Report on the Ph.D. thesis entitled "The fundamental metallicity relation through cosmic time: from $z \sim 0$ to $z \sim 1$" by Francesco Pistis (M. Sc.)

The doctoral dissertation presented by the candidate, M.Sc. Francesco Pistis, mainly deals with exploring possible evolutionary and environmentally hints in/from the Fundamental Metallicity Relation. This is done by comparing a local sample from SDSS with a sample at high redshift ($z \approx 0.6$) using the VIPER data for the first time, as far as I have understood. A preliminary and crucial step, in order to use properly this relation, is the study of the influence of all (or at least, most of) the possible biases which can affect it, both at the level of raw data selection (i.e. selection by quality of the signal, luminosity, ...) and of analysis (confronting different methods of comparison of data at different redshifts).

The thesis is 127 pages long including a 23 pages and quite detailed bibliography. It consists of: Chapters 1 and 2 which are mainly introductory chapters; Chapters 3 and 4, where the author describes the data sets which have been used for the analysis, the selection procedures which have been performed on them and all the possible biases which may be introduced (and which are analyzed in detail in the following chapters); Chapters 5 and 6, where the candidate describes the main results of his works, obtained using both "standard" tools (compared to literature) and more modern ones (machine learning); Chapter 7 provides a summary of previous chapters, while Chapter 8 displays possible future developments.

The present work is based on 7 journal papers, of which (at the time of writing this report): 2 are already published; 4 have been submitted; and 1 is in preparation. To be noted that 3 of them (one being already published), have the candidate as first author. The total number of citations is still low (8), but this may be due to the fact that they have been published just few months ago.

In the following, I will explicitly discuss, organizing them by chapters, some main criticisms and comments, with the most pressing (even for my curiosity only) to be clarified by the candidate highlighted in bold font. Before that, I have two general comments.

The first one is connected to the role of the candidate in the papers which are cited. In most of the thesis the subject *"we"* is used and I am wondering if the candidate could (generally) clarify what has been his role in all of them. The amount of work which has been performed on the data and at the computational level, seems to be to be quite huge and remarkable in many steps and aspects. Given that, I expect, of course, a collaborative effort, but I would like to know what has been done directly by the candidate.

The second one: in case of publication of this thesis, some detailed revision is needed. Some passages seem to have been written a bit in a hurry and would need a bit more of polishing. For example, in Chapter 1, it seems there is a missing paragraph at beginning of 1.1, starting with *"Successive missions..."*about *WMAP* and *Planck*. Moreover, many grammatical typos can be found in many places: just to give an example, but there are many cases like this, on pag. 78 *"The algorithm consists into give a score"*, while it should be *giving*.

1 Chapter 1

1. I am not an expert about the topic, but probably the definition of the SFE as *"number of stars formed per unit time per unit gas mass"* is a bit misleading because it could led to think that its dimensions are time^{-1} mass^{-1}, while it is actually time^{-1}.

2. **Pag. 8: how much general is the assumption that the SFE is constant? Is that confirmed by observations or simulations in any way?**

3. **From the last paragraph of Sec. 1.2: I would expect that both the gas inflow and the gas outflow would change Eq. 1.2, and the SFR would not behave like a simple exponential. Is there any model, with corresponding solution, which takes all terms into account?**

4. At the end of the first paragraph of Sec. 1.3, passive and active galaxies are introduced, but what these terms mean (which might not be the case for people not working in this field) is not specified anywhere before.

5. Pag. 10: it is written *"When SF galaxies are selected with stricter criteria"*. What does it mean exactly? Which criteria? And what does "stricter" mean?

6. **I am bit confused by the end of Sec. 1.3 probably because I am misunderstanding something. It is stated that *"the increase in SFR with redshift is expected to depend on the amount of cool gas available which reduces with redshift"*. But if we have *less* cool gas at high redshifts which is *available* for conversion in stars, should not we expect *lower* SFR at high redshifts?**

7. Pag. 13: considering that *"to further investigate higher redshifts, near-infrared spectroscopy is necessary"*, I am wondering if the candidate has any idea of what could be the contribution from JWST.

8. Pag. 15: Fig. 1.7 is referred instead of 1.9. Instead, Fig. 1.6 is never cited in the text.

2 Chapter 2

I do not have many comments about this chapter, except for the fact that, maybe because this is not my topic, the part introducing all equations of Sec. 2.5 does not look completely clear, and a more detailed explanation of all the involved terms would have been probably a better option.

1. Eq. 2.1: the dependence on electron temperature is not shown as much clearly as in Eq. 2.2.

2. Eq. 2.4: there should be a "=".

3. Eq. 2.5: is there any assumption about ω_2/ω_1 for which it does not appear in the equation (it should come from C_{12})?

4. The notation of Eq. 2.7 lacks a clearer explanation.

3 Chapter 3

In connection with the comment I made at the very beginning, in this section it is widely used the subject *"we"* (e.g., *"We build the normalized spectrum"*, etc...). I recognize that the amount of preparatory work seems to be quite varied and time expensive, but may the candidate specify what he has done directly in person?

1. Table 3.1: in *"Sample with properties"*, does "properties" mean M_*, SFR, absolute (?) magnitudes plus different flags?

2. Table 3.1: why different S/N thresholds have been considered for the hydrogen lines? Are they standard values or are they chosen by the authors in this case for some reason (to specify)?

3. **Table 3.4: I am confused by the interpretation of the KS test; what is written in the text seems to be contradictory and opposite to what should be expected. I understand that we *do* want the VIPER and the VVDS to be as closer as possible to have the same distribution. Thus, if all but one *reject* the null hypothesis, the comparison should not be possible. What I am misunderstanding?**

4. Table 3.5: although the percentage of SF is consistent between the two samples, that is not the case for the LINER and "Mix" classes. If the same cuts and selection criteria have been applied to the original samples, I would naively expect a more similar trend. Is there any reason to motivate why the samples are differently populated in those classes?

5. **Pag. 36: it is written that *"assuming a specific dust attenuation law, the choice of which can strongly alter the derivation of* M_**"*. After that, it is written that only one specific attenuation law is used. Is any bias expected then? If not, why?**

6. **Fig. 3.8: the control samples are always consistent among each other and with the VIPER sample in all cases except for the metallicity (and the redshift, but that is obvious). Considering that metallicity is one of the main quantities of interest in this thesis, I am surprised to not have read at the end of the chapter any further discussion about such a difference and if and how that is important for the analysis.**

4 Chapter 4

1. Pag. 47: while commenting Fig. 4.1, it is stated that it *"shows statistically negligible effects on the projections between the full composite region and only the SF galaxies inside this region"*. But from that figure it seems to me that effects are negligible only at small M_* and small SFR and $sSFR$, with deviations in other range maybe led by LINERs galaxies. Thus, how and why are these effects established as statistically negligible?

2. **Regarding the bias from the fraction of blue galaxies (Sec. 4.1.5), looking at Fig. 4.8, I don't understand the consequences of such a bias.**

3. Pag. 53: it is written: *"The MZR of the VIPERS sample does not show the characteristic flattening at high M"*. But from the corresponding Fig. 4.9 it seems that the VIPER sample lacks high mass galaxies. Could, thus, this assertion be clarified?

5 Chapter 5

1. **Fig. 5.1: the anti-correlation of the VIPER sample in the** $\log SFR$**-metallicity sub-space is quite strong and evident. It also seems to appear in Figs. 5.4 and 5.5 (the "convexity" of the profiles from VIPERS seem to be opposite with respect to SDSS). But I don't think I have read any possible explanation about its origin/source (I understand that the mass completeness discussed in Sec. 5.3 is not an explanation, as the anti-correlation seems to be independent of the mass completeness of the sample, if I interpreted well).**

2. Fig. 5.1: I am curious about the factor -0.32 to get the projection of minimum scatter. Is it fixed by theoretical considerations? Is it derived a posteriori from the given samples (thus not being fixed in general) under the only condition to minimize the scatter?

3. Fig. 5.2 is not really clear. Showing the surfaces from different viewpoints would have helped to understand more, i.e. to see the curvatures of the surfaces.

4. Pag. 63: I think that the statement *"Figure 5.5 shows bigger differences between the samples compared to the normalization with respect to the median value"* is not (even tentatively) explained.

5. Sec. 5.2.3: how the slope of the normalized metallicity-sSFR relation is calculated (local interpolation, finite differences, etc...)?

6. **Pag. 66: once again, I don't understand the discussion of the KS test because there are statements which are apparently in contradiction. On one hand it is written that *"From the point of view of the evolution of the MZR and metallicity-SFR relation, only the high* M_* *end of the MZR is comparable between low and intermediate redshifts"*. On the other hand I read *"The evolution between SDSS and VIPERS samples for the MZR at low* $\log M_*[M_\odot] < 10.75$ ... *is statistically significant."* So, what is statistically valuable and comparable, the difference in behaviour in the high or low M_* end? In addition to that, in other places it is written, instead, that the mass completeness is not a relevant bias. Why? How is that quantified?**

6 Chapter 6

1. For each ML step, were already available codes used? If yes, which ones? If not, where they written from scratch by the candidate?

2. I would have appreciated a bit more of technical details about the ML steps. Someone not knowing the topic could ask if the principal components have some precise physical meaning, or if are just a mathematical way to represent the parameter space with not a

straightforward connection to the physical quantities which have been chosen to describe the sample. How the physical quantities are projected onto the PC space? I have personally not understood exactly how the K-means clustering is performed: how the points are *"labelled"*? How are the distances in between each point calculated in the PC space in the WCSS algorithm? Why the number of neighbours has been fixed at $n = 20$ while searching for outliers?

3. Fig. 6.3: the left and right panels have exactly the same numbers. Is this a typo? Or SDSS and VIPER are really exactly equivalent from a PC point of view?

4. Fig. 6.5: why to choose 5? By which criterium? Does this choice hold only for the first two (shown) PCs, or all the main four one? Table 6.2: are these number referring to PC-1 and -2, or all four?

5. **I must admit that I have not understood the discussion about Figs. 6.6 and 6.7 and how to interpret them. If I am correct, the candidate wants to assess that the algorithm does not cluster galaxies in luminosity/redshift samples. If that is the case, as it is stated in the text, any "horizontal" correlation is dependent on the ordering of the clusters, but I think it could still be possible to investigate it by considering and comparing all possible combinations of the five clusters. Although, I see some "vertical" correlation. For example, the cluster 0 from SDSS, from Fig. 6.7, is made of galaxies with the smallest masses which are, accordingly, the less luminous (higher i-mag) and closest (lower z). In terms of SFR maybe the correlation is less evident, I agree, but still I cannot see the absence of any correlation quantified in an unequivocal way. In the case of VIPER, again, if I focus on the cluster 2 (green), I see that it almost covers the full high-end range of masses; that all clusters have a nearly constant SFR and, as a consequence, the sSFT for cluster 2 is lower than others. As well, it corresponds to the most luminous galaxies which are detectable on a wider redshift range. So, to conclude: I don't know if I have misunderstood the concept of "absence of correlations within the clusters", and I would like to have a clarification about that from the candidate.**

6. I have a curiosity about Fig. 6.13: why the cluster 3 (red) from the high mass galaxies in VIPER is not recognized by the algorithm as made of outliers although lying at the border of the 3σ level of distribution, while some outliers are even consistent with the 2σ?

7. Fig. 6.15: would not have been more helpful and interesting to plot the clusters and the "theoretical" expectations in the BPT diagram (as been done in other figures in previous chapter), so that one could have easily identified any overlapping of each cluster with a specific ionization-type region?

8. Pag. 88: *"Here, we found a correlation between the clusters and $D4000n$ break"*, but not much more is discussed about it.

9. I was mostly curious about the $D4000n$ break, because in cosmology it is used to identify and define the so called cosmic chronometers (mostly passively evolving early-type

galaxies) from which the Hubble parameter at various redshift can be measured, which is quite important for cosmological studies. I understand that in this thesis the role of these galaxies is quite marginal, entering mainly only the outliers description, so that no real insight from this point of view can be given. Am I right?

7 Chapter 8

I would appreciate a more clear discussion of how the open questions raised in this section are connected to the work of this thesis.

Conclusions

As a final remark, I want to stress that all the previous comments and questions are only raised to better understand the main novel results obtained by the candidate and to highlight the value of his doctoral thesis and work. In my opinion, the thesis meets all the necessary criteria set in the Polish Act - Law on Higher Education and Science and fulfills all the necessary requirements to be presented for the doctoral degree so that I recommend the admittance of M.Sc. Francesco Pistis for the defence.

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