Analysis Document: Weak Lensing Calibration with N-body Simulations

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1 Project Description

The goal of this project is to measure and improve the systematic uncertainty from mass-profile modeling in weak lensing. In the Weighing the Giants analysis, we assumed an NFW density profile. Does that bias the mean mass of the sample? How well can we constrain that bias? Starting at that point, I am to answer some of the following questions:

- What is the bias when you fit an NFW halo? What is the optimal radial range to fit, in terms of bias, systematic uncertainty and statistical uncertainty?
- Is the NFW profile the best description? Do other profiles work better? Are there other ways to measure masses (eg Mass Aperture) that are better?
- How does this evolve with cluster mass, and with redshift? Are there other predictors?
- How do we best handle the mass-concentration relation?
- How do we best handle the prior on cluster mass?

- Are there differences in predictions from different simulations, and from different ways to simulate lensing?

The simulations may also give insights into more advanced topics, such as

- Can we measure cluster concentrations?
- Can we measure cluster ellipticities, and over what radial range?
- If designing a survey from scratch, how should we do it?

There are currently three analysis efforts that these results feed into. The first is the Stanford Weighing the Giants work, with Steve, Adam, and Anja, working with wide field, ground-based observations from Subaru. Then there is the SPT efforts at low and high redshifts, with Tim and Joerg. The high-z work uses HST observations, whereas the low-z work uses Megacam @ Magellan. Finally, this work also feeds into the LSST effort.

2 Log

15 July 2013 Lots of development work during the March period that was undocumented. Basically, got running on Becker's simulations. Oddly, though, I couldn't reproduce his results. Not sure why, but it may be because I am only looking at the most massive halos while he was fitting biases to all halos. While there are many things that I can do to try to recover Matt's results, I don't know if I can. First step is to go back to my results, and just figure out what is going on.

01 Jan 2013 Start of this work package. First step is to clean up old code that I've been using in bonnpipeline and get it running with Will's simulated catalogs.

06 Aug 2013 I've recieved from Jiayi in Munich a distribution of SZ - halo center offsets from a new set of hydro sims they've done. I'm going to explore the offset distributions and effects on an NFW halo in an ipython notebook called: simulated_offsets.ipynb .

19 Feb 2014 After a long gap in this notebook, I'm back to updating it and tracking my research investigations. Since I've last updated, I've added two new simulation sets, the BCC and the MXXL. I've implemented a uniform analysis, and have begun exploring different radial ranges, as well as differences between the two simulations. I've updated all sections of the notebook to try to capture these pieces.

3 Guide to Files & Software

TODO: Fill in guide to software

4 Available Simulations

I have access to 3 sets of simulations:

BK11 : Cut-outs around massive halos at redshifts z=0.25 and z=0.5, used in Becker & Kravtsov 2011. Particles are extracted in a 20x20x400 comoving Mpc/h box around each cluster and are projected to form a 2-D mass map. From that, a shear signal is calculated, assuming sources are at z=1

BCC : A simulation of a large sky-area survey covering a continuous patch of sky. Three boxes of decreasing resolution are appended and are used to form a past light cone of the visible universe. The simulation ray-traces shear from each galaxy in the simulation to the observer at z=0. I have extracted galaxies centered around the lensed central position of each massive halo in the simulation. From Wechsler, Becker, and Buscha, priv comm.

MXXL : A large box where snapshots at particular redshifts are written to disk. I currently have only the z=1 snapshot. Sources are assumed to be at infinite distance (not infinite redshift). Shear is calculated by projecting masses to a plane in a box (of unknown size) around each massive cluster. I have 3 projections for each cluster.

5 Are the simulations consistent?

In the most basic test, do I recover the same bias as a function of mass from each simulation? There are a few reasons why I might not.

- Each simulation probes a different redshift range, so I should be able to probe redshift evolution. Thankfully, the BCC bridges between the MXXL and BK11, while I will also hopefully receive additional snapshots from the MXXL.
- Each simulation uses a slightly different cosmology. That may mean that the mass-concentration relations are different, which may again lead to different biases.
- Each simulation is at different resolutions, so there could be resolution issues, especially near the cluster centers.
- The BCC ray traces over a past light cone, whereas BK11 and the MXXL ignore LSS evolution and don't ray trace
- The density of points where the shear field is measured is different. Bahe+2012 shows that lower density surveys / noisier surveys average over / miss some substructure, leading to different bias results.
- There may be a bug in how I'm reading in one of the simulations. This was previously a problem with the BK11 simulations, where I first missed factors of h, and then there was ambiguity about at what redshift the lensing signal was calculated for.

5.1 Basic Analysis Approach

TODO: Fill in details here on how exactly I do my fits.

The fit is encoded in nfwfit.py.

5.2 Comparing Mass Bias vs Radial Range & M-C Relation

I've run mass fits with three mass-concentration relations: c=4, the Duffy08 relation, and allowing c to be fit freely.

I've run fits for 15 different radial ranges: 3 Inner radii 0.25, 0.5, 0.75 Mpc, and 6 outer radii 0.5, 0.75, 1.0, 1.5, 2.5, 3.0. The radial ranges are encoded as follows:

- 'r1' : '0.25 - 0.5 Mpc',
- 'r2' : '0.25 - 1.5 Mpc',
- 'r3' : '0.25 - 2.5 Mpc',
- 'r4' : '0.25 - 3.0 Mpc',
- 'r5' : '0.50 - 1.5 Mpc',
- 'r6' : '0.50 - 2.5 Mpc',
- 'r7' : '0.50 - 3.0 Mpc',
- 'r8' : '0.75 - 1.5 Mpc',
- 'r9' : '0.75 - 2.5 Mpc',
- 'r10' :'0.75 - 3.0 Mpc',
- 'r11' :'0.25 - 0.75 Mpc',
- 'r12' :'0.25 - 1.0 Mpc',
- 'r13' :'0.50 - 0.75 Mpc',
- 'r14' :'0.50 - 1.0 Mpc',
- 'r15' :'0.75 - 1.0 Mpc'

When I assume either c=4 or Duffy08, I see that the simulations produce different results. The slopes of bias vs mass change, as well as the absolute normalizations. The three simulations appear to respond differently as well. See figure 1. The curves seem to shift some between simulations. But since I am refitting the same halos, I would expect that the shifts in each simulation are real. Only when I go to cfree are the BCC and MXXL seemingly consistent. BK11 is hanging out by itself for some reason, which I don't understand.

The agreement between BCC and MXXL seems to be fit range dependent. The above fits covered a range of 0.5 - 3.0 Mpc. If we only look at the outer region for cfree, the fits are again good.

See figure 2.

However, if we look at the innermost fit region, 0.25-0.5 mpc, we can see that the two simulations are drastically different. BCC and BK11 show diving calibrations at the high mass end (0.2 for 10^{15} halos!?), but MXXL holds relatively steady up to the highest masses.

See figure 3.

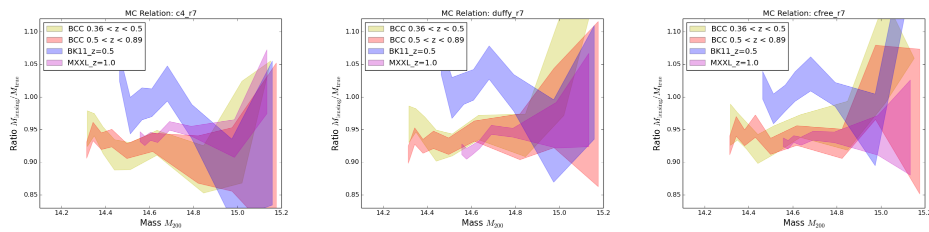

Figure 1: Plot of bias versus mass. Left: Assuming c=4, Center: Assuming Duffy08, and Right: C is a free parameter. The fit range is 0.5 - 3.0 Mpc. The width of each colored band represents the 1σ uncertainty in the median bias for each mass bin. Bins are adaptively created to balance signal-to-noise with exploring the mass range.

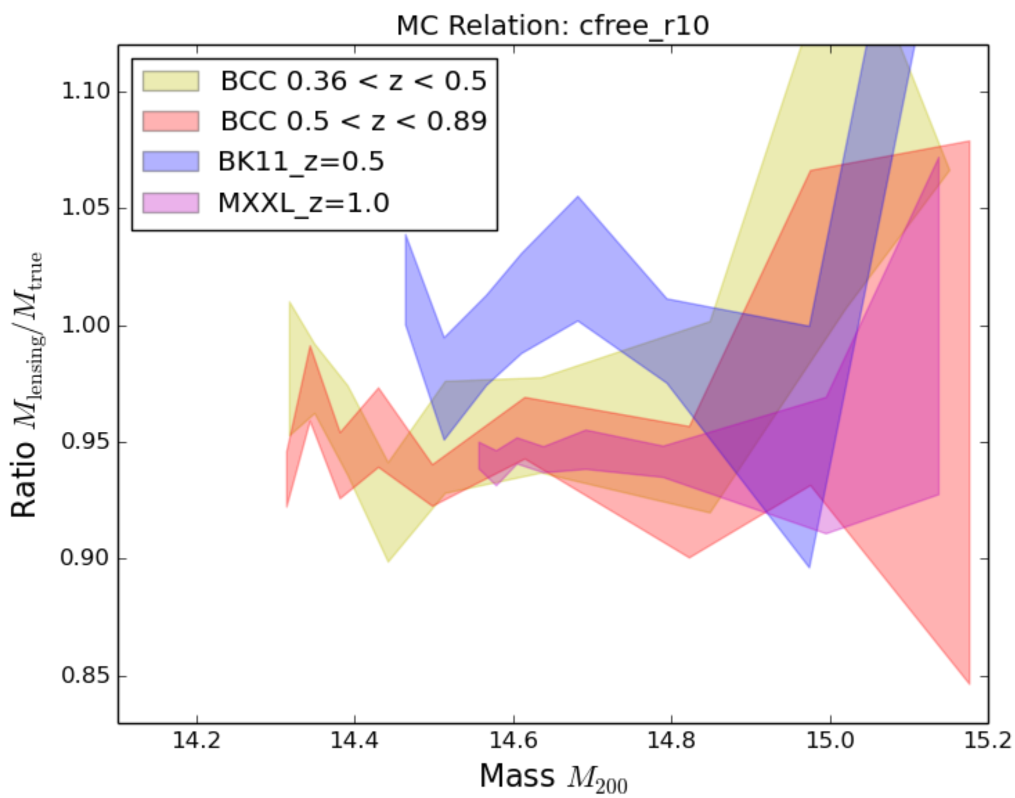

Figure 2: Same as fig 1, except now only for cfree and 0.75-3Mpc.

Could this be a resolution effect? Or maybe how the shear signal is calculated, since we are getting close to the critical curves, though I would think if that was the case BK11 and MXXL would be more consistent. We also should only be seeing shears of $g \approx 0.3$ at 250kpc for z=1 and 10^{15} solar masses.

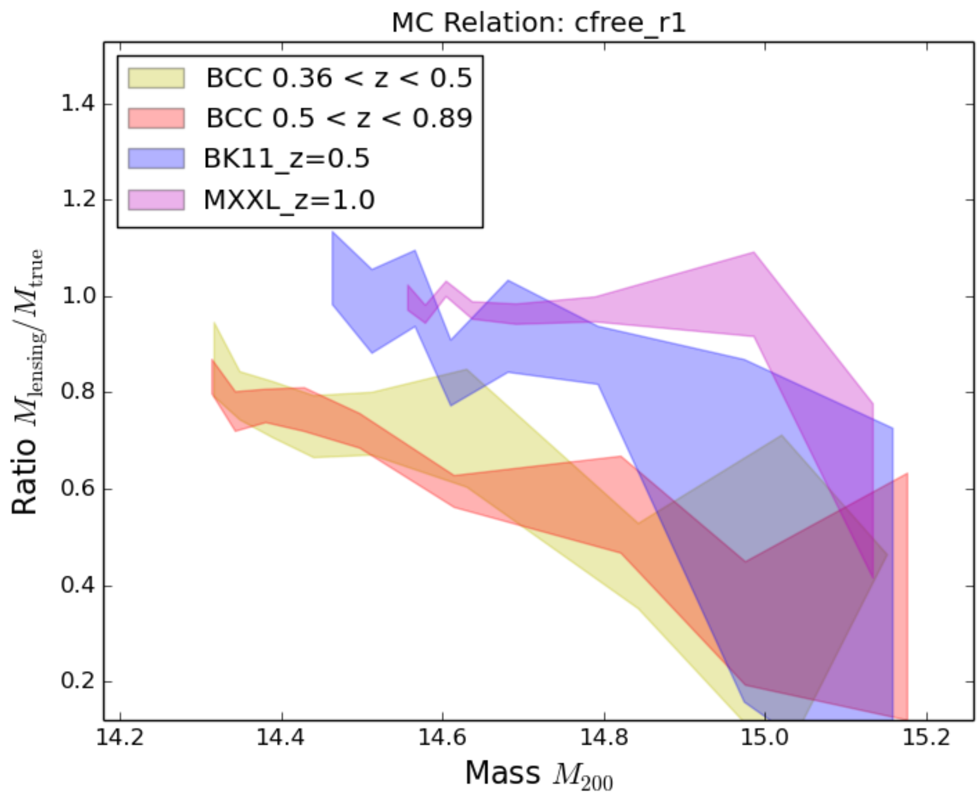

Figure 3: Same as fig 2, except for on a radically different scale.

6 Collaborators, Acknowledgements, Debts

6.1 Collaborators

Stanford

- Steve Allen
- Anja von der Linden
- Pat Kelly
- Adam Mantz
- Glenn Morris

Bonn

- Tim Schrabback
- Peter Schneider

SPT

- Brad Benson
- Joerg Dietrich

MXXL Simulations

- Stefan Hilbert

BCC Simulations

- Risa Wechsler
- Matt Becker
- Michael Buscha

BK11 Simulations

- Matt Becker