# Environment paper team response

**Bob**

*1. What is new and different here? We (GZ) and others have published many papers on such quenching questions and I don't see what is unique here? How is this different from the analysis and findings of (say) Gomez et al. 2003, Bamford et al. 2009, Masters et al. (yes, Im a author on these ;). I think it would be good to define your uniqueness and re-iterate that throughout.*

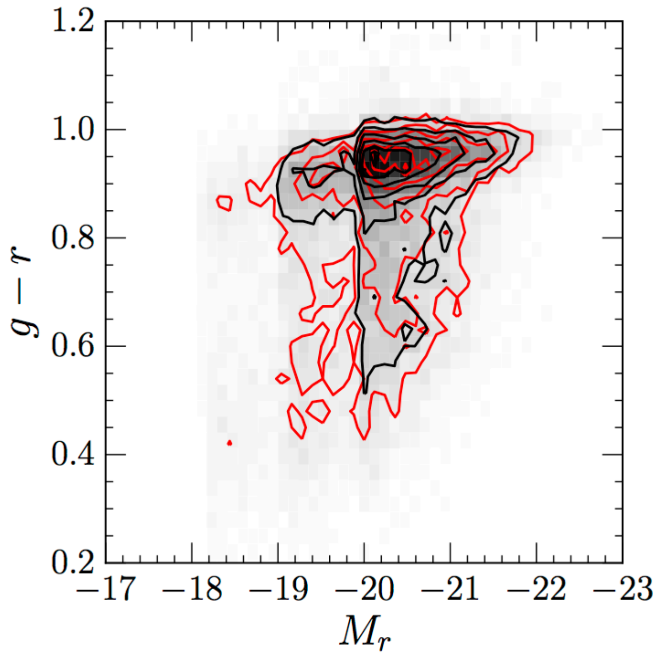
Gomez et al (2003) looked at the SFR and Hα EWs with the group centric projected radius in the SDSS and used these correlations to make assumptions about the quenching mechanisms. Bamford & Masters focussed on studying the changes in morphology with environment. This study builds on these works by looking at both the morphologies and using the colours to infer the time and rate that quenching occurs with group centric projected radius. I think this adds a lot of new information to the literature as it allows us to infer what mechanisms are actually occurring in the group environment from the time and rates that we find. I've tried to state this more clearly this in the updated draft.

*2. I am worried about the errors plotted - are they just sqrt(N) counting errors? I think you may need to investigate how errors in all the other parameters / quantities feed through into these plots. There are errors on the R200, colors, MPA-JHU measurements, fitted tau values, etc. One could do a bootstrap resampling exercise as well to see if the counting errors are representative. At the very least you need to say how the errors are computed and are the realistic?*

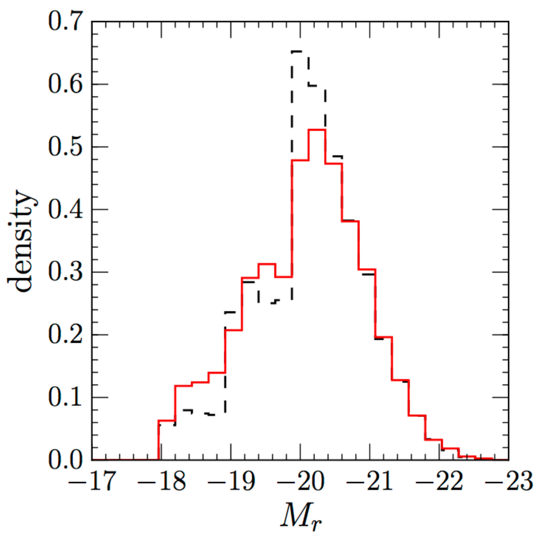
It is just counting errors that are plotted for the morphologies in Figures 4-7. In Figures 8-10 the errors are averaged across the errors calculated from the posterior probability distributions of each individual galaxy in a given bin, which reflects the errors in the colours used as inputs and the degeneracy of the model to give the inferred quenching times and rates. The errors on the calculated R200 are propagated from the errors on the redshift which are ~ 0.0005 and so are minimal and so do not make a big impact on which galaxies are in which R/R200 bins since we have such a large sample.

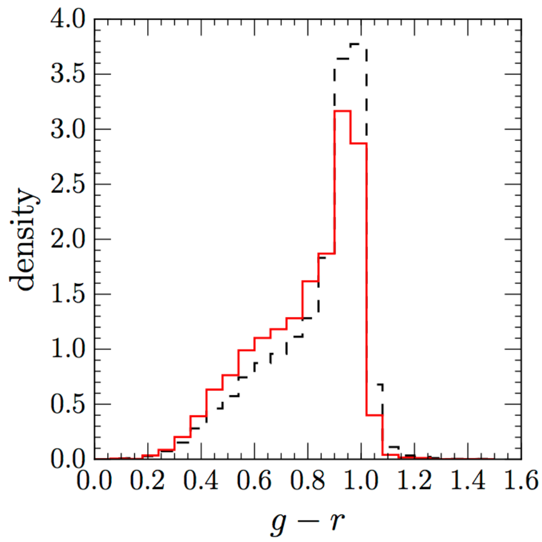
*3. What effect does constraining to the GZ-GALEX sample have on the results? I am worried about this being a biased sample (Figure 1 seems to say the only difference is in the mass scale between satellites and centrals, which seems odd to me). Maybe look at how others parameters compare with and without these constraints?*

We constrain all of these samples to $z < 0.1$ which has been shown by Wyder+07 and Yesuf+14 to give a complete red sequence despite the need for GALEX data. I've stated this more clearly in the paper. I've also made a colour magnitude diagram of the full Berlind sample (black contours) and the GZ2-GROUP sample (red contours) so we can see that there is no bias inherent from cross matching to GALEX at this redshift:




This plot isn't currently in the paper but I could be convinced otherwise. Similarly, here are the histograms in Mr and g-r for both the full Berlind sample (black dashed lines) and GZ2-GROUP sample (red lines). The full Berlin sample consists of three magnitude limited samples that I have collated, hence the sharp transitions at Mr = -19 & Mr = -20.




*4. Likewise, I am worried about the definitions of central / satellite galaxies (how noisy are those definitions?) and I have several worries about the noisiness in the definition of friends-of-friends groups! At low masses, they can have spurious groups (maybe half of low mass groups can be false) and the centroids of these groups is hard to quantify and thus noisy. How are these uncertainties accounted for? We discussed this in Bamford et al.*

To account for this uncertainty, I looked at the local neighbour density using the Baldry measure of Σ calculated by Bamford+09. I found that 90% had Σ > -0.8 which is the definition given by Baldry defining the Σ below which a galaxy would be classified as a field galaxy. I've therefore stated in the paper that we've got 90% purity in this sample. I also investigated the noisiness of the definition of central vs satellite as the most massive galaxy. I found that 96% of the satellites of the GZ2-GROUP sample have a difference in mass from their central that is greater than the error on their stellar mass.

*5. Are fiber collision constraints an issue e.g. for the velocity dispersions?*

Only 71 galaxies were flagged by Berlind as having fiber collision issues so I have removed these galaxies from the sample.

*6. Im not sure how your conclusions link to Figures 7 and 8? Overall, the stellar mass seems the most important variable while others seem to agree within the noise? Again, this was known before (Bamford et al.), so not sure what this adds?*

The conclusions of the previous draft were *very* much a placeholder for the argument about the 'bigger picture' of quenching that I knew I wanted to make as a summary of my past three papers with starpy, and so weren't padded out enough yet to relate back to the figures. I hope with this full draft that's become a lot clearer now.

The stellar mass is definitely one of the more important variables but I'd argue that so too is the mass ratio and the velocity dispersion. I think it's also interesting to see how N_group and the relative velocity aren't an issue. I think it definitely adds something here because we're showing that the quenching mechanism is dependent on the stellar mass not just the morphology, as in Bamford+09.

**Karen**

*Had a quick look today - sorry to take so long. I like the idea of another Galaxy Zoo paper addressing these issues. Been a while since Ramin's excellent work using correlation functions to look into this (Skibba et al. 2010 and 2012 I think). It's nice to pull in both morphology, mass and environment in one story. I know it's an early draft, but with my Project Scientist hat on, can I ask you to make sure to reference the previous work in this area done by the team (as well as outside it also!).*

I have made sure to refer back to the previous works of the GZ team in the introduction and discussion sections, especially Ramin's work as you pointed out. I hope now that paper is in full draft form this is now apparent.

*I find myself worried that the GALEX match will affect the grouping. Group identification is a challenge, so we'll need to pay some attention to that. This is not my area of expertise, but we have it in the term. How does the Berlind et al. catalogue compare to e.g. Yang et al.? Would it be worth having a look at a variety of environment measures (or explaining in the text why we don't).*

Firstly, see above in response to Bob's comment. I did first try and use the Yang catalog but when matched to both GZ2 and GALEX it only gave me ~ 40 group galaxies to investigate which wasn't nearly a large enough sample for what I wanted to study. I think this is because of the GALEX matching and the fact that the Yang catalog is more associated with the cluster/large group environment. We discussed why this might be on a GZ telecon about a year ago or so but I'm not familiar enough with the selection algorithms to claim anything else here. If anybody else has any insight into why this might be it would be much appreciated.

*I'd like to see Figure 1 with different subsets overplotted - e.g. you show all the sample for the comparison but what about when the GALEX match happens, what does that remove? Does the match with the group catalogue remove more? Also the removal of currently SF galaxies - does that overlap well with the GALEX non-detections?*

See the figure above in response to Bob in the CMD. GALEX doesn't remove certain areas of this space as the sample is $z < 0.1$ and so we are red sequence complete.

*The bar fractions seem a bit low in Fig 4.... what is your definition of a barred disc here?*

A barred disc is classed as p_bar > 0.5 in a sample of discs with p_disc > 0.43, N_edge_on_no > 0.715 and N_edge_on >= 20 (as stated in Section 2.4). So its number of bars/number of discs. Is it the field fraction that seems low with p_bar = 0.1? Or all the bar fractions?

*Could you plot average stellar mass as a function of r/r200 - I'd like to see how much that changes in your sample. Would be good to try to remove this from the trends in Fig 4 and 6 - is there enough data for a Peng et al. style 2D plot? The discussion in 4.3 needs to talk about the mass of the barred galaxies.*

This has been added in Figure 3 and shows that the stellar mass doesn't increase with group centric radius until the most inner bin. I've noted this and also noted that the conclusions still hold even if we disregard this bin which is biased by the increase in mass.

*Do we need to worry about more overlaps nearer the centre of clusters contaminating the merger fraction? What's the definition of a merger here?*

Good question, and one I'm not entirely sure how to answer. The definition of a merger is the same as Darg with p_merger > 0.4 (as stated in Section 2.4). Since there's no fibre collisions in the sample (see above response to Bob) I'd be confident in saying that overlaps aren't an issue but I think someone's going to need to back me up on that. If anybody has any insight about this I'd much appreciate it.

*Is it group velocity dispersion (or galaxy) plotted in Figure 8. I think group - but I got a bit confused by the discussion in 4.2 which seems to interpret is as a galaxy property.*

It is stellar galaxy velocity dispersion that's plotted in Figure 10 and I've tried to make this clearer in the text and caption that this is not the group velocity dispersion. It's a property that is often used in galaxy quenching studies and found to correlate with quenched fraction, colour etc.

**Ross**

*In section 2.1.2, you say that your sample is matched in redshift. I'm not 100% certain why this needs to be done, as all of your galaxies are at very low redshift. It might be necessary, I think it could do with a bit of clarification as to what this achieves.*

*I think your paper requires some physical motivation as to why you match in stellar mass-*

*I'm just wondering whether this is representative of the field sample. Again. this is probably just requires some motivation as to why it needs to be done. Again, I'm just wondering what the advantages and disadvantages to this are.*

I think matching in both redshift and mass is important because then there can be no argument that the differences arise due to redshift or mass dependencies in the samples. It means that the only way in which the two samples differ is in their environmental properties and so any differences we find between the two samples can be attributed to the environment. I understand your concerns about it not being truly representative of a true field sample as they may be lower in stellar mass, but then any differences we find could be argued to be because of stellar mass dependencies.

*Just a general note about Fig.2, the histogram bins probably need to be wider, as the data looks noisy.*

They have been made wider, see whether you think this looks better now. Thanks for pointing out!

*My main comments are about Figures 7 and 8. I feel that there are some nice, clear trends in Figure 7. However, I'm not sure your code is applicable to the science you're trying to achieve in Figure 8. In particular, the t+tau modelling gives an overall idea of quenching in a galaxy over a longer time period, so I don't think it will work for short term effects like RPS etc. I (personally) think there's some good morphological results in this paper, and some nice results in Figure 7--- I think there's enough in this for a good paper, maybe comparing these results for different measures of environment, different group catalogues etc.*

Actually the timescales for RPS aren't particularly short. In simulations by Emerick+16, Fillingham+16 (and others I cite in the paper) they find that the timescale for ram pressure stripping is on the order of Gyrs and so this is the timescale we can detect with starpy. Similarly, satellite infall takes place over ~ Gyrs and so we should be able to pick up the environmental effects during this infall with starpy. I think this is demonstrated across Figures 8-10 and reflected in the discussion in Section 4. Happy to have a big discussion about this though if you'd still like to convince me otherwise! :)