Please add your questions, comments, and topics for discussion here. Chairs may use this to inspire discussion topics (and/or call on you to ask your question directly) during the discussion sessions. Speakers are encouraged to add answers to any questions we do not manage to address during these sessions!

Monday Session 1

Andrew Cudd:

Ulrich Mosel: In any comparison of CC0pi data with generators or theories one has to consider also pion absorption. In the comparison shown neither SUSA nor the Martini model contain any such absorption. In https://inspirehep.net/literature/1644360 we showed that this reabsorption initially produced pions can amount up to sizeable changes of the X-section. Any further comparisons of 0pi data must take that into account. Care must be taken that pion production and absorption are linked by time-reversal invariance.

From Stephen Dolan in chat: To answer Ulrich's question: The SuSAv2 and Martini model Andrew showed on slide 12 has a pion absorption contribution added on top from NEUT or GENIE. It's a "frankenmodel" but the model drawn on the plots contains the relevant components.

Ulrich Mosel: On the question of how much the pion reabsorption contributes to 0pi events: 20% at very forward angles, only a few percent at larger angles (see paper cited above).

Alessandro: Then the discrepancies between models and data especially at forward angles --- and the advocated need of RPA, which is strange at this relatively-large momentum transfer --- could just be due to mis-modeling pion absorption?

Jon Paley: Alessandro, I can't speak for T2K, but in NOvA, which is at even higher energies, I think it would be extremely difficult to explain the low Q^2 suppression as a result of pion absorption (although we should take a closer look). We see strong evidence of low Q^2 suppression in 0-pi-like events. So that's not to say that pion absorption isn't contributing, but I doubt very much it's the whole story.

Ulrich Mosel: the numbers I quoted were for T2K. There all the previous, published comparisons simply ignored that reabsorption problem.

Jon Paley: Ulrich, sorry, I'm confused… don't they have an uncertainty on the pion absorption rate?

UM: in their published papers they simply compared with (among others) the pure QE predictions from SUSA/scaling.

Jon Paley: Sure, but the data points have an uncertainty included for the impact of pion absorption, no?

UM: I am not talking about the data: all I am saying is that they were comparing apples (scaling) with oranges (data). I discussed this at some length in the paper quoted above. This is essential when trying to compare particular 2p2h models with data!

Stephen: Where we compare to SuSAv2 we add pion absorption on top. The result is a CC0pi measurement. SuSAv2 gives us QE+2p2h, we then add the missing pion absorption from another model (and are clear about what we add in the papers).

UM: sorry, this may be so now, but in the T2K papers quoted this was not the case

SD: This is true for two comparisons in a 2016 CC0pi paper (and was noted). All the plots in Andrew's talk and the papers they refer to are making apples to apples comparisons where pion absorption components are added. This is written in the papers (page 16 right column in the paper we were discussing earlier https://arxiv.org/pdf/2002.09323.pdf).

Alessandro: Stephen, Jon, UM, thanks! So, the suppression in the longitudinal channel could be due to FSI, which becomes smaller at large Q^2 but should definitely be accounted for at lower Q^2. In electron-scattering, including them is important to reproduce the longitudinal response functions up to moderate values of the momentum transfer. There, two-body currents are small, so one can truly gauge the effect of FSI.

Raul GJ: SuSAv2-MEC model reproduces inclusive (e,e') data extremely well. It is not a microscopic model, but in an effective way it includes everything that can contribute to the inclusive signal, as the comparison with data shows.
Adding an additional contribution to SuSAv2-MEC would lead to breaking the agreement with the inclusive data.
In other words, SuSAv2-MEC model is complete, one should not add anything on top of it, it is designed to make predictions on inclusive signals, using it for exclusive channels is beyond its original scope.

UM: excellent Raul: finally someone who cares about theoretical consistency .-)

JP: Alessandro, just to be clear, FSI is included (and uncertainties on the FSI) in all results.

Alessandro: Thanks JP, but with FSI I really mean the quantum-mechanical ones. RPA is just an (approximate) example of them.

Guillermo Megias: As Stephen Dolan has mentioned, in recent publications we have added the pion absorption from another model on top of SuSAv2-MEC (1p1h+2p2h). For the moment, we do not have any RMF-based pion absorption model but we have found that we get a good agreement with CC0pi data when using SuSAv2 + 2p2h + pi-abs (https://arxiv.org/abs/1905.08556). This agreement with data is even better when using RMF + 2p2h + pi-abs.

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Ryan Plestid: Is there a simple picture that relates the transverse momentum imbalance & other kinematic variables introduced in the T2K talk, to the various nuclear effects mentioned? Is there a cancellation of e.g. FSI in one channel? Has this been demonstrated rigorously or is this based on physical arguments?

RIk Gran: A non-answer, but there is quite a bit of discussion across ~10 papers from the latest T2K and MINERvA measurements back to the initial sensitivity studies c.2015 . Those authors might point at the best subset to address your question. Maybe they are synthesized in a talk somewhere? Stephen Dolan, Luke Pickering, Xianguo Lu have worked on it back to the original papers, several new folks are also involved now.

Andrew Cudd: Following Rik's point, this paper introduces some of the transverse variables and how the nuclear model and FSI interactions affect their behavior:
https://arxiv.org/pdf/1512.05748.pdf

Stephen Dolan: For a more qualitative discussion of why different aspects of transverse kinematic imbalance are sensitive to different nuclear effects you can take a look at these two talks:
https://indico.ectstar.eu/event/19/contributions/409/attachments/313/414/sdolanTalk.pdf
https://minerva-docdb.fnal.gov/cgi-bin/RetrieveFile?docid=17864&filename=xlu_20180302_FNAL_final.pdf&version=2

Ryan Plestid: Thanks Rik, Stephen and Andrew :)

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Leo Aliaga:

Steve Dytman: How does Nova separate numu from numubar, nue from nuebar? Thanks, that makes the nuebar/numbar results you promise very tricky. I would be most interested in nue/numu! I look forward to visiting FNAL to discuss further.

Jon Paley: Steve, we don't have event-by-event separation. We do have some constraints based on data on the overall fraction of wrong-sign background in each beam. For the numubar analysis, the level of WSB is about 10%, so that results in about a 3-4% uncertainty on our measurements. For the nuebar, I agree, it's trickier since the fraction of WSB is much higher. We're considering some kind of combined nue + nuebar measurement (using both data sets together… two equations, two unknowns). Sadly, I would temper expectations for the nue/numu ratios… the uncertainties on these measurements are much higher than any differences one would naively expect to see. (I look forward to everyone coming back to FNAL! ;-)

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Mateus F. Carneiro:

Prabhjot Singh- On Slide 4 of Mateus talk. Do the SBND sensitivity lines with or without systematic uncertainties?

Mateus F.C.: Short answer, with systematic uncertainties.
For a more thorough description of the errors included (Figure 7):
https://www.annualreviews.org/doi/pdf/10.1146/annurev-nucl-101917-020949

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Adi Ashkenazi:

What is being done regarding neutron detection in LAr detectors (Arie Bodek

Steven Gardiner: In addition to the efforts mentioned in this morning's talks (e.g., neutrons in protoDUNE), the CAPTAIN experiment has a recent paper looking at the total neutron-argon cross section.

What are the main backgrounds to NC1p measurement?

Kirsty Duffy: See, for example, figure 4 (page 8) of https://microboone.fnal.gov/wp-content/uploads/MICROBOONE-NOTE-1067-PUB.pdf. The largest background is CC events that produce a proton, but some reconstruction failures cause us to miss the muon and mis-identify it as a NC1p event (we're working on a CC sample to constrain that background). The next-largest background is "NC proton + X", where we have an NC event that produces more than one proton (or some other particle along with the proton) that we miss. [In all cases here, I'm saying "a proton" to mean "a proton above detection threshold of p=300 MeV/c"] There can also be backgrounds from neutrons, if the neutron interacts in the argon to kick out a proton -- that could enter in the plot through any of the categories, I think, but the most obvious would be "Non-proton". Luckily that's estimated to be small - about 50 events out of ~5500 using the default interaction model in geant4 - but we are working on a systematic uncertainty to account for uncertainties in the neutron interaction model.

Steven Gardiner: ANNIE recently published a measurement of beam-correlated background neutrons for the BNB. The detector is in SciBooNE hall (closer to SBND than MicroBooNE), but their observations might be useful for getting a rough estimate of the level of background.

Juanma Franco: in slide 25, if I'm not wrong, the difference between the middle and right figures is that in the right you have extra cuts applied to the muon and proton kinematics (making both particles to be coplanar, constraints in the final proton polar angle, superior limit to the muon momentum, etc). What's the main contribution eliminated in the figure of the right that reduces the maximum to less than ⅓ of the maximum cross section measured in the middle figure? In other words, which extra cut applied to the muon and proton kinematics, not present in the middle figure, produces such a decrease of the cross section? I'm supposing it's related with the cuts applied and not with something related with the background contamination, etc.

Adi: Yes, the main differences are the requirement of 1 proton and the additional cuts: the colinearity cuts removes ~35% of remaining events, an additional cut eliminating events with energy deposition close to the vertex and unrelated to the 2 tracks, the coplanarity cut and the transverse imbalance cut, each removes roughly half of the remaining events after the previous one.

Juanma Franco:

As far as I know, these cuts should help to reduce background and enhance CCQE interactions but it's strange to me that those constraints
reduce the cross section that much. I have been trying to generate some theoretical curves in the PWIA to compare with microboone semi-inclusive data
and somehow I don't get too much difference comparing with the data without too many restrictions (of course FSI and 2p2h contributions aren't included
so we need to be cautious when comparing with data) but I do get a substantial difference when comparing with heavily restricted data. You can see here
the cross section as a function of the scattering angle where is shown both data sets and my calculations. I was wondering if there is any explanation to that difference
or if I'm missing some extra constraints that could reduce more the cross section.

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Aleena Rafique:

Prabhjot Singh- On Slide 3: The last bullet says that "begin data taking in late 2020s". When are the ND and the FD are expected to start data taking approximately?
Aleena Rafique: We plan to finish the detector construction by 2027 or so and start taking data right after but we don't have a definite answer yet.

Prabhjot Singh- On Slide 16: How do you define dcp resolution?
Aleena Rafique: The delta-CP resolution is produced using the 68% of the delta-CP values accounting for the expected systematics and statistical uncertainties.

Jon Paley: How well will the DUNE ND be able to constrain the nue/numu xsec ratio?

Jeremy Wolcott: Presumably the comparisons shown on p. 23 (and following) are to GEANT4, which includes its own hadron interaction cross section model. Are there any plans to make comparisons to predictions that use the pion (etc.) cross section models in, e.g., GENIE (or other generator)?

Aleena Rafique: You are right that these are produced using GEANT since the ProtoDUNE-SP is the charged test beam experiment. We plan to make measurements with GENIE using DUNE-ND in the near future.
→ followup (JW): GENIE can be run in a hadron scattering mode, and it would be interesting to compare those predictions to the ProtoDUNE cross sections :)
Aleena: Sure, good idea. I will pass along this request to the ProtoDUNE cross section group.

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Sam Jenkins:

Linda Cremonesi: First, great talk! And I’m really glad to see an improvement of this analysis coming out! My question was about dividing the FGD2 sample in the x and y layer, since you are already using the FGD1 sample to constrain the carbon interactions, in theory you could have just used the whole of the FGD2 sample without subdividing it? I’m only asking about this because in the previous iteration of this measurement, the migrations between FGD2 x and y layers were a pain (<- can’t find a better word for it!). So just using a “whole FGD2” sample would remove the problem of x and y migration, but maybe there’s something else I am not thinking about.

Margherita Buizza Avanzini: we are following the same procedure as we did here https://arxiv.org/abs/2004.05434, to extract the CC0pi xsec on oxygen and carbon (basically everything started from your analysis for CC1pi, Linda :-D). The reason to subdivide the FGD2 in two additional subsamples was to really select a water-enhanced subsample. For CC0pi, in FGD2x, we have something like 50% of events that are true CC0pi on O, while in FGD2y we have something like 60% of CC0pi on carbon. So, we decided to keep this separation (FGD1, FGD2x, FGDy) to be sure we have a sample that is representative of most of the water interactions. The goal is to reduce as much as possible the residual anticorrelation between the O and C xsec. For how the likelihood fitter that we use works, it is important to have “clean” signal samples. And here both C and O are signal, so you need C-enhanced and O-enhanced samples.
Also, for how the FGD2 is made, it is better to separate X and Y layers when evaluating detector systematics for this kind of analyses (O+C).
Anyway, you are right concerning migration (not sure Sam has already looked at this problem), but we have developed new systematics for this and noticed that in the end the impact on the total systematics is not that strong (everything is detailed in the paper I liked before, at least for CC0pi, but we have the systematics also for CC1pi).

Sam Jenkins: As Margherita is saying above, the procedure I'm following is the same as the paper above, and another CC0pi analysis that is yet to be fully finalised. I think it's best I let Margherita answer since she knows a lot more about these oxygen xsec extractions than i do!

LC: Thank you both Margherita and Sam for this thorough explanation! I see your point that from a fitter point of you having the two separate samples enhanced is definitely more powerful, especially because both C and O are signal and they probably don't have shape differences that you can use in your templates. Thank you!

London Cooper-Troendle:

Stephen: If I understand correctly you are suggesting explicitly using the MC truth in order to correct the data back to quantities we cannot directly measure in our detector (e.g. true neutrino energy or true energy transfer)? For me, the main goal of unfolding is only to remove detector smearing to allow our data to be compared with models and generators and not to correct for effects we cannot measure. If you do this then your measurement is only valid to the extent that your MC truth description of the effects we cannot measure is correct (which you can't be sure of by only showing agreement between the MC in the reconstructed space). Of course you can try to cover this with systematic uncertainties but I think the effects that you are trying to cover (like the amount of energy carried away by neutrons) come from physics that we struggle to define comprehensive uncertainties on. Why not aim to make a model independent unfolding in variables your detector directly measures?

Krishan Mistry:

Discussion session topic requests & questions:

Artur Ankowski: The effect of FSI on low Q^2 has been discussed in https://arxiv.org/abs/1506.03637 for MINERvA

UM: Indeed, the final state potential is quite essential, not only for inclusive X-sections. The same potential has to be used also for the final state cascade and, as far as I know, this is not the case in widely used generators. For example, when using a spectral function for the initial interaction then that FS contains implicitly effects of a potential. Then the outgoing nucleon should feel exactly the same potential. Same for SUSA, where again a potential is implicitly included in the scaling function. In both these cases, however, the potential itself is not really known and has to be put in by hand.

Artur: I can only speak for the spectral function calculation. In this case, the potential used to obtain the wave functions is the same as the one for FSI in the current analysis. This potential is determined in a phenomenological way, from proton scattering data.

UM: the spectral function is obtained from nuclear many body theory and thus contains already some potential which, however, is not known explicitly. The phenomenological potential that Artur mentions may be quite different from the one already contained in the SF.

Artur: Ulrich, most of the cross section for practical purposes comes from its mean field part, in other words from the wave functions in the potential used also to calculate FSI.

Camillo: Combined oscillation analysis has been done between SciBooNE and MiniBooNE and published in PRD, there you have an example of two different detectors in the same neutrino booster neutrino beam

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Monday Session 2

Laura Fields/Minerba Betancourt/Luke Pickering:

Artur Ankowski: When different processes will be treated as separable components of generators, internal consistency will be even larger concern than the dire issues we see today

Laura: Yes, we discussed this extensively at the workshop. Obviously, the goal is definitely the ability to generate neutrino interactions with a cohesive model from initial nuclear state to final state interaction. But in most cases we are currently quite far from that, and there are various reasons why separation of hard scattering / FSI would be useful at this point (e.g. for comparing models implemented in different generators to each other with a common FSI model, but other discussed in our white paper), while we work towards that goal.

UM: everything that was said by these three speakers was correct. It is a quite challenging program, both on the theory side, but also on the computational side. My question: who is going to do this all? A program with that width and importance can't be done by graduate students or postdocs who all move on after a while. This needs dedicated personnel. I hardly see this at universities, Fermilab has to step in!

LP: Completely agree, Ulrich. Minerba may have to give a more concrete answer, but the idea is to have support from FNAL. It would be good to get KEK/ICRR or Japanese universities involved too (Hayato-san will be the reference point here). NuSTEC is also another organization that could take some responsibility over the technical (computational/tools) side and also the scientific (modelling/consistency) side. But 'who is going to do this?' is something we are currently tackling in this effort.

Minerba: yes FNAL is going to provide some support, we welcome effort from all institutions, one institution only will not be enough effort for all the work that needs to be done. It is hard to get money from funding agencies to do only MC development ;)
I think there are components that could be splitted and post-docs or graduate students could help out and once they are done, the tools/components could be implemented in the big framework.

Marco Roda/Steven Gardiner:

Ulrich Mosel: Incl is a model where a projectile hits a nucleus from the outside. However, the situation in neutrino-induced reactions is quite different: the neutrino illuminates the whole nucleus so that the trajectories of outgoing particles start at different densities and in different potentials.

SD: We've adapted INCL++ to GENIE. Unfortunately, GENIE always put hadrons produced in the medium on-shell. We do that, then let

INCL++ take over. To be consistent, we should have the vertex properly medium-corrected, that comes a later day.

UM: This also shows quite clearly the limitations of any tabular approach: the tables would have to have much larger dimensions, because, e.g., density would also have to be a parameter.

SD: the developer theorists seem to like the table approach. Steven Gardiner can reply further if he wishes.

ClarenceW: For Marco's talk: 1) Are you using the MARES prior from the cited publication; in that case aren't you double counting the constraint since your tune and that publication uses the same data, and you're inputting their constraint to your tune? 2) Are your output parameter constraints on page 11 what you encourage experiments to use? They seem very very tight; perhaps they would be appropriate if our modern experiments were on free nucleons like ANL or BNL, but there is a large uncertainty in applying these parameters to predict nuclear data. Essentially, I worry the uncertainties are underpredicted in a nuclear environment, which will cause experiments to underestimate their systematic uncertainties from cross-section sources. Rik Gran: the community is still in a phase where this is an art, not an algorithm.   For every new observable (not just this talk) we take a critical look at the origin of the error bands on our input, and hopefully describe why a less aggressive error band, or a more targeted one, is helpful for the new observations.

MR, HI clarence.
1) So the prior on MaRES is the one from Q2 dependence data, while we only fit integrated cross section data. For sure the events might be the same, but there is not much more we can do.
2) well what we release is the fit on free nucleons. So if an experiment wants to increase the uncertainty because of the reason you mentioned it's totally reasonable. I guess the problem is a model problem. Can we use the free nucleon model on non-free nucleons?

Richie Diurba:

Luis AR: My concern with such tunings is how consistent with electron scattering the result is: all this physics is also present in electron scattering.
Steven Gardiner: Completely agree with this statement and Steve Dytman's response. The rate-limiting step is getting the Valencia calculation for electrons into GENIE. When that occurs, the tuning machinery will be ready to go for comparisons to electron scattering data.
SD: We are working on making GENIE elec scat code fully compatible with the neutrino side. That takes time and effort. In the meantime, we will take what we learn from elec scat and use it in our fits.

Gil Paz: What axial form factor are you using? MiniBooNE and lattice favor higher "axial mass" parameter.

Steven Gardiner: The nominal value of Ma for the base GENIE model that was used is 0.96 GeV. However, varying Ma has a very strong correlation with the QE normalization, so the tune results would imply a different effective value of Ma.

CM: 1) is the empirical model for 2p2h in GENIE based on MB data ?
2) MB re-weighted the pion bkg for their QE sample in a different way than other experiments, so maybe that is the source of the tension
3) Related to 1, if my memory is correct, then MEC+FSI are summed up in the empirical model.

Richie transcribing Steve D.: Yes but it is relatively flexible given what CCQE model used, so it is flexible for every dataset. I hope to use e data for GENIE empirical and then apply what is learned to neutrinos.

Steve D -> CM=Camillo?
1) Yes, for neutrinos. Unfortunately, it also depends on GENIE Llewellyn-Smith+RFG model.

2) As far as I know, MiniBooNE aimed to take out nuclear effects, but left them in the table so that we could all get to something that is same as modern measurements. That's what we all assume.
3) If my answer to 2nd point is correct, FSI is needed and therefore added.

Alexis: Regarding the normalization and axial mass. Combining with antineutrino data (for example MiniBooNE) would probably give insight to this, do you plan to combine neutrino and antineutrino ?

RD: That is a great suggestion and we have the framework already to do so. The overall goal was to focus on simple numu datasets, but we have the resources at the moment to do such a study. We will certainly discuss this for the future.

Chris Backhouse:

Iker de Icaza:

Luis AR: Wouldn't this BSM process interfere with the SM NC coherent photon emission?

Marco Roda: Hi Luis. In general it could be. But, in the specific case of the dark neutrino, photons are not possible in the final state. Only electrons. Also, as I was saying, the lifetime of the dark neutrino is very long, so the dark neutrino always leaves the nucleus, and then it decays.

Kajetan Niewczas:

Tuesday Session 1

Saori Pastore:

Luis AR: I'm wondering about the consistency between EFT MEC and phenomenological NN potential.

Luis AR: Also the applicability of the EFT MEC can be in question at high energy and momentum transfers that are present in few-GeV neutrino interactions.
JLB: We agree with data well up to and even beyond 2GeV of beam energy for electrons. We consistently nail the QE peak placement and correctly under predict the amplitude of the inclusive double differential cross sections, as this is currently a purely QE model.

JLB: It can also be noted that the distribution of the response density across many center of mass (total pair) and relative momenta that final state nucleons are not entirely back to back at all times upon interaction with the probe.

Artur: Can you predict this kinematic of final nucleons?
JLB: Yes. See our paper for double differential cross sections of two nucleon final states. We currently only show the final state leptonic variables, but these are commensurate with the nucleons. The full implementation in GENIE will utilize the full densities, not just the responses, which include a complete description of the lepton vertex, i.e., a full picture of the semi-final state before FSIs. Stay tuned for this full implementation with FSIs in the near future.

Artur: In two-nucleon final states, can you predict the kinematics of the second nucleon?
JLB: Yes, though currently the "exact" angular distributions are not computed, but are in principle possible to obtain. We plan to throw from several angular distributions at first in order to assess any discrepancies from data. The longitudinal and transverse components of each 1b, 2b, and interference scattering contribution to the total response density should give slight hints as to what the angular distribution is chosen.

Arie Bodek - how can there be interference between electron scattering in single body and two body currents when single body currents have on nucleon in final state ,and two body reactions have two nucleons in final state, unless the two nucleon
current ends up with a single nucleon in the final state,
Also what is your model say about the axial component of MEC and the relations between MEC structure functions in neutrino vs electron

Luis AR: Interference is possible if the nucleons in the one-body current are interacting ones. It's a matter of terminology. I'd actually not call them 1-body current anymore but many others do.

Oleksandr Tomalak:

Arie Bodek; In many analyses the distinction is made as follows. QE-like has only nucleons in the final state (no pions). So if a only a no pion cut is made, are electrons and muon difference greatly reduced since all the photons are included in the total energy? (Kevin McF: the electron cone size presented is appropriate for T2K and NOvA low Z detectors… it should be somewhat larger for liquid argon.)
Oleksandr Tomalak: Going to inclusive observables, electron vs muon differences usually reduce but it depends on experimental setup, i.e., definition of kinematics and cuts if they are still there. On slide 50, energy in cone observable is the closest to experiment. Indeed, electron vs muon differences are smaller than the most exclusive observable "soft photons only" in Slide 43.

Adi: Thank you for the talk! I have a few questions:

- For the radiative corrections in neutrino interactions you've mentioned the lower limit of radiated photon energy to be 20 MeV. What is the upper limit you're considering?
- Do you really consider a factor of Z in the radiative correction if the interaction is not coherent? (Ryan: To pick up a factor of Z\alpha you need things to be coherent i.e. soft-photon exchange.)

Oleksandr Tomalak: All photons below 20 MeV are included in all observables. In collinear and inclusive observables, we consider all photons allowed by kinematics.
Oleksandr Tomalak: Z factor was not included in CCQE part of this work. Only scattering on "free" nucleons is presented.

Jeremy Wolcott: The CCQE calculations are already fantastic. I am wondering whether it's possible to do something similar for pion production -- which is a very important part of the sample for NOvA and for DUNE. (Or whether you are able to speculate on how similar/different the corrections might be.)
Oleksandr Tomalak: It is possible to do something similar for pion production but it will be more involved technically and most likely more model-dependent for radiation of hard photons.
→ JW: Even with those provisos, given the absence of any other estimates, it would be *extremely* interesting. Consider this an invitation, if you are interested! :)
Oleksandr Tomalak: Sure, it is one of the future directions.

Beata Kowal:
Bryan Ramson: This type of treatment has been extensively explored at higher energies with DIS between lepton-nucleus and hadron-hadron interactions but involves an approximation with the transverse Q2 being much less than the scale of the problem. (https://arxiv.org/abs/1509.04766) Given the overlap in the resonance region for CC interactions, is there an opportunity to expand the polarized treatment from QE to DIS? I.e. how difficult is it to carry these polarized terms through higher energy interactions?

Luis AR: Alam, Sato and I considered QE + RES (using DCC) + DIS following Hagiwara et al who did it earlier. There will be a flash talk about it tomorrow

Sasha Tomalak: 1. Do you always use PCAC for the pseudoscalar form factor in your studies? 2. Which observable can be particularly interesting for extraction of axial form factors at GeV energies?

Atika Fatima: We have also performed the axial dipole mass variation in our work. The perpendicular component of polarization is quite significant for the extraction of axial dipole mass.

Luis AR: The contact term in the non-res amplitude was not included by FN but is required by chiral symmetry.

Teppei: Did you consider Cabibbo-suppressed CCQE? (Lambda production by anti-neutrino). NOMAD studied Lambda polarization in numuDIS and I am wondering if that is relevant for CCQE region. Also, from decay product (pi + p) you can get outgoing Lambda polarization. LArTPC can in principle reconstruct Lambda with some efficiency. (Bryan R.: Do you have a link for this study?)
Nuclear Physics B 588 (2000) 3–36
Nuclear Physics B 605 (2001) 3–14
These are 2 LAmbdaDIS polarization papers from NOMAD (TEppei)

Atika Fatima: Reply to Teppei: Our group (M Sajjad Athar and S K Singh) has done the studies for the polarization components of the Lambda produced in antineutrino reaction. You can find the details in Phys Rev D 98, 033005 (2018). ←I will check, thanks! (Teppei)

Aaron Meyer:

AM: Small correction to the question asked after the talk --- empirically, LQCD observes that the Delta becomes stable at 280MeV.

Or Hen: Are there any thoughts about EFT lattice matching to bring these calculations to nuclei? There has been some nice work on that for light nuclei and it could be a promising avenue for reaction physics…

Mike Wagman (reply to Or): this is super important but challenging because we want LQCD to provide input for the transition region where neither low energy EFT or perturbative QCD converges, so it's not clear exactly what EFT one should match to. We can use lattice results for few-nucleon systems to test and validate nuclear models in this region though, which in a more handwavey sense is matching to effective nuclear models. As LQCD multi-nucleon calculations reach physical quark masses and get systematics fully controlled this will be important to study.

Aaron Meyer: I agree with what Mike has said and I don't think I have anything constructive to add here. Matching LQCD to EFT is certainly the way forward and there should be a roadmap in place for how to go about this as the LQCD calculations improve. It might be easier for EFTs to match onto high-Mpi LQCD calculations until LQCD can actually achieve physical pion masses, although I don't know the implications/difficulties of raising the pion mass from the EFT side.

Baroni:
How important is it in the single nucleon sector for instance in the calculation of g_A, to keep track of the exponentially suppressed finite volume corrections?

Aaron Meyer (response to Baroni): In the single nucleon state, the finite volume corrections are not so relevant. They typically go as exp{-MpiL}, with MpiL~4. For most of the ensembles used in typical calculations, leading order chiral perturbation theory tells us that these corrections are around 1% or smaller. The more relevant issue is with the finite volume corrections of the excited state corrections coming from Npi states and nucleon resonances, which have power-law finite volume corrections and are much more relevant. There are some studies that look at excited state contamination in these computations, but none have used Npi (qbar+4q) operators which are empirically seen to be absolutely necessary to extract information about the relevant states.

Baroni:
Thank you for the useful answer!

Fernando Alvarado:

Arie Bodel The important thing is to compare FA(Q2) for data vs calculation. FA(Q2) is available for neutrino D and pion electroproduction,

Luis AR: we're using EFT and therefore are limited to low Q2, we can't consider the axial ff at all available Q2 either in lattice or experiment. But we certainly extract the low q2 dep of the ff even if it's useful to quote the radius as a benchmark quantity.

Ulrich M: Isn't it courageous to extract axial radii? After all, even the 'normal' vector radius of the proton still presents a problem!

Luis AR: we provide the chpt based mpi extrapolation which is valid regardless of the lattice results themselves. "Data" might improve in the future as Aaron said, but the chpt approach remains, only the LEC values will change.

Aaron Meyer (response to Arie): The axial radius is not meant to be a stand-in for the form factor over the entire Q2 range. It is merely a definitive quantity that can be used as a benchmark when comparing different extractions of the form factor. Using a dipole mass only makes sense in the dipole parameterization, whereas the axial radius is defined as a slope of the form factor and so makes sense in the context of other axial form factor parameterizations.

UM: I quite agree with that. I was just wondering if one could trust any axial radius from lattice, as long as the em radius is not under control.

AM (response to UM): Perfectly valid question. I haven't scrutinized the vector radius in as much detail as the axial radius, but my understanding was that the vector radius had fewer problems within the quoted uncertainties. There's some justification for expecting less excited state contamination in the vector channel than the axial channel. In the axial channel, the axial current can excite a pion while the nucleon is treated as a spectator only (up to the necessary momentum injection). This seems to enhance the coupling to Npi excited states, I think because it requires fewer gluon exchanges. For the vector current, the nucleon can only be a spectator when transitioning to the Npipi channel, which chiral perturbation theory tells us is suppressed with an additional factor of the spatial volume (phase space factor) relative to the Npi state. Noting this, the vector form factor/vector radius is the canary in the coalmine for LQCD - if the vector FF can't be reproduced, then the axial FF should be suspect.

AM (continued response to Arie): For the z expansion, the axial form factor is very nearly linear in z, so having the axial radius will give you information about most of the form factor Q2 range. So even if the axial radius doesn't contain the full Q2 information, it gives a great deal of information about the form factor already.

Narbe Kalantarians:

Khoirul Faiq Muzakka:

Arie Bodek - The chorus data is on lead and the ccfr/nutev is on iron, so nuclear correction are different,

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Tuesday Session 2

Manuel Alejandro Ramirez Delgado:

Jon P.: In the data-preservation project, are there selection criteria applied, eg, fiducial and containment?

Rob Fine: @Jon A little more detailed of an answer to your question: We plan to include low-level objects (calibrated hits) in addition to the higher-level objects that are inputs to current analyses, so that a future analyzer could, in principle, implement new reconstruction techniques. We won't be providing the same level of support for that kind of new reconstruction out-of-the-box, as we plan to do with higher-level reco/reproduction of existing analyses. The idea is to make it "easy" to e.g. change the fiducial volume or signal definition of an analysis and "possible" to e.g. implement new PID

Amy Filkins:

Tejin Cai:

Andy F: Is there any physics reason to expect delta pT_x to be asymmetric? It seems counter-intuitive to me.
Tejin (reply to Andy): We think it's the underlying resonant pions that contribute to the asymmetry. I think there are measurements on pions that see asymmetry. Our signal definition is the QELike definition, so events with pion absorptions leak in.
Andy F (reply) - would this allow us to measure delta polarisation in-medium? (perhaps this should be directed at a theorist?)
Oh, also what are the prospects for measuring these same variables in antineutrino mode? Would we expect the asymmetry to be reversed?

Tejin(reply): Maybe with enough data, MINERvA's ME result might be sensitive to these effects. Here is our LE pi0 analysis: https://arxiv.org/pdf/1708.03723.pdf, Fig 22 actually shows slight hints of asymmetry in the pion azimuthal angle. For antinu the processes are slightly different -- for the combined hadronic system we have proton+pi-, for QELike there is only neutron. I think it can be done (QELike-wise some of the variables can't be measured, if there is a proton it's all FSI, still neutron's position could tell us whether it's asymmetric to the other direction), but we don't have enough manpower.

Yiyu Zhang:

Erin Conley:

Diego Aristizabal:

Michelangelo Pari:

Wednesday Session 1

Ayami Hiramoto:

Rik Gran: very nice talk. As you collect higher statistics samples, the multiplicity of protons will be very interesting. 200 MeV/c momentum is 21 MeV KE. The emulsion technique is a nice way to access these very-low energy hadrons. There was discussion in an earlier day of this workshop, how do we account for (can we constrain) specific FSI processes like pion absorption. This should show up as events with several nucleons in the final state. Do you think the eventual data set will have sensitivity to something this specific?

AH:@Rik Gran Thank you very much! Yes, giving such kind of constraints is one of our aims. We have relatively large uncertainty on momentum reconstruction (~30% for multiple scattering and ~5% for range) but we can achieve small uncertainties for the angle and multiplicity measurements (although not achieved yet in the pilot runs). The dominant background for the multiplicity measurement is cosmic ray but we can estimate its amount precisely. I guess we can start such kinds of analyses with the data set of the first physics run but more data is desirable. RG: Looking forward to more! With MINERvA data, I personally study the smallest energy deposits in single detector elements (we are underground, no cosmics). I like to imagine what detail we can not see, but you will see it !

Jesus Gonzalez Rosa:

JLB: What does "BC" stand for?

Jesus Gonzalez (reply)
BC stands for Bosted-Christy parametrization. References:

P. E. BOSTED and M. E. CHRISTY , Phys. Rev. C 81, 055213 (2010)
P. E. BOSTED and M. E. CHRISTY , Phys. Rev. C 77, 065206 (2008)

Also,
PDF (denoted as GRV98) reference: arXiv:hep-ph/9806404

The reference for the other parametrization Bodek-Ritchie (BR) can be found in the presentation.

M. Rafi Alam:

Asia Sobczyk: For some results (slide 17) DIS and DCC polarisation curves don't match, even though the cross section from slide 16 does. Do you know why?

Luis AR: I'm not sure… We'll check and let you know. <-Asia: Ok, thanks!

Luis AR: The relevance of different structure functions is different for cross section vs polarization and the matching can be different for different structure functions so, in general, good matching in cross section doesn't grant good matching in polarization. This is a general argument: a more specific answer requires checking different terms for a given kinematics. We should do this indeed. Thanks!

Giorgio Pintaudi:

Jon Paley: is the mechanism for the changing response of the detector over time understood?

Giorgio: Because of design flaws in the frontend electronics (based on the SPIROC2D chip), a satisfactory detector calibration is very difficult to achieve, no matter how hard we try to calibrate the threshold (using the S-curve) and the gain (using the dark noise finger plot). The nature of the design flaws is now understood but they cannot be solved without completely redesigning the frontend boards.
Moreover the MPPC* model that we use in WAGASCI is a new generation one with very low dark noise (remember that we need the dark noise to do the online calibration and to draw the finger plot from where we derive the gain). All these issues together make the online calibration a daunting task.
These issues can be somewhat alleviated by sophisticated software analysis and test-bench of the electronics. I worked a lot on the software analysis tools but there is still room for improvements. The test-bench of the electronics is still to do : that is why the simulation of the electronics is still undone.
The conclusion is that we still do not fully understand the electronics response but even with a complete understanding the operation of this kind of electronics is still very difficult.

*the MPPC (Multi Pixel Photon Counter) is a type of APD developed by Hamamatsu Photonics

Ryan Plestid:

Artur: we used the effective momentum approximation, generalizing EMA', for neutrinos in https://arxiv.org/abs/1404.5687

Ryan: I was being "loose" in my language. EMA and mEMA were both called EMA in this talk. The mEMA is applied also by the Ghent group, in GENIE, for astrophysical neutrino detection. To the best of my knowledge *every* paper that accounts for the Coulomb distortion is citing Engel's paper (happy to see a counter example)

GiBUU uses some semi-classical propagation in a Coulomb field. I could not discern from the documentation whether e.g. the amplitude is affected of there is just a semiclassical propagation of leptons inside the nucleus. @UM do you have any comments?

I am also interested to know how these effects are included in NEUT and NuWro

Kevin: is it obvious that it should be the "axial" radius describing the size rather than the charge radius?
Ryan: r_A was just "radius of nucleus A" not axial radius. Apologies for the confusing notation.
Kevin: @Ryan, I should have guessed! I'm lecturing on nuclear physics right now! So charge radius it was…
Ryan : @Kevin kind of. In the toy model I take a charge distribution which sources the Coulomb potential and this is treated as an independent parameter from the characteristic size of the binding potential. Obviously in real nuclei these are

correlated, but it helps to separate the two scales explicitly. There is an interplay between the two scales.

In this talk r_A refers to the characteristic size of the bound-state wavefunction. In a realistic application you have a hierarchy like 1/\sigma_\perp < r_A << 1/E_\nu \sim 1/(1 GeV).

UM: Do we have any guesses on how big these changes are (and their dependence on energy)?

Ryan: The shift in momentum \delta k \sim V(0) \sim Z\alpha / r_A \sim Z/(A)^0.33 \alpha / 1 fm \sim Z/A^(0.33) x 1.4 MeV . The relevant energy scale in the transverse direction is \sigma_\perp \sim \sqrt{ Z\alpha / r_A^2 } \sim \sqrt{ Z/A^0.666 \alpha / 1 fm } \sim Z^0.5/A^0.3 (20 MeV).

Work on estimates for relevant phenomenology are underway. I do not have anything I would quote publicly right now.

NatalieJ : sorry, had to miss your talk because I was teaching - so probably you discussed this, but how low in energy can you go with this technique ?

Alexis Nikolakopoulos:

Andreas Kronfeld: Do you have in mind using KDAR as a test? By test, I mean first carrying out the comparison of model w/ low Q^2 data, and when one is “happy” with the model, then check how well it does for KDAR.

I think significant constraints are already set by (e,e’) data in this kinematic region, which is often not taken into account when experiment specific tuning is considered, which can be for several good reasons.
On the other hand, some of the parameters that are fixed in such tuning are specific to neutrinos and don’t affect (e,e’), e.g. the axial current parameters and effective binding energies or energy shifts that take into account that the interaction is charge-changing instead of NC.
Comparison to KDAR data can serve as a test of these corrections in a region that is non-trivial, but in which one doesn’t have to worry so much about different competing interaction mechanisms.

Gustavo H Guerrero Navarro:

Atika Fatima:

Andreas Kronfeld: what did you use for the default SU(3)-symmetric form factor? For example, is it a calculation or an Ansatz?
AF: It is a calculation. You can see our paper for reference Phys Rev D 98, 033005 (2018).
AK: @Atika, in a quick skim, it seems to me you derive relations between the nucleon->hyperon form factors and nucleon-> nucleon form factors. Then you take the vector parts from parametrizations of data, and the axial from the dipole Ansatz. Is that right?
Atika: Exactly, that's what we have done.
Andreas: I suppose as lattice QCD matures, you can simply swap out your 2018 choices for those—to see how they work, changes, etc.
Atika: Thanks for the suggestion. We may try it in the future.
Andreas: 👍

Luca Doria:

Luis AR: are you going to analize exclusive pion electroproduction?

LD: Pion production is something we will consider but our priority for now is finish inclusive cross sections and then move to the proton channel first. We have also a special spectrometer dedicated to pions which we can use.
If the interest for pion production is high, we could adapt our program.

Luis AR: Thanks. Pion pion production measurements at MINERvA and MiniBooNE have raised questions and there are no corresponding data with electrons.

UM: There are indeed very few charged-pion data with electrons, but there are lots of data for photoproduction on nuclei, mainly from TAPS at MAMI and at ELSA.

Adi: Thank you for this interesting and exciting talk! Do you intend to measure any exclusive results?

LD: We plan to do the proton channel first. Before moving to exclusive, we have to prove all out techniques with a series of inclusive measurements though.

Adi: Are you using also polarized targets?

LD: No polarized targets for now. We had a polarized 3He gas target in the past.

Adi: When are you scheduled to run with Argon?

LD: Soon (weeks/month) we will have a jet target test and we will try Argon, hopefully.

Adi: Can you please specify the expected Luminosity for the Argon run?

Adi: Can you use all 3 spectrometers?

LD: Yes, they can be all put in time coincidence. In principle you can do one or two-nucleon Knockout measurements for example.

Adi: Are you able to trigger on two spectrometers and veto a third?

LD: Offline is possible, online I do not think we have this trigger but it can be implemented (FPGA).

Adi: There are many interesting avenues one can pursue at MAMI, we'd be happy to talk about them.
We'd also be very happy to supply more updated models. If we won't find your email, please contact us: adi@fnal.gov

LD: Adi, thank you very much. We are in a "testing" phase and we focus on completing the Inclusive data but the real aim are specific relevant final states.

Adi: Following Steve Dytman's comment, indeed we have many inclusive results already from past experiments, and we need more exclusive measurements. To my knowledge, we don't yet have Argon results in MAMI available phase space. It will be great to see also those.

LD: we can discuss about the missing kin. space. Thanks.

UM: There is a much more updated model: GiBUU, gibuu.hepforge.org

LD: Thank you for the suggestion: we'll look into this.

Minerba: Is the MAMI collaboration planning to do any MC tuning with the collected data or getting involve with other tuning efforts?

LD: I am personally very interested in getting involved in generators. The collaboration itself does not have plans on this. We are just starting the experimental program.

Minerba: Good, Adi and I will get in touch, thank you very much for great talk

Artur: while this point is not very relevant for the main message of your talk, I would like to point out that there are many data sets available at 145 deg between 120 and 560 MeV, by Barreau et al.

LD: Yes, thanks. We are aware of the Barreau data. Unfortunately I did not include them in my plots.

Guillermo Megias: Thanks for this very interesting talk! On slide 10, are you using the QE or the RMF contribution for the "Megias et al. full" curve?

LD: I have to ask Miha which QE curve he used in this plot. Thanks for pointing this out.

Guillermo Megias: I remember I sent some results to Miha Mihovilovic some time ago for the particular case 885 MeV, 70º but not for 36º. Are you using the SuSAv2-Genie implementation for the 36º case? In case you need more predictions, please let us know: megias@us.es

LD: I do not know if Miha run the code himself. I will ask him more details on this.

UM: There were actually data taken at MAMI some while ago, at a little lower energy:

LD: Thanks.

Investigation of multiparticle final states in ^{12}C photoreactions

P. D. Harty, I. J. D. MacGregor, J. Ahrens, J. R. M. Annand, I. Anthony, R. Beck, D. Branford, G. E. Cross, T. Davinson, P. Grabmayr, S. J. Hall, T. Hehl, J. D. Kellie, T. Lamparter, J. A. MacKenzie, J. C. McGeorge, G. J. Miller, R. O. Owens, M. Sauer, R. Schneider, and K. Spaeth

Phys. Rev. C 57, 123 – Published 1 January 1998

You can find more papers from these experiments by doing:
f a grabmayr and date before 2000 in INSPIRE
These were photoproduction experiments all in the Delta region.

Steven Gardiner: Very interesting talk! Can you elaborate a little more on the expected capabilities for the low-energy electron measurements which could inform supernova neutrino interaction modeling? What is the minimum usable beam energy? Would you be able to perform exclusive measurements of nucleon and/or gamma-ray emission at these energies?

LD: Steven, we will have a 150 MeV accelerator with high currents and spectrometers. One can do eN scattering e.g. on Argon in the SN interesting range. I'm just starting to think about a possible project in this direction and I was actually reading you work on MARLEY.
I got to know MARLEY within the DEAP-3600 collaboration where we are looking also for solar neutrinos.
Maybe we can discuss together about this at some point. I will contact you.

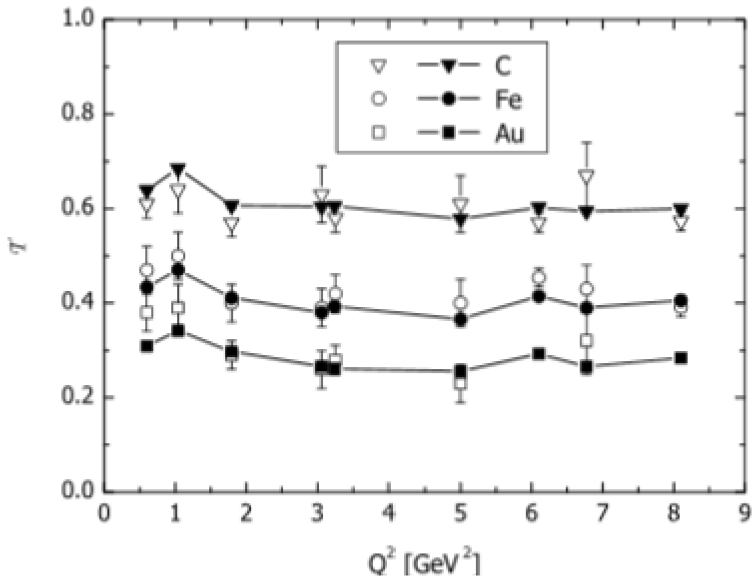
Steven Gardiner: Thanks for the helpful response and your interest in that work. Yes, I'd be happy to talk with you about possible projects.

Wednesday Session 2

Julia Tena Vidal:

Noemi Rocco: can you please discuss a bit more how Pauli blocking is implemented for pions? Is it strongly model dependent or is there a common prescription used for the three event generators? Does it play a big role in the results you are showing?
Steve D: basic idea in implementation of Pauli blocking is to suppress events where the recoil nucleon is below the Fermi sea. It is done in different ways in the generators.

UM: in 2002, nearly 20 years ago, one of our students, Jürgen Lehr, calculated already the transparencies with an early version of GiBUU. The results are in his thesis:
https://www.uni-giessen.de/fbz/fb07/fachgebiete/physik/institute/theorie/inst/theses/dissertation/previous/lehr_diss
And here is the main result for the transparencies:



Taken out of: http://th-www.if.uj.edu.pl/acta/vol40/abs/v40p2571.htm
It is strange, that now, 20 years later, that work is already forgotten
Steve D: It's not forgotten, we refer to it in our paper. It's in a different kinematic regime than our work.
UM: Why reinvent the wheel.

Noemi: Do you expect A independence even for other observables?
Steve D: yes, certainly. This is very inclusive, NO specification of final state. As data considered gets more exclusive, more A dependence will be seen.

JLB: Have you checked for any presence/absence of nucleon position correlations for the transparency? Could the energy cutoff effect this?

Alessandro Lovato:

Luis AR: Does the QMC configuration change as the cascade evolves?
AL: We keep track (evolve in real time) of all the interacting particles, whereas the background ones are frozen until they interact with a propagating particle. Suppose you start

with one particle, there is an interaction, then we keep track of two particles (keeping the other A-2 frozen). If these two interact with a third, we keep track of the third and so on.

JLB: When more than one particle is within the interaction cylinder, does the probability go as a binomial distribution?
AL: The length of the cylinder dl is small enough that there is almost always only one particle in it. In the (extremely rare) cases where there is more than one particle, we only interact with the first;
JLB: If you want to consider more than one rescattering, there is a simple method developed here (see Eqs. 11-15 and surrounding discussion). Of course, for small nuclei, this is a small effect. Considering the inputs for these calculations are known up to calcium-40, there could be larger effects thereafter.

JLB: Are the momentum vectors of the nucleon scattering centers at all radii assumed to be isotropically distributed? Or is there a correlation in their directionality at various radii?
AL: The positions and the isospin of the nucleons are sampled from ground-state wave functions of 12C, so they retain all correlations among radii and isospin (hence the difference between np and nn nucleon-nucleon distributions)

UM: So here is my point again: as soon as you have a few nucleons already knocked out, your QMC groundstate becomes irrelevant. For example, there is nothing in your method to avoid knocking out 12 nucleons from a C12 nucleus. Actually, in the old calculations by Lehr that I quoted above, we did treat the time-dependence of the groundstate and did not work with a frozen-density approximation. Nowadays, to save time, we actually rescale the cross sections so that it becomes less and less probable to knock-out a further nucleon.
AL: We use QMC configurations rather than nuclear densities, keeping track of all "active" particles (i.e. particles that have undergone at least one scattering). When studying 12C, the probability of knocking out > 2 particles in 12C much smaller than knocking out 1 nucleon and so on. This is at least as good as re-scaling the density, but in principle it is more accurate than that. However, I agree that non-interacting particles are kept frozen.
UM: indeed, it is the configuration of the non-interacting particles that makes problems. Of course, the high-multiplicity events appear more frequently at higher energies, but already at DUNE that is a problem.
AL: We know where the non-interacting particles are; we just do not time-evolve them. However, in 12C it is very unlikely to knock out more than 3 particles. Let me try recovering the plots
NR: For the "simple" mean free path approach we also account for this effect by rescaling the proton and neutron densities every time a particle interacts.
LP: Typical experimentalist question. What is the path to getting this work into experimental simulations, does it need supercomputers to run? Or can inputs be generated and then events thrown on single CPUs in reasonable time (say 1M evs < 24 hours for reasonable).
AL: We do not need supercomputers for our simulations, yet. A few MPI ranks are sufficient for all practical needs (up to now!) The code is on GitHub and you are welcome to download and use it
LP: 🤩 I couldn't see the link to the repo in your slides, are they in the Isaacson paper, or could you link here if not?
AL: Here it is: https://github.com/jxi24/IntranuclearCascade

Alessandro Baroni:

Noemi Rocco: Would it be possible to add explicit pions?

AB: Yes it would require adding nonlocal terms (that connect different lattice sites) in the Hubbbard-like Hamiltonian.

Efrain Segarra:

Arie Bodek: One has to be careful about mixing Fermi motion and EMC effect. Since Fermi motion dependence on Q2 does not follow QCD. So the PDF analysis will not work;

Or (comment on discussion following Efrain's live response to Arie's question above): Nothing that Arie said is incorrect and at the same time I think Efrain is correct that its out choice if we want to separate the effects or not. Its not clear to me that separating them, as it currently done in generators, is better than removing fermi-motion effect in the generator for these processes and using just the nuclear PDFs that account for all effects together. We have to keep consistency but have more than one way to do it.

Arie: What you say is correct, if we only have DIS. But when it comes to matching a DIS region and resonance region there is a problem in two inconsistent approaches.

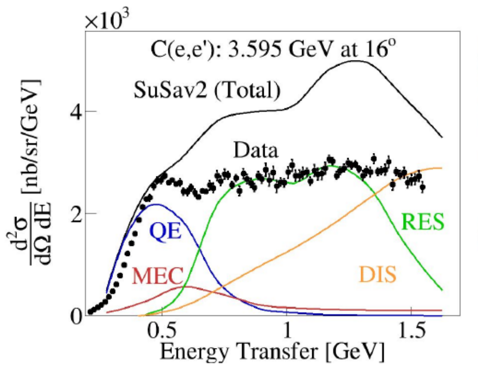
Afroditi Papadopoulou:

Arie Bodek, One reason why Resonance region model shown has issues is because it is well known that DIS curves averages the resonances, so one cannot add DIS and resonances models, the models used in electron scattering a a Duality base curve for DIS which is multiplied by a resonance modulating function in the resonance region.
I have several other comments that are easier in person.

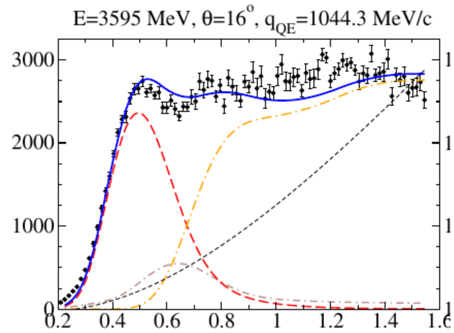
Artur: I second Arie's comment. I will be discussing it in detail in my talk on Thursday. Both GENIE 2 and 3 suffer from double counting in the region where higher resonances contribute.

Stephen: It's important to point out that when we see SuSAv2 making exclusive predictions, it's the well validated inclusive model that's been GENIE-ified to make exclusive predictions (in a similar sort of way we do for other models in the generators). It's also worth noting that the "SuSAv2" part is only the QE+MEC. The RES and DIS are from GENIE.

Raul GJ: Maybe I missed from the talk, but the SuSAv2 implemented in the generator does not match the original SuSAv2. This is what you showed



And this is the original result ([PHYSICAL REVIEW D 94, 013012 (2016)]())



Maybe you should refer to SuSAv2 differently, something like GENIE-SuSAv2 or something else.

Steve D: Interesting, I think we should explore this difference further.

Stephen: I think the QE should be almost identical (it looks similar by eye) and the MEC should be very close (HT tables vs SuSAv2 parameterisation causes some small differences). The difference here is the inelastic part which has nothing to do with SuSAv2 in the top plot. I agree it makes sense to preface SuSAv2 shown like this as SuSAv2-GENIE or similar.

Raul GJ: I see, thanks for the clarification.
I think it is obvious but I will comment on it anyway. There is a clear double counting in GENIE, with the RES +DIS. If one has a RES that already by itself matches the inclusive data then the DIS should not be there.

Steve D: GENIE works hard (within the Frankenmodel paradigm) to avoid double counting, not always possible. Since the DIS below the cutoff (W<1.7 GeV until recently, now 1.9 GeV), that's the best example of double counting. I see the S11(1535) peak in both RES and DIS. Above the cutoff, there is no resonance contribution.

UM: In her plots I am missing an essential contribution to the inelastic cross section: the 1pi background contribution is completely missing, but nearly 30 years of research around the Delta-hole model have shown that that must be there! This is even textbook wisdom by now: see the book by Ericson and Weise on pi-N-Delta physics. Has that component, which should contribute on the left side of the Delta peak, been tuned away?

Guillermo Megias: As commented before, the SuSAv2-Genie results only contain QE+2p2h, the RES + DIS contributions in Genie comes from other Genie predictions. The SuSAv2-inelastic model is also available to be implemented in generators (at least for electrons, for the moment). It can be implemented for the full inelastic regime (RES+otherResonances+DIS) or for some particular regimes (for example, removing the Delta resonance region related to 1pi emission) by introducing some cutoffs in the invariant mass (W) integral of the model.

Comments for discussion

Camillo Mariani to Everyone (1:40 PM)
I think that we should specify what do you mean by accuracy and we should start including systematics on the model and MC prediction then we can consider inclusive or exclusive model predictions, but we will have the associated errors

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Thursday Session 1

Michael Nieslony:

Jon Paley: Nice talk, it's so great to see the great progress ANNIE is making. On slide 15, any idea what's going on at early times in the plot on the left?
Michael Nieslony: Hi Jon, thank you. Do you mean the "Selection cuts for neutron capture" plot on slide 15 or the "PhaseI background rate plot" on slide 16?
JP: Sorry! I meant 15
MN: Ok, no problem. So I think this combination of a slightly smaller rate at the beginning convoluted with an exponential decay at later times is expected, but I can try to find a source for that.
Steven Gardiner: Michael should correct me if I'm wrong, but I think the deficit at early times is due to the time it takes for the neutrons from the source to thermalize. The capture cross

section is far larger for thermal neutrons than it is for fast ones. We saw a similar effect in Phase-I calibration measurements using a 252Cf fission neutron source. In Fig. 6 of the Phase-I paper, there's also a "bump" in the neutron signal that shows up several microseconds after the source trigger. The falling part of the distribution fits really well to a decaying exponential (with the correct time constant for thermal neutron capture), but the initial part had to be modeled with Geant4 to get the transport/thermalization right. I suspect something similar is going on with the Phase-II calibration data using the AmBe source.
MN: Yes, thanks a lot Steven for the detailed explanation. The thermalization process also in this case seems to be responsible for the observed behavior.
JP: Thanks,@Steven and @Michael!

JS: What is the energy threshold for the neutrons' detection?
Steven Gardiner: ANNIE uses neutron capture on Gd to detect the neutrons, so there is no threshold.
Michael Nieslony: Yes, thank you Steven. If anything we might have more problems with very high energetic neutrons that are not contained.
Thanks!

Tejin Cai: What is the angular resolution of detecting the neutrons at ANNIE?
Michael Nieslony: So the Gd capture signal is rather uniform since it emits multiple gammas. We have some cuts to identify these neutron capture signatures. In addition we will have some vertex reconstruction on the neutron position, so we will know where the neutron capture happened and can link that to the measured neutron capture efficiency for the respective position.

Roberto Petti:

Ryan Plestid: Have you quantified the systematic uncertainty on the C-background event rate? How sensitive is your subtraction to nuclear physics uncertainties. I.e. suppose we mismodel ^{12}C cross sections, could we significantly contaminate the "H" sample?
RP: the background subtraction is model-independent as it is based on the graphite targets, which are designed to have identical acceptance as the plastic CH2 ones. The technique does not work without graphite for the reasons you indicate.

Ryan Plestid: Are there any systematic uncertainties due to different detectors between the CH2 sample and the graphite sample? What are the detectors surrounding each target material?
RP: each target is 1-2% of radiation length and they are designed to have the same equivalent thickness and identical acceptance (they are interleaved throughout the volume).

Un-Ki Yang: I have a comment on the discrepancy (15~20%) at low x region (x<0.1) between neutrino data and charged lepton data, which was interpreted as a different nuclear effect between neutrino and charged lepton scattering. In my CCFR data analysis, I proved that two dataset (neutrino and muon) are consistent within 5%, if heavy charm quark production was properly treated. See *Phys.Rev.Lett.* 86 (2001).
It seems that authors of this paper (PhysRevC.96.032201) was not aware of the CCFR paper.

RP: Thanks for the comment. I used the plots simply as illustration, as you know there are many outstanding discrepancies. The point is that in the absence of an accurate calibration of the neutrino energy scale from data we can never be sure and have to rely on MC/model corrections.

Steve D: Please explain how same measurements can be used to measure both cross section and flux. I am likely missing something.

Arie Bodek - Yes, you are missing something, low nu method is used to measure neutrino flux.

Zara Bagdasarian:

Luca Doria: what is the energy threshold with a WbLS for seeing Cherenkov+Scintillation signal?

Jon Paley: great talk! On slide 17, what kind of systematics are assumed in these sensitivities? How will nu-A-scattering-associated systematics be constrained for the THEIA LBL measurements?
Hi Jon, thanks for your question. We assign independent normalization uncertainties of 2% (5%) on each of the nu and antinu appearance signal(background) modes. We do not explicitly include the nu_mu disappearance samples, but the choice of uncertainty for the appearance samples assumes some systematics constraint from the disappearance samples. You can find the full paper here: https://doi.org/10.1140/epjc/s10052-020-7977-8
Not sure I understand what you mean by nu-A-scattering
JP: nu-A = neutrino-nucleus. I think DUNE is assuming similar uncertainties on the normalization of nu_e/nu_mu cross sections, and there the detectors have the same nuclear target. So it would seem to me that you would need to include additional uncertainties for the fact that THEIA and the DUNE ND have different nuclear targets (similar to T2K).

Thursday Session 2

Matthew Heath:

Steven Gardiner: Nice talk! Can you say a little more about the status of the neutrino-induced neutron measurements? Those data will be really interesting as well.
Thanks for the question Steven. I can't say much more than this but it is getting close to finalizing data cuts and signal/background predictions before box opening.
SG: Thanks a lot. I'm looking forward to those results whenever they're ready.

Joanna Sobczyk:

Luis AR: Do you take into account the uncertainty in the truncation of the perturbative expansion?

JS: Do you mean truncation for the chiral hamiltonian? Or about truncation at the level of coupled cluster?

Luis: I was thinking about the power counting expansion for the currents.

JS: Oh, ok. So we expand the currents to the same order as our chiral Hamiltonian. And for the longitudinal response it's actually easy, because we just have the leading order term (we do not include 2-body part)

Luis: I understand you need to use the same order for currents and Hamiltonian for consistency. But you have (systematic) differences if you compute your observables (or responses) at different orders in the chiral expansion. From your answer I can see that for the long you only have leading order, but at the beginning you showed higher orders. We can discuss later if you want to follow the talk ;)

JS: Sorry for the confusion. What I meant, is that the currents have to be consistently derived. When one does this, the 2-body pieces appear (even 3-body, and so forth). But depending on the operator, they appear at different order of the chiral expansion. The transverse one appears at lower order with respect to the longitudinal one. And this is consistent with what is observed: the transverse response is enhanced a lot by the 2-body currents.

Arie Bodek: Questions, (1) in the simple models of PWIA is the Coulomb Sum rule just the Pauli suppression? (2) Also, why does it not converge to 1.0 at high Q on your plots?

JS: Coulomb sum rule -> 1 for q-> \infty at least the 1-body contribution, with nonrelativistic kinematic. So this limit is not something we expect our models to precisely fulfill. In the PWIA the CSR is an integral over the energy of RL, which is the integral over the momentum, of initial nucleon weighted by the momentum distribution, with an appropriate delta conserving function.
Arie Bodek - Thanks, the deviation from 1.0 in the PWIA model comes from Pauli suppression, is that correct? So can we take your deviation from one as an approximate Pauli suppression that can be compared to simple Pauli suppression models in PWIA (which at present must be applied in the existing MC generators)l

Vishvas Pandey:

Andy F: On slide 21 - does this large difference also show up in the lepton energy spectrum? 50-100% discrepancies *should* be measurable in the coming years (but low energy electrons may be better measured in energy rather than angle)

Steven Gardiner: Yep, see Fig. 16 from. Energy and/or angle measurements would be very useful as a nuclear modeling constraint!

AF: Woah! Big differences!
Alexis: As you can see from the paper linked by Steven, the differences are indeed big, but can be fully understood by realising that the MARLEY cross sections only take Fermi and Gamow teller transitions into account, i.e. there is a limited strength into the continuum. The CRPA calculations however take higher order multipoles into account.
The nice thing about a multipole expansion is that the different multipoles do not interfere, such that the higher-order ones can be added, however it is important to make sure that the different MPs are consistent to avoid possible double counting issues or inconsistencies in underlying nuclear properties.

Vishvas: Thanks Steven, Alexis! Andy, this is uncharted territory, no measurements yet. Hopefully in the next few years, measurements at SNS/LANL will be helpful. Though, measurements in lepton kinematics are still challenging. Measurements in lepton kinematics will help with understanding the underlying nuclear structure, e.g., what is shown in the reference Steven mentioned, allowed (Fermi and Gamow-Teller) vs forbidden (more higher order multipoles) transitions contributions as a function of neutrino and lepton energy.

Kirsty Duffy: On slide 16 (pdf page 23), when comparing CRPA to GENIE nue/numu predictions: what are the differences there? Is GENIE massively overpredicting at higher omega, or is CRPA an additional component that should be added as well (not “instead of”)?

Alexis: Vishvas may be more qualified to answer this question, as he made the comparison to GENIE calculations. At first it seems like a normalization issue, but the comparison was extended to a more quasielastic regime and there GENIE gives more sensible results in comparison to CRPA.

As there are many different versions and underlying models in the generator is it however hard to say if this is a general GENIE result, or results from a specific tune.

Vishvas: Kirsty, I would say this is kind of expected, as you see in the (e,e’) plots on the left. FG overpredicts strength and does not have the shape (low-lying excitations in the nucleus) that you would expect at these energies. FG is trying to predict a QE peak, which isn’t there. Of course, it looks worse at more forward-ish scattering where small energy/momentum is transferred. But if we integrate leptonic variables out and look at total cross sections, the differences kind of wash out, and absolute cross sections are more comparable.

NatalieJ : This is physics that is not present in an Fermi gas calculation, you need a mean field model, preferably with long-range correlations on top, to see this strength and effects.

Artur Ankowski:

Sasha Tomalak: Which quantities do you want to extract from electron scattering data? Form factors or transition amplitudes? Or is it more a test of FSIs and what is in GENIE? Which level of precision can be achieved in the vector sector and how will it propagate to neutrino cross sections?

Artur: Electron scattering can be used to validate our understanding of the ground state properties (the distribution of energy and momentum of nucleons), structure functions, as well as to test hadronization. LDMX experiment, one of the ongoing efforts, will be able to collect 10^14 events within 6 months of running. That gives you a sense of uncertainties.

Atika Fatima: For which parameters LDMX experiment gives a sense of uncertainty. Is it about the uncertainty in the determination of form factors or in the understanding of the medium effects?

Artur: LDMX will study all aspects of pion production, collecting both inclusive and exclusive data. They will be useful to understand FSI, nuclear transparencies, hadronization, pion and nucleon spectra etc

Luke Pickering: The plot on S2 is misleading. The dcp bias only appears because the atmospheric parameters are fixed. There is a degeneracy in the osc prob that means if you misunderstand the missing energy in erec you get DM32 wrong, but you get dcp correct. Dcp is very much stats limited for a while at DUNE, even with large missing energy misunderstandings. This would not happen for an actual oscillation measurement unless they performed appearance-only fits and published without ever looking at their mu-like spectrum. In this eventuality they would either see their DM32 value disagreed with NOvA/T2K and know something was wrong, or everyone has DM32 wrong but because we doing join disp/app fits, dcp is remarkably robust because of the aforementioned degeneracy. (To be clear, escat work is incredibly important and I am not trying to comment on the motivation, but plots such as these have the potential to be seen by people without the nuance of understanding required to know this isn't really a DUNE failure mode).

Artur: I agree that a realistic analysis of the impact of nuclear effects for DUNE has not been performed yet. It would be very important to do such a study, with a realistic estimate of the cross section uncertainty. At this stage, however, we are dealing with cross section issues which should be resolved before DUNE starts collecting data, rather than uncertainties that unavoidably will enter the oscillation analysis.

Rik Gran: There have been theory studies of in-medium effects on Delta. They must be especially severe for a baryon resonance in the nucleus. Are any of these past studies already consistent with your conclusion about the shifted Delta peak ?

Artur: There is a paper by Arie and Tejin https://arxiv.org/abs/1801.07975 which could be easily implemented.

UM: Now, that there are problems with the implementation of various mechanisms, what value does it then have to tune GENIE to data??
I also wonder how that affects published data. For many data published by MINERvA GENIE versions < 3 were used, both to compare with and for determining acceptances and efficiencies. Do Artur’s results mean that the error bars on these data should be larger?

Artur: I couldn’t agree more. Tuning is well justified to constrain the parameter of the model, but should not be used as a remedy for its flaws.
It is very difficult to assess how the neutrino data are affected because they are always based on tuned versions of GENIE, so this becomes a question that only the collaboration can answer.

SteveD: GENIE was tuned to neutrino-D data, not electron-D data. When I last looked at eD data, disagreement wasn’t as bad. More investigation is required.

Artur: Neutrino data are always flux averaged, so the problems may be much harder to notice. Underestimated delta and overestimated higher resonance may compensate each other in such cases, but this doesn’t mean they do not affect the results, only that they are harder to notice.

SteveD: agreed

Un-ki Yang: the Bodek-Yang (BY) model is designed to describe all inelastic contributions including resonances at inclusive level. Obviously, it will be a double counting if we add the BY contribution to the resonance prediction. The implementation of the BY model should be also checked if the BY prediction from Genie highly overestimates the electron scattering data which was used to construct the BY model.

Artur: we are well aware of this. Unfortunately this was not taken into account when GENIE was developed, so this issue persists to this day.

Arie Bodek; some of the issues are not difficult to solve. First the implementation of BY model should be checked since it should agree with the electron scattering data by definition. Second the model should be matched to a resonance model around W=1.8 since both the BY model and the resonance model used should agree if they both agree with electron scattering data. Third the Delta/single pion Optical potential should be included to bring the delta to the correct position. The Delta optical potential has been included in an updated version of the Bosted-Christy fits.

Artur: I absolutely agree. The implementation issue should be resolved as soon as possible. These are very large discrepancies and there is no reason for them to plague GENIE at this point.

UM: Arie, let us rather say the pion background contribution should be added. Ascribing the shift of the resonance to a Delta potential is very misleading, even though it may have the desired effect.

Arie Bodek - The models, e.g. Bosted-Christy model Fermi smear neutron and proton cross section, the only way to make the Delta work is to use a Q3 dependent binding energy shift. For Psi scaling QE peak one calls this an energy shift parameter. You can call it a Q3 dependent energy shift for pion production. But you are correct, if you can provide a procedure to add pion background model that can be added to the Bosted-Christie, or GENIE MC that shifts the single pion peak it would also work.

Libo Jiang:

Data was taken in 2017 for all targets.

Sasha Tomalak: Could you point to the literature about your technique to estimate errors of radiative corrections? Slide 14

CM: you could find the references in the cited paper but mostly they came from the Mo and Tsai formalism. Let me add them in the following for completeness. Actually even better, there was a very complete workshop at Jefferson lab on precise radiative correction in 2016:

https://www.jlab.org/conferences/radiative2016/

Our paper: PRC 100, 054606

L.W. Whitlow, Ph.D. thesis, Stanford University, 1990, SLACR-357 (unpublished).

Mo and Tsai: https://doi.org/10.1103/RevModPhys.41.205

Sasha Tomalak: Thanks, I am familiar with this paper. I am more wondering regarding uncertainty on radiative corrections. Will try to find it in your references.

Artur: Compared to other sources of uncertainty, the one on radiative correction is small, but obviously accounted for. To quantify, we estimate the uncertainty on radiative and Coulomb corrections to be 1.00%, compared with the total uncertainties of 2.75% for argon and 2.39% for titanium, see Table V in Phys.Rev. C 103, 034604

Raul Gonzalez Jimenez:

Luis AR: There are QE-like events with a proton in the final state and an absorbed pion which cannot be distinguished experimentally and will distort Enu reconstruction.

UM: I looked into that in https://doi.org/10.1103/PhysRevLett.112.151802 There I found that events with 1 and only 1 proton (and X neutrons) can rather reliably be used for energy reconstruction using the usual QE-based method. These 1p events also included the reabsorbed pion events. The event rate with that restriction was down to about ⅓ of the total.

Raul GJ: Thanks UM, we will read your paper carefully.

JMUdias: real pions produced in the same nucleus and that are absorbed in the same nucleus, may contribute to the ‘only one proton’ in the final state, as Luis pointed out, in the same way as they may contribute any other processes where the final proton, while traveling across the nucleus, knocks-out/produces other particles that may or may not be detected in the experimental signal of ‘at least one proton in the final state’. These processes are about 50% of the total contribution with (at least) a proton in the final state. If the experimental signal is a mix of these events in larger or smaller measure, depending on the selectivity of the experiment to different channels, that would translate, in Raul’s calculation, to a ‘distortion’ of the ‘effective’ spectral function.

Raul GJ: A real pion is produced in the nucleus, therefore we have a cross section for that process. Then the pion is reabsorbed in the same nucleus, then we have another cross section for that other process so that the actual final state is one proton. Then we sum this final cross section (?) with the one from a pure QE scattering (without pions) in order to get the final answer. That is a classical description of the whole process. It is not necessarily the true answer. For example, since this pion absorption and QE have the same final state we should sum amplitudes not cross sections.
That real pion production and absorption is an intermediate step that happens (if it happens) inside the nucleus, there is no way to measure that. We measure the actual final state: one proton.
I am not saying real pions cannot be absorbed, of course, there are pion-nucleus experiments that need that. But it's a different process.

Alexis: A lot of the problems with such a higher energy process, specifically real pion absorption would show up of course in missing energies above the pion mass. This means that the situation can really only be improved by being able to exclude these high-missing energy states from the experimental signal, i.e. lower detector thresholds. Even in that case, the uncertainty on the energy (with a 1mu 1p final state) is just inherently there. The energy and error here are thus a lower bound, to some extent you cannot do better on an event by event basis unless you can remove a region of missing energy from the signal.
Given the results for Argon measured at Jlab, a missing energy region of ~80 MeV seems to be very well populated, which puts again somewhat of a lower bound on the precision unless one can remove this from the signal.

Raul GJ: I agree, our approach is not the final answer of course.

Astrid Blin:

Alexis: Are there any relatively simple implications for more phenomenological modeling ? Or can we only use these calculations as benchmark by comparing amplitudes/multipoles, if so, are they available (and up to what value of Q^2)?

Luis AR: We did some work on multipoles upon request by the referee. The amplitudes are available or can be produced if requested. In the absence of good low energy data, the kinematic validity range is not defined besides the fact that q0 and |vec{q}| should be below around 600 MeV for the chiral series to converge. As this is an inelastic process we need to constrain both q0 and q or Q^2 and W if you prefer.

Alexis: Thanks for your answer.

Rik Gran: Thank you for the nice talk, very well presented. Probably you said it and you can emphasize: on slide 14 you benchmarked your new work against the HNV model at low Enu. Do the two models share many of the same ingredients? Can you say again which innovations account for the differences -- mostly the use of the Delta and N together as the caption and your spoken emphasis was. Or are there other innovations coming from the use of the photoproduction data? What are the prospects to continue insight to higher Enu where many neutrino experiments operate but outside the range of the photoproduction data ? Partly I'm picturing that you are demonstrating threshold effects that diminish when more kinematic space is included, but this may be a naive guess.

Astrid: Thank you, Rik, for your questions. In fact, the approaches in our work and with the HNV model are only tangentially related. We show the HNV model in our plot since those works are benchmarks for neutrino-induced pion production. In the HNV model, only tree-level interactions are considered, and no EFT such as ChPT is being relied on for a systematic ordering of contributions. In addition, the HNV model was optimized for intermediate energies rather than the threshold region. They also consider the Delta(1232) excitation, though.
So the innovation in our work, in a nutshell, is that we 1) use a predictive and systematic EFT, renormalizable order by order, 2) appropriate for the low-energy region, 3) and without too much uncertainty from unknown (fitting) constants, due to the constraints already set by other channels/beams (this should also address your question of how the photo-/electroproduction data fits bring additional insight).
The concerns of going to higher Enu are not the lack of photoproduction data: in fact, there are photoproduction data galore at intermediate and high energies, for virtually every conceivable polarization combination. So here the bottleneck is the predictive power of the theory: ChPT is a low-energy theory, and the closer we get to the 1 GeV scale, the worse will its convergence be, until it eventually breaks down. Therefore, the range of applicability is

restricted by that. That being said, so far we have not implemented the effects of the Delta(1232) at the peak energy: we would need to implement the width associated with the pole. This extension can still be done within ChPT, allowing for pushing towards intermediate energies around the Delta(1232) peak.
In my view, any further efforts towards higher energies will have to be done outside of the scope of ChPT - this does not preclude from having combined studies of ChPT+some smooth transition into other intermediate/high-energy theory/model. But it would not be solved with ChPT alone.
Can you explain your naive guess better, please? Probably you mean that in the energy range we are considering it is mainly the opening of the neutral and charged pion channels that are extremely well described, which however has little effect/relevance at higher energies? Did the above text reply to this point partially?

Suggested topics for discussion session:

UM: I made - half jokingly - the comment last evening that things started to go wrong when experimenters took over the generator development. This, of course, was a need since high energy theorists are usually not interested in mundane things such as generators; they like BSM physics, strings and multiverses.
Triggered was my comment by what I have seen over the last few days: a never ending desire to tune one generator to data and no desire to question or discuss the underlying implemented physics.
Neutrino experimenters spend a lot of (too much?) time tuning their favorite generator to data. If v. 3.y suddenly fits what version v 2.x did not describe, they are happy and encourage everyone to use only the latest version. Physics and we theorists, on the other hand, are left behind because it is never really documented in any detail what physics has been changed from one version to another and what we can learn from it. From a nuclear physics point of view: many generator developments are nothing else than reinventing the wheel, simply because of this firewall between high energy physics and nuclear physics (which, however, does not forbid to read papers from the other, far side!). I have seen here lots of examples for that: a good example is the discussion that erupted here early on about the effects of RPA that were clarified by the work of the Ghent group some years ago, but obviously not digested (read?) by many.

SteveD: GENIE tries hard to update its models. In fact, v3.0 had numerous new models, In particular complete Valencia LFG+QE+MEC set of models. For the moment, GENIE collab has only tuned to deuterium neutrino data. We will keep tunes separate from core models.