa*, which is at the base of the Hubble tension;

2 Minor comments:

1. there are various typos scattered through the text, just to point out some of them: *summetries; renormalisability; neccessary; so fas; cosmologcal; loverlooking;*

2. the nomenclature is not always uniform (actually, the provided list of abbreviations is not fully exploited) e.g., we have (capital) *General Relativity* vs *general relativity*; Ω_m^0 vs Ω_m; Ω_k^0 vs Ω_k; and other similar cases;

3. in some cases nomenclature style is "wrong": for example "Planck", when referring to the ESA satellite, should be written in italic, because it is convention in the astronomical community to use this type of font for space-based missions; Type Ia Supernovae is not *SN1a*, but SNeIa;

4. pag. 12: in Eq. 2.5, if the convention $c = 1$ is being used, it should have been stated clearly. Moreover, I suppose that ϕ is the gravitational potential, but that is not stated clearly;

5. pag. 37: about the constraints on GW speed: I can see a Δt, which makes me think about a time difference, although it should be a velocity difference. If one would like to include Δt, then the expression should be written in a different way, see Eq. 1 in ref. [108];

6. pag. 43: I guess that the correspondence between signs of the curvature and the type of geometry is inverted;

7. pag. 43: *"in one of our previous papers"* this might be a refuse from the printed article, as "our" in the context of this thesis, might be misleading because it does not refer to a work of the candidate;

8. pag. 43: continuity equation for radiation has suffix "m";

9. pag. 78: Eq. A.1 should be more correctly referred to as the transverse comoving distance;
10. pag. 79: within the thesis, it has been claimed that also the Pantheon SNeIa has been used, but this is not described in the related section of the Appendix, and this should be done, because there are some differences w.r.t. JLA;
11. pag. 80: Eq. A.12, there is a c (speed of light) missing in the equation;

The thesis looks very professional, is well-written, but maybe it would have helped to *have more details in some (many) technical and mathematical issues which underlie the* theoretical background. On the same level, as most of the thesis deals with constraining Lorentz invariant models and with comparing them to cosmological data, it would have helped to have an even short but more explicative description of the state of the art of the present observational tests. A concrete section would have surely helped also to state more clearly the newest and most important achievements accomplished by the candidate, and on which this thesis is based, which are anyway important.

But these are just formal requirements; concerning what matters, i.e. the content, in my opinion the thesis fulfills all the necessary requirements to be presented for the doctoral degree so that I recommend the admittance of Mr. Nilsson for the defence.

Szczecin, 09.07.2020
dr hab. Vincenzo Salzano, prof U.S.
Institute of Physics, University of Szczecin