David Schaich – Research Interests

My research focuses on strongly-interacting quantum field theories in particle physics. Primarily, I am interested in using lattice gauge theory to obtain quantitatively-reliable, non-perturbative predictions from such theories. While this approach to quantum chromodynamics (lattice QCD) is a mature field, the application of lattice gauge theory to strongly-interacting theories beyond the standard model is undergoing rapid development. My research includes projects in both lattice QCD and models of electroweak symmetry breaking through new strong dynamics, as well as studies of improved computational techniques needed to make this work possible.

Much of my recent effort has been dedicated to performing a lattice calculation of the **electroweak S parameter**, which I carried out as a member of the Lattice Strong Dynamics (LSD) Collaboration. Initial results provide evidence that theories of electroweak symmetry breaking through new strong dynamics may be in better agreement with experiments than previously thought. Explaining the significance of this study requires a brief review of the decades-long effort to understand the origin of elementary particles' masses.

In the standard model of particle physics, elementary particles gain mass as a result of the spontaneous breaking of an electroweak symmetry that unifies the weak and electromagnetic interactions. While this generic picture of electroweak symmetry breaking (EWSB) has been strongly supported by experiments since the 1970s, the physics underlying this process remains unknown. A natural possibility is that EWSB is driven by the dynamics of some new strong force at the TeV scale. Such "dynamical" EWSB results from a process much like spontaneous chiral symmetry breaking in QCD.

Because strong interactions make perturbation theory inapplicable, models of dynamical EWSB have often been treated by analogy to QCD. A large number of TeV-scale resonances (analogues to the ρ, π, etc. of QCD) are predicted, and may be seen at the CERN Large Hadron Collider (LHC). In the absence of direct evidence for new particles, decades of effort have gone into using precision electroweak measurements to constrain models of EWSB. S parameterizes such measurements, and provides the tightest constraints on new strong dynamics.

Experimentally, the S parameter is small and negative, $S \approx -0.15(10)$.[1] If we imagined taking experimental information on the spectrum of QCD and scaling it up to the electroweak scale, we would find $S = 0.32(3)$. Recent lattice QCD calculations performed precisely this scaling, and obtained consistent results. For a theory of new strong dynamics with N_f fermions transforming under the gauge group $SU(N_c)$, the QCD analogy would then suggest $S \sim 0.3 \frac{N_f}{2} \frac{N_c}{3}$, in considerable disagreement with experiment even for the minimal QCD case $N_f = 2$ and $N_c = 3$.

The calculation I performed with the LSD Collaboration found that the S parameter in $SU(3)$ lattice gauge theory with $N_f = 6$ can be smaller than expected from the analogy with QCD, and in certain cases even smaller than for $N_f = 2$. This result realizes a long-sought hope of dynamical EWSB, and motivates ongoing lattice studies of the S parameter in theories with even larger N_f.

This raises one of the main difficulties of lattice exploration of **strongly-interacting theories beyond the standard model**: we face an overwhelming array of possible models, which cannot all be studied in detail. Instead we must map out isolated islands in the theoretical sea, and attempt

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[1] S depends on a "reference Higgs boson mass", here 1 TeV.

to understand how the properties of a given theory (its phase diagram, S parameter, spectrum, etc.) depend on its most basic features such as N_f and N_c. Close contact with model builders is also critical, to focus our attention on the most relevant and interesting possibilities.

Another difficulty is that, unlike lattice QCD, we do not have extensive experimental information to guide us. Without this means of checking the systematic effects of working in a discrete spacetime, we need to carefully ensure that our lattice calculations provide reliable information about continuum physics. This produces a significant increase in computational cost, in addition to the larger costs of using larger N_f, and costs caused by non-QCD dynamics.

In particular, the LSD Collaboration uses domain wall fermions (DWFs), a formulation that adds a fifth dimension to the lattice in order to obtain good chiral and flavor symmetries. As the fifth dimension becomes infinitely long, $L_s \to \infty$, chiral symmetry becomes exact even at finite lattice spacing, and its spontaneous breaking can be more cleanly studied. Even at the finite $L_s = 16$ we use, DWFs are much more computationally expensive than other common formulations such as staggered or Wilson lattice fermions.

With other members of the USQCD Collaboration, I am currently exploring a "mixed-action" approach, which we plan to apply to a large-scale project to begin in 2011. This method uses computationally inexpensive staggered fermions for the sea quarks, but theoretically clean DWFs for the valence quarks in the final part of the calculation. We avoid some of the costs associated with DWFs by limiting their use; however, improved numerical techniques for chiral fermions (discussed below) are badly needed by the entire lattice community.

Although lattice studies of physics beyond the standard model present significant challenges, they also provide new opportunities to collaborate with theorists and phenomenologists studying physics at the LHC and at other (e.g., dark matter) experiments that explore the weak scale. Theories of physics beyond the standard model generally involve strongly-coupled dynamics, if in some cases only at very high energies such as the scale of supersymmetry breaking. Interaction between lattice theorists, model builders, and experimentalists will become increasingly important as our understanding of electroweak symmetry breaking and physics beyond the standard model advances.

In **lattice QCD**, my work has focused on exploring the strange quark content of the nucleon, part of an ongoing project at Boston University. Although strange quarks are not among the valence quarks that determine the quantum numbers of the proton and neutron, the presence of strange sea quarks has measurable effects on nucleon form factors. In particular, the strange scalar matrix element $\langle N|\overline{s}s|N\rangle$ plays a role in searches for dark matter: in some models it can provide the dominant coupling between the dark matter candidate and the nucleon.

Because the strange quark is not a valence quark, diagrammatically it appears in closed loops that couple to the nucleon itself only via the gauge field. Such "quark-line disconnected diagrams" present a severe computational challenge. The main computational cost of lattice gauge theory is inverting the lattice Dirac operator, conceptually a large, sparse matrix. Exact computation of disconnected diagrams would require a number of inversions proportional to the lattice volume. In practice, we estimate the result stochastically, which still requires many inversions, and introduces a new source of statistical error.

In recent years, calculations involving disconnected diagrams have benefited greatly from the application of multigrid algorithms that dramatically decrease the computational cost of each inversion. Multigrid algorithms represent the physical system on a succession of coarser grids, adaptively determining the best representation of the system on the coarser levels.

Applied to the Wilson fermions used in our current disconnected diagrams studies, multigrid algorithms can reduce costs by up to an order of magnitude, a dramatic illustration of the potential benefits of **improved numerical techniques** for lattice simulations. In this case, the algorithmic advance has made possible a new direction of research: performing disconnected diagrams calculations that involve the light (up and down) quarks in addition to strange quarks.

At the same time, graphics processing units (GPUs) have produced comparable performance improvements in certain calculations. GPUs can sustain enormous rates of computation, but memory and bandwidth constraints make it difficult to apply GPU computing to many common calculations. These sorts of difficulties will likely become more severe as we move toward exascale computing in coming years. Cheap and rapidly evolving GPUs can be an ideal testbed for developing software for exascale computing.

I am involved in ongoing efforts to apply GPU computing and multigrid algorithms more broadly, especially to domain wall fermions. Much remains to be done to perfect these methods, and to search for other improvements. In addition to providing tangible benefits to the entire lattice community, developing improved numerical techniques and applying novel hardware to lattice calculations can provide opportunities for students to become involved in this field of research. We often use simpler systems than QCD in this work, such as the two-dimensional $U(1)$ Schwinger model, or even graphene. These less-elaborate computational projects can be ideal ways for students to play an active role in this research program from an early point in their studies.