Topical Collaborations for HEP

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I. INTRODUCTION

A recommendation from the 2023 LQCD-extension III review stated that the DOE Office of High Energy Physics (HEP) and USQCD should explore "topical collaborations":

> *DOE HEP and LQCD-ext III should seriously consider taking the lead in the creation of one or several topical collaborations focused on neutrino nucleus interactions with the goal to provide experimentally relevant predictions. The NP Topical Collaborations could serve as a model here.*

Since the review was held together with the DOE Office of Nuclear Physics (NP), it seems likely that the panel was alluding to the program of topical collaborations (TCs) that NP has supported since 2010. In this document, we review briefly the NP program, discuss advantages of the NP and similar frameworks, and spell out three candidate TCs in which lattice QCD plays a vital role to impact the DOE HEP experimental program.

The NP program has been discussed in a presentation [1] to a recent Town Hall meeting [2] of HEPAP's Particle Physics Project Prioritization Panel (P5) [3], from which the following summary is derived. The mission of the NP TCs is to bring together leading nuclear theorists to collaboratively focus on solving challenging problems central to advancing knowledge in nuclear physics. The projects are aligned with Nuclear Physics program priorities, and will interpret the results of current experimental programs at NP facilities with the aim to realizing their full scientific potential. In the current (2022) round, there are five TCs, four of which are funded solely by NP with another in which DOE HEP is a partner. These TCs are highly leveraged: based on the 2016–2021 program, each TC supports around 10 Ph. D.s and 10 postdocs. In addition, the four 2016–2021 TCs provided funding to bridge six junior faculty positions. Another six bridged positions are planned for the five TCs in 2022. Despite the large numbers of individuals scientists involved, the funding is $2.0–2.5M over five years, *i.e.*, about three FTEs/year. The NP TC programs have been proposal-driven: any group of theorists with a compelling idea can put in a proposal. In addition to making a good science case, the leveraging of funding, the bridging of faculty positions, and the overall structure of the collaboration(s) should lead to outcomes that are sustainable for many years.

The NP framework might be too broad in scope to be (solely) the outcome of a review of LQCD-ext. III. Thus, the three possible TCs discussed below are designed to be relatively independent scientifically, even if lattice QCD is a common element. For each, we have in mind collaborations of lattice and non-lattice theorists and experimentalists who will work together on a set of common physics goals, each group complementing the other. In some ways this is similar to the hugely successful Muon $g-2$ Theory Initiative [4] which grew out of discussions among USQCD Collaboration members. This type of coordinated research among a diverse set of researchers is essential to address the many issues attendant to a complex scientific undertaking like the Standard Model (SM) value of the anomalous magnetic moment of the muon and to reach consensus that is broadly supported by the larger community. An important difference with this proposal is that both communities will bring essential skills to accomplish common physics goals compared to the Theory Initiative where the groups largely have worked independently along existing lines of research. For the HEP TCs, working closely together from the start is important to create the necessary enthusiasm and support to launch and sustain these efforts.

In the following, we assume a budget in the range $0.5-0.75M per year, per TC, in line with NP TCs, and we assume these funds will be highly leveraged through other university grants and/or funds in order to support the needed critical mass of researchers (students, postdocs, and faculty) for each project. University bridge positions are crucial to ensure sustainability of the research effort spurred by each TC. We note that over the past decade around a dozen HEP-oriented professors in lattice QCD retired but only four assistant professors were hired, three with a RIKEN-BNL Research Center bridge. Three lattice-oriented, HEP-supported laboratory staff have retired and two were hired over the same period.

The following sections detail three possible HEP TCs designed to sustain future research activities that address DOE HEP experimental priorities where lattice QCD plays a vital role, buttressed by important non-lattice inputs.

II. NEUTRINO-NUCLEUS INTERACTIONS

As the most important DOE-HEP experiment in the next two decades, DUNE will require an extensive theory toolkit [5] to maximize its physics output. Central to the experiment is the fundamental interactions of neutrinos with matter, namely quarks inside nuclei. Like any experiment, one needs to know the energy of the incident probe, but that is not directly measurable and reconstructing it from the final state is difficult or impossible without a model of the nuclear physics of the struck nucleus. Lattice QCD can provide the crucial information in the first step of this model, the neutrino interaction with the struck nucleon inside the nucleus.

At low energies (say, up to $\sim 1\ \text{GeV}^2$) for quasi-elastic scattering, the axial form factor of the nucleon is needed to describe the interaction. As in the case of the muon $g-2$, it can be calculated directly using lattice QCD. It is difficult to extract it from data, although the recent Minerva measurement of $\bar{\nu}_\mu p \to \mu^+ n$, with the proton in the hydrogen of a hydrocarbon target, is promising [6]. The lattice calculation employs similar methods to very successful calculations of meson form factors (see, *e.g.*, ref. [7]). Further, lattice calculations of the vector form factor, which is well measured in electron-proton scattering, provide a cross check.

At higher energies, up to the DIS region, a complete description requires more and different information. For example in the resonance region the $N \to \Delta$ transition form factor is needed. Beyond this pions are produced, so the cross section is determined from the hadron tensor [8]. In these cases, lattice QCD is even more important because of the lack of other methods. Like the form factors, the transition form factors and the hadron tensor will be used in many-body effective field theory to obtain the neutrino-nucleus cross section, so a close collaboration between different sub-fields will be needed to produce the results.

Nucleon-level lattice-QCD calculations are only the beginning, because the targets in experiments are large nuclei. Lattice-QCD will have to be combined with effective-field-theory techniques and nuclear many-body theory [5]. Theorists with the needed expertise need a background in nuclear physics, but there are several examples of nuclear theorists, lattice experts, and neutrino phenomenologists working together already (for example, refs. [9–13]). The aim of this TC is to strengthen and extend these grass-roots collaborative efforts and continue the development of a common framework proposed in white papers [5, 8].

III. PARTON DISTRIBUTION FUNCTIONS

To search for physics beyond the SM, the Large Hadron Collider (LHC) collides protons on protons which requires a precise understanding of the parton structure of the protons to successfully interpret the underlying interactions, especially new ones, if any, that produced the detected particles. The interpretation follows from a theoretical description of the collision in terms of quarks and gluons which requires knowledge of their initial distributions inside the colliding protons. These parton distribution functions (PDFs) then allow for the computation of the needed cross section which can then be compared to experiment.

Even though the fundamental structure of hadrons in QCD has long been an active area of research, several significant challenges remain to reach the level of precision needed for discovery of new physics (or for an adequate theoretical understanding of the SM) [14]. On the lattice QCD side two of the biggest ones are the intrinsic noise of Monte-Carlo simulations which is exponentially bad for baryons and the so-called inverse problem, or relating quantities computed in Euclidean space-time to the physical quantities in Minkowski space-time. The former requires improved techniques of noise reduction and/or improved computational algorithms and is also intimately related to the problem of excited state contamination. The latter is a more general problem that also affects phenomenological determinations of PDFs from experimental data, where modern statistical analysis techniques some of which have a close relation to Machine Learning can be used to address this problem [15, 16]. In the case of pseudo- and quasi-PDFs [17, 18] the ultimate connection of the lattice matrix elements to the $\overline{\mathrm{MS}}$ light-cone PDFs is done through multi-loop calculations in perturbation theory. Once promising methods are identified, they need to be developed and applied at scale, *i.e*, to calculations with large lattices (volumes), small spacings (for the continuum limit), and physical masses which represents a massive computational challenge.

Lattice calculations of PDFs can be combined with phenomenological results for greater precision than either can attain alone. We therefore envision a close collaboration between the two communities to enable our physics goals. We note that interest in work along these lines has appeared recently [14, 16, 19–22].

IV. QUARK FLAVOR PHYSICS

Quark-flavor physics is perhaps the area of HEP most significantly impacted by lQCD to date. However, to go beyond the precision determination of the muon $g-2$ and the Cabibbo-Kobayashi-Maskawa (CKM) mixing matrix of the SM, significant challenges must be overcome in order to find possible new physics in the rare decays of heavy and light mesons. In fact theory lags experiment in most of the interesting cases, and a concerted effort between lQCD, perturbative QCD (pQCD) and data-driven phenomenologists is urgently needed in order to catch up. Such a collaboration will take advantage of, and leverage, ever increasing precision of lQCD for meson calculations to probe for new physics up to the 1000 TeV scale.

The demonstrated ability to compute direct CP violation in $K \to \pi\pi$ decays (ϵ') [23, 24] opens up exciting possibilities to discover new physics [25] if the theory precision can be improved to the level of the experiments (KTeV and NA48), or better. To get there one- and two-loop pQCD calculations related to operator renormalization and matching are needed. There are unphysical divergences to be isolated and subtracted too. Isospin corrections are also important. All of these issues can be addressed by combining expertise across lattice, pQCD, and phenomenology domains. In the next five years lattice simulations with four flavors of chiral lattice fermions will become available that suggest a one percent determination of ϵ' is possible if the necessary theory tools are in place.

A similar scenario holds for the K_L-K_S mass difference, long-distance contributions to indirect CP violation in neutral kaon mixing, and rare kaon decays. Again, increasing lattice capability is opening opportunities for new physics if the required effort is matched on the pQCD side. There is interest in this community to sustain these efforts if the necessary support is available.

Lattice QCD calculations combined with experimental measurements of exclusive semi-leptonic B decays have led to the most precise determinations of the CKM matrix elements. Duplicating this success for rare decays of heavy quarks will be more challenging but could pay huge dividends. Rare B meson decay measurements show interesting hints of deviations with the SM. The $b \to s$ transitions can lead to final states with two or more hadrons ($K^{(*)} \to K\pi$, *e.g.*), which are challenging but methods to handle them are being developed on the lattice side [26, 27]. The pQCD issues with renormalization and matching discussed for kaons have overlap here too, and there are phenomenological considerations as well. Precision in neutral $B_{(s)}$ meson mixing still lags experiment, and agreement among various lattice calculations is imperfect, thus improvement is needed here too.

In the next five years and beyond new experiments at ATLAS, Belle-II, CMS and LHCb will take and analyze a trove of new data. To leverage these, the theory side must keep up with a concerted effort to improve the precision of all aspects of the calculations. For example, the next step in CKM determinations (which are fundamental to detecting deviations from the SM) will require electromagnetic effects. Here not only are new and more precise lQCD calculations needed, but also collaboration between experimentalists and event-generator writers is needed, so that long-distance radiation is treated consistently.

V. SUMMARY

Lattice QCD is entering the sub-percent precision era for mesons. A large, sustained effort for baryons is needed to reach even the ten-percent level which can nonetheless impact DUNE and experiments at the LHC. To find new physics on the rare and precision frontier and take full advantage of coming leading-edge lattice calculations requires crucial input from phenomenology and perturbation theory. We propose topical collaborations in HEP theory similar to the successful NP model to search for new physics in the most effective way, across HEP.

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[2] P5 Townhall Virginia Tech (27 June 2023).

[3] Particle Physics Project Prioritazation Panel (P5) (2023).

[4] The Muon $g-2$ Theory Initiative.

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