Paper	*Red-sequence galaxies with young stars and dust: The cluster Abell 901/902 seen with COMBO-17* -- arXiv:astro-ph/0506150
Preface	This paper is a careful and comprehensive piece of work, addressing the presence of dusty star-forming galaxies in a large galaxy cluster complex. The existence of dust enshrouded activity in galaxy clusters, though not a new idea, has suffered from limited study and ambiguous evidence. Many groups have, or have begun, to study this phenomenon at radio and/or mid-infrared wavelengths, but this paper is one of the few pieces of work which attempts to study active dust enshrouded cluster galaxies in the optical, through a careful analysis of spectral energy distributions. Their multiwavelength data are unique and their results are interpreted in the broader context of galaxy cluster evolution. Though quite long and detailed the main ideas are not hidden, and the technical discussions will be of value to others working in the field. I am very impressed with the thoroughness of the work.
Comments	The main improvement could be done in shortening the lengthy discussion of Section 5, since we can pick up repetitions, and conciseness would make the conclusions clearer. The name of the population "dusty red population", might not be appropriate, since not all have dust, and in any case the level of extinction is quite low. It might be more appropriate to call them "intermediate population"? Typos: -- Their is --> There is --esacping --> escaping -- We try in consider --> We consider I'm sorry for the slow refereeing for this paper. The blame for that lies not on the paper, which I think is very interesting and provides a nice addition to what is proving to be the reference dataset for galaxy evolution out to z=1. I do, however, have a few comments and remarks that I would like to see addressed, and would like to see the updated paper although I expect it would require only minor modifications. Let me just summarise my main concerns before I go through more in detail below: - Merger influence. One of the main results of the paper is that the decline in star formation is not dominated by major mergers. While the case is reasonably convincing it lacks a discussion of the relationship between star formation and the visual appearance of mergers. In other words could a large amount of the star formation you see be related to either early or late stages in a major merger where you are unable to identify the galaxy as a major merger?

- The influence of old stars in heating the gas is ignored throughout, but I am worried that it would introduce systematic uncertainties with luminosity, perhaps in particular in the local sample.

- It is unclear what the physical role of the irregular class is and how it fits into the picture provided by the paper.

Overall the paper is well written and very clear.

Detailed comments:

Abstract: I find it a bit odd to refer to M_* > 10^10 Msun as massive given the fact that they contribute a large fraction >80% of the total stellar mass density in the local universe. Personally at least, I view "massive" as being at least above the median mass. The paper is clear about what you mean by "massive", but even so you might want to define it in terms of the XX% most massive galaxies if possible.

Section 2: For the astrometric accuracy: When shifting the astrometric solution to 2MASS, did you match EIS to 2MASS or did you match the Spitzer data to 2MASS? If the latter, why did you not match directly to 2MASS, and if the former why not match EIS directly to 2MASS and then match the Spitzer data to the corrected EIS matches?

2.2 Classifications:

There is one aspect of your classifications which complicates the interpretation of your results: What is the physical nature of your irregular category? In the classical morphological definitions in the local universe Irregular is taken to mean Magellanic irregular usually with the Magellanic clouds as reference types.

Since these are faint/low mass systems it is unclear where to fit in your systems into the picture. In particular it is unclear to me whether they, at least the faint end, could mostly be systems where the low luminosity components of interacting systems are invisible, or whatever they are? I turned to your Appendix B to see if the GOODS data could shed some light on this, but you don't discuss this.

You might want to discuss this in more detail in other papers, and my apologies if I have overlooked it in your existing ones, but it would be a great help if you do discuss this class somewhat more - at present it is glossed over and I read it as if you equate this with the type of Irregulars which then begs the question of what your Irregulars will turn into as they certainly are more massive than local Irregulars (which also seem to be in agreement with what we found in Brinchmann & Ellis 2000)

2.3 - this was a little bit confusing to read. And I am still unclear on a couple of points:

- Are the 24mu sources that were blends of two COMBO-17 sources included or excluded from the final catalogue. I presume excluded, but don't understand exactly why since you know the match & separation and you know the 24mu flux, is the PSF too poorly known for the 24mu data to exclude an appropriate division of flux between the two sources?

- Have you double-checked the source matches using, say, radio observations of the areas?

- Is a uniform upper limit for the non-detections acceptable (ie. is the noise spatially uniform?)

- Are there any 24mu sources that were not matched to the C17 catalogue? It would be nice to get a number for this either in 2.3 (my preference) or in Appendix A.

3.1

- missing a "the" in front of Lagache et al.

3.2.

- Ignoring the fraction of IR light from dust heated by old stars, while not a very big effect could lead to a systematic bias with stellar mass in your systems. The low SFR/<SFR> systems would presumably have a larger fraction of dust-heating from old stars than the high SFR/<SFR>. Thus I am worried that perhaps you are systematically overestimating the SFR at high mass in your systems relative to low mass. (This is obviously addressed to great length in Bell 2003 so presume it is easy to address)

- Averaging over inclination angles (as done e.g. in Charlot & Fall 2000) might be ok for sample averages but for a relatively small sample like the present I doubt it will solve things.

- You claim that 0.3 dex is an appropriate systematic/random uncertainty, but comparing the SFRs in your figure 1 to Fig 1 in Bell 2003 it seems to me that a) the scatter in Bell 2003 is more like 0.5 dex in the same range of SFRs (log SFR>0.5 or so) and b) isn't the overlap between your sample and Bell 2003 a bit small to be that certain about the systematics? Also, in the conversion from 24mu to TIR flux you quote a factor of 2 uncertainty, which I presume is larger than the uncertainty in converting to TIR in Bell 2003? Do you have an alternative estimate of the systematic/random error?

- Finally, it isn't clear to me that adding the UV to the SFR indicator in fact improves things (although Bell 2003 indicates that it does). For instance Flores et al (2004 - A&A 415) showed that Ha derived SFRs in fact agree well with the IR-estimated SFRs as long as Ha was carefully corrected for extinction. Have you tried their prescription or would the results change if you use a TIR-only calibration for the SFR?

3.3

You do not discuss any possibility of underestimation of the stellar mass because of obscuration. Franceschini et al 2003 (A&A 403, p501) claim that the presence of dust can lead to up-to a factor of five uncertainty in the stellar mass. Are you able to rule this out in some way? This is also a paper I would like to see a reference to in your paper as it does discuss several aspects of what you are doing, such as deriving SFR/M* estimates and looking at those compared to the stellar mass, and their results seem to be in some disagreement with yours (c.f. their Fig 10 to your Fig 4. It would be very helpful to explain where the difference comes from (as far as it is possible)

Section 4

Fig 1: What are the uncertainties indicated by the cross in the lower right?

Also in Fig 1 - it would be good to have some more detail on what relation you have taken from Adelberger & Steidel (2000), what this is based on and how you transformed to your quantities (ie. from their L1600 to your UV luminosity etc. This is necessary to ascertain how well the local and distant universe agree.

Second paragraph: You talk about IR luminosity density, yet you quote luminosities. I presume the assumption is that you have a volume limited sample and can therefore sum up all luminosities, but given that a density should be per volume I suggest you either change the text to reflect that this is truly an integrated luminosity density or that you quote values per Mpc^3 (given your concern about cosmic variance I guess the former is to be preferred)

It would also be good to have a rough estimate on the uncertainty in the ratio of IR to UV luminosity density.

5.1.

Second paragraph, line 3: "rapid massive star formation" - why massive?

Third paragraph, second to last line: "owing to their lower SFRs and dust contents..." - how do you know that these systems ("Irregulars") have low dust content?

I was intrigued by the fact that Pec/Int galaxies with red optical colours show strong 24mu emission - are these red also when looking at UV-optical colours?

Fifth paragraph, second line: You state that there is an interplay between galaxy morphologies and SFHs - while I do agree that we know this in the local universe and this would extend to higher redshift, I can't see that your results in this section have put any very strong constraints on the star formation histories of the galaxies?

5.2

- Is it meaningful to quote relative morphological mixes when the morphological mix is (at least locally!) a strong function of luminosity? Do you see any such relation in your data? It might be more enlightening to show the relative contributions to the IR density as a function of luminosity (although that is essentially captured in the LFs).

- It would be nice to know how much different morphological types contribute to the total IR density in the local universe

Page 14: The galaxies that you classify as mergers will only be classified as mergers for a certain short period of time. However you use your number counts to directly constrain the contribution of mergers to the total SFR decline. This is an uncertain assumption however - the simulation by Barnes (2004) of the Mice is a case in point - at the stage that the Mice are at now they have not reached their peak star formation, in fact the peak will be reached after the two galaxies have merged. It is not clear to me that at that point your classifications will be able to classify this as a merger - likewise if you go along the Toomre sequence of mergers, you find several galaxies at advanced stages of mergers that require careful
observations to be classified as mergers or merger remnants at present. Thus if a large number of galaxies in your sample have elevated SFRs because of a recent major merger whose influence you
cannot see, the true contribution of major mergers to the total decline in SFRs will be underestimated by your method. Thus your method will only be able to place a lower limit to the total major merger rate.

That said, I think that the fact that so much of the star formation is in undisturbed spirals indicate that my concern is relatively unimportant, but I would like to see it addressed (dismissed if possible :)

Section 6.

- For calculation of SFR/<SFR> did you really do what you said in the text, ie. calculate M* (z_gal)/t_gal? Or did you take into account the recycling fraction to adjust for mass lost over time? The difference
is substantial of course.

- I am confused about the local sample and its relation to the distant sample:
 * The local sample is K-selected whereas the C17 sample is essentially B-selected - this ought to mean that the fraction of strongly star forming galaxies is lower in the local sample?
 * Further the local sample is 2177 whereas the C17 sample is 1306 (if I understood the numbers correctly) - thus even after correction for overdensity it seems overabundant with respect to the local sample.

* Given the morphology-density relation there is likely to be a rather different morphological mix in the local and distant sample - how does this affect your results?
* One would expect that a larger fraction of the low-redshift IR emission would come from old stars - this would mean that SFRs at low redshift might be systematically over-estimated compared to the high redshift case.

- I also think it might be appropriate to compare also to some of the other local/distant results such as Brinchmann & Ellis (2000) & Franceschini et al (2003) for SFR/M* and possibly Gavazzi et al (2000) for the local (or Brinchmann et al 2003 for that sake).

- Finally a very interesting question with your samples is whether they will map onto each other? Ie. is the low redshift sample an example of what the high redshift galaxies will turn into 6 Gyr time?

Anyway, I don't think the results will change in any way, but as it stands I feel it lacks a bit of detail.

Figure 4 - Again, are the error-bars medians? - Also, and this is relevant for Fig 5 too: The SFR/<SFR> quantity is dependent on h because of the time-scale introduction so since you have chosen to keep the h factors you should keep that for SFR/<SFR> too.

7.1 - Again a comparison with Francescini would be helpful here.

7.2 - Figure 6 - to be consistent with other plots you should indicate the h-dependence of the luminosities.

- Also the Chandra image goes only down to a certain depth - how much of the total 24mu emission could be due in part to AGN contribution? (ie. of the population not detected by Chandra). Strictly speaking your AGN contribution is a lower limit to the AGN contribution

- I was surprised you used the Cohen calibration for X-ray SFRs and not something a bit more thorough and perhaps more appropriate for comparison with your work like Colbert et al 2004 or Persic 2004. I
don't think their use would change any results, but you might want to check.

8. Again I disagree that your Chandra comparison gives an upper limit to the AGN contribution.

Appendix B: Related to my earlier comments about luminosity-morphology correlation, I was wondering if the correction factor you derive from the GOODS area depend on luminosity? Also it would be good to quote the numbers for reference.

That's all. Hope it is all easy to address!

Paper	The Evolution of the Optical and Near-Infrared Galaxy Luminosity Functions and Luminosity Densities to z~2 -- arXiv:astro-ph/0505297
Preface	The authors present a study of the evolution in the galaxy luminosity function in the B-band from low redshifts to z=2 and in the U and J-band out to z=1. The sample is based on photometric redshifts from multi-colour imaging by the GOODS team, which in case of the J-band LF comes from a field with 130 sq.arcmin in size. I have some important issues with this paper, mostly concerning a lack of comparison with existing literature, the need for a more convincing presentation and a certain incompleteness of their work, which in the given form does not merit publication in The Astrophysical Journal.
Comments	(1) What are the results presented in this paper? a. it presents B-band LFs out to z<=2 probing into the redshift desert at z>1.3. Fine. This has been presented by Poli et al. 2003 out to z=3.5 (which is not even cited), and more recently by VVDS from a much larger and more significant sample of even spectroscopic redshifts! The authors mention handwavingly that their results agree with VVDS, but see no need to substantiate the claim with any figure. b. it presents U-band LFs out to z<=1. Why only to z=1, not to z=2 like for the B-band? The authors have the data for that. Would they not see any evolution more clearly with a wider redshift range? Again, the VVDS appears to present much more significant results. And while we are at it: Statements are made about the colour dependence of the evolutionary trends. Then, why not R-band-LFs as well, which are certainly possible with the photometry the authors have? Then they could compare the results with those from LCIRS that also probe into the redshift desert as the authors claim they do. Chen et al. (2003) is mentioned as an example for a survey, but there are no further comparisons. c. it presents J-band LFs out to z<=1. Feulner et al. 2003 and Pozzetti et al. 2003 have presented J-band LFs in similar redshift ranges before, in the latter case using spectroscopic redshifts from the K20 survey, which is only ~1.5 mag less deep when compared on the same magnitude scale. They are not even cited, let alone any comparison being offered. Instead, they cite K-band LF results from Drory et al. (2003) and initiate a debate about Drory's failure in trying K corrections into the restframe K-band. If they were trying to quantify evolution in the J-band, they must surely also be interested in local zeropoints, so what about the Cole et al. (2001) J-band measurement from 2dFGRS, which is cited briefly but ignored otherwise. d. it discusses a comparison between the data and semi-analytic models.

It picks Somerville, Primack & Faber 2001 for a comparison of luminosity functions and integrated luminosity densities and claims fairly good agreement. First I fail to see the good agreement anywhere. The obvious disagreement between the strongly evolving predicted and the non-evolving observed luminosity density seen in Fig. 8 and 10 is blamed on the unobserved domain of faint galaxies, while good agreement is again claimed for the observed mag range as compared in Fig. 7. A brief look at that figure makes clear that the shape of the predicted LF changes very little. Hence, the evolution in the luminosity density is a result of the LF shifting as a whole along the magnitude and/or density axis. In case of a Schechter function, this would mean M* and phi* change while alpha stays constant. And M*/phi* evolution propagates right into luminosity density evolution. In other words, the unobserved faint domain offers little contribution to the evolution, while in the observed range, the data do not evolve a lot (see also measured M*/phi*), but the models evolve across them, from overestimating the characteristic luminosities at z>1.5 to underestimating them at z<1. The evolution of this mismatch (factor of ~2 in L*) matches the evolution of the mismatch in the luminosity density (factor of ~2 between z=0.6 and 1.7). Hence, the problem arises from disagreement in the observed mag range.

Summarizing the first impression, it appears as if the authors had always "kind-of-wanted" to write a paper on the subject (there was a 2002 AAS contribution on this), and didn't get around to it for a long time. Now, the literature left them behind, they threw some results together along the shortest possible route, left out many obvious but complementary results they could have produced, and just ignored almost entirely the 2003-2004 literature in the field for their discussion, except for mentioning handwavingly that they are consistent with COMBO-17 and VVDS. Then they compare their data with a model (which may well be right...) wiping the crucial disagreement (no evolution in their data) under the carpet, and drawing all attention to what could not have been observed yet.

(2) Is the presentation convincing?

a. I miss a few diagrams which describe the sample, such as a redshift-magnitude diagram. There is just no way to get a feeling for the input catalogue that went into the LFs. How is the type split done, can we see illustrations? What about the restframe colours in the sample - do they evolve?

b. The LFs could be overplotted with a couple of relevant LFs from the literature so one could see differences in depth and the size of error bars, or systematic differences between this paper and previous results. This would be interesting for both J-band results as well as B-band LFs, where good agreement with COMBO-17 and VVDS is claimed. Especially, it would allow to see, how the strong evolution found in Ilbert et al. (VVDS) and Wolf et al. (C17) fits in with their weak evolution. It would be revealing to see the plots.

They may have singled out these two surveys for a comparison because they are large and have produced relevant publications to date. But where the authors probe the same domain as COMBO-17 and VVDS, their results are less significant, and where they probe other domains, they ignore the existence of other surveys and their results. Again, a plot comparing the Pozzetti LF and luminosity densities with the results in this work would be desirable.

Minor points:

3. The organization looks funny in the appendix, with a Section "A." and "A.1" - If one has only one sub-section, why introduce an additional hierarchical level?

4. Generally, subscript labels in mathematical expressions should be set in {\rm } when they are words rather than mathematical symbols.

5. What happens to AGNs (of high or low luminosity) in the sample? Please specify. Are they recognized and ignored, or included at the right or wrong z?

6. Abstract: they observe VERY little evolution in M^*_U - this could be interesting. But when looking at what Ilbert measure from a presumably very reliable sample, how can the measurements be reconciled? This should be discussed.

7. They say, they measure evolution "from z=0.1 to 1", which is misleading because the redshift bins are rather wide. How about quoting the median redshift in the sample or better the volume mid-points of the first and last bin, 0.39/0.88?

8. On p.5 they say, "Using the available 13 ... and 7 ... passbands " -- isn't it 7 and 13?

9. On p.7 they discuss photo-z errors propagating into M_{abs} errors But they also propagate into K-correction errors! Because with a wrong z you look at the wrong lambda point in the filter set. If the SED is flat in f_{nue} that does not matter, but for galaxy SEDs different from that it matters. How much, e.g. for M_B of a red galaxy with +/-0.1 in z?

10. On p.7, it is actually mostly M^* which is biased by the photo-z errors, but not alpha if the survey is deep enough to see the flat part of the LF.

11. On p.9 they mention cosmic variance and they present estimates of the error contribution to the LF. Is the CDFS redshift histogram consistent with the 1-sigma variation calculated? One could use the VVDS redshifts to show that. Given the all the rumors about peculiarity of the CDFS, it would be useful to substantiate or refute the rumors once and for all. Their cosmic variance discussion looks like a good place to address this question quantitatively - a good way to support the LF measurements and provide something new as well.

12. On p.10, Sect. 4.1 they fit a Schechter function to the 1/Vmax points. Does that not lead to a biased M^* given the width of the mag bins at 0.5 mag? How do they compare

with STY derived M* values? The bias could exceed 0.1 mag for steep functions also the bias is redshift-dependent because of a changing visibility of the faint, flat part, and hence change the evolution found.

13. On p.10 they mention how different types are more or less prominent in different passbands and they could just confirmingly say that this means different mean colours by design of the type definitions. They could plot diagrams of colour (in their restframe bands UBJ or more) over redshift and demonstrate that the mean colours match the M*_a-M*_b colours in the LFs.

14. On p.14 they say models are in broad agreement with observations in Fig. 8. No. If you need proof, just look at the B-band...

15. On p.17 they say they are deeper and that's why they get a better M* and alpha. They should discuss the reason in detail rather than using a generic "we are deeper"-argument. Fig. 3 and 6 indeed show clearly that the all-type LF is not a single Schechter function. Maybe, it can be modelled as a sum of two Schechter. The bright end gets flattened by early-types, and the faint end gets steepened independently by starbursts, so any fitted alpha depends explicitly on which population dominates the M-range for the fit. And at high z the blue population is so much brighter and more numerous compared to the early types that the early types don't stick out as a hi-L hump anymore, and the LF looks steep as a whole. Locally, the starbursts are almost irrelevant, while the early types have quite an impact -- this is why 2MASS does not "see" the steep alpha!

16. Fig. 1-: The LF of red galaxies should have a more positive alpha after ignoring the population that makes a rise at the faint end (see e.g. 2dFGRS and COMBO-17 procedure on this - would make it easier to compare then). Any comments on the faint upturn? Is it possibly dust-reddenend blue galaxies such as edge-on disks, coming in primarily at the fainter end of the early-type LF after extinction

17. Fig. 8-: Isn't cosmic variance bumping up the luminosity density in the center z-bin? See n(z)'s from VVDS and others. Does that not limit the interpretation in terms of evolution measured from a small field? Ties in with the discussion on cosmic variance.

18. Fig. 10-: I like it, esp. the black and grey error bars! This figure makes clear how much disagreement there is between the measurements, which may be more a consequence of cosmic variance than method.

19. Tables 2-4:
- errors in M*/phi* look sooo tiny when alpha is fixed. Why is that? Is it a result of finding 1-sigma errors by cutting a line across the 1-sigma contours in a 2-D (alpha,M*) plane, instead of marginalizing over the 2-D distribution?
- does M*_UB really drop from z=0.6 to z=0.9? Or is a CDFS-specific overdensity at z~0.6 responsible, which acts together with phi*/M* uncertainties?

Paper	*Photometric calibration of the COMBO-17 survey with the Softassign Procrustes Matching method* -- not yet on arXiv
Preface	This paper addresses the question of how to refine the calibration of multi-colour data sets using an advanced form of stellar locus matching between the data set and a model. This is a problem worth being solved even though some solutions exist, and some of the frontier problems in this area are neither addressed by this work nor by previous work. The paper chooses a point-set matching procedure known from other areas of data analysis and brings that into astronomy. As such the paper describes only a moderate scientific achievement, however, it is often appaling how little awareness there is in the astronomical community of algorithms that were developed elsewhere, and so there is clearly a case for showcasing application transfers such as this one. In principle, I would like to see this paper published then, however, I have a couple of major concerns.
Comments	1) First of all I don't know the meaning of a "GEMS" catalogue. This paper uses data from the CDFS field of the COMBO-17 survey, which have been published by Wolf et al, first in 2004 and then aparently with a calibration update in 2008. The GEMS survey appears to be a two-band HST survey, an entirely different thing. The authors should clarify whether they are using the 2004 or 2008 version of the catalogue, and remove confusing GEMS references. 2) Secondly, in Sect. 3, COMBO-17 is described as using a single standard star for calibration. After ressearching the literature, it appears that this was only the case for the 2008 version of the CDFS table, while the 2004 version is calibrated with two stars. Again, this will need clarifying. 3) Looking at Table 2 and investigating the ZP offsets, I wonder whether the authors here have confused the flux units or magnitude system. COMBO-17 was compared to other data sets in the past, e.g. MUNICS, see the Wolf et al. (2008) paper in which a calibration update was published. That update was a moderate change in the grand scheme of things, not more than 20% at the extreme ends of the spectrum. Here, we are looking at ZP offsets stretching to nearly a factor of 3. I find that hard to believe. Since the filter wavelengths strectch nearly a factor of 3, and the offsets scale nicely with central wavelength of the filter concerned, I find it much more likely that the authors of the present paper have dropped a unit of lambda in their unit conversions. The authors go on to show that photometric redshifts derived after their massive recalibration nicely match the original COMBO-17 ones with no gain in precision. I don't think that is a likely outcome in the case where the COMBO-17 data are grossly miscalibrated. I have minor comments, but will reserve these until the major ones are addressed.

Paper	*Massive Elliptical Galaxies: From Cores to Haloes* -- arXiv:astro-ph/0512175
Preface	I wish to recommend the paper for publication in the Astrophysical Journal after some revision. I generally like the spirit of the paper and the use of the data for the presented purpose. However, I feel that several issues are not presented with the desired clarity and also statements are made on the basis of values taken from the literature which have been chosen with insufficient care.
Comments	Section 4: 1. I am puzzled by the fit in Fig. 2. It looks way too steep to the eye, and in fact a line of M_dyn \propto M_star would naively look better. Is there an issue with the weighting of the points? How strong is then your case for proposing that dark matter is more important in more massive galaxies based on your own data, ignoring that other literature may suggest it (but not your data, I believe)? 2. You cite Padmanabhan et al. 2004 to derive dynamical masses. Pad04 suggest that these M_dyn values have systematic errors of ~30%. Furthermore, you said yourself that the stellar masses are uncertain to ~0.1-0.2 dex from IMF issues alone. A diagonal line fit to the data in Fig. 2 would show that you find dynamical and stellar masses to be consistent within 0.1 dex. Given the large errors no case for dark matter can be made within R_e from your data, although suitable choices of IMFs and velocity structures can allow dark matter given the large uncertainties. Hence, I don't see that your (Sect. 4) "mass fraction derived lies in between the cosmological value (O_m/O_b ~ 6) and the value for spiral galaxies." What is the value for spiral galaxies you assume? I believe, your cores of ellipticals seem to contain as little dark matter as those of spirals given your data. Next you say "This is consistent with the baryon distribution being set by violent relaxation during the collision of two spirals (Mamon 1992), one of the proposed fomation mechanisms for massive elliptical galaxies of this type." Well, but this is in fundamental opposition to the monolithic scenario your paper investigates and for which it claims consistency between data and models. In fact, in Sect. 8.1 you argue that the concentrations obtained in hierarchical models are not observed in your galaxies. I find that confusing. You conclude Section 4 with references to Romanowsky et al. (2003) and Dekel et al. (2005), saying "it is not clear that such an explanation [i.e. the Dekel one] could account for our results which therefore present a challenge to such models." NO. This is beside the point. Rom03 and Dek05 agree on the data, but draw different conclusions on the basis of different assumptions about the velocity structure. Your data and results make no statements about the velocity structure whatsoever. Your dynamical masses are purely a product of the Pad04 velocity assumptions. How then do you want to confront the argument between Rom03 and Dek05? I believe, you are not challenging either one. You probably want to say, that data such as those of Rom03 would be expected even in large samples of ellipticals like yours, not only in the few examples studied by Rom03.

My suggestion: Could you clarify the errors on your numbers for the reader and then adjust the weight of your conclusions, and phrase more concisely what and what not it is that your data support?

Section 5:

3. You define your sample as 2040 galaxies selected with completeness limits from an initially much larger sample of almost 9000 galaxies. You say in Section 2, that your subsequent results are based on this complete sample. In Fig. 3, you show in fact the completeness limits and say that the fits have been derived only from the sample above. Now:

a. In Fig. 2 you probably restricted the fit to the complete sample as well. Could you indicate the mass limit there as well, especially since your mean M_star/L turns out to be different from the one assumed in the selection in Fig. 1.

b. In the middle panel of Fig. 3 you draw a horizontal completeness line at log M_star ~ 10.85, while your original selection was a cut in L_r, that translated into a log M_star = 10.9 on the basis of M/L=4. Your mean measured M/L is higher (~4.8 from Fig. 4 it seems), so your resulting completeness line should on average be at 10.98. While that is only a little difference, I am concerned about the strong slope difference in M_star(sigma) compared to L_r(sigma). If it is correct, a horizontal line in L_r would translate into a completeness line in M_star with quite a slope.

c. You could draw an approximate completeness line at a suitable angle into Fig. 4 as well. This may help understanding why the fit looks so incorrect when compared to the whole set of points (of the incomplete sample).

4. I am generally very confused about the fits:
a. The L_r(sigma) fit and the (M_star/L_r)(sigma) fit look like not fitting the data points, but too steep.

b. I also do not understand at a fundamental level, how you can do any fit while dropping points below the completeness line when your fit is almost parallel to the completeness line? Could you illuminate that? A completeness line perpendicular to a fit would be of no concern but how do you deal with incompleteness in L_r at any given sigma? Does that not bias you? If yes, it would bias you to a too flat fit.

c. I am deeply concerned about the stability of fits when comparing Fig. 3 and 4. When I compare the M/L ratio of the M-fit divided by the L-fit with the fits to the M/L points, I hope to recover the same relationship within some error. In contrast I actually find:

M-fit/L-fit: M_star/L_r ~ sigma^(2.06-3.48) = sigma^-1.42
vs. M/L-fitted: M_star/L_r ~ sigma^+0.33

and

M-fit/L-fit: M_dyn/L_r ~ sigma^(3.55-3.48) = sigma^+0.07
vs. M/L-fitted: M_dyn/L_r ~   sigma^+1.18

A slope inconsistency of 1.75 and 1.11, respectively, to be contrasted with the slope errors you formally quote as 0.03 and 0.01 - can you comment?

Section 8.1:

5. You find mean initial concentrations of <c> in the range from 3 to 9 assuming monolithic collapse and adiabatic cooling. Then you say, referring to Wechsler et al. (2002) that massive galaxies as you study them are predicted to have c an order of magnitude higher. However, Wec02 shows in Fig. 9 a <c>=13 for galaxies which are less massive than your completeness limit, i.e. 1.5-2.5 e12 in M_tot which is 0.6-1.0 e11 in M_star(<R_e) assuming your alpha and baryonic to dark matter ratio. Specifically, galaxies with >4 e10 M_star(<R_e) or >1 e12 M_tot are shown to reach all the way down to c~4, although this requires a recent major merger (since z=1). The latter is expected to happen in the hierarchical model. So, I don't see any disagreement at an order of magnitude level. Since Wec04 talk about dark matter only, and not dark plus baryonic matter, the concentrations measured in these two frameworks will necessarily be different. This weakens the value of your comparison as it stands. So, maybe just weaken the suggestion that your results would be bad news for the hierarchical scenario, or substantiate the argument better if you can.

6. There is already some weak evidence for a change in concentration of massive elliptical galaxies with redshift from the measurement of the size-mass relation by Trujillo et al. (astro-ph/0504225). Do you wish to comment in your paper on that? What predictions would the monolithic and the hierachical scenario make for that?

Section 8.2:

7. Why do you choose the solar metallicity cooling curve, when your source SD93 offers a range of metallicities including nil metallicity as expected for pristine material undergoing a first collapse? It makes a real difference in your plot and I suggest you replace the solar metallicity for the 'nil' curve.

Section 9:

8. In the second paragraph you say, "we find a slope of M_star(<R_e) \propto sigma^2.059, less steep than previous work (Thomas et al. 2005) based on a study of an order of magnitude fewer galaxies". Apart from the fitting issues mentioned already above, which make me suspicious of this slope (your M_star/L is almost flat, remember), I don't like the implication of the remark on the Thomas study about the order of magnitude fewer galaxies - because, your dynamical masses are based on Pad04, who have done very similar work to you and find a steeper slope based on an order of magnitude MORE galaxies than you use. Clearly, sample size is not the issue. If you are after justifying your result being different from others, there must be a different argument.

Some minor comments:

9. In Sect. 1 you say "we find results which are intermediate between Rom03... and Dek05". I don't understand the claim of finding intermediate results (see item 2 above), as your numbers are based on the Pad04 intermediate assumption, plus I do not see strong evidence for dark matter given the uncertainties.

10. In Sect. 1 you say "Defining alpha...we find alpha ~ 20 provides a good fit". Later you do consider values of >10 generally. I share your opinion, that the value is uncertain to at least a factor of 2. But I would indeed say that in the introduction as in "values of 20, probably at least >10...", otherwise the reader will expect you have done a very accurate job in constraining alpha later.

11. Start of Sect. 3 says M_star ~= M_b for ellipticals with little gas content, although later you argue that more than half the mass may be in hot coronal gas. Why not say right here, that you are talking about "within the effective radius at least" or something similar?

12. In the conclusions you say "we cannot confirm the results of Rom03". Honestly, I don't believe you can rule them out either (see above), so I don't find this conclusion fair (whether you or I believe Rom03, is a different question, I just don't see that it is your data that make a significant statement on that).

13. Some words missing, chosen sub-optimally, or typos:

Sect. 1: ditribution --> distribution
Sect. 2: work presented B03 --> by/in B03
Sect. 7: model in compatible --> is
over-density when the top hat --> at which the top hat
redshift,z, -> redshift z
sigma is large so --> so large
Sect. 8.2: definied -> defined
Sect. 9: have been used --> has been used
less abundant that --> than

Please do not misinterpret my comments, I am simply motivating a clean-up that will make the paper look much more consistent and natural even to the critical reader.

Paper	*Metallicity effects on cosmic Type Ib/c supernovae and gamma-ray burst rates* -- MNRAS, 2012, 423, 3049
Preface	This paper attempts two objectives: (I) It tries to reproduce present-day supernova Ib/c rates in irregular galaxies using a fiducial model of their star formation history, their chemical evolution and a couple of model parameters (II) It compares the cosmic GRB rate history with the cosmic star formation rate histories of various authors. Here the cosmic SFR is actually represented by the SN Ib/c rate, which in the chosen model is proportional to SFR. Step I is supposed to motivate the choices made in step II. Throughout the paper, special emphasis is put on the Calura & Matteucci model of galaxy evolution. I understand the motivation for this work, but at present I find the case, the presentation and the implementation confusing.
Comments	About part II (which is the ultimate goal of the paper): 1. The statements are made that the GRBs are an important tool for measuring the cosmic SFR history (given that they highlight the star formation, thereby follow the SFR), and that the cosmic SFR history in this paper is measured especially thanks to GRB observations (first paragraph of Sect. 4). In the end, one of the two major findings of the paper is that the GRB rate history follows the SFR history (=the SN Ib/c rate history assuming that is strictly proportional to SFR and doesn't show the otherwise expected metallicity-dependence). This is a perfectly circular argument. 2. The statement is made that only 10^-4 of all SNe Ib/c make GRBs, a value that is somewhat lower than some other literature values reported before, using a different methodology and data. This statement is based on the comparison between an observed GRB rate and a SN Ib/c rate predicted from a model that the paper itself considers an unrealistic hypothesis in the light of current models of stellar evolution. Clearly then, we have no means to predict the SN Ib/c rate accurately and reliably enough to draw new conclusions on the fractions of Ib/c's making GRBs. So, this is a void statement. 3. The paper ends with the conclusions that appear fuzzy and trivial. I copy: 3a. "If the galaxy formation redshift is assumed to be z_f=10, all cosmic histories of SF observationally derived, produce cosmic SN rates which differ little from each other up to z=8." First of all, the model adopted by the authors has no redshift dependence of the SNR/SFR ratio as the minimum mass for WR stars is kept fixed, irrespective of metallicity. Hence, the z_f of the galaxies has no impact. The first part of the statement should be dropped. The reason why SNRs differ little in a proportional model is that SFRs differ little. That should be said. 'Two decades of work on the SFR history have led

	the community to agree on the SFR history.' - that is the real statement here, and this is an observation of the literature, not a conclusion of the author's work. 3b. "In particular, there is not a clear descent of the CSFR and SN rates up to z=8; therefore, all the models which predict a fall for z<8 in the CSFR underestimate the amount of SF at high redshift." The authors are saying that all literature curves of the SFR history they have chosen to show agree in particular in their high-z behaviour; so if anyone else came along and suggested an SF history that was steeply declining to high-z, that person would suggest an SF history that would end up much below the other SF histories shown here at high-z, and it would actually be an underestimate (because we believe the SFR histories in the literature). That is a very profound and trivial statement, and not at all a conclusion from the authors work. 3c. "Studies of GRBs… have suggested that pure monolithic models of galaxy formation in which massive spheroids form stars at very high rate and at very high redshift cannot be ruled out." I don't know how you back this statement up from your paper, other than it could be a general rephrasing of the state of the present literature. However: From the comparisons of the observed GRB rate history with the descriptions of the SFR history, I can see only one model that is ruled out and that is the one where the SNR or SFR goes up by a factor of 100 when we go from z=7 back to z=10 (because the GRB rate stays low). Wouldn't we see those GRBs if they were 30-50 times more common in that z bin?). The GRB rates you use in your paper (though it is a good choice of literature source) suggest that if anything can be ruled out at z>7, then it is the black solid line model (I believe Calura & Matteucci?). At least this form of high SFR at high z is then very unlikely. 4. You also claim in the introduction to your conclusions that "observations suggest that GRBs occur mainly in metal poor objects at variance with the expectations from stellar evolution.", and in other places you insist that galaxies, even irregular ones must follow the mass-metallicity relation. I believe the first statement has long been shown to be wrong. Check e.g. Svennson et al., 2010, MNRAS, 405, 57, to see that the mass difference between CC-SN hosts and GRB hosts is not particularly large (based on Swift data as you use them). Then the metallicity difference should not be very large either. In fact, I do not believe that the GRB host galaxy observations discredit any expectations from stellar evolution so far. If anything, some particular choices of GRB progenitor models (e.g. Yoon & Langer) predicted that you could only make GRBs from low-metallicity stars, and they had to modify their models with time to accomodate the observations of supposedly relatively normal-metallicity GRB hosts. It was the theoretical side that needed to dream up new ways of making normal-metallicity GRBs (which they are doing with more and more success). About part I now:

5. You check different models of making SNe Ib/c by predicting from a historic evolutionary model of the galaxy its present-day SFR and metallicity, and compare it with a present-day average observation of the SNR.

I would have thought a better way is to measure the present-day SFR as well, or if you can't do that, derive it from galaxy relations in the literature, such as a mass-SFR relation and a mass-metallicity relation - then you could see whether the two observations at the same cosmic epoch are consistent with each other assuming different parameter constraints in the making of SNe Ib/c.

Instead you are introducing an additional uncertainty by running a model for those galaxies and producing some results, of which we don't know whether they agree with literature observations. The size of this uncertainty is in no way characterised, and it is certainly increasing the error bar in the comparison of Fig. 3.

Looking further at your model for the two irregular galaxies, I am puzzled to find that galaxies which differ by a factor of 70 in present-day mass are expected to have identical SFR histories (apart from a factor 70 in the normalisation, of course). That seems to disagree with observations of a non-linear mass-SFR relation for galaxies, e.g. Noeske et al. with SFR ~ M^0.65. If e.g. the Noeske relation holds in the mass range of your lowest mass galaxy, then your model underestimates the SFR and SNR normalisation together by a factor of 70^-0.35 ~ 4 or 0.6 dex between the two galaxies. Move up the lines of the lower galaxy relative to the larger galaxy by 0.6 dex, and you get perfect agreement with the Mannucci data.

The statement is then made that Fig. 3 tells us that the solid line for the lower-mass galaxy is ruled out, while it is the line suggested by stellar evolutionary theory (which then motivates the choices for the rest of the paper).

I disagree. Either I apply the toy correction I just suggested and any disagreement disappears to within 1-sigma; or I attach an error bar region to the solid line and it becomes consistent with the data point within 1.5 sigma at the most. So, I don't believe that anything theoretical is being ruled out here - the approach is too unreliable to be quantitatively meaningful.

As a result, part II of your paper gets discredited as well, and the question remains, which statements are left that you make from your own work and that are reliable.

Minor comments:

6. I do observe that most of Sect.3 "Results" is actually a model setup choice and should be in Sect. 2, furthermore Sect. 2 has subsections 2.0.1 without a 2.0, and a 2.1 with no 2.2 … on the same hierarchy.

7. Typos are abundant, even in author names in citations.

	8. You notice how your two model galaxies are very close to the best fit of Maiolino, but you don't say that the larger one of the two model galaxies falls way above the data of Lee. In fact, that one is a galaxy smaller than the LMC and deemed supersolar in your model - that should be unrealistic. 9. Figs. 7 and 8 have incomplete labels (should say 'log') and are redundant. Fig. 7 can be dropped if Fig. 8 is relabelled on the right y-axis with 'log R… +4' I am a little bit at a loss to see the contribution of this paper.

Paper	*Improving constraints on the growth rate of structure by modelling the density-velocity cross-correlation in the 6dF Galaxy Survey* -- arXiv:1706.05205
Preface	The paper “Improving constraints on the growth rate of structure by modelling the density-velocity cross-correlation in the 6dF Galaxy Survey” presents a novel approach on how to combine galaxy position and galaxy peculiar velocity information over the same area of the sky to obtain constraints on the logarithmic growth factor. The paper develops a novel theoretical methodology which is later applied to simulations and real data. The obtained constraints on the data can potentially be reduced when some aspects of the model are improved, as the authors states at the end of the paper. The paper is well written and presented, and therefore I recommend it for publication in MNRAS after the following comments are addressed.
Comments	General comments. 1. The authors have not mention anything on the impact of a potential velocity bias term could have in their results. How such term would affect the growth factor results and how would it enter in your covariance matter formalism. 2. In section 2.2, second column, line 10. The authors state that considering high-mass haloes impacts their ability to recover the growth rate. What is the mass of these high-mass haloes? >10^13.5? Which is the fraction of these high mass haloes with respect to the the rest of haloes (i.e. Num_halo(m>10^13.5) / Num_halo(m>10^13))?. Do the authors know the case of this limitation? Is it perhaps related to some sort of high-mass-halo velocity bias or non-Poison shot noise term? 3. Eq. 7 – 10 display the covariance of delta(x) in terms of the cross and auto power spectra of the delta(k) field. From the derivation in Appendix A, I have the impression that the power spectra quoted in those equations is not the theoretical power spectra, but the theoretical power spectra convolved with the window function of the survey. The authors should comment why they are ignoring the effect of the window function of the survey when applying this methodology to the data and how this assumption could affect their results. 4. At the beginning of section 3.8. I don't understand the sentence “In this case, the largest scale described by the velMPT-breeze power spectrum correspond to kmin=0.0025”. At so large scales, the prediction provided by MPTbreeze for the power spectra (both Pmm, Pmtheta and Pthetatheta) should be essentially equal to the Plin power spectrum. So, by using the prediction from CAMB for Plin the authors could have access to an arbitrary small kmin values. In the case of the data, the authors might take then the kmin corresponding the largest scale of the survey (i.e. kmin~Lsurvey/(2pi)) and ignore (or leave it for future work) the implementation of the super sample covariance terms.

5. In section 3.8 the authors set kmin=0.15 value. How robust are the findings in terms of fs8 if this parameter is slightly varied (let's say between 0.10 and 0.20)?

6. In section 4.1, second column, line 35. The authors show they don't obtain any explicit improvement when constraining fs8 (the error is always ~0.018). The authors speculate that this might be because: i) they do not include any observational systematic in their simulations (I understand they refer here to the effect of sigma_obs here); ii) in GiggleZ n_v>>n_g, and therefore the constraints on fs8 are highly dominated by the velocity field. These findings point out the importance of performing the tests on the model with synthetic data with similar features than the actual dataset (although this is not always possible due to computational limitations). I would like to ask the authors whether it is possible to downsample the number density n_v of GiggleZ to a number which matches the actual ratio of n_g/n_v of the data (rough calculation gives n_v ~ 2.37*10^-5) and re-do the analysis to see whether in that case adding the cross-covariance produces a gain of similar order than the observed by the data (although the absolute values will be different). This would partially solve point ii). In addition, point i) could be addressed by modifying the GiggleZ halo velocities according to a distribution expected by sigma_obs: for instance if you expect sigma_obs to be a Gaussian distribution with a certain mean and variance, input by hand such distribution in the velocities of GiggleZ to make it closer to what you expect to have in the dataset.

7. Section 4.2. The authors interpret the badd=0 as the model with no additional bias parameter required. Later, they show how this case is very disfavored by the data. However, the authors don't mention also the special case where badd=bfit. This case could also be interpreted as the model does not need an additional bias parameter, but just to increse kmax -> kadd=1. Looking at Fig. 5 and 6, this solution seems to be disfavoured “only” by 2sigma, both for data and sims.

Minor comments/typos.

A. After Eq. 6, please define “a” and “H” if has not done it before.

B. In section 3.4, the authors include the observed error in eta from the fundamental plane, sigma_obs, but they don't describe it. Could the authors describe it or provide specific reference to other papers where it has done so.

C. In Fig. 5 caption. I believe the authors mean “The black dashed line” instead of “The lack dashed line”.

D. In Fig. 4. Authors should provide the units of sigma_v either in the plot itself or in the caption.

E. In section 3.6 the authors use CAMB to generate the Pmm power spectrum. Do this correspond to the linear power spectrum provided by CAMB, or the the non-linear power spectrum also provided by CAMB which makes use of HALOFIT? Please, clarify this in the text.

	F. In section 3.6. In linear theory, Pmtheta and Pthetatheta are equal to Plin. I am assuming then, that the authors are using the 1loop (2loop?) SPT provided by velMPTbreeze. Please state the followed approach. If this is correct, why do the authors use Pmm provided by CAMB/HALOFIT and Pmtheta and Pthetatheta provided by velMPTbreeze? Although the authors have their own right to use the model they want for each power spectra, would not be more consistent to use all power spectra provided by MPTbreeze and velMPTbreeze at the same loop order? G. In section 4.1, second column, line 32. It would be useful if the authors could provide the correlation factor between bfits8 and fs8. Also it would be useful to provide such parameter for the data case, between beta and sigma8 in section 5. H. Table 4: I would suggest to include an extra column with the fs8 value (and its errorbar) and an extra row with the case where badd has been treated as a free parameter, so the reader can compare the numbers. Also, from Fig. 8, the author may add an extra case where badd is a free parameter. When these points are added, the authors may comment how the errorbars on fs8 are affected by having badd as free parameter. I. At the end of section 5.2, the authors mention how sensitive is the fs8 best-fit parameter to the cosmology chosen, in this case, WMAP and Planck. The author may mention here, whether this mild dependence may also be used (or could be used potentially in future surveys such Taipan) to constrain the Alcock-Paczynski parameter, alpha_iso.