| Wednesday, 24.06.2020 | | | | |
|---|---|---|---|---|
| Time UTC | Time CEST | Speaker | Subject | |
| 12:50 - 13:00 | 14:50 - 15:00 | | Announcements | |
| 13:00 - 13:25 | 15:00 - 15:25 | Tamara Davis | On redshift measurements | Q&A |
| 13:25 - 13:50 | 15:25 - 15:50 | Kenneth Wong | H0 from strong gravitational lenses | Q&A |
| 13:50 - 14:15 | 15:50 - 16:15 | Dominic Pesce | The Megamaser Cosmology Project | Q&A |
| 14:15 - 14:40 | 16:15 - 16:40 | Bernard Schutz | Standard Sirens | Q&A |
| 14:40 - 15:10 | 16:40 - 17:10 | Panelists & speakers | Discussion | Q&A |
| | | Panelists: S. Birrer, S. Suyu, R. Anderson, O. Lahav | | |
| | | Link to YouTube Video | | |

**Tom Shanks** 1:49 PM
Is crowding in the LMC an issue for  detached eclipsing binary method?

## 09 Tamara Davis - Redshift measurements

✍️ There are already 6 questions with 4 upvotes. Here are the most popular ones:

**The different group velocities for NGC4993 seems to have a huge error bars, compared to the 150/250 km/s. Are these measured directly from the group distance?**
*by Anonymous | 3 upvotes*

- **Tamara Davis** 5:58 AM

*The oft quoted 150-250km/s error bars are due to non-linearities in the reconstruction of the velocity field from densities. They don't take into account the uncertainties due to the data, sampling, and selection effects. The larger error bars we use try to take these other effects into account as well. We also use peculiar velocity measurements directly, whereas the reconstruction assumes a model to get the velocities from densities. The methods are different and subject to different statistical/systematic uncertainties.*

**could there be effects due to the fact that the dipole seen in the CMB may not be the "local"**
*by Anonymous | 1 upvote*

- **Tamara Davis** 5:58 AM

Yes, I think what you are referring to is what I mentioned as the fact that we share some of our bulk motion with nearby galaxies. So correcting from heliocentric to the CMB frame actually overcorrects the redshifts of nearby galaxies. Since velocities are sourced from very large scale modes, this effect can persist out to quite large scales (zerror~10^-4 at z~0.2). See Section 4.3 and Figure 5 of 1907.12639. This is what we try to correct when we correct for local peculiar velocities.

**Have you looked into the combination of peculiar velocities and the effect of pixel-scale non-linearity (e.g. due to detector stitching)?**
*by Richard Anderson (ESO) | No upvotes*

**Do we need to adapt the fitting routines to accurately account for errors both in x- and y-axis simultaneously?**
*by Richard Anderson (ESO) | No upvotes*

- **Tamara Davis** 5:58 AM

*Ideally yes.*

**what is the extent of the effect expected on time-varying dark energy inference**
*by Anonymous | No upvotes*

- ***Tamara Davis*** *5:58 AM*

We looked at this for the Dark Energy Survey supernova cosmology, see Table 1 of 1811.02374. Since for dark energy one uses the higher-redshift supernovae it is not as much as a worry as for low-z H0 measurements. For this particular sample we considered a possible systematic redshift bias of 4e-5, and found it gave an uncertainty of 0.006 in w, which had not been considered in previous analyses. Combined with peculiar velocity uncertainties, which have long been included, the redshifts uncertainty gave 0.012 in w. Other surveys with different redshift ranges, and different numbers of supernovae will give a different answer, and time-varying w will be more susceptible than this to potential biases because of the extra flexibility in the model.

Latest question

**The peculiar velocity indeed depends on the smoothing scale (or group membership). In the context of H0 from GW170817 see e.g. Nicolaou et a;. arXiv:1909.09609**
*by Anonymous | No upvotes*

## 10 Kenneth Wong - H0 from strong gravitational lenses

✍️ There are already 10 questions with 16 upvotes. Here are the most popular ones:

**How accurately do you estimate the external convergence (the impact of Millennium Sim based on WMAP cosmology and one choice of SAM model) ?**
*by Anonymous | 5 upvotes*

- **Kenneth Wong** 3:12 AM
  The typical uncertainty on the external convergence (k_ext) ranges from ~3% to ~6% for our current sample, but it depends on the particular field in that lenses that lie in more overdense lines of sight have larger median k_ext and a wider spread in the k_ext distribution, while the opposite is true for lenses in more underdense fields.  As far as the impact of the choice of simulation/cosmology, while this could in principle be a source of bias, we use relative galaxy number counts. instead of absolute number counts to mitigate such dependence, so any effect would be second-order.  We do have a work in progress being led by Sampath Mukherjee to quantify differences between simulations and SAMs. Finally, in Millon et al. 2020 (https://ui.adsabs.harvard.edu/abs/2019arXiv191208027M), Figure 4 shows that when you don’t apply our k_ext correction, you get a trend of H0 with k_ext, which is unphysical, but applying our correction removes this trend.

**Is it reasonable to average over each individual lens-based H0 measurement? That is, are they drawn from the same distribution?**
*by Anonymous | 3 upvotes*

- **Kenneth Wong** 3:12 AM
  When looking at the individual H0 likelihood distributions from the individual

lenses, they are statistically consistent with being drawn from the same distribution (i.e., the spread of H0 values is consistent within the uncertainties), so there is currently no evidence that they are not drawn from the same distribution.  Since the data sets for individual lenses are independent, we can multiply their H0 likelihoods.  We can further incorporate correlations between lens properties and H0 through our upcoming hierarchical analysis being led by Simon Birrer.

**What is the relation between the mass along the line of sight and Omega_M that goes into the lens modeling?**
*by Anonymous | 2 upvotes*

- **Kenneth Wong** 3:12 AM

As mentioned above, the k_ext correction is based on the Millennium Simulation cosmology with fixed Omega_m.  In the modeling, Omega_m is allowed to vary freely with a wide prior of [0.05,0.5].  However, Omega_m has almost no impact on the inferred H0, so this does not affect our results in any significant way.

**Among your big collaboration, are you planning to "double blind" modify data before analysis to avoid H0 confirmation bias?**
*by Antoine Mérand | 2 upvotes*

- ***Kenneth Wong*** *3:12 AM*

We are currently working on a validation on hydrodynamical simulations in an upcoming paper, as well as an analysis of a lens that currently does not have a measured time delay.

**When you said "high cadence in a single season" is enough to get accurate time delays, what is high cadence exactly? once a day? more frequent?**
*by Anonymous | 1 upvote*

- ***Kenneth Wong*** *3:12 AM*

  High cadence is (nearly) once per day.  See Courbin et al. 2018 (https://ui.adsabs.harvard.edu/abs/2018A%26A...609A..71C) for more details.

Latest question

**Are you including the velocity dispersion in your mass modeling? How much would that help? Would it be work to remodel some lenses if you get 2D kinematics?**

*by Stefan | 1 upvote*

- ***Kenneth Wong*** *3:12 AM*

  Yes, we are incorporating the velocity dispersion in our mass modeling, which does help mitigate some degeneracies. We are also working on incorporating 2D kinematics in the future, which would potentially improve the constraints by quite a bit. See Yildirim et al. 2020 (https://ui.adsabs.harvard.edu/abs/2020MNRAS.493.4783Y).

***Kenneth Wong*** *3:12 AM*

**“The most precise H0 measurement comes from combining (only) 6 QSOs. could the H0 value be ‘pulled up’ by (at least one) outlier?”**

The individual H0 distributions for individual lenses are statistically consistent with one another, i.e., none of them are an outlier that is significantly pulling the distribution in either direction. You could arbitrarily remove the lens with the highest H0, but it wouldn’t shift our combined result by more than 1-sigma.

**“When you say that the WL analysis agrees with the theoretical modeling for LOS effects, quantitatively, at what level do they agree?”**

Depending on the filtering scheme used on the WL data, the k_ext posterior distribution agrees with the weighted number counts k_ext within either 1-sigma or 2-sigma. See papers by Olga Tihhonova for details (https://ui.adsabs.harvard.edu/abs/2018MNRAS.477.5657T and https://ui.adsabs.harvard.edu/abs/2020MNRAS.tmp.1655T).

**“Is there a significant correlation between the convergence and H0?”**

Copy/pasting from above: in Millon et al. 2020 (https://ui.adsabs.harvard.edu/abs/2019arXiv191208027M), Figure 4 shows that when you don’t apply our k_ext correction, you get a trend of H0 with k_ext, which is unphysical, but applying our correction removes this trend.

**“How will your analysis methodology be adapted to handle a large lens pipeline?”**
Our upcoming hierarchical analysis paper will reanalyze the entire sample and can be adapted to include any future results we get.  Also, we have ways to more systematically analyze lenses to improve speed and uniformity, e.g. Shajib et al. 2019 (https://ui.adsabs.harvard.edu/abs/2019MNRAS.483.5649S)

## 11 Dominic Pesce - The Megamaser Cosmology Project

✍️ There are already 4 questions with 7 upvotes. Here are the most popular ones:

**GW is currently based on 1 system, how many is maser Ho based on?**
*by Anonymous | 3 upvotes*

- ***Dom Pesce*** *4:24 PM*

*The maser H0 measurement is currently based on 6 systems.*

**By how much the 22GHz maser sample size could be increased with ngVLA?**
*by Anonymous | 3 upvotes*

- ***Dom Pesce*** *4:24 PM*

*The ngVLA will be ~10x more sensitive than current radio facilities, enabling us to see ~3x farther away and opening up ~30x more volume.  So with no additional improvements in sampling efficiency, etc., we'd expect to see the sample size increase by a factor of ~30.*

- **Rachael Beaton** 4:29 PM

**ngVLA** !!!!

- **Dom Pesce** 4:31 PM

ngVLA is going to be fantastic 🙂

**How concerned are you about satellite constellations such as STARLINK?**
*by Anonymous | 1 upvote*

**Question for D Pesce is "G Efstathiou suggests that there is a 3.5 sigma discrepancy in the geometrically calibrated Cepheid distances to NGC4258." Comments?**

*by Tom Shanks(Durham) | No upvotes*

- **Dom Pesce** **4:24 PM**

I'm not familiar with this discrepancy and so I have no comments on it.

- **Dom Pesce** **4:24 PM**

**> Are you performing a specific search for masers in galaxies that host stellar standard candles?**

The MCP itself did not explicitly perform such a search, but many of the AGN that are nearby enough to calibrate standard candles have naturally been searched for 22 GHz maser emission as part of these larger surveys

## 12 Bernard Schutz - Standard sirens

✍️ There are already 2 questions with 10 upvotes. Here are the most popular ones:

**Are there any systematics involved in the event detection (e.g. numerical template construction) that could bias the distance estimates?**
*by Richard Anderson (ESO) | 6 upvotes*

- **Bernard Schutz** 5:50 PM
  *There are definitely possible systematics in the template families, particularly in parameter estimation, such as measuring masses and spins. To get a handle on them, we have built several families using independent methods, and we always run parameter estimation codes using more than one on each event in order to estimate the differences. All families include post-Newtonian inspiral calculations matched to results from numerical simulations of mergers. At this point, these possible systematics are below the dominant uncertainties for most events. The binary neutron-star event GW170817 was treated especially carefully in this regard, because of its high signal-to-noise ratio and because of the importance of the H0 measurement. Our template families do not yet incorporate parameters*

*for measuring spin components in the orbital plane, or for dealing with eccentricity. We do have families that parametrise tidal effects in neutron-star mergers and higher radiation modes. As our detector sensitivity improves, we will also upgrade our templates, so that their systematics will not become a limiting factor.*

**Will dark or bright standard sirens first reach a <5% measurement?**
*by Richard Anderson (ESO) | 4 upvotes*

- **Bernard Schutz** 5:50 PM
  I addressed this in my talk a bit. It is hard to guess the answer. Bright sirens need to be identified electromagnetically, and new telescopes may make a big difference: Rubin Observatory and SKA, for example. Dark sirens benefit particularly from improved detector sensitivity, because that slims down the position uncertainty region. Perhaps the answer is that bright sirens could win the race if they get to 5% before the A+ upgrade to LIGO, while after that dark sirens have a very good chance.

## Discussion Panel 3

**Saurabh Jha**

Jun 24, 2020

6

Is the CMB frame the right one to be using for local H0 measurements?

**Anonymous**

Jun 24, 2020

4

Which constraints on dynamical dark energy models could you summarize from gravitat. lensing and so on h0 ?

**Anonymous**

Jun 24, 2020

1

Is not good approach to average the different ho values from the different methods.is necessary to understand the différences to reach the définitive value.