Quark-Gluon Plasma or ‘Classical’ Hadronic Physics? FREE

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Bertram Schwarzschild's article ("Have Heavy Ion Collisions at CERN Reached the Quark–Gluon Plasma?" PHYSICS TODAY, May, page 20) gives a good description of the three observations on which the CERN groups based their announcement of the discovery of a new state of matter. The article also briefly alludes to other more conventional explanations of the observed phenomena, but does not mention that detailed theoretical descriptions of all three, based on "classical" hadronic physics, exist in the literature and do rather well in explaining the data.[1]

These hadronic transport codes follow the ultrarelativistic heavy-ion collision all the way through the very high energy densities to the final hadrons that reach the detector. (These models are not fully "classically" hadronic, because they invoke string degrees of freedom to describe high-lying hadronic excitations; they do not, however, invoke any liberated quark–gluon plasma.) These models have been extensively tested at lower bombarding energies over the last 15 years and have repeatedly proven their value there. The arguable crux of these models is that they attribute many observed effects to final state interactions between "classical" hadrons in the late expansion phase of the reaction. To describe these observations, the models need transition rates for rather exotic hadronic processes as an input—input that cannot directly be measured. Believers in the quark–gluon plasma (QGP) see this as a weak spot of the hadronic scenarios.

On the other side of the debate are theoretical analyses based on a QGP scenario. Here, however, the only reliable models that exist are idealized equilibrium models; and neither the nonequilibrium-dominated formation of the QGP nor the hadronization into the final particles is described in any detailed and tested way. There are promising attempts at constructing a parton cascade, but how hadrons become partons and how partons become hadrons again at the end of the reaction is not yet understood.

Schwarzschild's comment that "the *cognoscenti* . . . argue that an unambiguous demonstration of the quark–gluon plasma will have to wait for the RHIC data" expresses a lot of hope for some unforeseen, unexpected, but striking signal that can be understood directly without a detailed description of the dynamics of the ultrarelativistic heavy-ion collision.

What will probably be needed to come to a conclusion, even after the RHIC results have come in, is a new generation of theoretical descriptions of the development of the ultrarelativistic heavy-ion reaction. These new descriptions must be able to follow the time-dependence of the reaction from its initial nonequilibrium state of two ordinary nuclei in their ground states with very high relative momentum, through the equilibrated QGP phase of the partonic constituents of the hadrons, and on to the final state of again "classical" hadrons in the detectors. New theoretical developments, also in the form of large-scale numerical simulations, are desperately needed. For the relativistic energy domain, such a development has taken about 15 years. Based on the experience we have gained there in developing theoretical methods and codes for such simulations, I have considerable hope that new developments for the ultrarelativistic domain may not take as long. However, real progress can be made only with the joint efforts of experimenters and theorists alike.

Reference

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Surface Instability Spikes

I read the interesting article on G. I. Taylor by Michael P. Brenner and Howard A. Stone (PHYSICS TODAY, May, page 30). In the cover caption (page 5), the authors explained the cones or spikes caused by the surface instability of the magnetic fluids by saying, "The mathematical structure of these cones was first investigated by G. I. Taylor in 1964, at age 78."

To the best of my knowledge, the surface instability of the magnetic fluids was first investigated experimentally and theoretically by M. D. Cowley and R. E. Rosensweig.[1] Taylor investigated the surface instability of water, oil, and mercury under a strong electric field.[2,3] In such a case, similar but not identical spikes appeared on the surface of the liquid. The authors might argue that the surface instability of the magnetic fluids under the magnetic field is equivalent to this electric instability. However, strictly speaking, the two phenomena are different. In addition, the magnetic fluid's surface instability effect is far stronger than that of electric instability. The uniqueness of the magnetic fluid's surface instability is verified by just this extreme strength alone.

I think it is unfair to attribute the discovery of the surface instability of the magnetic fluids to G. I. Taylor even from the theoretical viewpoint.

References

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BRENNER AND STONE REPLY: Susamu Taketomi correctly notes that the original study of the instability of a ferrofluid to a transverse magnetic field was by M. D. Cowley and R. E. Rosensweig. However, Taketomi's claim that our figure caption is incorrect is false.

The caption mentioned the conical peaks on the interface, which were first calculated by Taylor well before Cowley and Rosensweig's 1967 analysis. Figure 5 of their paper shows a layer of ferromagnetic fluid with hexagonally shaped surface distortions where "isolated highlights represent peaks." These "peaks" are the cones shown in the cover graphic and mentioned in the caption. Cowley and Rosensweig do not investigate the shapes; they perform a linear stability analysis calculating the spacing between the peaks.

The "first mathematical investigation" is, of course, different from the first observation of a physical phenomenon. Hence, Taylor *was* the first to investigate the mathematical structure of these conical shapes.

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