# Thursday July 9 - Recent Results from Merging BNS and BH-NS

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## Brainstorming Q&A:

1. Fundamental questions:
    a. In the context of producing theoretical predictions useful to constraining multimessenger observations:
        i. What contributes the largest to the uncertainties in current BNS simulations, and what can be done (assuming existing computational resources) to address these?
        ii. What are the most important theoretical predictions made from BNS simulations in the past e.g., 5 years, that observers need to know about?
            1. Riccardo: two types of contributions --
                a. a simulation bank for parameter estimation (results that are quantitatively reliable)
                b. basic qualitative results on processes and mechanisms -- e.g., which mechanisms can power a SGRB jet? Is the central engine a BH or a NS? What types of ejecta can be produced at the different evolution stages? Which kind of kilonova would be produced by each of these components? What is the variety of EM counterparts that we can expect from the different scenarios?
            2. Manuela: Alessandra asked this morning whether simulations could tie velocity & mass of the fastest ejecta (with e.g., radio observations) and set further constraints on the EOS compared to what can be done with the kilonova emission.
            3. Alessandra: Are polarization measurements useful for constraining magnetic fields (in relation to the hypothesis of large scale magnetic fields in the disk+BH system formed after merger)?
            4. Francois: There has been some work done in this direction by Kiuchi et al
            5.
    b. In terms of tying BNS simulations to observations, what's more important to get right: magnetic fields or microphysics?
        i. Julian: Depends on what you care about / what you are looking for in your simulation
        ii. Riccardo: Depends also on the system. Consider these cases:
            1. If remnant is a long-lived NS, B-field is very important.
            2. If prompt collapse, B-field is not as important

iii. Zach: Inspiral gravitational waves insensitive to B-fields; microphysics crudely important
iv. Riccardo, Francois: Peak GW frequencies & amplitudes may shift in presence of magnetic fields
    1. Riccardo: resolution dependent as well -- predictions not very reliable yet. Maybe viscous effects could kill oscillations, curtailing the post-merger GW signal (see Shibata paper), but this is not settled
    2. Francois: David R's viscosity isn't as strong as Shibata's
    3. Bruno & Francois: Maybe Shibata's viscosity is too high, measured alphas much lower.
    4. Julian: Stress due to correlated MHD turbulence poorly classified by single value of alpha (it's not an alpha disk). Single "mean" alpha value misleading
    5. Milton -- Shibata points out that the value of alpha even varies due to resolution.
    6. Francois: See David's 2020 paper on this topic -- uses a spatially dependent alpha prescription based on super high res simulations by Kenta
    7. Julian & Francois -- very difficult to infer actual values for e.g., alpha from current simulations due to resolution, subgrid model limitations
v. Riccardo: clearer that microphysics is super important in the early post-merger. Any other situations in which one or the other is most important?
    1. (For potential kilonova observations, the nuclear physics uncertainties play a huge role)
vi. Milton: In case of ergostars, don't need neutrino modeling to get high Gamma outflows. Maybe if you want to study SGRBs, you need magnetic fields, but amount & nature of ejecta depends enormously on microphysics

Ian:
http://arxiv.org/abs/1906.03150

2. Magnetic field-related questions
    a. Can large-scale poloidal B-fields be created in a BNS remnant, which would help/might be necessary to launch jets? How could future simulations settle this question?
    b. Other than insufficient numerical resolution (which could be addressed e.g., with subgrid modeling), what difficulties stand in our way to properly model magnetic fields?
    c. In which aspects and/or evolution phases is the lack of resolution really crucial for MHD effects?

d. How sensitive are simulations to the initial (pre-merger) magnetic field structure and orientation? Are initial exterior magnetic fields important? What about an internal toroidal component?
    i. Are there plans to relax certain simplifying assumptions regarding initial magnetic fields -- i.e., not using purely dipolar-like fields (which we know aren't stable over long timescales)
        1. MIlton: Paper released a couple months ago: Not enough poloidal fields with dipole moment aligned with J_orbital -> no jet
        2. Riccardo: The survival time of the remnant may have a big impact on whether jet forms (those results are based on very short-lived HMNS)
        3. Bruno: Initial magnetic field also plays a very important role
        4. Manuela: How is interior initial magnetic field in NS important/
        5. Riccardo: Purely poloidal or purely toroidal mag fields are unstable (in context of single NS). If we want something axisymmetric (still simple), it would be better to have at least a poloidal field that extends to the exterior plus a toroidal field inside. "Twisted torus" configuration would be desirable, as it stabilizes interior B field (but toroidal field needs to be dominant for stability)
            a. first twisted-torus mag field model in GR
            b. [https://arxiv.org/abs/0903.0556](https://arxiv.org/abs/0903.0556)
            c. poloidal field instability confirmed in GRMHD
            d. [https://arxiv.org/abs/1105.3971](https://arxiv.org/abs/1105.3971)
            e. twisted-torus model with large toroidal component
            f. [https://arxiv.org/abs/1306.2803](https://arxiv.org/abs/1306.2803)
            g.
        6. Milton: NICER indicates there's a toroidal component as well
        7. Julian: Is there a threshold beyond which instabilities weaken, such that the steady state is e.g., dominated by subgrid geometry
        8. Riccardo: Note the (Alfven) timescale for instability, ~3ms for 10^16 G field, which scales linearly with this (weaker B fields, longer timescales). Literature indicates 20-90% toroidal field is needed for stability (in terms of mag energy), see Braithwaite 2009
        9. Julian: What about effects of rotation -- how does this impact stability of B field
        10. Riccardo: Rotation will have an impact on B-field stability studies. Studies with rotation done in Newtonian regime, see Geppert, Rheinhardt 2006.
        11. Julian: 2D vs 3D very different
        12. Riccardo: Extending to 3D could greatly increase the variety of stable B field configs. NICER results indicate certain multipoles can be stable over very long timescales. Also, stresses could build up on the crust, and breaking the crust could result in an EM

signal (see magnetar burst activity). Something to be studied in the future.

e. In what ways could (el.) does resistivity play a role (in the post-merger phase)?
    i. Zach: Numerical resistivity already exists in all GRMHD simulations. Has it been settled that this resistivity is less than what we'd expect Nature to provide? (No)
    ii. Ian: Looking into formulation of resistive GRMHD in the ideal limit (Alex Wright, see https://arxiv.org/abs/1906.03150). Extending WhiskyMHD to implement this modified formalism. Have a couple 1e12 Gauss simulations repeating ca 2012 calculations from WhiskyMHD. No differences in inspiral/early (~2ms) postmerger phase. 5-10ms postmerger: resistivity confines B-field to core more than without resistivity. Effects smaller than other numerical errors. Promising formalism, but difficult to disentangle physical & numerical effects unless higher B-field strengths explored.
    iii. Manuela: How high?
    iv. Ian: If we are to assume effects linear in B-field strengths. Marginal at 1e12Gauss, then maybe at 1e14Gauss it'll be more important.

f. Manuela: Any insights from observation about how strong B fields should be from BNS?
    i. Bruno: In many cases, we'd expect B fields to be weak during inspiral
    ii. Milton: 1e12-1e13 Gauss. Problem is Alfven timescale is very long to model instabilities. Maybe take a cue from Shibata & start with (artificially) strong B field that will eventually be realistically strong after merger.
    iii. Riccardo: B-field of magnetars are much stronger inside than outside. Magnetic energy needed to power giant flares is much larger than what is inferred from outside the field, at least 10x. A good initial field for BNS inspiral could be 1e12 for surface poloidal field, but 1e13 for internal (poloidal and toroidal) field.
    iv. Julian: In BNS simulations, start with too strong B field but still amplification saturates at a much higher level. What would happen with a weaker B field? Same final saturation level?
    v. Riccardo: Yes, it's likely. When weak B-fields are just "along for the ride", amplified by instabilities (KH, MRI), saturation comes when the field becomes dynamically important. So we might expect the final saturation level to end up being similar irrespective of the initial field.
    vi. Julian: Interesting that it saturates at around the same time, implying that there are multiple e-foldings.
    vii. Riccardo: Yes, first K-H, then MRI after 30ms. What happens otherwise will be very dependent on resolution.
    viii. Manuela: In the post-merger phase, we should get an important idea of what impact the magnetic field will have.

ix. Riccardo: Yes, and the longer-lived the remnant, the more important the magnetic field will be. Magnetic fields in the (long-lived) remnant are not just a source of viscosity -- production of helical structure also has an important impact.

3. Subgrid modeling questions:
    a. Do we really need subgrid modeling to properly model MHD instabilities, or would it be equally inconsistent to artificially amplify the initial B-fields? Which is the worst poison?
        i. Manuela: There are some neat subgrid models that capture reconnection
        ii. Julian: Too small scales to be useful here. Very important here to properly model current sheets. Prefer assumption that's easier to understand -- subgrid changes evolution; starting out with stronger field results in easier interpretation
        iii. Milton: Do we really need subgrid? We can do high & low resolution -- need K-H & MRI at the beginning, then MRI "resolvable" later
        iv. Bruno: Need ~10x resolution a la Kiuchi to "properly model"
        v. Ian: Kiuchi indicated we are nowhere near convergence, so we're not solving a well-posed problem. Idea of the subgrid model is to provide closure to the unresolvable physical scales. Papers in numerical analysis of turbulence demonstrate convergence is impossible, and need statistical models to get any confidence in the result. Thus subgrid may be the only path forward to get reliable result
        vi. Julian: Large discrepancies in physical scales make it nearly impossible to obtain convergence. In ordinary turbulence, energy large->small scales; if we get energy flows correct at resolvable resolutions, maybe the subgrid energy flows are not as important.
        vii. Ian: Are we confident we know which scales aren't important?
        viii. Julian: Radiation transport may be insensitive to subgrid, except for case of fluid scattering against photons, which may be a good thing. This is a problem dependent statement, however. In our case, are we in the right neutrino opacity regime?
        ix.
    b. Should subgrid model building for magnetic field dynamics include realistic microphysics?
        i. Ian: Most subgrid models (very few in GR) rely on the idea of scale separation, limited interaction between different scales. Many models are phenomenological with minimal inputs (good because cheap), but not built upon e.g., experimental data.
        ii. Bruno: Experimental?
        iii. Ian: Measure energy cascades at small levels, couple back to larger scales. Unfortunately we cannot access such data for BNS.

        iv. Riccardo: In our case, we must calibrate subgrid models using high resolution, then going to subgrid. Is it true we always need calibration against experiment, or is there another way?
        v. Ian: Could close by moving to another model instead. E.g., use kinetic energy calculations. Also e.g., turbulence due to interacting superfluid vortices there are theoretical calculations. So we're probably stuck using something phenomenological, based on other theories. Problem with moving to an ideal GRMHD at higher and higher resolutions is that the physics may violate the assumptions.
        vi. Bruno: How about local, high-res simulations?
        vii. Ian: Possible issue with gauge-invariance of subgrid models. Not clear that Newtonian subgrid model can be properly extended to GR
        viii.
    c. How do we validate sub-grid models? How to explore limitations?
        i. On a related note, could we agree as a community to share subgrid model source codes publicly after publication, so that different flavors can be compared?
            1. Bruno: Spritz code is publicly available, containing 2015 subgrid model. Disclaimer: Haven't tested with Spritz yet. Check out Zenodo (search for Spritz) & arXiv (Spritz paper)
4. How important is neutrino heating/reabsorption for certain effects? What about neutrino-antineutrino annihilation? How difficult is it to model those?
    a. Francois: heating/reabsorption: Important to get composition of the wind, so one can understand subsequent r-process. Annihilation: It could matter, and may clear out the polar region, but very difficult to do properly without proper transport: leakage schemes can't do it, and closure schemes are very inaccurate due to the fact that polar regions (where this is important) these schemes are worst behaved. Need e.g., Monte Carlo calculations, or possibly a subgrid model based on MC
    b. Riccardo: If energy deposited doesn't make a huge difference, would it make sense to treat heating/reabsorption in *post processing*?
    c. Francois: Dan is doing this now. Makes sense conceptually, but would be good to validate.
5. Julian: What is the typical energy density of neutrinos in postmerger disk as compared to the pressure in the gas?
    a. Francois: It's small, in my experience.
    b. Julian: How small?
    c. Francois: Total energy in neutrinos 1e-3mc^2, 0.1% rest mass.
    d. Julian: Then they probably wouldn't do much in the way of turbulent dissipation.
    e. Francois: Indeed, based on recent simulations
    f. Manuela: On what timescales?
    g. Julian: Not a matter of timescale. Question was about whether scattering of fluid motion against neutrinos is an important contributor to turbulent dissipation. In

ordinary disks, photons' thermal energy can be greater than thermal energy in gas. This case is fundamentally different.
    h. Manuela: Effect on accretion?
    i. Julian: More likely to affect cooling.
    j.
6. Ergostars are interesting objects. Milton, how did you set up the ID? How could they form in astrophysical scenarios?
    a. Milton: Here are links to the ergo star papers
        i. https://arxiv.org/pdf/2002.01473.pdf https://arxiv.org/pdf/1907.03765.pdf
        ii. Would be happy to chat about tomorrow (had to run to another meeting)
        iii. Here is my paper of the effect of the Bfield orientation https://arxiv.org/abs/2001.09153
7. BH-disk initial data (BHNS/BNS/collapsar remnant) simulations: what is the dependence on initial conditions? How much do they differ going from merger to collapsar context?
    a. Agnieszka: Started with 1D alpha-disk simulations with Kerr BH, currently working with Torus (Fishbone-Moncrief & Chakrabarti) initial data embedded with typically poloidal B field.
    b. Riccardo: How do you set the field?
    c. Agnieszka: Vector potential A_phi only nonzero component, use pressure/density isocontours. Normalize B-field strength according to Pgas/Pmagnetic
    d. Riccardo: If A_phi is set by pressure then the B-field is constrained inside torus, right?
    e. Agnieszka: Yes. During simulation the B-field evolves though
    f. Riccardo: So there's a transition phase?
    g. Agnieszka: Yes.
    h. Julian: This indeed is the most common approach
    i. Agnieszka: Yes, but there are more complex approaches
    j. Julian: Tsch. tried to pack magnetic flux onto the horizon very rapidly, a very special choice.
    k. Manuela: What about TCAN?
    l. Julian: Goal is to use realistic A-fields in remnant disk from BNS simulation
    m. Riccardo: Is there the idea to manually paint the B-field onto the disk?
    n. Manuela: This is a possibility, to begin with
    o. Julian: It may be a stepping stone, but we'd prefer to just use the remnant BNS disk self-consistently modeled using B fields.
    p. Riccardo: F. Cipolletta -- could you comment on mag initial data for BH-disk?
    q. Cipo: Working on implementing B field in Fishbone-Moncrief using sphericalNR, following EHT code comparison project's approach (purely poloidal B field).
    r. F. Armengol: Handoff of magnetic field & BNS disk in parallel. So far BNS disk tests have been performed in pure hydro. Should be straightforward to xfer B-field data from IGM or Spritz because both evolve the A-fields.
    s. Riccardo: Importing A-field data is best to avoid nonzero divB
8. Vassili: Regarding helicity. How can you get a helical structure from zero initial?

9. Riccardo: Helical structure should not be confused with “magnetic helicity”, an integral MHD quantity that is conserved in ideal MHD (and has to do with field topology).
10. Vassili: Are we relying on numerical resistivity to do physics?
11. Riccardo: The emergence of helical structures is not related to numerical resistivity, it is driven by the dynamics of the system (mostly due to mag winding, which is well resolved). If you change resolution, you obtain the same effect, helical field in other words is not generated by numerical resistivity.
12. Vassili: Am I just conflating large-scale field & helicity (defined on small-scales)
13. Riccardo: Right. Twisting a dipolar field into a helical field does not change the topology, nor the helicity. Apart from this, numerical resistivity will cause nonconservation of helicity, but the effect is very small (irrelevant for our description).
14. Beany: Is helicity a bulk effect?
15. Riccardo: No, helicity is an integral. Think of it as the number of times magnetic field lines are linked together. If all field lines exist as separate rings that are not linked, helicity is zero.

## Key References:

- Wright & Hawke 2019: arxiv.org/abs/1906.03150 (A resistive extension for ideal MHD)
- Ruiz et al 2020: effect of the Bfield orientation https://arxiv.org/abs/2001.09153
- Ruiz: Here are link to the ergo star papers https://arxiv.org/pdf/2002.01473.pdf https://arxiv.org/pdf/1907.03765.pdf