RHIC: From dreams to beams in two decades

Gordon Baym
Department of Physics, University of Illinois at Urbana-Champaign
Urbana, IL 61801, U.S.A.

This talk traces the history of RHIC over the last two decades, reviewing the scientific motivations underlying its design, and the challenges and opportunities the machine presents.

1. THE VERY EARLY DAYS

The opening of RHIC culminates a long history of fascination of nuclear and high energy physicists with discovering new physics by colliding heavy nuclei at high energy. As far back as the late 1960’s the possibility of accelerating uranium ions in the CERN ISR for this purpose was contemplated [1]. The subject received “subtle stimulation” by the workshop on “Bev/nucleon collisions of heavy ions” at Bear Mountain, New York, organized by Arthur Kerman, Leon Lederman, Mal Ruderman, Joe Weneser and T.D. Lee in the fall of 1974 [1]. In retrospect, the Bear Mountain meeting was a turning point in bringing heavy ion physics to the forefront as a research tool. The driving question at the meeting was, as Lee emphasized, whether the vacuum is a medium whose properties one could change; “we should investigate,” he pointed out, “… phenomena by distributing high energy or high nucleon density over a relatively large volume.” If in this way one could restore broken symmetries of the vacuum, then it might be possible to create abnormal dense states of nuclear matter, as Lee and Gian-Carlo Wick speculated [2].

The physics discussions at Bear Mountain focussed on astrophysical implications of unusual states of matter such as pion condensates and Lee-Wick matter in neutron stars, high energy cosmic rays, stable abnormal nuclei, as well as fanciful applications, e.g., by A. Turkevich to manufacture and custom tailor superheavy materials, even to make a high temperature superconductor, and by G. Vineyard to use abnormal nuclei as active components in a breeder reactor. The worry engendered by the latter’s suggestion that seeds of unusual states could set off a global catastrophe was calmed by the observation that “Lee-Wick theory indicates that $10^8$ or $10^9$ [abnormal superdense nuclei] have already been produced on the moon, and that the moon is still there, albeit with large holes” – an approach still invoked to support the safety of high energy nuclear collisions [3]. Schemes for accelerating heavy ion beams were also addressed: H. Grunder described possibilities of injecting heavy beams from the SuperHILAC into the Bevalac (a project in fact completed in 1984), G. Cocconi mentioned thoughts at CERN of transferring ions up to $^{16}$O from the PS into the ISR and eventually into the SPS [4]. Most important for RHIC was K. Prelec and A. van Steenbergen’s proposal of constructing a booster ring to inject fully stripped ions with $A \geq 200$ into the AGS.
One should remember that at the time of the Bear Mountain meeting, the idea of quark matter as the ultimate state of nuclear matter at high energy density had not taken hold. The asymptotic freedom of QCD had only been shown the previous year \[5\]. Rather, in addition to Lee-Wick abnormal matter, possible states under consideration included the Hagedorn hadronic resonance gas \[6\], as elucidated by Frautschi and Lee, Leung, and Wang \[7\], and mean field hadronic models, most recently that of Walecka \[8\]. Although the concept of quark matter was mentioned as early as 1970 by Itoh in the context of neutron stars \[9\], and described before asymptotic freedom by Carruthers in 1973 \[10\] – as “quarkium, a bizarre Fermi liquid” – the decisive step was the paper of Collins and Perry the year following Bear Mountain \[11\]. Their motivation was to understand the equation of state of matter, as needed to set an upper limit on the maximum mass of a neutron star, a problem just discussed by Rhoades and Ruffini \[12\] using the Hagedorn hadronic equation of state. Their crucial realization was that ultrahigh temperature as well as ultrahigh baryon density corresponded to the asymptotic regime of QCD, rather than a hadronic regime, and thus the ultimate state would be a weakly interacting ”quark soup.”

Several other early meetings were seminal in the eventual conception of RHIC, including the “first workshop” on ultrarelativistic nuclear collisions at Berkeley in May 1979 \[13\], the 1980 GSI Workshop \[14\], the 1980 joint Japan-U.S. seminar at Hakone \[15\], Helmut Satz’s meeting in Bielefeld, which was instrumental in bringing theorists together to think about ultrarelativistic collisions and quark matter \[16\], and the second conference in the Quark Matter series at Bielefeld in 1982 \[17\]. Plans for the fixed target heavy ion facilities at the AGS \[18\] and at CERN \[19\] were well under way by early 1983.

The critical event in establishing RHIC was the open “town” meeting of the U.S. Nuclear Science Advisory Committee (NSAC) at Wells College in Aurora, New York, from July 11-15, 1983. The role of NSAC, which advises both the NSF and DOE, is to coordinate nuclear science policy in the United States. In the Spring of 1983 the immediate job of the Committee, which was chaired by John Schiffer and of which I was a member, was to write a five year long-range plan for nuclear physics, and in particular to recommend the next major construction project to follow the just approved 4 GeV electron accelerator, CEBAF, at the future Jefferson Lab. Rather than dividing nuclear physics by experimental facilities, the Committee’s approach was to study the basic science questions: nuclear symmetries, quarks and QCD in nuclei, extreme states of nuclear matter, nuclei and the universe, etc. I found myself chairing the subcommittee on extreme states of nuclear matter. The other members of the subcommittee were Arthur Kerman and Arthur Schwarzschild, who were on NSAC, as well as Miklos Gyulassy, Tom Ludlam, Larry McLerran, Lee Schroeder, Steve Vigdor, and Steve Koonin.

The main issue to be decided at Aurora was whether the next facility would be a hadron or heavy ion machine. The hadron machine was proposed by Los Alamos as the successor to the meson factory, LAMPF. This machine, LAMPF II, would inject protons from LAMPF into a 16-32 GeV synchrotron to generate a K-meson beam as well as pion, muon, neutrino and \(\bar{p}\) beams. The most specific heavy ion project was the VENUS accelerator at Berkeley, a two ring superconducting accelerator for both fixed target and colliding beam experiments. As chair of the subcommittee on nuclear matter under extreme conditions, I was to make the scientific case for pursuing heavy ions. My
intention, at a talk I would give in the middle of the meeting, was to conclude with a statement that “the highest priority for the field is an ultra-relativistic heavy ion collider [of] E/A \gtrsim 30 \text{ GeV} in the center of mass, with A up to uranium.” But then a remarkable bit of news arrived Monday evening, the first day of the meeting; the High Energy Physics Advisory Panel (HEPAP), which advises DOE on high energy facilities, had just decided to abandon the problematic Colliding Beam Accelerator (CBA), the 400 GeV on 400 GeV proton collider at Brookhaven – whose construction was well under way – in favor of building the then named Desertron, eventually the SSC. Our subcommittee realized immediately the remarkable opportunity this decision opened to nuclear physics, and in my talk Wednesday morning, I argued the proposal to build a colliding beam heavy ion accelerator in the CBA tunnel \cite{20}. With the next day’s favorable vote of the attendees at the meeting (27 to 11 with one abstention), RHIC – although not so named yet – had entered the conceptual stage.

As Schiffer summarized the deliberations to Jim Leiss, the Associate Director at DOE for High Energy and Nuclear Physics, and Marcel Bardon, the Director of the Physics Division at NSF \cite{21},

> Our increasing understanding of the underlying structure of nuclei and of the strong interaction between hadrons has developed into a new scientific opportunity of fundamental importance – the chance to find and to explore an entirely new phase of nuclear matter. In the interaction of very energetic colliding beams of heavy atomic nuclei, extreme conditions of energy density will occur, conditions which hitherto have prevailed only in the very early instants of the creation of the universe. We expect many qualitatively new phenomena under these conditions; for example a spectacular transition to a new phase of matter, a quark-gluon plasma, may occur. Observation and study of this new form of matter would clearly have a major impact, not only on nuclear physics, but also on astrophysics, high-energy physics, the broader community of science and on the world at large. The facility necessary to achieve this scientific breakthrough is now technically feasible and within our grasp; it is an accelerator that can provide colliding beams of very heavy nuclei and with energies of about 30 GeV per nucleon. Its cost can be estimated at this time only very roughly as about 150-200 million dollars. It is the opinion of this Committee that such a facility should be built by the United States expeditiously, and we see it as the highest priority new scientific opportunity within the purview of our science.

2. THE BEGINNINGS OF RHIC

As an immediate followup to the Aurora meeting, Arthur Schwarzschild and Tom Ludlam of BNL convened a task force which met from August 22-24, 1983 to begin to set the parameters of the future heavy ion collider \cite{23}. The members of the Task Force from outside BNL included J. Bjorken, C. Gelbke, H. Gutbrod, A. Kerman, C. Leeman, L. Madansky, A. Mueller, I. Otterlund, A. Ruggiero, L. Schroeder, G. Young, W. Willis, and myself. At this stage all the civil engineering, including a $^4\text{He}$ refrigeration system for superconducting magnets, was in place for the now abandoned CBA; the challenge was
to “stuff a collider” into the pre-existing tunnel.

The first issue was the maximum energy of the collider. The leading consideration was to achieve a “clean” central rapidity region, i.e., with small net baryon density. Experiments at the ISR indicated that the projectile and target fragmentation regions in pp collisions were two units of rapidity wide; nuclear effects were expected possibly to double this number [24]. Thus a lower bound on the energy would be 50 GeV/A per beam. (The “about 30 GeV per nucleon” in the Schiffer letter, above, was based on this requirement.) A compelling reason to go to 100 GeV/A was the possibility of producing high energy jets, and studying their propagation through the nuclear collision volume. This process, which is being realized at RHIC today, remains important as the closest one can come to carrying out deep inelastic scattering to probe the matter in the collision volume. Indications from cosmic ray experiments at the time, particularly the Si on Ag JACEE event, were that energy densities would be of order several GeV/fm$^3$, an estimate that has been well substantiated by subsequent collisions at the SPS. Such energy densities were felt then, as now, to be adequate to produce a quark-gluon plasma.

The Task Force stressed the importance of the beams having a large dynamic range in energy and mass number, to allow systematic studies with increasing mass number of the projectile nuclei, over a range of energies from the future SPS program (equivalent to 10 GeV/A on 10 GeV/A) on upward. It also recognized the need to be able to run pp and pA collisions, as well as AA, to be able to study the onset of new collective physics with increasing size of the projectiles. Designing the machine for pp collisions presented a delicate political issue, since one did not want to appear to be resurrecting the CBA. On the other hand, the capability of colliding protons has enabled the development of polarized proton beams for the RHIC spin program (or RHIC, as it could be known).

Achieving a large luminosity was not a critical issue, since the cross sections for central events are so large. The Task Force set a minimum luminosity of $10^{25}$ cm$^{-2}$sec$^{-1}$, with the possibility of eventually upgrading to $10^{28}$ cm$^{-2}$sec$^{-1}$ to study rare events. As the experimentalists have now experienced, RHIC’s initial luminosity of ten percent of its design luminosity $2 \times 10^{26}$ cm$^{-2}$sec$^{-1}$ already produces a rather healthy event rate.

The report of the Task Force also sketched out the rudiments of a physics program, utilizing at least three intersection regions, with at minimum two large solid angle detectors and one small solid angle experiment. The RHIC experimental program took fuller shape at a number of workshops, including that in Berkeley in September 1984 [24] and at BNL in April 1985 [29].

The Third Quark Matter meeting at BNL in September 1983 played a particular role in building community support for RHIC. The meeting was permeated, as Allan Bromley noted at the Round Table discussion of prospects for future experiments, with “a sense of enthusiasm, excitement, . . . , a feeling of adventure in the air.” In my Concluding Remarks at the meeting, I laid out what seemed at the time like a reasonable timetable for construction of RHIC, starting, after all needed reviews, in October 1987, with first beams in October 1992. [Hans Gutbrod, I recall, immediately stood up and asked whether it really had to be that long!] The formal RHIC proposal, which was issued in August 1984, sketched out an even more optimistic timeline, with a project start in October 1985, and first colliding beam tests in July 1990. Little did we imagine! [27]
Figure 1. Phase diagram of nuclear matter in equilibrium, and how it can be explored in ultrarelativistic heavy ion collisions, from the 1983 NSAC Long Range Plan [22].

3. THE SCIENTIFIC GOALS

The scientific base presented to NSAC in 1983 for carrying out ultrarelativistic heavy ion collisions remains central in the goals of RHIC today. The basic questions asked were, “What is the nature of nuclear matter at energy densities comparable to those of the early universe?” and, “What are the new phenomena and physics associated with the simultaneous collision of hundreds of nucleons at relativistic energies?” [22] As the 1983 Long Range Plan put it, the most outstanding opportunity opened by an ultrarelativistic heavy ion collider is “the creation of extended regions of nuclear matter at energy densities beyond those ever created in the laboratory over volumes far exceeding those excited in elementary particle experiments and surpassed only in the early universe.”

Now, as in 1983, nuclear matter at baryon densities well above nuclear matter density, $\rho_{bn}$, or at excitation energies corresponding temperatures of hundreds of MeV is a terra incognita. Knowledge of its properties remains scant. The proposed and probably naive equilibrium phase diagram, reproduced from the 1983 Long Range Plan in Fig. 1, shows the familiar low-temperature low-baryon density regime where the degrees of freedom are hadronic, the high temperature or high baryon density regime where matter is expected to be a quark-gluon plasma, and the uncertain transition region between these two phases [28]. It also shows the expected liquid-gas phase transition at low density, and a possible region of pion condensed matter. The regions of the diagram explored by the nuclear fragmentation regions and the central region in ultrarelativistic collisions are also shown.

The heart of the program remains discovering the properties of nuclear matter under
extreme conditions. Beyond simply mapping out its phase diagram one would like to learn from ultrarelativistic heavy ion collisions thermodynamic properties of high energy density nuclear matter, including its entropy and equation of state, the nature of its excitations, e.g., quasiparticles and collective modes, how it transports energy-momentum, baryons and other conserved quantities, how it emits particles, stops hadronic and quark projectiles and otherwise dissipates energy. These are tough challenges, which will only be met with considerable theoretical modelling of collisions.

Much of the motivation for learning about dense matter has historically come from astrophysics, and neutron stars in particular. Such applications were in the forefront at the Bear Mountain workshop, and at the NSAC Aurora meeting, and were behind Collins and Perry’s studies of the quark-gluon plasma as the ultimate state of matter. Understanding the properties of dense matter remains crucial for determining the structure of neutron stars, their mass-radius relation and thermal evolution, their upper mass limit and the transition to black holes, as well as for answering the question of whether there can exist a distinct family of quark stars. They also enter in working out how old stars undergo gravitational collapse and subsequent supernova explosions. New observations on compact x-ray sources, from the Rossi X-ray Timing Explorer, the Chandra X-Ray Observatory, and other space telescopes, are beginning to give information on strong field gravity as well as opening the possibility of directly measuring neutron star masses in “quasi-periodic oscillation” objects (QPO’s), thereby confronting theoretical expectations of dense matter. However, while heavy ion collisions will provide experimental information on hot dense matter—potentially useful in studying merging of binary neutron stars—they will not directly measure properties of cool matter in quasistatic neutron stars; learning about such matter will require sufficient theoretical understanding of the hot regime to allow extrapolation to sub-MeV temperatures. Finally, the appeal of being able to reproduce, in the central regions in ultrarelativistic collisions at RHIC, conditions in the Big Bang at times from about one microsecond until the time of nucleosynthesis, albeit under very dynamic rather than quasistatic conditions, is irresistible. Discovering how matter hadronized in the early universe, whether via a sharp first or second order phase transition, or via a crossover, and determining the associated entropy changes would be a remarkable contribution of RHIC to cosmology.

Ultrarelativistic heavy ion collisions continue to offer promise of opening a new window on QCD, particularly on large distance scales not reachable in few hadron collisions. Over the last two decades the list of questions that experiment can address, and possible answers, has become quite refined. A major advance has been the realization of the sensitive dependence of the initial states formed in the collision volume to the partonic structure of the incident nuclei. How well, though, does the initial phase come to local thermal equilibrium? Can one measure and accurately predict the time scales? How can one extract the effective interactions between quark and gluon degrees of freedom at distances of 5-10 fm? How do the long-range unscreened color magnetic interactions affect the structure of quasiparticles in the plasma and their interactions? Can one see evidence of a color superconducting state? What is the nature of the deconfinement transition? How is fragmentation into hadrons affected by the presence of a dense cloud of excitations, i.e., how does a quark-gluon plasma “vulcanize” into hadronic matter? What is the role of chiral symmetry breaking in the transition; does it lead to detectable disordered chiral
condensates? These are all issues whose resolution will require considerable interplay between theory and experiment.

In addition, as the 1983 Long Range Plan noted, ultrarelativistic collisions may produce a spectrum of unusual objects as the plasma expands and hadronizes; the current list of hopefults includes multiquark states, hadrons with heavy quarks, extended droplets of large strangeness, and multi-baryon states of unusual chiral topology. Even pi-mu and other exotic atoms can be formed in collisions. But most importantly, we must remain prepared for nature to surprise us in the way it reveals the physics of this unexplored regime, as it did in first presenting neutron stars in the form of pulsars.

4. THE AGS-SPS FIXED TARGET PROGRAMS

The AGS and SPS fixed target programs, which began experiments in 1986 have served very importantly as a warmup to RHIC; the carrying out and analysis of the experiments, given the complexity of the final states, has been a non-trivial feat of the nuclear and high-energy community. Thanks to the fixed target program, RHIC is not beginning in a vacuum; rather, the experimentalists, as well as theorists, are battle-hardened from the AGS and SPS.

Beyond establishing the lay of the land in high energy collisions, the fixed target experiments have produced tantalizing results. The experiments to date show clearly that many secondary interactions take place early on in the collisions, producing behavior well beyond that seen in pp collisions. Identification of directed and elliptic flow has shown that the dynamics in the collision volume are collective [30]. Inclusive measurements together with careful analysis of two particle correlation (Hanbury Brown–Twiss) data [31] have given a detailed picture of the evolving collision volume, and have shown that the experiments have produced matter at unprecedentedly high energy density, an order of magnitude beyond that in laboratory nuclei and certainly in the expected region for plasma formation. The experiments to date have provided thermodynamic information on the early stages of matter and freezeout conditions in the collisions [32].

One of the first indications of new physics to emerge from the fixed target program is the enhancement of strangeness compared with pp collisions, first seen at the AGS by E802 in K⁺/π⁺ ratios [33], and studied in multistrange baryons at CERN, most recently, by NA49 and WA97 [34]. The second is the suppression of the J/ψ, as studied by NA38 and then NA50 [35], which appears to defy explanations in terms of nuclear absorption. While it is very tempting to ascribe the suppression to screening in a plasma, as proposed by Matsui and Satz [36], we do not fully understand how a nascent J/ψ would be quenched in a hot strongly interacting hadronic soup. A further indication of unusual physics is the excess of low mass dileptons observed by CERES and HELIOS/3 at CERN [37], which points to a decrease of the rho mass in the hot stages of the collision.

Despite suggestive hints [38], the experiments have not yet identified a quark-gluon plasma. On the one hand, we do not understand the strongly interacting hadronic state near the deconfinement transition well enough to rule it out. One cannot simply go from asymptotic cross sections to deriving the properties of dense matter, and thus be able to assert with any certainty that this is not the matter present. Such an approach does not work in condensed matter physics or in nuclear physics; one cannot, for example, derive
the properties of nuclei simply using nucleon-nucleon scattering cross sections. On the other hand, we do not yet understand the quark-gluon plasma well enough to rule it in. Pictures based on perturbative QCD are neither trustworthy nor adequate at the energy densities present. Neither are lattice QCD calculations at a stage where they provide accurate guidance, particularly since they have not yet dealt satisfactorily with finite net baryon density, as is present in the fixed target experiments.

It will be the role of future experiments at RHIC to characterize the matter in the collisions. To show that a quark-gluon plasma has been produced will require providing evidence for color deconfinement, e.g., delineating the effective degrees of freedom of the matter as those of quarks and gluons. While creating and identifying a quark-gluon plasma is an exciting goal, in a basic sense it is only one part of the larger question. Matter created at RHIC with effective interhadron separation much less than the diameter of hadrons will, under any circumstances, be very different from standard nuclear matter from the stockroom. Whatever form such matter takes, it will be interesting in its own right. Its degrees of freedom will certainly not be the familiar hadronic ones. It may correspond to the simple theoretical picture of a weakly interacting quark-gluon plasma; more likely it will be intrinsically strongly interacting and, one should hope, much more complicated and richer. Discovery of the high energy phases of matter would only be the beginning. We should continue to bear in mind Kozi Nakai’s question at the 1983 Quark Matter meeting in Brookhaven, “What is the next step after we find it [the quark-gluon plasma]?”

ACKNOWLEDGEMENTS

The beginning of the RHIC physics program gives us a good opportunity to thank all the people who over the years have contributed so much to the development of RHIC. In addition to those mentioned in this brief history, and other unsung principals from BNL and outside, we owe a special debt of gratitude to the late Herman Feshbach, who understood and first set in motion, particularly through the Nuclear Science Advisory Committee, the process in the nuclear community that led to RHIC; to Nick Samios, who recognized from the beginning the importance of RHIC and put the full resources of BNL behind it; to Dave Hendrie whose support and guidance within DOE was crucial at all times; and to Satoshi Ozaki for masterfully bringing RHIC to completion.

Barbara Jacak’s valuable comments on this paper are greatly appreciated. This work has been supported in part by National Science Foundation Grant PHY 98-00978.

REFERENCES

1. Report of the workshop on BeV/nucleon collisions of heavy ions – how and why, Bear Mountain, New York, Nov. 29 – Dec. 1, 1974 (BNL-AUI, 1975).
2. T.D. Lee and G.-C. Wick, Phys. Rev. D9 (1974) 2291.
3. P. Hut and M.J. Rees, Nature 302 (1983) 508; W. Buzna, R.L. Jaffe, J. Sandweiss, and F. Wilczek, Rev. Mod. Phys. 72 (2000) 1125; A. Dar, A. De Rujula, and U. Heinz, Phys. Lett. B470 (1999) 142; S. Glashow and R. Wilson, Nature 402 (1999) 596.
4. Despite the appeal of the ISR to the heavy ion community, the building of LEP would take priority in the eighties, with consequent decommissioning of the ISR.
5. D.J. Gross and F. Wilczek, Phys. Rev. Lett. 30 (1973) 1343; H.D. Politzer, Phys. Rev. Lett. 30 (1973) 1346.
6. R. Hagedorn, Nuovo Cim. 15 (1960) 434.
7. S. Frautschi, Phys. Rev. D3 (1971) 2821; H. Lee, Y. Leung, and C.G. Wang, Ap. J. 166 (1971) 387; Y. Leung, and C.G. Wang, Ap. J. 181 (1972) 895.
8. J.D. Walecka, Ann. Phys. NY 93 (1974) 491; S.A. Chin and J.D. Walecka, Phys. Lett. B52 (1974) 24; see further refs. in G. Baym and C.J. Pethick, Ann. Rev. Nucl. Sci. 25 (1975) 27.
9. N. Itoh, Prog. Theor. Phys. 44 (1970) 291.
10. P. Carruthers, Coll. Phenom. 1 (1973) 147.
11. J.C. Collins and M.J. Perry, Phys. Rev. Lett. 34 (1975) 1353.
12. C.E. Rhoades, Jr. and R. Ruffini, Phys. Rev. Lett. 32 (1974) 324.
13. First workshop on ultra-relativistic nuclear collisions, LBL-8957, UC-34c, CONF-7905107, May 1979.
14. Workshop on future relativistic heavy ion experiments, R. Bock and R. Stock, eds., GSI, Darmstadt, October 7-10, 1980.
15. High-energy nuclear interactions and the properties of dense nuclear matter, Proc. Hakone seminar, K. Nakai and A.S. Goldhaber, eds. (Hayashi-Kobo Co., Tokyo, 1980.
16. Statistical mechanics of quarks and hadrons, Proc. Int. Symp. H. Satz, ed., Univ. Bielefeld, Aug. 24-31, 1980 (North-Holland Publ., Amsterdam, 1981).
17. Quark matter formation and heavy ion collisions, Proc. Bielefeld Workshop, May 1982 M. Jacob and H. Satz, eds. (World Scientific, Singapore, 1982).
18. Brookhaven proposed building the heavy ion facility at the AGS, with a transfer line from the tandem and a cyclotron booster to inject heavy ions, in Jan. 1983. Proposal for a 15A GeV heavy ion facility at Brookhaven, BNL 32250.
19. Proc. workshop on SPS fixed-target physics in the years 1984-1989, Feb. 1983, J. Manelli, ed. CERN 83-02.
20. Thoughts of an ideal heavy ion collider were very much in the air at the time. Bjorken had already suggested in informal conversations at Fermilab in March 1983 [the time of his hydrodynamic scaling paper, J. D. Bjorken, Phys. Rev. D27 (1983) 140] building a collider in the CBA tunnel with center-of-mass energy $\sim 50$ GeV/A using superferric or other low cost magnets.
21. J. Schiffer to J. Leiss and M. Bardon, Aug. 5, 1983.
22. A long range plan for nuclear science, DOE/NSF, Dec. 1983.
23. Report of Task Force for relativistic heavy ion physics, Nucl. Phys. A418 (1984) 657c. As the plans for RHIC came more into focus, the Task Force was succeeded by an ad hoc panel which met in December 1983, the RHIC Technical Committee, chaired by Bill Willis, which met in April 1984, the RHIC Review Board, chaired by Allan Bromley, which met in May 1984, and eventually by the RHIC Policy Committee, chaired by Herman Feshbach, which met regularly from 1991 through 1995.
24. R. Annishetty, P. Koehler, and L. McLerran, Phys. Rev. D22 (1980) 2793; W. Busza, Nucl. Phys. A418 (1984) 635c.
25. Proc. workshop on detectors for relativistic heavy ion collisions, L. Schroeder, ed., LBL 19225, UC-37, CONF-8403137 (1984).
26. Proc. RHIC workshop: experiments for a relativistic heavy ion collider, P.E. Haustein
and C.L. Woody, eds., BNL 51921 (1985).

27. Indeed, uncertainties remained in DOE throughout the eighties about whether to build RHIC. In December 1988, on Dave Hendrie’s urging, I went to Washington, as Helmut Satz did earlier, on a mission to convince the recently appointed DOE Director of the Office of Energy Research, Robert O. Hunter – a former plasma physicist – of the validity of the new (quark-gluon) plasma physics that RHIC would reveal, and the wisdom of building a machine to study it. In the succeeding months the first funds for RHIC construction were put in the U.S. Federal budget that started in October 1990. However, RHIC, in the face of stringent budget limitations in the nineties, continued to be fragile, a point particularly brought home when the SSC was terminated by the U.S. Congress in 1993.

28. Two notable refinements of the phase diagram in the intervening years are the reduction of the estimates from lattice gauge theory of the expected zero baryon density transition from $\sim 200$ MeV to about $\sim 150$ MeV, and the theoretical discovery of color superconducting states at low temperature and high densities in the deconfined phase. [See the review by K. Rajagopal, Nucl. Phys. A661 (1999) 150c.]

29. C. Miller, these Proceedings.

30. J. Barrette (E877 Collaboration), Nucl. Phys. A661 (1999) 329c; H. Schlagheck (WA98 Collaboration), ibid. 337c; J. Bächler et al. (NA49 Collaboration), ibid. 341c; M.C. Abreu (NA50 Collaboration), ibid. 345c.

31. U. Heinz and B. V. Jacak, Ann. Rev. Nucl. Part. Sci. 49 (1999) 529.

32. See summary and plot of thermal model analyses in P. Braun-Munzinger and J. Stachel, Nucl. Phys. A638 (1998) 3c.

33. T. Abbott et al. (E802 collaboration) Phys. Rev. Lett. 64 (1990) 847; Phys. Lett. B291 (1992) 341; Y. Akiba et al. (E859 collaboration), Nucl. Phys. A590 (1995) 179c; L. Ahle et al. (E866 collaboration), Nucl. Phys. A610 (1996) 139c.

34. C. Höhne (NA49 collaboration), Nucl. Phys. A661 (1999) 485c; F. Antinori et al. (WA97 collaboration), ibid. 130c.

35. M.C. Abreu et al. (NA50 collaboration), Phys. Lett. B477 (2000) 28.

36. T. Matsui and H. Satz, Phys. Lett. B178 (1986) 416.

37. B. Lenkeit (CERES collaboration), Nucl. Phys. A661 (1999) 23c; A.L.S. Angelis et al. (HELIOS/3 collaboration), Eur. Phys. J. C5 (1998) 1.

38. CERN symposium, Feb. 10, 2000. See, e.g, CERN Courier 40 (May 2000) 13; B. Schwarzschild, Phys. Today 53 (May 2000) 20.

39. Nucl. Phys. A418 (1984) 377c.