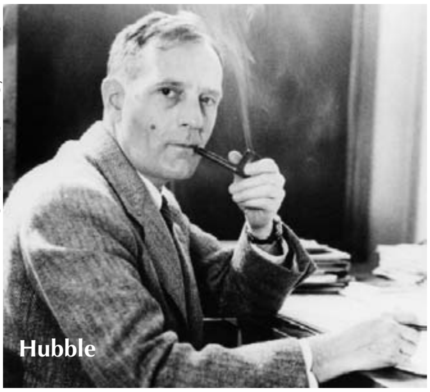
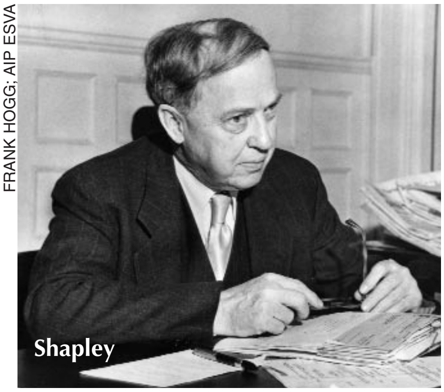
HALE OBSERVATORIES; AIP ESVA



Hubble

FRANK HOGG; AIP ESVA



Shapley

missed a great discovery) by adopting what he called “the general principle of omitting the great isolated clusters in discussing the general distribution of the background on which they are spotted.”[3]

In his magnum opus, *The Inner Metagalaxy*, Shapley summarizes the work on the large-scale structure of the nearby universe that he and his associates at the Harvard College Observatory had carried out over the preceding quarter century.[4] That work revealed stunningly large variations in galaxy density. For example, the Shapley–Ames survey of galaxies brighter than the 13th magnitude showed that the number of galaxies in the Galactic Northern Hemisphere is twice as large as that in the Southern Hemisphere.[2] Furthermore, the Shapley–Ames mapping of the sky showed that the clumping of galaxies occurs on scales much larger than the approximately 1-milliparsec size of typical clusters. The first truly three-dimensional large-scale survey of the universe was made by Valérie de Lapparent and colleagues,[5] who published radial velocities and positions for a large sample of galaxies that are brighter than magnitude 15.5. Their observations revealed for the first time that the spatial distribution of galaxies exhibits a rich texture of filaments and bubbles. Perhaps the most stunning example of the large-scale inhomogeneity of the distribution of galaxies in nearby regions of the universe is provided by the so-called Shapley Concentration, an enormous supercluster of half a dozen massive galaxy clusters.

Astronomers now generally believe that the cosmic structure revealed by the large-scale distribution of galaxies resulted from the gravitational amplification of primordial density fluctuations, which were subsequently modified by other physical processes such as gas dynamics, radiative cooling, and photoionization. It is thought that such processes contributed to the striking dependence of the morphology of individual galaxies on the density of their environment.

In summary, Hubble mainly studied galaxies and the distance scale in the universe. His work was complemented by Shapley’s, which mostly concentrated on the clumpiness of galaxy distribution in space. We now know that the study of both individual galaxies and the inhomogeneity of their distribution provide vital clues about the evolution of the universe.

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More on Numerical Lattice QCD

The article by Carleton DeTar and Steven Gottlieb (PHYSICS TODAY, February 2004, page 45) misleads readers from outside lattice field theory (LFT) about its past, present, and future. At best, the article may present a consensus in the collaboration, known as MILC, of which the authors are a part.

In their historical overview, the authors miss too many of the field’s truly remarkable achievements. The renormalization group, a conceptual organizing principle of all field theories, was first concretely formulated in LFT. A non-gauge result relevant to particle physics was that the Higgs mass must be less than 700 GeV. Duality and the role monopoles play in it also originated in LFT, and so did confinement and finite-temperature deconfinement. The first decade of LFT has been extremely productive and has had a long-lasting impact on theoretical particle physics and field theory. One could call this period the bosonic era of LFT, and it is an illustrious one.

The inclusion of fermions, a much needed step beyond the bosonic era, has preoccupied a large fraction of the community. Fermions had a conceptual defect in their original formulation by Kenneth Wilson 30 years ago. Only quite recently has that problem been finally solved. The solution constituted important theoretical progress, validating continuum ideas in a fully nonperturbative setting and restoring precise chiral symmetry.

A significant physical step was the formulation of the valence approximation and the discovery of its surprising numerical agreement with experiment. Technically, a most important development was the discovery of an algorithm that could take us beyond the valence approximation to truly ab initio numerical quantum chromodynamics (QCD). Neither of these two steps originated from MILC, although the collaboration made contributions at later stages. Both the surprises surrounding the valence approximation and the algorithms making it feasible to go beyond it are important advances.

After the initial formulation of LFT and the associated renormalization group ideas, it became clear that the approach to continuum could be sped up by fine-tuning the lattice action. This "improvement," while important in practice, lacks the theoretical novelty of the above-mentioned achievements.

The recent calculations reviewed in the article go beyond the valence approximation and attempt to improve the approach to continuum by a logarithmic factor relative to previous simulations. That these calculations required the use of an unfounded artificial suppression that purportedly reduces the number of sea quarks by four was not mentioned by the authors. Without the artificial suppression, the violation of taste equivalence would indeed have been logarithmically improved, but there would have been fourfold too many sea quarks. No effective field theory representation of the actual simulation exists that also includes the artificial suppression of sea quark contributions.

A correct and direct way to full QCD is known, based on exact lattice chiral symmetry. Today's computational cost holds us back, but a few more years will almost certainly bring us the power we need to do the calculations right and to present to the rest of the particle physics community accurate numbers that were obtained directly from the QCD Hamiltonian, with no additional assumptions. It is wrong to present the two methods of including fermions, one based on Kogut–Susskind fermions and the other based on lattice fermions with exact chirality, as being on equal theoretical footing. The true objective of numerical QCD—to assist theoretical analysis to produce numbers that, if they disagree with a correct experiment, imply the discovery of new physics—will eventually be attainable, but only with truly chiral fermions.

I disagree with the authors that lattice QCD has matured; rather, its practitioners have, and their relentless pursuit of computer resources seems to have drained some of them of the self-discipline required when presenting results to the rest of the particle physics community.

Unlike experiments in nuclear or particle physics, lattice projects do not have to be big in terms of personnel. We should rethink the policy that concentrates almost all of the computing power in the hands of a few large collaborations like MILC. That policy has tended to stifle individual thinking, imaginative risk-taking, and self-criticism. Now is a good time to do that rethinking, because an alternative exists: Small but reasonably effective commodity clusters (groups of standard personal computers networked to act as one computational resource) have reached prices affordable for small groups and even individual researchers. More money should go to small research groups, or even single researchers, and should be earmarked for purchasing computer clusters. No science would be lost if the funds of large collaborations were restricted to make this possible.

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DeTar and Gottlieb reply: Herbert Neuberger is a principal developer of the exact chiral overlap formulation of lattice fermions, a significant achievement in lattice field theory. The domain wall formulation, also cited in our article, is closely analogous. Although Neuberger apparently would have preferred an article reviewing lattice field theory, highlighting his contributions, we were instead asked to focus on recent computational successes of numerical lattice quantum chromodynamics. Those successes were achieved with an inexact chiral formulation that includes an approximation known as the "fourth root trick" to produce the correct number of quarks.

The question under lively debate is whether the approximation is justified, not strictly as a local lattice field theory, as Neuberger insists, but as a controllable approximation to continuum QCD. The international community is divided between skeptical purists and practical optimists who are encouraged by recent successes.

Some 40 physicists outside our collaboration currently use gauge configurations that we generated with the inexact chiral formulation, so our optimism is widely shared. Three collaborations other than ours contributed to the promising results we featured. So far, nothing comparable has come from the vastly more computationally expensive, exact methods.

Our purpose was to view the history and goals of numerical lattice QCD in unified terms, discussing past and present successes and looking forward to progress that will come, possibly through exact methods. Measured objectively by results, the formulation of improved algorithms for the inexact method and the companion analytic formalism for overcoming its limitations was unquestionably a nontrivial advance.

The MILC collaboration's computer allocations amount effectively to modest grants to each of us. Pooling them makes progress, such as that described, possible. We also participate in and support small innovative projects. On a national level, very substantial resources being developed by the US lattice gauge theory community under grants from the Department of Energy will be devoted to domain wall and overlap fermions. This is as it should be.

In the early days of quantum field theory, many physicists, including Paul Dirac, abhorred the poorly justified treatment of the infinities encountered in renormalization prescriptions. Of course, we now understand full well how to justify those prescriptions. Neuberger's logic, however, would have deprecated the early remarkable successes of perturbative quantum electrodynamics.

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Fascinating Pluto

The special PHYSICS TODAY issue on planetary diversity (April 2004) describes many fascinating objects in our solar system. But the controversy over Pluto's planethood and the interest in new objects beyond Neptune shouldn't mask the fascinating nature of Pluto itself. A recent consortium in which we participated used a total of nine telescopes, in Hawaii and California, to observe Pluto when it passed in front of a star. From the way the starlight faded, we could tell that Pluto's atmosphere has expanded since it was detected in another occultation 15 years ago. From spikes of intensity in the fading starlight, we could even tell about kilometer-scale structure in Pluto's atmosphere. We hope that the *New Horizons* spacecraft gets to Pluto before the expansion is inevitably reversed, which may cause the atmosphere to condense on the surface or freeze and fall as snow.

We are planning to observe more occultations by Pluto and other objects in the outer Solar System in the upcoming years. This effort often requires extensive international collaborations, so we would like to hear from others interested in collaboration. Large telescopes are not required for observing the brighter occulted stars.

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