Monday, 22.06.2020				
Time UTC	Time CEST	Speaker	Subject	
12:50 - 13:00	14:50 - 15:00	Richard Anderson	Welcome & announcements	
13:00 - 13:25	15:00 - 15:25	Adam Riess	SH0ES	Q&A
13:25 - 13:50	15:25 - 15:50	Rachael Beaton	Chicago-Carnegie Hubble Project	Q&A
13:50 - 14:15	15:50 - 16:15	Silvia Galli	Planck and the CMB	Q&A
14:15 - 14:40	16:15 - 16:40	Lloyd Knox	The theoretical picture	Q&A
14:40 - 15:10	16:40 - 17:10	Panelists & speakers	Discussion	Q&A
		Panelists: B. Javanmardi, E. McDonough, D. Huterer, J. Mould, W. Freedman		
		Link to YouTube Recording		

01 Adam Riess - SH0ES

✍️ There are already 7 questions with 40 upvotes. Here are the most popular ones:

What's your favorite theoretical explanation for the problem?
by Richard Anderson (ESO) | 17 upvotes

Didn't Tully-Fisher distances previously
by Tom Shanks (durham) | 7 upvotes

What does a complete sample of SNe Ia refer to? All measured on a single photometric system?
by Anonymous | 7 upvotes

Can we also exclude the crowding needed to decrease the tension from 5 to 3 sigma? (i.e. not to remove the whole 5 sigma, but just enough to remove discrepancy)
by Valeria Pettorino (CEA Paris Saclay) | 4 upvotes

What can you gain by getting more optical color information for more distant Cepheids?
by Richard Anderson (ESO) | 3 upvotes

Latest question

In recent works, you assume zero host reddening to a disk field in N4258. However, Macri+06 found evidence to the contrary. How are these results reconciled?
by Taylor Hoyt (uchicago) | *No upvotes*

Adam Riess 9:30 PM

- Let me respond to a few questions for me above. I was asked how far out we have done the direct crowding measure with Cepheid amplitudes. I misspoke and said 20 Mpc (NGC 1559) its actually 25 Mpc (NGC 2525) and this distance now includes half of our calibrator sample and we get the same H0 when we only use the closer half.
- I was asked if you had some unrecognized crowding (like a couple of sigma tension with the amplitude data) could you lower the H0 tension to 3 sigma? The answer is not really, by adding two lower tensions, the product of unlikely events is preserved.
- Knox asked Wendy and I why NGC 3370 Cepheid and TRGB disagree? My understanding is that NGC 3370 (and NGC 1309 and 3021) are a) beyond the range for TRGB b) TRGB was measured by a different team and c) TRGB was measured near the spiral outskirts but not really in the halo. The completeness for TRGB-like stars is only ~50% and loss of TRGB stars (and their colors) could impact the TRGB measurement. So I think this object is not in TRGB's comfort zone.
- What is a complete sample of SNe Ia mean? It means to include all the SNe within a certain redshift range that pass other selection criteria that are independent of distance or magnitude.
- **Lloyd Knox** 17 days ago thanks, Adam for addressing my question.

02 Rachael Beaton

✍️ There are already 7 questions with 21 upvotes. Here are the most popular ones:

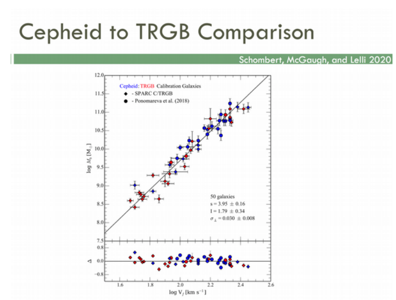
How many more galaxies do you expect to have both Cepheid and TRGB distance measurements for soon? How many are needed to explore the discrepancies?

by Matthew Colless | *9 upvotes*

- **Rachael Beaton** 17 days ago

Great question. We have a program on-going to homogeneously process as many SNe Ia hosts with Cepheids as possible. I think we can get the number to 20, which at least is a distribution that we could divide the sample and learn something.

We could also did deeper into local distances -- there are a couple of analyses that show overall agreement between the techniques, but like I try to emphasize we need to make sure we are comparing apples-to-apples in terms of understanding differences in more distant galaxies.



Here's an independent comparison where galaxies are plotted by external data and we can compare the scales.

- **Marina Rejkuba** 17 days ago

Hi @Rachael Beaton , nice talk! I have a follow-up on this question: in NGC 5128 (Centaurus A) the Cepheid distance (Ferrarese et al. 2007) has a much larger uncertainty than TRGB distance (Harris et al. 1999, Rejkuba 2004). That is I believe mostly due to difficulties in dealing with extinction corrections for Cepheids. Cen A is an elliptical and as such an interesting “different environment” where one can compare Cepheid, TRGB, PNLF, SBF, Miras, etc distance determinations

- **Matthew Colless** 17 days ago

Thanks for the detailed response. (And great talk, btw!)

- **Rachael Beaton** 17 days ago

Thank you! oh, hey it is not a terribly time in Austrailia!

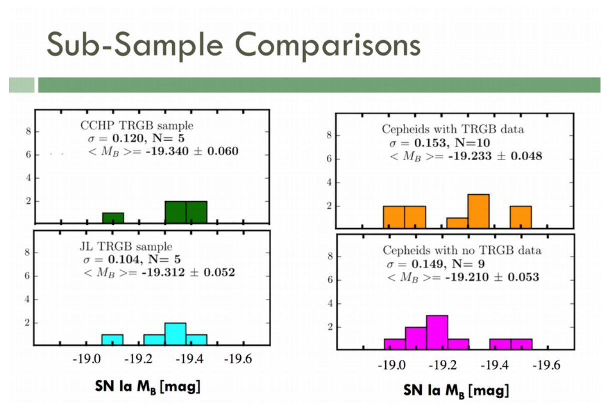
Good point @Marina Rejkuba -- we have not fully mined what we can learn in the nearby Universe!

Could you say more about how your H0 result depends on individual galaxies : how do you vary if you exclude a given one?

by Anonymous | 3 upvotes

- **Rachael Beaton** 17 days ago

  I don't have a full jack-knife result at hand, but that would be an interesting thing to do. I can say that doing some break-ing down of the samples from Freedman et al. 2019, we just consistent calibrations with sub-samples.



- **Wendy Freedman** 17 days ago

I did a series of jack-knife tests and the results do not vary significantly.

- **Rachael Beaton** 13 days ago

[because it is missing on the backup slide and I cannot edit the original post]
I should note the plots I showed here are from Freedman et al. (2019) and discussion of sub-samples is presented there. The plots in the talk were also in Freedman et al. (2019). The differences in the Ceph and TRGB distances are shown both in the talk and in the paper.

How strongly does the adoption of Omega_M=0.315 with on uncertainty from Planck affect your H0 result? (Eq. 6 in Freedman+2019)

by Richard Anderson (ESO) | 3 upvotes

How much of an issue is the tip of the AGB for you in practice?

by Richard Anderson (ESO) | 2 upvotes

- **Rachael Beaton** 17 days ago

Good question. our injection tests include AGB stars above the TRGB matched to the star counts about the TRGB (for the luminous ones) and then extending below the RGB where it is really asymptotic. We do think that our methods include a consideration for contamination in the metal-poor halos.

Usually in old, low mass stellar populations we don't have to worry too much because of the relative time scales that low mass stars spend on RGB vs. AGB.

Where you **really** have to worry is in young populations where the more massive stars are populating different parts of the space -- you can see those populations in some of the more challenging measurements in disks and there it might be impossible to get a precise and reliable answer.

Do you consider any extinction coming from the host galaxy? Especially when the orientation of the galaxy is not perpenticular to ous?

by Anonymous | 2 upvotes

Latest question

Tully's EDD database gets -4.01 for NGC 4258 as well (Madore field), Reid et al gets -4.03 with Madore field.
by Adam Riess | 1 upvote

- **Rachael Beaton**

Here's our "all TRGB" comparison from the forthcoming paper. I don't have the individual studies labelled here -- but the TRGB's themselves are in good agreement with uncertainties. I would have to double check how the Rizzi et al. 2007 calibration is employed in N4258, even in the halo it has a red-TRGB, so the color-component in the EDD calibration might be part of it. Deep Anand might have more to add, but I don't see him here.

CCHP X. The TRGB in NGC 4258 15

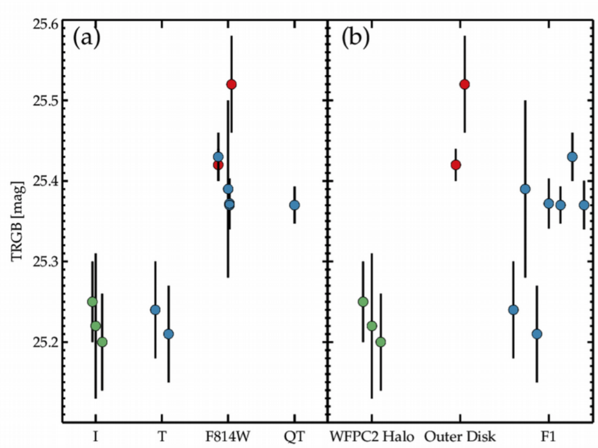


Figure 6. Comparison of TRGB measurements from the Literature. (a) The prior TRGB measurements are grouped by the photometric system and color-coded by the *HST* field being studied. (b) the prior TRGB measurements are grouped and color-coded by the *HST* field being studied. These visualizations reveal systematics between the filter systems and hint at possible differences between the field locations.

- **Rachael Beaton** 17 days ago

@Taylor Hoyt have you thought any more about the EDD result?

- **Adam Riess** 17 days ago

That is from Jang and Lee 2017 for NGC 4258 and Madore set

NGC 4258
-4.021 ± 0.067
-4.017 ± 0.067
[illegible]

- **Marina Rejkuba** 17 days ago

Rizzi et al. 2007 calibration is -4.05

- **Rachael Beaton** 17 days ago

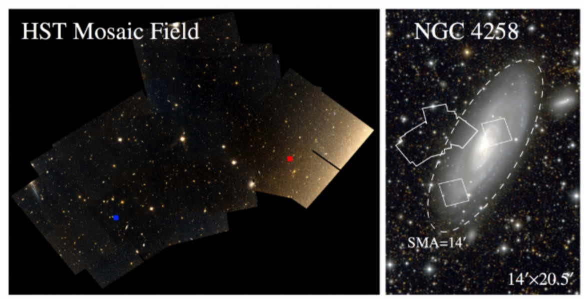
Interesting -- the Tips themselves are identical on the blue-TRGB. There is a bit of a turn down in the N4258 that might be it?

- Adam Riess 17 days ago

But 0.04 mag is quite a shift, no?

- Rachael Beaton 17 days ago

For our new work we use everything outside of a projected SMA of 14 arcmin, because we do see gradients in the surface brightness.



- **Adam Riess** 17 days ago

Friday you said -4.02, no?

and that it agreed with Jang and Lee to 0.01?

Did you add new NGC 4258 extinction?

- **Rachael Beaton** 17 days ago

Oh, sorry I might have not been super clear -- our TRGB agrees within 0.01 mag

There is a tiny shift in the 4258 maser distance -- but I don't think that is enough.

- **Adam Riess** 17 days ago

Have you added extinction for NGC 4258? I think your slide said so?

- **Rachael Beaton** 17 days ago

Internal extinction?

- **Adam Riess** 17 days ago

yes

- **Taylor Hoyt** 17 days ago

We find no evidence for reddening in the fields we have chosen to use for the TRGB calibration

- **Adam Riess** 17 days ago

So is the TRGB peak in NGC 4258 still m_I~25.38+/- 0.03? as from past analyses?

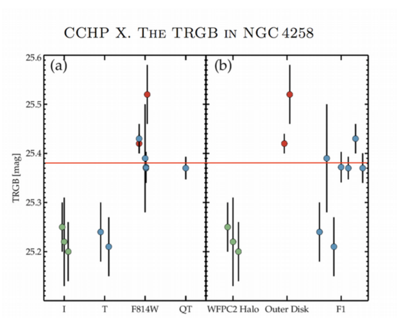
(Tully EDD 25.43, Madore 25.36, Macri 25.39.

- **Taylor Hoyt** 17 days ago

It is based on a selection that is different than went into each of those fields. For ref, Macri+06 found I= 25.42 not 25.39

- **Adam Riess** 17 day

The line I added is the mean people have claimed in lit.



M_TRGB
mag
-4.01

- Taylor Hoyt 17 days ago

which literature? Is that a mean of every point on that figure?

- Adam Riess 17 days ago

This is from Tully’s EDD for NGC 4258

That is what Rachael sent, I echoed back

- **Taylor Hoyt** 17 days ago

Yes, Tully et al. do use the Rizzi+07 calibration

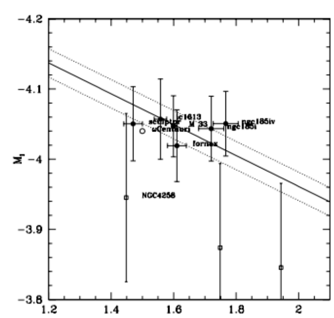
(referring to your screenshot of M_TRGB from the EDD)

- **Marina Rejkuba** 17 days ago

This is screenshot from Rizzi+07. Note the dependency on color.

points, was assumed to be the final error. To derive the zero point for the *HST* flight system, again we used the conversion relation presented in Sirianni et al. (2005) for ACS and the one in Dolphin (2000a) and Holtzman et al. (1995) for WFPC2, applied to a representative star having $V - I = 1.6$ and $M_I = -4.05$. The conversion of the magnitudes of this star into the *HST* flight system allows us to derive the following relations:

$$M^{ACS}_{F814W} = -4.06 + 0.15[(F555W - F814W) - 1.74],$$
$$M^{ACS}_{F814W} = -4.06 + 0.20[(F606W - F814W) - 1.23],$$
$$M^{WFPC2}_{F814W} = -4.01 + 0.18[(F555W - F814W) - 1.58],$$
$$M^{WFPC2}_{F814W} = -4.01 + 0.15[(F606W - F814W) - 1.12],$$



- **Adam Riess** 17 days ago

So what F814W ACS system apparent tip for NGC 4258 do you get and how/why different than JL17?

- **Taylor Hoyt** 17 days ago

That should probably wait for the paper.

For reference, JL17 get I_0 = 25.357 mag. With the Reid+19 distance, that would be -4.04 mag

It would appear that a value of -4.01 is more discrepant with literature values, than -4.06?

- **Rachael Beaton** 17 days ago

For those without the paper up on the desktop, here's the Jang & Lee Table.

THE ASTROPHYSICAL JOURNAL, 835:28 (17pp), 2017 January 20

Table 6
A Summarv of the TRGB Calibrations in This Studv

Table 3
Approximation of the Color–magnitude Relation of the TRGB

Galaxy	α	β	δ	rms
	($F814W_{TRGB} = \alpha(Color_{TRGB}{}^{a} - 1.1)^2 + \beta$	$(Color_{TRGB}{}^{a} - 1.1) + \delta$)		
Entire color range, the same α and β values for all galaxies				
M105			26.124 ± 0.011	0.026
N3384			26.099 ± 0.011	0.028
M81			23.780 ± 0.010	0.029
N3377	0.159 ± 0.010	−0.047 ± 0.020	26.201 ± 0.012	0.024
N253			23.698 ± 0.010	0.035
N4258			25.357 ± 0.011	0.035
N300			22.433 ± 0.009	0.012
N3077			23.852 ± 0.013	0.020
Blue RGB stars with F606W–F814W ⩽ 2.5, the same α and β values for all galaxies				
M105			26.124 ± 0.011	0.026
N3384			26.106 ± 0.016	0.021
M81			23.780 ± 0.016	0.015
N3377	0.182 ± 0.025	−0.070 ± 0.038	26.203 ± 0.016	0.030
N253			23.698 ± 0.013	0.030
N4258			25.362 ± 0.015	0.037
N300			22.437 ± 0.012	0.010
N3077			23.854 ± 0.017	0.022
Entire color range, different α and β values for each galaxy				
M105	0.106 ± 0.050	0.047 ± 0.085	26.093 ± 0.035	0.027
N3384	0.107 ± 0.052	0.024 ± 0.088	26.090 ± 0.039	0.029
M81	0.157 ± 0.050	−0.044 ± 0.070	23.782 ± 0.046	0.031
N3377	0.063 ± 0.166	0.037 ± 0.235	26.199 ± 0.060	0.026
N253	0.307 ± 0.084	−0.229 ± 0.132	23.729 ± 0.042	0.034
N4258	0.116 ± 0.074	0.020 ± 0.134	25.338 ± 0.059	0.035
N300	0.249 ± 0.252	−0.164 ± 0.262	22.465 ± 0.061	0.012
N3077	0.135 ± 0.150	−0.037 ± 0.155	23.859 ± 0.042	0.020
Weighted mean	0.139 ± 0.025	−0.023 ± 0.039		

Zero-point		
4258	LMC	N4258 + LMC
± 0.067	−4.002 ± 0.101	−4.015 ± 0.056
± 0.067	−3.998 ± 0.101	−4.011 ± 0.056
± 0.073	−4.004 ± 0.096	−4.016 ± 0.058
± 0.068	−4.012 ± 0.101	−4.024 ± 0.057
± 0.068	−4.007 ± 0.101	−4.019 ± 0.057
± 0.125	−3.973 ± 0.139	−3.984 ± 0.121
± 0.069	−3.995 ± 0.092	−4.007 ± 0.060
± 0.068	−3.968 ± 0.106	−4.015 ± 0.057
± 0.068	−3.964 ± 0.106	−4.011 ± 0.057
± 0.074	−3.970 ± 0.102	−4.013 ± 0.060
± 0.069	−3.978 ± 0.107	−4.025 ± 0.058
± 0.069	−3.973 ± 0.107	−4.020 ± 0.058
± 0.126	−3.939 ± 0.143	−3.976 ± 0.121
± 0.069	−3.961 ± 0.098	−4.005 ± 0.062

And here's the tip:

- [Adam Riess](#) 17 days ago

ok, here is the Tully TRGB measurement

-IR	T814	T8_lo	T8_hi	606
	mag	mag	mag	m
	25.43	25.40	25.46	

- [Rachael Beaton](#) 17 days ago

So, I normally compare to the BLUE TRGB (the flat part that we use in CCHP). I'll have to do some more homework to see if use slightly different MW extinctions.

- **Adam Riess** 17 days ago

so 25.40-29.40=-4.00. Is that right? (and that is the low end)

(I looked at the average of 10 other SN hosts between CCHP and EDD and they are quite consistent so I wouldn't easily discount methods)

- **Taylor Hoyt** 17 days ago

It is unclear whether the Tully et al. pipeline selects for blue, metal-poor TRGB stars or not. The ML method is also not the same as ours

Not quite.

I saw that the 10 overlap between CCHP and Tully are fainter on EDD

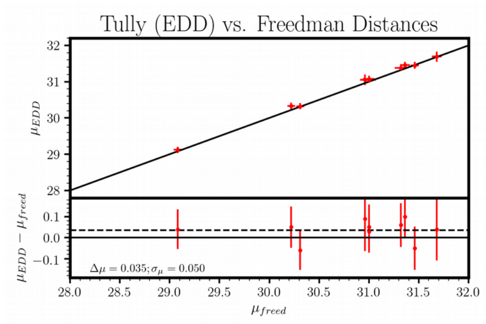
- **Adam Riess** 17 days ago

right but why NGC 4258 different than the other 10 SN hosts in common?

I’ll make a plot of the comaparison and send it to you later today

- **Taylor Hoyt** 17 days ago

No need! thank you though. I have also done this analysis. Here it is.



- [Adam Riess]() 17 days ago

Nice, so N4258 looks just like the others?

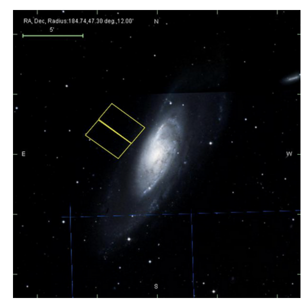
scratch that comment, I don’t se NGC 4258 listed, can you add it?

- **Taylor Hoyt** 17 days ago

Sure. I think that's a good idea. Give me a second.

- **Adam Riess** 17 days ago

Thanks



- **Taylor Hoyt** 17 days ago

While I plot this up, I want to point out that the EDD m_TRGB value does **not** include a foreground reddening correction.

- **Adam Riess** 17 days ago

EDD field for NGC 4258

yes, but the distances for all hosts are done the same way

- **Taylor Hoyt** 17 days ago

If you compare distance moduli

The reddening correction is included in the distance moduli quoted by EDD, not in the apparent TRGB magnitudes.

- **Adam Riess** 17 days ago

yes, so EDD distance modulus is 29.42 +/- 0.04 but since NGC 4258 has a maser modulus of 29.40 then the abs mag must be 0.02 mag fainter than Tully TRGB assumes (which was -4.01 in their table). So I think that is -3.99. Does that make sense?

- **Taylor Hoyt** 17 days ago

The premise of this discussion is that EDD finds consistently fainter distances than the CCHP, including both SN hosts and 4258, the geometric anchor. So, we have to make sure we are on the same system.

- **Adam Riess** 17 days ago

well is CCHP measuring the Madore et al field for NGC 4258? what tip do you get for that?

Which fields are you using?

The offset between EDD and F19 (and JL17) can be in the calibration so can we just compare the apparent F814W tip in ACS in the Madore field before extinction? EDD says 25.43 (after foreground A_I=0.025) that is 25.405. JL17 gets a median 25.375. What do you get?

- **Taylor Hoyt** 17 days ago

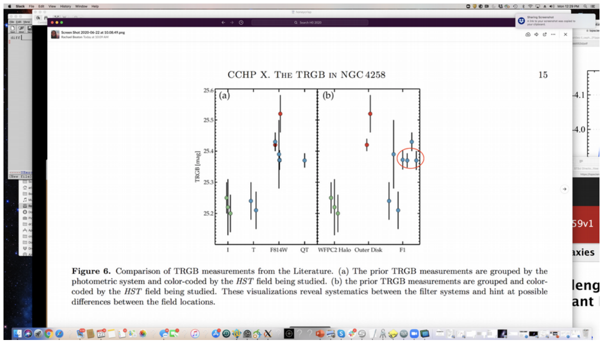
If N4258 follows the same trend as the rest of the galaxies, then the difference is purely in the methodology of measuring the TRGB

If there were a disagreement in 4258 alone, it would have bucked the trend

- **Adam Riess** 17 days ago

What tip do you get then for NGC 4258?

I guess its these points I circled) off the plot Rachael shared, 25.37 after extinction correction like JL17? So 25.37-29.40=-4.03.



(If you guys no longer are interested in this thread it would be kind of you to let me know so I can drop it. Normally at a conference we can run ~0.03 mag to ground by comparing measured quantities..)

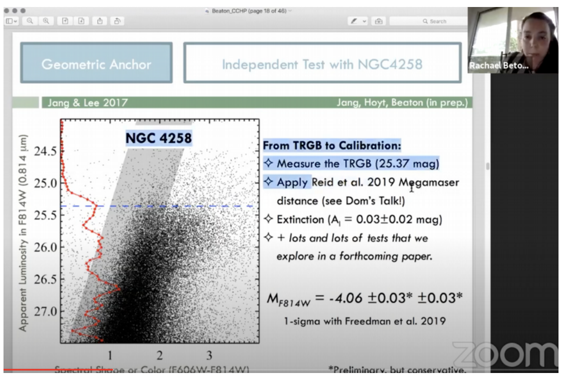
- Taylor Hoyt 17 days ago

I highly appreciate this discussion, and am grateful to have the chance to provide clarifications. I think it's best that we clarify these % differences. One quick thing, the JL17 value is 25.357 mag, not 25.37 mag.

- Adam Riess 17 days ago

what tip value do you get for ACS F814W Madore field? did you say?

Rachael's slide said 25.37



- Rachael Beaton 17 days ago

Yes, that I think I accidentally took the number on my slide (25.37) from our new analysis instead of JL17 (25.357), but I also rounded everything to the nearest hundredth in our convseration last week (and in my comparison slides, so I had 25.36 in my head for JL17).

- Adam Riess 17 days ago

so which is which now?

- Rachael Beaton 17 days ago

So our new tip is delta = 0.01 from Jang & Lee, but with a 0.03 preliminary statistical uncertainty

- Adam Riess 17 days ago

what value is that?

- Rachael Beaton 17 days ago

sorry, edited my previous post. New analysis (preliminary) = 25.37; Jang & Lee 25.357 (rounds to 25.36, I round bc of binning issues)

- Adam Riess 17 days ago

ok, and that is after Milky Way extinction?

- Rachael Beaton 17 days ago

before extinction

so, I quote on my slide A_{I} = 0.03 (again round to hundredth, but it is 0.025 raw from the maps)

I have not had a moment to check the JL17 paper to see if they used, say A_{I} = 0.02 (edited)

- **Adam Riess** 17 days ago

any idea why that is so different (0.06 mag) than EDD? see here . Not the calibration, just the tip which EDD gets as 25.43, http://edd.ifa.hawaii.edu/get_cmd.php?pgc=39600

- **Rachael Beaton** 17 days ago

Not without seeing their ML fits and Luminosity function. The EDD doesn't apply a color cut and there is clearly a metallicity slope to their CMD. It is possible that they get a fainter tip because of the contribution from the redder stars that turn down.

In theory, we could run our method without the color cut, but it wouldn't be a 100% diagnosis.

Let me check my notes on the Rizzi paper -- they might have used that field in that paper.

I have a note that the EDD mean color is F555W-F814W = 2.12 mag (edited)

And, skimming, we normally truncate around F555W-F814W = ~2.0 mag for the "blue edge" (translating from F606W-F814W color ranges following J&L)

So it would seem like they are using the redder and fainter stars in their fitting, which could skew things fainter.

- **Adam Riess** 17 days ago

but that is the same range as other hosts, no? Trying to understand why 0.06 mag fainter when usually only 0.03 mag fainter

- **Rachael Beaton** 17 days ago

Rizzi et al. 2007, actually gets 25.52 (outer) and 25.45 (inner) in the Macri et al disk fields using the same underlying algorithm as EDD.

- **Adam Riess** 17 days ago

so outer vs outer is 25.52-25.37 which is 0.15 mag!

- **Rachael Beaton** 17 days ago

Yeah, unfortunately the details of this level from the EDD are hard to work out without seeing their fits.

- **Adam Riess** 17 days ago

ok, I can ask them for the LF

- **Rachael Beaton** 17 days ago

Yeah, those numbers from the Rizzi paper are the inner disk and outer disk compared. That paper takes the mean of the two as their NGC4258 tip (I separate them out for the point of getting a sense of what we're looking at).

- **Adam Riess** 17 days ago

could this difference explain the difference H0 for IRTF and bTF calibrated by RGB?

- **Rachael Beaton** 17 days ago

Yeah, I didn't say it as eloquently as I should, but taking random literature values for TRGB without looking into the details of where the field is can lead to the same level of problems as someone taking a Ceph measurement without checking that the PL was set up the same.

Not a perfect analogy

- **Adam Riess** 17 days ago

but EDD did use the Madore field so not a random field

- **Rachael Beaton** 17 days ago

oh, right for N4258 that is true, but the metallicity is the gotcha there.

- **Adam Riess** 17 days ago

but in F814W, no metallicity dependence (says F19)

- **Rachael Beaton** 17 days ago

Only for the blue side of the TRGB -- beyond a color of F606W-F814W > ~ 1.7 the metallicity effect cannot be ignored

- **Adam Riess** 17 days ago

but if far out in the halo, would this be the same (low metallicity) for all hosts with halos?

- **Rachael Beaton** 17 days ago

Not necessarily, N4258 has an "inner spheroid" like the MW and M31 that is the remnants of its ~most recent accretion. we can see it in the Dragonfly-Array SB profile (Merritt+ 2016 I think, in ApJL) and it is also visible in the shallower imaging. Our more detailed study is going to quantify that variation.

- **Adam Riess** 17 days ago

ok, but I am uncomfortable if each calibrator has something different than a regular host. Take the H1 maps you showed of the Macri field in NGC 4258. Its as far out (20 kpc) and along the axis as the regions around some SN hosts you use (e.g., NGC 3370, 1309, 3021) so I am not sure why we exclude the field for NGC 4258 but not for the others.

- **Wendy Freedman** 17 days ago

The point is that the Macri field that you use (taken for the purpose of studying young Cepheids) is in the DISK along the MAJOR axis. You can see (by eye) the dust lanes and young blue supergiants. The earlier MINOR axis field is in the HALO. Our new analysis is based on data even farther out in the halo. You can't argue that 20 kpc is comparable when you land right in the (crowded) disk full of dust, The point is that you need to get away from disk contamination.

A separate question, why do you worry about extinction in the LMC and yet make no correction for dust in your analysis of the TRGB in NGC 4258? If you compare the Cepheid moduli in this field (and as shown by Macri et al. 2006), there is substantial reddening in this disk field. Yet you make no correction at all for extinction. Note that you would find better agreement with the halo field if you did.

- **Adam Riess** 17 days ago

We do correct for the foreground extinction for the outer field. We don't correct for internal extinction because a) the Cepheids in the field have colors that match the unreddened Cepheid locus (see Nataf et al. 2020) so if the Cepheids don't have it, TRGB should be even less.

Ok, I think I tracked down the 0.03 mag difference in NGC 4258 TRGB we kept discussing. The apparent Tip value from Jang that Rachael was using she thought was provided before extinction (this Slack thing is useful!) and so she applied the Milky Way extinction of A_I=0.03 but Jang's value already included Milky Way (I made this mistake too before) so this would be double correcting. I confirmed this with Jang. So the NGC 4258 Tip would be -4.03. This explains half of the difference with the EDD data for NGC

4258 which gives 25.43 (reported tip, includes color correction)-0.03(extinction)-29.40(NGC 4258 maser distance)=-4.00 for the same field. It would be nice to understand the remaining 0.03 mag.

Additional question in the channel + answers

Mario Hamuy Jun 22nd at 3:40 PM

Burns et al 2018 analyzed the CSP data calibrated with Cepheids and obtained H0=72.7, whereas Freedman et al 2019 obtained 69.8 from the same CSP data set calibrated with the TRGB technique, that is a 4.2% difference. Only 3% can be attributed to the TRGB vs Cepheid calibration. Could you explain why there is an additional 1.2% difference between Burns and Freedman in the analysis of the CSP data set?

- **Rachael Beaton** 17 days ago

Hi @Mario Hamuy -- I am not totally sure. My understanding from Chris is that there is that correction for host mass is part of it. We also have, more or less, uniform uncertainties in our distances so that the weighting in the mean is slightly different. I tried to show that in our comparison of the Ceph to TRGB SNe Ia. I'd love to understand this more because it keeps me up at night.

- **Adam Riess** 17 days ago

There is a 1% difference between the CSP and Pantheon/Supercal data set. Because the calibrators come from broad set, I think Pantheon is more accurate

- **Rachael Beaton** 17 days ago

That's true! I have a plot somewhere with the Panethon as calibrators.

03 Silvia Galli - Planck and the CMB

✍️ There are already 9 questions with 32 upvotes. Here are the most popular ones:

Do we know c_s(z) well enough to infer reliable r_s ?
by Anonymous | 9 upvotes

Answer from Silvia

#1 Lloyd actually answered this question. c_s depends on the physics of how baryons and photons interact as single fluid and this impacts the CMB in many different ways (e.g. c_s enters in the sound horizon, but also in the amplitude of polarization at l>100, since the velocity gradient which sources it depend on c_s).

Planck and WMAP agree at l<1000 but its the Planck data at l>1000 that cause tension in H0. Could CMB systematics eg beam correction, affect Planck results?
by Tom Shanks (Durham) | 9 upvotes

What type of CMB systematics are you looking for in your H0 analysis to explain the H0 tension?
by Anonymous | 5 upvotes

Answer from Silvia

#2+3 You can look at the 2015 likelihood paper arXiv:1507.02704 Fig. 35 for TT and C10 for TE and EE, and to the likelihood paper 2018 1907.12875 Sections 3.6-3.11 for more tests, specially in polarization. We looked at many different effects, such as foreground cleaning, data cuts, frequency dependences, beam uncertainties, multipole splits, calibration uncertainties, priors on nuisance parameters, etc… and none of them could really substantially change the value of H0 to a level that could solve the tension.

When you add the BAO constraints to WMAP, are you self-consistently using the WMAP sound horizon scale for the BAO measurements?
by Matthew Colless | 4 upvotes

Answer from Silvia

#4 Yes WMAP+BAO takes the rs (at baryon drag) from WMAP (and any case, rs from WMAP and Planck agree well). WMAP gets rd=148.5+-1.2, Planck 147.09+-0.26

How strongly does the Planck result depend on the assumed perfect flatness of the Universe?
by Richard Anderson (ESO) | 3 upvotes

Answer from Silvia

#5. Answered

Latest question

could the 2 sigma tension between ACT/SPT and planck be due to some region of sky that affects planck but not the limited sky CMB expts eg cold spot ?
by joe Silk | 1 upvote

Answer from Silvia

#6 This analysis 1712.01986 didn't find large variations of cosmo parameters across the sky . Also, we did estimate parameters using different galactic masks retaining different fractions of the sky (40-70% after apodization), obtaining consistent results.

04 Lloyd Knox - The theoretical picture

✍️ There are already 8 questions with 37 upvotes. Here are the most popular ones:

Should a model resolve both the H0 and S8 tensions to be considered successful? are we ready for "several" discoveries?
by Anonymous | 11 upvotes

- **Lloyd Knox** Jun 22nd at 5:43 PM

This question was asked: "Should a model resolve both the H0 and S8 tensions to be considered successful? are we ready for "several" discoveries?"
My answer: I'm in general not in favor of any rules like this. Let's rely on good judgment and the arguments that support it. But to respond to the specifics of your questions, from my perspective the S8 tension is not as strong as the H0 problem. Perhaps the true cosmological model has this S8 tension and statistical fluctuations plus perhaps removal of a small amount of systematics is the answer? I would like to hear other answers to this question. I am certainly not an expert on the cosmic shear data.
If a would-be solution makes S8 tension even worse, that's another matter.Related: there are theory papers out there that ignore BAO. I think this is generally a mistake. BAO plays a big role in constraining the solution space.

- **Rachael Beaton** 17 days ago

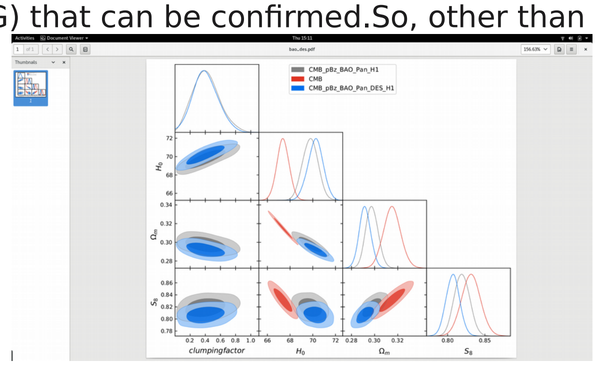
This is helpful context! thank you! I get asked this and flub through.

- **Suhail Dhawan** 16 days ago

Very helpful! Just to add to your point about BAO's, the high-z Type Ia supernovae provide complementary constraints to the BAOs so a combination would be even more constraining for the late-time expansion history predictions of some models.

- **Levon Pogosian** 16 days ago

Hi Lloyd, enjoyed your talk yesterday and also your really useful paper with Millea. I agree with your comment about the importance of the BAO. And, it’s really challenging to get H0=74 and keep CMB and BAO at all redshifts happy.I have, of course, my favourite model at the moment. Karsten Jedamzik and I have looked at baryon inhomogeneities induced by primordial magnetic fields (PMF) which speed up the recombination process and lower the sound horizon (https://arxiv.org/abs/2004.09487). There, CMB+BAO appear quite happy with H0 around 70.5 \pm 0.6, depending on which other data you include, while resolving the S8 tension. I am attaching a plot with a global fit of this model to P18(+lensing)+BAO+Pantheon+DES+RiessH0 with and without including DES. “CMB” is the Planck LCDM fit. There is substantial independent motivation for PMFs of that strength (0.05 nG) that can be confirmed.So, other than failing to get all the way to H0=74, this proposal seems to satisfy all you Model X criteria, in terms of being predictive, well-motivated/conventional physics etc. Would be happy to hear your and other people’s questions/thoughts/criticism.

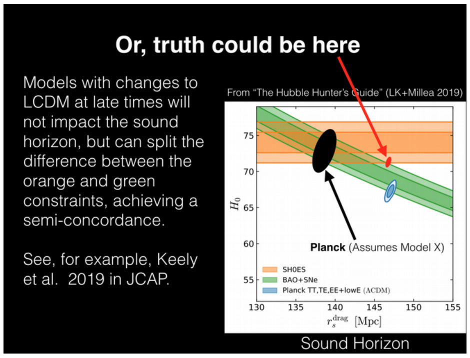


Are there any exotic late time (z <=1) expansion history model that still seem viable to explain the tension?

by Suhail Dhawan | 11 upvotes

- **Lloyd Knox** Jun 22nd at 10:02 PM

Let me address this question: "**Are there any exotic late time (z <=1) expansion history model that still seem viable to explain the tension?" from @Suhail Dhawan with a slide. (Although Keeley et al. look at z < 3).**



- **Graeme Addison** 17 days ago

Here are a couple examples of phenomenological late-time modifications that include all the main data sets. Zhao et al. https://arxiv.org/abs/1701.08165 (freedom in w(z) for DE, includes multiple turning points),
Raveri https://arxiv.org/abs/1902.01366 (modifying gravity at late times; I recall quite a few effective extra params are needed).

arXiv.org

Dynamical dark energy in light of the latest observations

A flat Friedman-Roberson-Walker universe dominated by a cosmological constant ($Λ$) and cold dark matter (CDM) has been the working model preferred by cosmologists since the discovery of...

arXiv.org

Reconstructing Gravity on Cosmological Scales

We present the data-driven reconstruction of gravitational theories and Dark Energy models on cosmological scales. We showcase the power of present cosmological probes at constraining these models...

- **Vivian Poulin** 17 days ago

along these lines; let me advertise my work too 😉 https://arxiv.org/pdf/1803.02474.pdf
The visual argument from Lloyd against late universe modification can be understood as follows: the (perpendicular*) BAO measures theta_D= r_D / dA (with rD calibrated on Planck) while SN1a measures mu= 5log10(dL) + cst (where the constant would be calibrated via e.g. SH0ES); the problem is that within GR (at least) dL = dA * (1+z)^2;

so that once calibrated BAO and SN1a effectively provide measurement of the **same** quantity (given that their redshift overlaps). Because of the mismatch in the calibrator (that is the “Hubble tension”,) dA^BAO and dA^SN1a are incompatible; that is completely independent on any model, it just relies on the assumption dL = dA * (1+z)^2. This indicates that resolving the tension requires to change the calibrators: either SH0ES calibration constant is incorrect (within LCDM there is no tension!) or one needs to change r_D from Planck. This is what is illustrated in Lloyd’s figure.*a similar argument could be made with the parallel BAO measuring rD*H, and leading to discrepant H measurement.

- **Suhail Dhawan** 16 days ago

I didnt quite follow what "split the difference between the orange and green constraints" means

- **Graeme Addison** 16 days ago

If you don’t change the sound horizon, and only allow a vertical shift upward from the Planck LCDM constraint in Lloyd’s plot, you can’t get perfect agreement with both the orange and the green bands. So “split the difference” in the sense you’d take a bit of a hit in the likelihood / goodness of fit from both the distance ladder and BAO+SNe in this scenario.

How can you make recombination occur a little bit earlier or a little bit later?
by Jeremy Mould | *6 upvotes*

- **Lloyd Knox** 10:09 PM

Jeremy MOuld asks, "**How can you make recombination occur a little bit earlier or a little bit later?" @Jeremy Mould.**
This is something we do consider in the Hubble Hunter's Guide. By the way, let me take this opportunity to say, since it did not come across in my talk, that a lot of what is in HHG is consideration of possible avenues one might explore in alternative model space and the challenges that one will face if one heads in that direction. The success of the standard recombination scenario, which, via diffusion damping and polarization generation has a big impact on observable spectra, is quite impressive. I did show in my talk the huge impact of diffusion damping on the TT spectrum. So the observables are really sensitive to the history of recombination, and therefore ways we can mess with it are quite constrained.Here's what we wrote in HHG, "In this subsection we consider reducing this conformal time by reducing zd by having the baryon drag epoch

end at a higher photon temperature. Such a solution was in fact presented by [74]. However, the question remains of the underlying physics that would lead to a high-temperature recombination. In principle, it could be achieved with time variation of the fine structure constant, since a stronger electromagnetic interaction would lead to recombination at a higher temperature. Based on CMB power spectra [75] find the change in the value of α between recombination and today to be $\delta\alpha/\alpha = (.7 \pm 2.5) \times 10^{-3}$. Since atomic physics energies are linearly proportional to α, this indicates only sub-percent changes in recombination temperature are permissible. These are too small to achieve a 7% change in the sound horizon.The failure of α variation as a way to get to small r ? s is a specific example of what we expect to be true in general: changes to the physics of recombination sufficient to change the sound horizon by 7% will wreak havoc on the shape of the damping tail. Admittedly, we have no proof that such a solution is not possible. But it seems highly unlikely that new physics alters r ? s by changing recombination, while having an acceptably small impact on the shape of the CMB damping tail. The unlikeliness is underscored by the fact that recombination occurs out of chemical equilibrium – the relevant atomic per-particle reaction rates are not much faster than the Hubble rate. The particular details of the ionization history resulting from this out-of-equilibrium recombination are marvelously consistent with the shape of the damping tail. Thus the task is more challenging than simply reproducing a generic equilibrium ionization history at a higher temperature."74] C.-T. Chiang and A. Slosar, arXiv e-prints 1811, arXiv:1811.03624 (2018). [75] L. Hart and J. Chluba, Monthly Notices of the Royal Astronomical Society 474, 1850 (2018).

I like this use of Slido and Slack!! Thank you, @Richard , @Sherry and any others responsible for getting this set up.

Do we need more parameters than just the 6 from LCDM? Would this mean that LCDM fails at describing our Universe?

by Anonymous | *3 upvotes*

Is it really necessary to keep invoking dark physics? What's your take on inhomogenous cosmological models?

by Felipe Olivares | *3 upvotes*

Latest question

Do you think the conclusion from your Hubble hunter's guide paper, that the EDE is the model that works best so far still holds?

by Anonymous | 1 upvote

Discussion Panel 1

✍️ There are already 19 questions with 29 upvotes. Here are the most popular ones:

Are there plans to make the end-to-end analysis pipelines (incl enhanced data products) public?

by Simon Birrer | 6 upvotes

So if the CMB H0 measurements depend on the model, would it be enough to find the right model X solve the discrepancy?

by Felipe Olivares (INCT) | 6 upvotes

naive question: there is (probably) discrepancy in H0 values between Planck vs late universe, is there similar difference wrt other cosmological parameters?

by Anonymous | 4 upvotes

(to the panel:) What's the most surprising new result you saw today, and why did it surprise you?

by Richard Anderson (ESO) | 4 upvotes

To Lloyd by Suhail Dhawan: Are there any exotic late time (z <=1) expansion history model that still seem viable to explain the tension?

by Richard Anderson (ESO) | 3 upvotes

Latest question

Tom, the Key Project TF calibration paper is Sakai et al. But you're right about the 1980s Aaronson-Huchra-Mould IRTF calibration based on M31 & M33 alone.

by Jeremy Mould | No upvotes

Dragan Huterer Jun 22nd at 6:06 PM

Just to clarify/repeat both the Q and A from Tom Shanks to me earlier today. The Q was roughly: why invent new LCDM-extension models when you can just study the local density field, e.g. counting galaxies, and learn more about a possible Hubble bubble, and perhaps explain the H0 tension. My response was that one should certainly do that (map out the local density field) - it's after all an important research branch of cosmology - but that, from the theory-expectation side, pure LCDM has absolutely no chance to allow a Hubble bubble big enough to explain the observed H0 tension. And, to the extent that our universe looks very close to LCDM (at large scales esp) from a huge variety of observations, I would want to see a totally convincing measurement of the Hubble bubble before believing we are abandoning LCDM in this particular direction.

- **Graeme Addison** 17 days ago

Also we should bear in mind that the H0LiCOW/STRIDES lensing systems are not ‘local’. The sources are at $0.7<z<1.8$, the lenses at $0.3<z<0.7$ (Wong+2019). Their H0 results are completely consistent with SH0ES and now have comparable precision. How can one explain this through any local density effect?

- **Tom Shanks** 17 days ago

Isn't there always a 10 percent systematic in the lens modelling?

- **Graeme Addison** 17 days ago

No, according to the systematics error analysis from Millon et al. https://arxiv.org/abs/1912.08027 (covers this and some other recent claims / concerns).

arXiv.org

TDCOSMO. I. An exploration of systematic uncertainties in the...

Time-delay cosmography of lensed quasars has achieved 2.4% precision on the measurement of the Hubble Constant, H_0. As part of an ongoing effort to uncover and control systematic uncertainties,...

- **Tom Shanks** 17 days ago

OK. D

- **Dragan Huterer** 17 days ago

I am personally skeptical of the claimed strong-lens accuracy in getting H0 mainly because of worries about freedoms in modeling the lenses. At any rate would be great to see confirmed (or denied) by another team.

- **Tom Shanks** 17 days ago

I quickly looked at the paper - they mention the mass sheet/slab issue but where do they actually include it in the systematics budget. They mention shear but that too is immune to mass sheet/slab issue.

- **Graeme Addison** 17 days ago

Sounds like good things to bring up in the discussion after Kenneth Wong’s talk on Wednesday…

- **Lloyd Knox** 17 days ago

We have Cepheid-calibrated supernova distance measurements to z =0.6. We have LCDM+CMB-calibrated distance measurements via BAO to z = 0.6. These don't agree and are unimpacted by a void. The level of disagreement, though, is admittedly not as severe as the 4.2 sigma H0 discrepancy.

- **Fred Courbin** 17 days ago

About lens models: one can invoke super flexible models (assuming they are physical at all), but the current data do not call for this.

Another thing is that all out data, light curves, time delays, images, codes to measures delays, modeling codes are all public and open source on github. Our master students are able to use these codes, so in principle anyone can use them and run whatever model is desired.

- **Tom Shanks** 16 days ago

Lloyd, I take the point. But what the local underdensity suggests is that the perfect LCDM fit to the SNIa Hubble diagram can't be as exact as claimed. And previous "gold standard" distance indicators have had their problems. Take eg Tully Fisher - it only got as far as Virgo before it had to be recalibrated because of disagreement with KP Cepheid distances.

Tom Shanks 6:30 PM

I'll try to show what you want in 1 slide in Friday's panel discussion.