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Report on the doctoral thesis "Study of dark energy and modified gravity and their influence on the cosmological parameters of the universe" by Michal Vraštil

The nature of two fundamental components of the standard cosmological model, dark matter and dark energy, remains elusive. While the former might be explained by a yet undiscovered WIMP and the latter purely by the cosmological constant, alternative explanations have been proposed in the form of modified gravity theories. The two main observational tests for such theories on the cosmological scale concentrate on their influence on the spectrum of the cosmic microwave background anisotropies and on the density perturbation power spectrum.

In the presented thesis the author concentrates on the latter, simulating large-scale structure formation in chameleon gravity. Current state-of-the-art cosmological simulations are based on a combination of N-body simulations and hydrodynamics, both methods placing huge demands on supercomputer time. The goal of the thesis was to test numerical approximation schemes that enable evolving density perturbations beyond the linear regime, in order to provide a less demanding technique for computing the predictions of modified gravity theories.

Skipping to the main conclusion, the approximation schemes may be used to obtain results at spatial scales down to the baryon acoustic oscillation (BAO) scale, but not further down closer to galaxy-cluster scale. The main original results were published in an article in MNRAS (Vraštil & Habib 2020). Clearly, the extent of research and numerical simulations behind this article is much larger than indicated by the presented selected results. In spite of the more modest scale and resolution used in the study, developing practically single-handedly the N-body simulation code against which the approximations were tested is an impressive feat.

The first four chapters of the thesis provide a general background on cosmological evolution, modified gravity theories, cosmological simulations and approximation schemes. As indicated by the generated Turnitin report, large parts of the text of the first two chapters are copied verbatim from the author's Master's thesis, to an extent that is more than just eyebrow-raising, as discussed further below in "Other comments". In the rest of the report I provide specific

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comments on the original results appearing in Chapter 5, followed by other comments on the thesis, and questions for the defense.

Science comments:

In Section 5.2.1 and Fig.5.8 the range of validity of the pseudo-linear approximation is taken as the point where its power spectrum crosses the nonlinear power spectrum. However, even at lower k the pseudo-linear approximation cannot be considered as valid, since it strongly overpredicts the power spectrum (by up to a factor of 2). Similarly, Fig.5.8 indicates that for k ~ 1 h Mpc^{-1} the approximations do not appear to be reasonable, contrary to the discussion on p.103.

I could not find information on the initial power spectrum at z=200 used for the simulations (although one may guess what is usually used). Nevertheless, the BAO peak discussed in detail in Sections 5.1.3 and 5.2.3 appears in the simulations via the input initial power spectrum, and its location is primarily given by linear evolution in ΛCDM.

Other information that I missed includes the numerical simulation parameter combinations used for the results shown in Chapter 5.1, the redshift for the results in Fig.5.15, or results for z=0.5 in Fig.5.2 for better comparison with Fig.5.5. There is no comment on the PM (particle-mesh?) results that do not appear in the article but do appear in Fig.5.1 (where they seem to trace the nonlinear results perfectly) and in Fig.5.5 (where they exhibit what looks like artifact oscillations). It would have been interesting to see the PM results compared with the approximations in Fig.5.6. Similarly, there is no comparison of the computational time demands of the different methods, which would appear to be important given the goal of speeding up the simulations.

The presented figures are mostly adequate and well selected. I have comments only on a few specifics. In Fig.5.7 the differences between the curves are too large for the relative plots in the bottom four panels to be of much use (showing blown-up artifacts etc.). The vertical axis range in Fig.5.10 does not show results with power spectrum lower than the FPA or FFA approximations. Similarly, Fig.5.11 does not show results with power spectrum larger than the pseudolinear prediction and its horizontal range is much reduced, so that it is hard to correlate with Fig.5.10 above or other Figures. Finally, Fig.5.2 might have benefited from including a plot of $P_{lin}(k)$ for the different redshifts for better interpretation of the results in other panels.

Other comments:

Twenty-one Sections of the thesis (1.3.3 through 1.3.9, 2.2 through 2.3.3, 2.3.5, 2.4.1 through 2.4.6) are entirely or in large parts copied verbatim from the author's Master's thesis (Vraštil 2015). For example, pages 33 - 41 or 50 - 55 are practically fully cut-and-pasted. The fact that these parts are marked in italics (in the version submitted on 14 September 2020) does not make such “reusing” or “taking over” acceptable. It goes beyond my comprehension why the author compromises a thesis including interesting original results by playing fast and loose with most basic academic standards. The texts written for earning a master's degree cannot be cut-and-pasted for earning a doctoral degree.

Continuing in a similar vein, Section 3.3.1 follows closely the description of the numerical method in Chapter 19.6 of Press et al. (1988) without citing it as a source. Many figures come from properly cited references, but including the full original captions un-adapted to the text of the thesis and without explaining their notation.

Questions for the defense:

1) According to Fig. 5.2 there is more structure on small scales using the truncated Zel'dovich approximation (TZA, panel c) than using the Zel'dovich approximation (ZA, panel b). Shouldn't it be the other way round? According to Fig.5.1 at redshift 1.8 (bottom panel) TZA power is much lower than ZA at large k (small scales), indicating the opposite.

2) According to Fig. 5.1 at redshift 1.8 (bottom panel) the relative difference of TZA from P_lin is negative at k > 0.1 (small scales). At z=0 (top panel) both TZA and ZA are negative at the same scales. According to Fig.5.2 at these scales only ZA at z=0 is negative, TZA at z=0 and both at z=1.8 are positive. How can both figures be right?

3) To interpret the Gaussian fits to the BAO peak, how wide was the interval used for the fitting? Was the Gaussian fitted to ξ as in eq. (5.3) or to r^2 ξ as in the preceding text? Either way, clearly the region of the peak is strongly influenced by the general "background" slope of the dependence. Wouldn't it be better to fit the Gaussian after subtracting the locally linearized background dependence?

4) Fig.5.12 is fascinating but at the same time puzzling. First, its version in the thesis is different from the corresponding Figure 13 in the paper. Why doesn't the vertical axis extend to now (a=1, z=0)? Why doesn't the horizontal axis extend to k < 0.01 as in most other figures, extending instead to k > 1 where FPA cannot be trusted (as per 5.2.1)? Why doesn't the color scale include values < 1 which should be present as per Fig.5.9? Finally, what are the dark vertical bands (most prominent for three values of k < 0.1)? Does this mean there are spatial scales valid throughout cosmological evolution for which chameleon gravity systematically predicts larger power than standard ΛCDM cosmology?

5) According to Fig.5.14, the BAO peak with chameleon gravity is typically 20% higher and 20% narrower than in standard ΛCDM (with FPA, even more when using FFA). Does this mean more accurate measurements of the peak could be used to confirm or rule out chameleon gravity? How about comparison with measurements, e.g., Anderson et al. (2014) or Alam et al. (2017)?

In conclusion, while the presented thesis documents the author's capability of performing original, advanced and largely independent scientific research, his inclusion of the equivalent of ~18 full pages copied verbatim from his Master's thesis documents his lack of regard for basic research and academic standards. Based on this reason I cannot recommend the acceptance of the thesis in its submitted form.

David Heyrovský

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