Cosmology before noon
(large-scale structure at $2 < z < 6$)

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Large-scale structure at $2 < z < 6$

The opportunity:

- For the last decade CMB surveys have dominated constraints on ΛCDM+ models, with LSS in a supporting role.
- Progress requires we rebalance this.
  - Current $z < 1$ LSS-only constraints on ΛCDM parameters are (nearly) competitive with those from CMB ...
  - ... in the future LSS should overtake CMB for some cosmological constraints.
  - Steady, incremental improvements become qualitative change — "Quantity has a quality all its own" (Stalin)!

Continuous advances in detector technology and experimental techniques are pushing us into a new regime, enabling mapping of large-scale structure in the redshift window $2 < z < 6$ using both relativistic and non-relativistic tracers ...

Next-generation science drivers

In the absence of a clear signal of new physics currently ... I will consider high-precision tests of the SM and GR with a focus on LSS

- Expansion history (BAO)
- Curvature
- Primordial non-Gaussianity ($f_{NL}^{\rm loc}$, $f_{NL}^{\rm eq}$, $f_{NL}^{\rm orth}$)
- Primordial or induced features, running of n_s
- Dark energy during MD
- DM interactions, light relics ($N_{\rm eff}$) and neutrinos

Probe metric, particle content and **both** epochs of accelerated expansion ... with high precision

Maximizing S/N

I want to maximize the S/N for new, BSM, physics

- There are many possible extension to our SM (ΛCDM+GR).
- To my mind none are more compelling than others.
- If theory can't give us guidance, maybe phenomenology can?
  1. Work where inference is clean.
  2. Look where we haven't looked before (frontier!).
  3. If you don't know how to maximize S, then minimize N!

Push to higher redshift, in the epochs before cosmic noon!

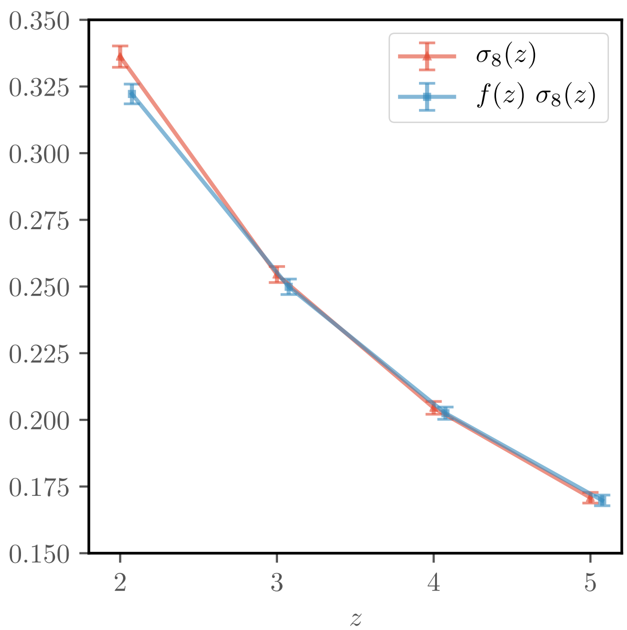
Advantages of high z

Moving to higher z gives us four simultaneous advantages:

1. Wide z range leads to rotated degeneracy directions.
2. Larger volume.
   - More than $3\times$ as many "linear" modes in the $2 < z < 6$ Universe than $z < 2$.
   - Large volume $\Rightarrow$ small errors at "low" k, increased dynamic range to break degeneracies.
3. More linearity and correlation with ICs.
   - Get "unprocessed" information from the early Universe.
4. High precision theory.
   - Low k modes are under good "theoretical control" using PT, little need for "nuisance parameter marginalization".
   - Everyone loves PT when you can use it – QED, Fermi liquids, CMB, ... LSS!
   - Theory becoming very advanced: lots of cross-fertilization with GR, CM and theory colleagues. New ways of merging N-body and PT techniques.

LSS at high-z offers many of the advantages of CMB anisotropy!

One example: growth rate



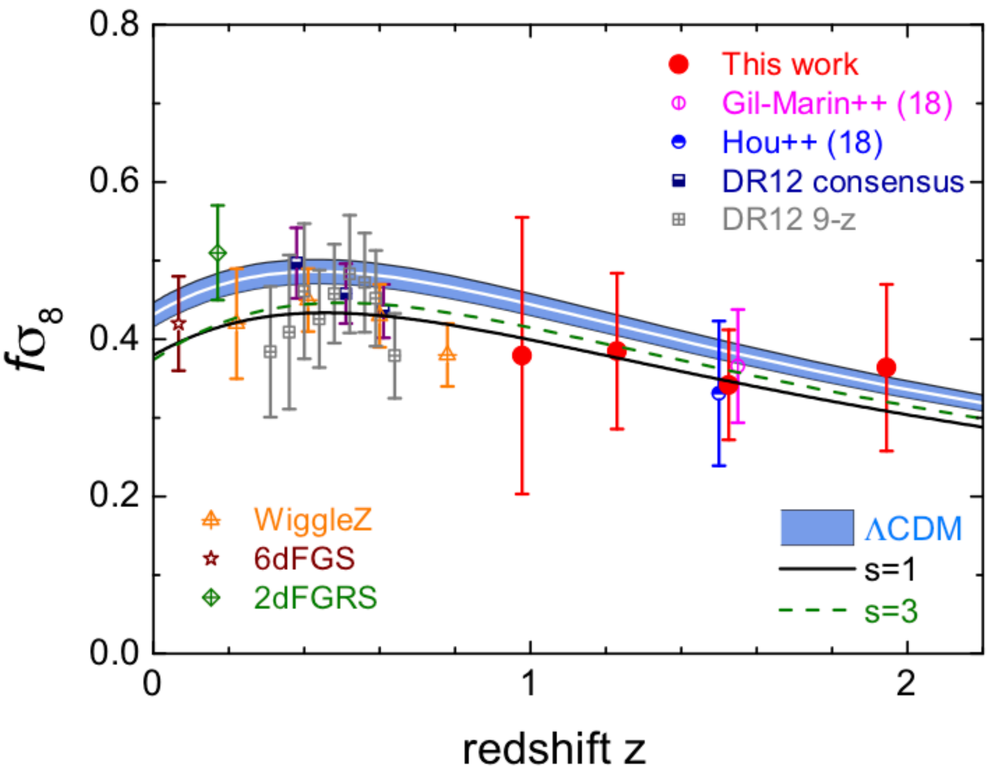
- Between $z \simeq 10^3$ and today, fluctuations grow by $\sim 10^3$.
- GR+ΛCDM predicts growth very precisely.
- Marginalizing over unknown parameters, growth is predicted to 1.1% vs. z (dominated by m_ν uncertainty).

Is GR+ΛCDM right?

[Along the way test gravity model, expansion history, contents, ...]

Growth rate

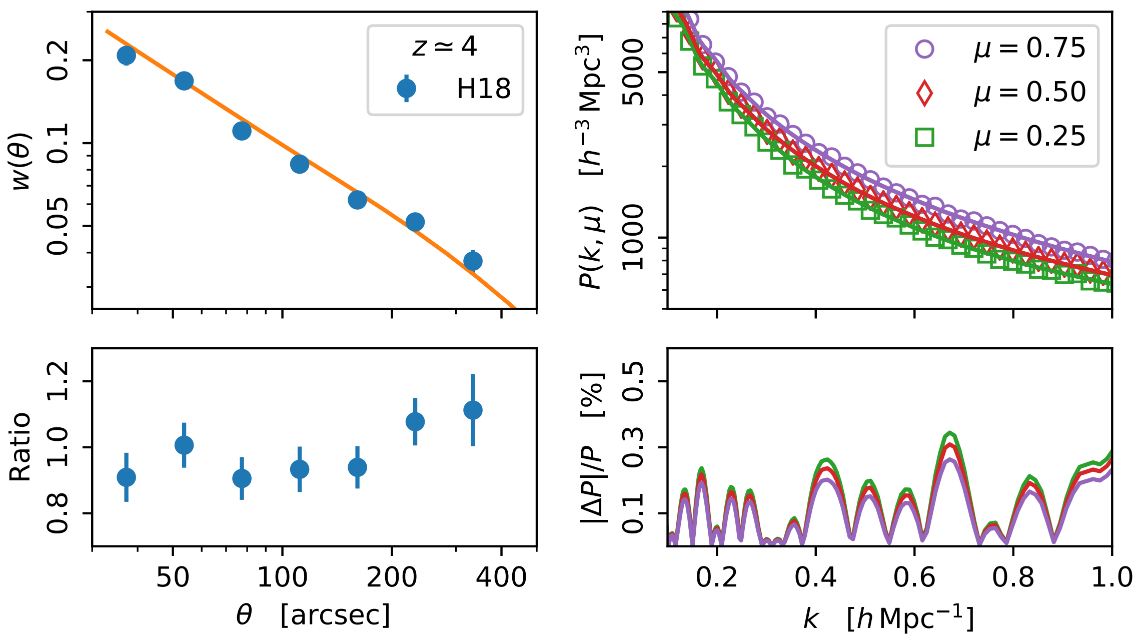
We are far from making a 1% test ...



Zhao+19

Theory "error"

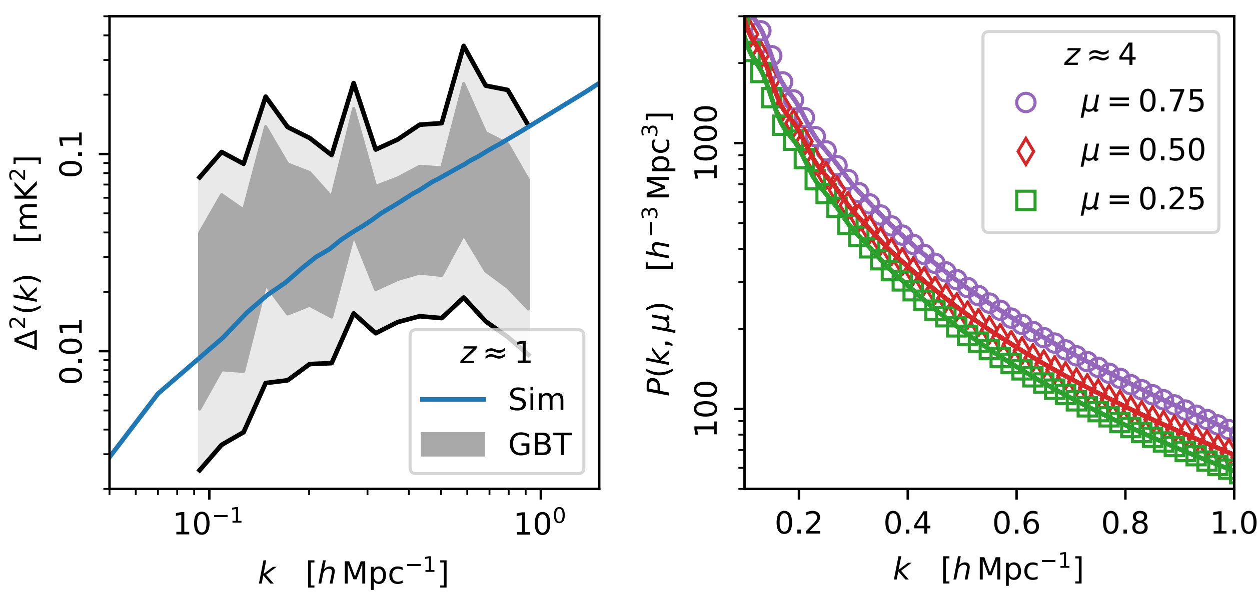
Out-of-the-box comparison of two, public, theory modeling codes



Over half the sky, within $3.5 < z < 4.5$ there are over a billion modes out to $k = 1\,h\,\mathrm{Mpc}^{-1}$!

Theory "error"

There's nothing special about galaxies here ... HI would work too!



What probes of the $2 < z < 6$ Universe will we have?

What tracers can we use to probe the $2 < z < 6$ Universe?

- We can build upon deep imaging surveys (LSST).
- We can make use of planned CMB surveys.
- We will have satellite data (SPHEREx and Euclid + Roman?).
- We want spectroscopic information where possible.
  - Galaxy and QSO redshift surveys.
  - Intensity mapping.

CMB = lensing at high z

We are witnessing a rapid scaling up of CMB experimental sensitivity as we move into the era of million-detector instruments!

- A natural "by-product" of next generation CMB surveys to constrain primordial gravitational waves is high fidelity CMB lensing maps – probing the matter back to $z \simeq 1100$.
- It's hard to do cosmic shear at $z > 2$.
- Lensing is sensitive to mass, not light.
- By using a relativistic tracer it gives access to the Weyl potential.
- But lensing is projected ...
- ... lensing + galaxy surveys offer redshift specificity, higher S/N and lower systematics. Natural synergies: greater than sum of the parts!

Tracers of LSS at $2 < z < 6$

- There are lots of galaxies at high z, and we have pretty efficient ways of selecting them.
  - Dropout, or Lyman Break Galaxy (LBG) selection targets the steep break in an otherwise shallow F_ν spectrum bluewards of 912Å.
  - These objects have been extensively studied (for decades!).
  - Selects massive, actively star-forming galaxies – and a similar population over a wide redshift range.
  - LBGs lie on the main sequence of star formation and UV luminosity is approximately proportional to stellar mass.
  - A fraction of these objects have bright emission lines (LAEs).
- BBN $\Rightarrow$ there's lots of Hydrogen as well!
  - Hyperfine (mag. dip.) transition of H I ($p + e$ spin-spin coup.)
  - Very rare transition per atom ($\propto \mu^2/\lambda^3$):
  - Little absorption or confusion (no line at 710 MHz!).

Many ways of using this information

- There are many ways of combining these data to get at the science I emphasized earlier.
- You've no doubt seen (or will see!) many forecasts from individual surveys.
- Spectroscopic observations at high z are key!
  - LSS evolves – if we don't know at what z the objects are we don't know what epoch we're measuring.
  - Need to reject interlopers, weight tracers, ...

Thoughts

- With SPHEREx/LSST/Euclid/Roman will have deep imaging/target catalogs for optical spectroscopy
  - Combine data to calibrate photometry on large scales?
  - For dropout selection deeper u-band imaging is valuable.
- The community is already planning or building next-generation instruments.
  - To determine "observational costs" need pilot studies, R&D.
- Need to develop and build new multi-survey phenomenology.
- Need to develop and build new multi-survey analysis tools.
- Would gain from funding experiment-agnostic "phenomenology" schools to train the next generation of "theoretically sophisticated observers" and "observationally savvy theorists" who can work across surveys.
  - Could bridge back-to-back collaboration meetings.

Conclusions

- There are **many** (quasi-)linear modes left to map!
- These will allow precisions tests of SM and GR, and improve constraints on parameters by substantial factors (or find something new!).
  - Already (several) percent-ish level constraints at lower z are turning up much-discussed "tensions".
- If theory can't give us guidance, maybe phenomenology can?
  - Work where inference is clean.
  - Look where we haven't looked before.
  - If you don't know how to maximize S, then minimize N!
- The best observational approaches are still TBD.
  - Pilot programs and R&D
- This presents an interesting, and very 'principled', theoretical challenge.
  - ... and no doubt there will be a large role for simulations (theory, mocks, end-to-end), new ML tools and "big data" too.

The End!