Absorption Line Strengths in Cluster Galaxies from \( z=0.8 \) to \( z=0 \)

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Leiden 2006

A. Weiss: We want “models that give ‘nice’ results.”

Collaborators: Illingworth (Lick), van Dokkum (Yale), Franx (Leiden)
The Sample

• **Abell 2256 (z=0.06)**
  - 18 (17) Galaxies
  - Spectra, from KPNO 4m, courtesy of I. Jørgensen

• **Abell 2390 (z=0.23)**
  - 26 Galaxies
  - Spectra from Keck/LRISb

• **CL 1358+62 (z=0.33)**
  - 55 Galaxies
  - Spectra from Keck/LRISr

• **MS 2053-04 (z=0.58)**
  - 41 (40) Galaxies
  - Spectra from Keck/LRISr

• **MS 1054-03 (z=0.83)**
  - 33 (31) Galaxies
  - Spectra from Keck/LRISr

Breeze quickly by this. Just mention 1358 is the reference.
The E/S0s in CL 1358+62: The Reference Sample

R=21mag-limited sample, with a few extras below R=21 for giggles.

55 galaxies from E thru Sbc

15 < S/N < 80 (per A)

33 E, E/S0, S0 galaxies

(see Kelson et al 2000abc for more details)

This work on the E/S0s in CL 1358+62 was just accepted for publication in ApJ and is now astro-ph/0606642
CL 1358+62: Example spectra

LRIS red, 3x45min, S/N from 15–80 per A. sigma_inst=60km/s
1. Velocity Dispersions: \( \chi^2 = | \{ G - \left[ P_M(B(\sigma, v) \circ T) + \sum_{j=0}^{K} a_j H_j \right] \} \times W |^2 \)

2. Indices measured from the "raw" (unconvolved) spectra (where the resolution is about 60km/s)

3. Errors in the bandpasses computed via

\[
R \approx \tilde{R} = P_M(B \circ T) + \sum_{j=0}^{K} a_j H_j \\
\tilde{N} = G - \left[ P_M(B \circ T) + \sum_{j=0}^{K} a_j H_j \right] \\
V_{F_p} = \frac{1}{\lambda_2 - \lambda_1} \sum_i^{n} \tilde{N}_i^2 \Delta \lambda_i \\
V_{F_p} = \frac{1}{n} \sum_i^{n} \tilde{N}_i^2 \\
\varepsilon_{F_p} = \sqrt{\frac{1}{n^2} \sum_i^{n} \tilde{N}_i^2}
\]
CL 1358+62: Details
(Because They Are Important)

Measuring the indices in the 3 exposures of each galaxy, we have computed the distributions for each index, normalized by the expected errors and sample size.

If the errors are correctly estimated, then the distributions should look like Gaussians with standard deviations of unity.

The key to our method is that the determination of the variances in each bandpass does not rely on simple photon statistics.

That our errors have Gaussian distributions is also critical for later analysis.
CL 1358+62: Details
(Because They Are Important)

4. Doppler and Instrumental broadening corrections done simultaneously:

\[ X_{cor}|_G = X|_G + \left[ X|_{TIDS} - X|_{B(\sigma)\sigma T} \right] \]

Why additive and not multiplicative?

Because these are not real equivalent widths and the pseudo-continuum can move dramatically over significant changes in the total broadening.

The estimate of

\[ \left[ X|_{TIDS} - X|_{B(\sigma)\sigma T} \right] \]

is done using each galaxy’s best-fit BC03 model SED.

This also preserves the dynamic range of the indices under higher resolution, while compressing/transforming to the lower resolution Lick system.

Perhaps mention Taylor expansion aspects?
CL 1358+62: Details
(Because They Are Important)

Note there are error estimates for these corrections!

The “plateaus” indicate that the pseudo-continuum is moving around.

Note that the locations where zero correction is required changes with each index.

Errors estimated by using the BC03 template derived using age/Z from fits over different wavelength ranges.
CL 1358+62: Science!
(The science may be important, too)
Question:

How are these observations best-described using the state-of-the-art multi-parameter stellar population models?

We want the most homogeneous set of parameters possible.

Therefore we use only those galaxies that have

\[ \text{H}\delta A, \text{H}\gamma A, \text{CN}2, \text{Ca}4227, \text{G}4300, \text{Fe}4383, \text{Fe}4531, \text{and C}4668 \]

Now down from 33 E/S0s to 19 E/S0s (though a few are fainter than R=21mag).
The first step towards solving a problem is first admitting you have one.

Wait, we can’t quite do science yet.
CL 1358+62: More Details
(doh!)

The first step towards solving a problem is first admitting you have one.

Hello Everybody.
CL 1358+62: More Details
(doh!)

The first step towards solving a problem is first admitting you have one.

Hello Everybody.

My name is Dan Kelson.
Hello Everybody.

My name is Dan Kelson.

And I have systematic errors.
Systematic errors are bad:

1. Jacobians do weird things because the magnitude of systematic errors in each index vary with respect to the dynamic range.

2. Hessians turn into gibberish, and render it impossible to compute valid covariances.

How bad is it?
How bad are the systematic errors? ... beats me. I undoubtedly have all three kinds:

(1) the ones i know i don’t know
(2) the ones i don’t know i don’t know
(3) the ones i know i know

Well then, let’s get rid of them! How?

Because we know a few things:

(1) $<z_f> \approx 2.4$ for massive E/SOs in clusters

(2) the 2nd derivatives in the model grids are fairly small, especially the mixed partials
In an ideal world, free from systematic errors, every index ought to return the same age and metallicity.

So we compute the model (Thomas et al...) indices for a population with

\[
\begin{align*}
\text{age} & = 7 \text{ Gyr (equiv. to } z_f=2.4) \\
[Z/H] & = 0.3 \text{ dex} \\
[\alpha/Fe] & = 0.2 \text{ dex} \\
[\alpha/N] & = 0.0 \text{ dex} \\
[\alpha/C] & = 0.0 \text{ dex} \\
[\alpha/Ca] & = 0.0 \text{ dex}
\end{align*}
\]

The models are offset by the difference between the predicted indices and the mean of the indices for those E/S0s with \( \sigma > 200 \text{ km/s} \).

(more or less the above values are “known” quantities)
(and our results are insensitive to modest changes to these values anyway)
CL 1358+62: The Modeling

So now we can almost do the \( \chi^2 \) minimization:

\[
\chi^2 = \sum [(X_{\text{model}} + \Delta_X - X)/\sigma_X]^2
\]

Where \( X = \text{HdeltaA, HgammaA, CN2, Ca4227, G4300, Fe4383, Fe4531, and C4668} \)

We need a smooth version of the Thomas et al. grids.

This is done by fitting \( X_{\text{model}} \) as a bivariate Bspline in \([Z/H]\) and log \( t \), where the knot coefficients are then fit as multivariate polynomials of the abundance ratios.

By “zeropointing” the models to my data, the SSP parameters are no longer absolute, but are relative to the reference parameters.

But think of what we’ve gained: valid formal errors and an homogeneous set of SSP parameters!
**CL 1358+62: The Modeling**

<table>
<thead>
<tr>
<th>X</th>
<th>( \frac{\partial X}{\partial \log t} )</th>
<th>( \frac{\partial X}{\partial [Z/H]} )</th>
<th>( \frac{\partial X}{\partial [\alpha/Fe]} )</th>
<th>( \frac{\partial X}{\partial [\alpha/N]} )</th>
<th>( \frac{\partial X}{\partial [\alpha/C]} )</th>
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<tr>
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<tr>
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These estimates are valid at the reference point.
**CL 1358+62: The Modeling**

**The Zero-point Offsets for Recalibrating the Models to the Data**

<table>
<thead>
<tr>
<th></th>
<th>$X$</th>
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<th>$X_{(\text{CL1358})}$</th>
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<tr>
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<td>4.422</td>
<td>4.000</td>
<td></td>
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</tr>
</tbody>
</table>

These are the Deltas at the reference point.
R=21mag selection cut is actually fuzzy as expressed in sigma, because of scatter in the Faber-Jackson relation.

Selection effects are even a big deal in this sample. Any plots of age vs sigma have to take into account a fuzzy selection cut (even in a so-called volume limited sample like a cluster). In the field it is far worse since one has Malmquist bias.
What?? We have nitrogen going gangbusters and carbon is asleep at the wheel!

Note too that calcium shows no significant variation as well.

Is the N variation a modeling error? Given the extreme sensitivity of C4668 to carbon abundance, it is remarkable that my data give a slope of 0 (within the errors).
These three had not been fit and so a match to their slopes is actually a prediction of the population trends derived from the other indices.
Are there any sanity checks?

Data from Nelen et al. for those galaxies with low or no OIII.

All the “wide” indices look well-matched.

All the “narrow” indices are not well-matched:

i.e. several Fe indices and Ca4227 shows some oddities, too.

Such narrow indices have the largest corrections for broadening...

Is it my problem, or theirs?

Note that our trends of Z/H,Alpha/N and zero Alpha/Fe variation with sigma match incredibly well the indices of Hdelta,Hgamma,CN,Mgb,Mg2,...
Where does my alpha/Fe discrepancy come from?

If I try to construct indices “the old-fashioned way”, by first convolving my spectra to the IDS resolution, then measure the indices, and then fit a polynomial to the multiplicative corrections using an old metal-rich BC03 SED, and then fit the models to the results... I get $\text{Alpha/Fe} \propto 0.27 \log \sigma$

However, once nitrogen is introduced, none of this matters at all...

Stress that once N is included, none of this seems to matter.
Do we have any other sanity checks?

Using the BC03 models we have predicted the B-V colors of these galaxies given their SSP parameters ($\log t, [Z/H]$).

The observed B-V colors come from WFPC2 data, using apertures equivalent to that used for the spectroscopy and also simulating the seeing during the exposure.

The slope is $0.97 \pm 0.11$ mag/mag.
But how the heck do you get the nitrogen abundance ratio to correlate with sigma?
But how the heck do you get the nitrogen abundance ratio to correlate with sigma?

The answer: You don’t!

For closed-box models which self-enrich, the Z-dependent AGB yields for nitrogen predict a slope of -1 for this diagram.

The slope here is $-1.07 \pm 0.10$ dex/dex.

This explains why the CN–sigma relation is *so* tight. It is not only sensitive to both N and Z but the N is *determined* by Z.
If I had not done the differential analysis, and simply fit the models straight, then we would have recovered, essentially, the same basic trends, though it appears as if we would have claimed that the S0s were slightly younger than the Es at fixed sigma.

But...
Differential vs. Straight Fitting

While similar, general trends would have been found, we would have gotten:

Differential Age vs Z

Straight Age vs Z

Differential N vs Z

Straight N vs Z

This is not to necessarily say that YOU suffer from these issues specifically. This is simply a lesson for those of us who have NO control over many of the systematics!
Quick Summary of the E/S0s in CL 1358+62

- Using only those E/S0s with homogeneous set of 8 indices (HdeltaA, HgammaA, CN2, Caq4227, G4300, Fe4383, Fe4531, and C4668), we have fit for the relative SSP parameters using the Thomas et al models.

- The resulting parameters are homogeneous and have reliable covariance matrices.

- At fixed sigma, the E and S0s appear to have the same age and abundance patterns, down to our limit of R=21 mag (M* at z=0.33)

- Scatter in ages is consistent with that inferred from colors and M/L ratios.

- Steep, tight Z/H-logsigma relation (0.9+-0.2dex/dex) with 0.06 dex scatter.

- No significant Alpha/Fe-sigma correlation. Scatter is Alpha/Fe is 30% larger than expected from errors. Does the old Alpha/Fe-sigma correlation come from historical treatment of broadening corrections? It’s going to be very difficult to make the blue act differently unless the models are totally hosed... it may be easier to tweak up the narrow red Fe indices.

- Steep, tight Alpha/N-sigma correlation, probably due to the N being secondary, and thus really correlated with Z/H. Thus E/S0s had sustained self-enrichment (self-basting). Carbon and calcium show no significant variation.
Quick Summary of the E/S0s in CL 1358+62

While these results go counter to much of what has been discussed previously, it makes for a nice bed-time story:

E/S0s had similar star-formation histories as field spirals (of the same mass) before they entered the high-density environment.

That’s the end of the story, with no complicated scenarios to produce a strong correlation of Alpha/Fe with sigma (which theorists seem to have trouble producing).

Given the typical magnitude of the second derivatives, it is going to be difficult to get radically different results out of the blue (Thomas et al) models. Unless these models are complete junk, and I doubt this given (1) the colors match up perfectly, (2) all of the “broad” index-sigma relations match up with data on nearby galaxies.

But let me move on...
What about that whole “Evolution” business, Dan? We didn’t ask for a lesson on how you found this stuff hard.

There may be some subtle differences but qualitatively, this looks cool!
Working backwards in time...

CL 1358+62 ($z=0.33$)

Abell 2390 ($z=0.23$)

Abell 2256 ($z=0.06$)
And ever further back...

CL 1358+62 ($z=0.33$)

MS 2053 ($z=0.58$)

MS 1054 ($z=0.83$)
And a comparison...

CL 1358+62 (z=0.33)

MS 1054 (z=0.83)

RX J0152 (z=0.83), from Jorgensen et al.
Quantifying these comparisons: Nitrogen

The N-sigma relation looks to be fairly constant... modulo noise, errors, etc. There is a mild hint of some variability in the zeropoint.

I wouldn’t rule out remaining, unknown systematic errors.
Quantifying these comparisons: Carbon

Carbon looks pretty boring in all of the clusters.
Quantifying these comparisons: Calcium

Calcium looks pretty boring, too.
The Alpha/Fe results look fairly consistent (i.e. no slope) but Abell 2256 appears offset. My hunch is that it is a systematic error somewhere since this is still preliminary.

What were my aperture corrections for that cluster again?
Since we now “know” that $D[\text{Alpha}/N]$ is a surrogate for $D[Z/H]$, we can re-fit the models and constrain the two parameters to be equal (but with opposite sign). (If we hadn’t done that, we’d still have similar looking plots but with larger scatter).

Are those zeropoint offsets real? I do not know yet but I am suspicious.
Quantifying these comparisons: $\log t$

Hey, this looks a lot like age evolution, assuming these really are co-eval populations (with a $<z_f>=1.7\pm0.4$).

But are they actually co-eval? Let us re-examine the devolution we did before.
Devolution.

By $z=0.58$, many of the CL1358 galaxies with low sigmas have “negative” age. What do we do with them?

By $z=0.83$, 25% of the CL1358 sample, and $1/3$ of the E/S0s have moved to regions with strong Balmer absorption. 20% of the sample now have negative age. What do we do with them?

Since we “know” the ages of the CL1358 galaxies, we can look at where they’d be at earlier epochs.
eXtreme Devolution.

By \(z=0.83\), 25% of the CL1358 sample, and \(1/3\) of the E/S0s have moved to regions with strong Balmer absorption. 20% of the sample now have negative age. What do we do with them?

These clusters simply do not have the population of massive blue galaxies one would expect if these “negative age” galaxies were present and forming stars.

**Ergo, we assume they have not entered the cluster yet!**

By \(z=1.27\), only half of the sample “remains”, in this scenario, despite the fact that we calculated a mean epoch of formation of 1.7!

More complicated modeling is called for to better assess selection, biases, etc.
Summary

• Qualitatively, the clusters look reasonably consistent with simple evolution/devolution of CL1358+62’s galaxies

• The N-sigma relation seems to be present in all 5 clusters, with possible (small) zpt offsets (real? systematic error?)

• Carbon and calcium, again, no significant variation.

• Alpha/Fe: bupkis (but what does this indicate?)

• Z/H-sigma looks “similar” (though the noise limits to what extent we can say this) in all 5 clusters, with some possible zpt offsets (real? systematic error? environmental effect? and yes, it anticorrelates with the N offsets)

• Once again, we see that cluster galaxies in the past look younger than those at lower redshift. We inferred a mean $z_f=1.7\pm0.4$ (so far) but this is biased by the fact that we can only use those galaxies that are in the cluster at each epoch...

• Devolution of the low-z clusters to high-z gives us a picture where we must account for many zero/negative age galaxies that simply do not appear present in our high-z CM diagrams.

• Should we assume they have simply not entered the clusters yet and that substantial evolution has been occurring, despite the apparent uniformity of ages at low-z?