1 Introduction

A major reason for studying the collisions of heavy nuclei at very high energies is the hope that we might observe a new state of matter, the “Quark-Gluon Plasma”. In this Proceeding I outline a different picture for the high temperature phase of SU($N$) gauge theories: it isn’t a plasma — which in this case means a weakly interacting gas of essentially massive quasi-particles — but a condensate of $Z(N)$ spins.

That the high temperature phase of a SU($N$) gauge theory is like the low temperature phase of a Z($N$) spin model is an old and familiar story. The role of a global Z($N$) symmetry, and its relationship to confinement, was first introduced by ’t Hooft. That Z($N$) spins form a rigorous order parameter for the deconfining phase transition in pure glue theories was shown by Polyakov and by Susskind. Svetitsky and Yaffe demonstrated that if the deconfining phase transition is of second order, that the Z($N$) spins determine the universality class of the theory. Indeed, the Z($N$) spins are the standard order parameter used on the Lattice to pinpoint the deconfining phase transition.

What is novel in the following discussion is a mean field theory which relates the pressure to the behavior of the Z($N$) spins. I first review the usual quasi-particle picture of the high temperature phase, and then discuss how I was led to the model of a Z($N$) condensate. The advantage of a mean field theory is that while it clearly breaks down at a critical point, experience in condensed matter systems tells us that it often works well away (and sometimes even relatively near) a critical point. While the model is extremely simple, it does make numerous detailed predictions, which can be tested by numerical
simulations. My presentation is meant to be pedagogical, ignoring as many technical details as possible.

2 Quasi-particle Models

I largely consider the theory without dynamical quarks. Of course this is a serious limitation, and yet I will argue that in fact the quenched limit can tell us interesting things, at least for three colors.

At very high temperatures, by asymptotic freedom a plasma picture is bound to be a good description. As the temperature \( T \to \infty \), the pressure \( p \sim n_\infty T^4 \); \( n_\infty \) counts the numbers of degrees of freedom in a textbook fashion. There are perturbative corrections to this result; the series starts with terms \( \sim O(g^2) \), but due to many-body effects, the next term is \( \sim O(g^3) \), so the correct series is in fact an expansion in \( \sqrt{g^2} \). Heroic calculations\(^5\) have given us the series to \( \sim O(g^5) \).

Unfortunately, the series behaves extremely badly. Balancing just the \( g^2 \) and \( g^3 \) terms tells one that the series starts to fail when \( \alpha = g^2/(4\pi) \sim 1/20 \), which corresponds to ridiculously high temperatures, \( \sim 10^7 \text{GeV} \) or so. This series has been studied with Pade approximations.\(^6\) Parwani claims that useful information can be extracted, which is possible, given the number of terms computed.

Even so, it is not difficult to understand why naive perturbation theory may fail. In ordinary perturbation theory, one is computing about the trivial, perturbative vacuum, in which all fields propagate at the speed of light. Through scattering in the thermal bath, however, particles develop an index of refraction different from unity, which heuristically we call a “thermal mass”. (While handy, the nomenclature is misleading, because gauge invariance is not violated.) The thermal mass of the gluon is\(^7\)

\[
m_g = \frac{\sqrt{N}}{3} gT. \tag{1}
\]

In fact, the series is an expansion in \( \sqrt{g^2} \) precisely because of these thermal masses.

Thus it is useful to develop an expansion in which the effects of thermal masses are automatically included. Phenomenological approaches to gases of massive quasiparticles have been developed.\(^8\) More recently, approaches based upon hard thermal loops were developed by two groups, by Andersen, Braaten, and Strickland, and by Blaizot, Iancu, and Rebhan.\(^9\) There are crucial differences between these two approaches: the approach of Andersen et al. is manifestly gauge invariant, but even reproducing the \( \sim g^2 \) term in the free
energy requires a two (!) loop calculation in an effective theory. The approach of the Blaizot et al gets the $\sim g^2$ term right, and with some sleight of hand, the $\sim g^4$ term as well, but it is not manifestly gauge invariant. After these resummations, the free energy is presumably better behaved than the ordinary perturbative expansion. (Although due to the complexity of the calculations, the really crucial test of computing higher order corrections cannot be carried out.) Both groups agree that $p/T^4$ appears to be constant down to low temperatures, several times the transition temperature, $T_c$; as discussed below, this appears to agree with Lattice simulations.

Let us take the formula for the thermal mass, $m_g$, in Eq. (1) seriously. Coming down from infinite temperatures, the ratio of $m_g/T$ grows slowly as the temperature decreases, due simply to the logarithmic increase of the QCD coupling constant. This agrees with careful studies on the Lattice, such as by Laine and Philipsen.\footnote{10}

### 3 Lattice results

The interesting question is to match the behavior of the free energy down to $T_c$. Here one must appeal to numerical simulations on the Lattice. I stress that the data in the quenched limit is extremely close to the continuum limit; for example, between different groups, the results for the ratio of the critical temperature, to the square root of the string tension, agree to within $\sim 5\%$; the difference is due to how the string tension is extracted, not $T_c$ per se. The Lattice tells us several things. First, that $p/T^4$ is constant from a temperature $\kappa T_c$ on up, equal to about 80\% of the ideal gas value; $\kappa \approx 2$. The pressure drops suddenly from $\kappa T_c$ to $T_c$, and is essentially zero below $T_c$. This reflects the fact that $T_c \sim 265$ MeV is low in the pure glue theory; at such temperatures, the probability to excite glueballs, which are heavy, $\geq 1.5$ GeV, is very small.

The standard way of understanding this behavior of the pressure is to use a bag model. The pressure is then $p = n_{\infty} T^4 - b$, where $b$ is the bag constant. By adjusting $b$, one can obviously get the pressure to vanish at some point.

This is in direct contradiction to other data given to us by the Lattice. In particular, it is known that for three colors, the deconfining phase transition is weakly first order. For example, the ratio of the energy at $T_c$, to the ideal gas value, is $\sim 1/3$. This is very different from the bag equation of state, which by construction gives a similar ratio $= 1$.

The latent heat, however, underestimates how much correlation lengths grow near the deconfining transition temperature, $T_c$. Coming to the transition from below, the string tension at $T_c$ is a factor of ten smaller than at zero temperature. Coming to the transition from above, the ratio of the screening
mass (as defined from the two point function of Polyakov loops), divided by
the temperature, decreases by a factor of ten, as one goes from $2T_c$ to just
above $T_c$.

A historical comment is in order. Originally, simulations for three colors
at a small number of lattice steps in the imaginary time direction, $n_t$, found a
strong first order deconfining transition. Going to larger $n_t$, at first the APE
group claimed to find a second order transition, in direct contradiction with
very general arguments for a first order transition. While it was soon found
by the Columbia group that the transition is first order, the confusion was
extremely instructive, for it demonstrated how weakly first order it really is.

The order of the phase transition for three colors can be understood as
follows. For two colors, the deconfining transition appears, from numerical
simulations, to be of second order. Indeed, calculations of critical exponents
agree very well with the prediction of a $Z(2)$, or Ising, spin model. For four
colors, old simulations indicated that the transition is of first order. For vari-
ous reasons, Tytgat and I were skeptical of this result, but we were wrong.
In agreement with older simulations, recent work by Ohta and Wingate clearly
demonstrates that the transition is of first order. (For four colors, large $n_t$ is
necessary to separate a bulk transition of the Wilson action from the deconfin-
ing transition at nonzero temperature.) There is still work to do, though: it is
not known how strongly first order the deconfining transition is in the contin-
um limit. If we assume that it is strongly first order, then the transition for
three colors is weakly first order because it is near the second order transition
for two colors. Presumably, four colors is like any larger number of colors,
so that the transition is of first order as $N \to \infty$. This agrees with general
arguments on the Lattice by Gocksch and Neri. Also, it demonstrates that
in at least this one instance, three colors is not close to infinity, but to two.

4 Back to Quasiparticles

How can a weakly first order transition be described within a quasiparticle
model? Take non-interacting quasiparticles, with masses which vary with tem-
perature. Since the particles are by definition noninteracting, in order for the
pressure to nearly vanish at $T_c$, the quasiparticle masses must be heavy, so
that their free energy vanishes in a Boltzmann fashion. This has been done
by several groups. Typically, the one loop formula for the thermal mass in
Eq. (1) is assumed; one then allows the coupling constant to grow in such a
fashion that the pressure is correctly fit. Essentially, one correlates the Landau
pole in the coupling constant with the decrease in the free energy at $T_c$. (For
a different, phenomenological fit to the pressure, see.)
I suggest that this fit is physically unnatural. Consider, for the sake of argument, the case of two colors. Again, the pressure, the energy, etc., nearly vanish at $T_c$. Surely the pressure for two colors can be fit by such a model, with increasing masses. We know, however, that since the transition is truly of second order, that the mass for $Z(2)$ spins exactly vanishes at the critical temperature. So we are describing an exact critical point by a gas in which the masses are becoming very heavy, not even light! I find this odd; one would like a model to fit all aspects of the physics, not just some. The same argument applies for three colors, where the transition is weakly first order, or as I prefer to call it, nearly second order. Conversely, if the deconfining transition is strongly first order for four or more colors, then such a fit may be reasonable.

I note that the decrease of the screening mass was described by Peshier et al. There are actually two screening masses: one for the screening of static, electric fields, $\sim A_0^2$, and one for the screening of time-dependent fields, $\sim A_i^2$. In ordinary perturbation theory, these two masses are strictly tied together, since they both arise from the gluon polarization tensor, with one $1/\sqrt{3}$ times the other. Peshier et al. describe the mass for $A_0$ as an integral over the $A_i$ quasiparticles; as the $A_i$ fields become heavy, the $A_0$ fields become light. It is not clear to me, though, how these two very different screening masses can arise from the same polarization tensor. See, however, my comments at the end.

5 Effective Model

How then can one describe the behavior of the pressure? The impetus for my work was a talk given by Eric Braaten at the Aspen Center for Physics, in August of 1999. After describing his work, Hans Pirner asked him if he had considered the coupling of condensate fields to the quasiparticles; Braaten said no. (I think Pirner meant pions.) My immediate thought was to couple the field for the Polyakov loop to the quasiparticle masses.

I then introduce the $Z(N)$ Polyakov loop:

$$\ell(x) = \frac{1}{N} \, \text{tr} \left( \mathcal{P} \exp \left( ig \int_0^{1/T} A_0(x, \tau) \, d\tau \right) \right) ; \quad (2)$$

where $\mathcal{P}$ is path ordering, $g$ is the gauge coupling constant, $x$ is the coordinate for three spatial dimensions, and $\tau$ that for euclidean time. The $\ell$ field is real for two colors, and complex valued for three or more. In published work I call the operator in (2) the thermal Wilson line; in these proceedings, in deference to European custom I call it the Polyakov loop.
The $\ell$ field transforms only under the global $Z(N)$ symmetry of 't Hooft, $\ell \to \exp(2\pi/N)\ell$. The expectation value of $\ell$, $\langle \ell \rangle = \ell_0$, behaves exactly opposite to that of an ordinary spin system: it vanishes in the low temperature phase, and is nonzero in the high temperature phase.

My first thought was then to couple the Polyakov loop to the screening mass, taking $m_g \sim gT\ell$ for gluons, and $m_q \sim gT\sqrt{\ell}$ for quarks. These powers of $\ell$ are natural guesses, given the general expressions for hard thermal loops. However, the quasiparticles then become light near the transition, and even if one adds a bag constant to fit the pressure, one can’t fit the energy, since light fields inevitably have a lot of energy.

Instead of marrying the Polyakov loop to hard thermal loops, I ended up throwing out the quasiparticles altogether, and developing a theory entirely in terms of the Polyakov loop. The usual way in which to model the magnetization in a spin system is by mean field theory. For three colors, I take:

$$\mathcal{V} = (-2b_2|\ell|^2 + b_3(\ell^3 + (\ell^*)^3) + (|\ell|^2)^2) b_4 T^4.$$  \hspace{1cm} (3)

The terms $\sim |\ell|^2$ and $\sim (|\ell|^2)^2$ appear for any number of colors, since they are invariant under a global symmetry of $O(2)$. The term $\sim \ell^3$ is special to the three colors, reducing the global symmetry to $Z(3)$. The cubic invariant drives the transition first order; for the sake of simplicity, since the transition is nearly second order, for now I ignore $b_3$.

At the minimum of the potential, for $b_3 = 0$, $\ell_0^2 = b_2$. Thus whatever the behavior of $\ell_0(T)$, one can trivially obtain this by adjusting $b_2(T)$. In an ordinary spin system, $b_2$ is negative below $T_c$, and positive above; for $Z(3)$ spins in a gauge theory, one simply requires the opposite, positive $b_2$ below $T_c$, and negative $b_2$ above.

The potential in (3) is totally standard: my only contribution is to add an overall factor of $T^4$ in $\mathcal{V}$. This arises because the Polyakov loop, as an exponential, is of necessity a pure number, without any mass dimension. Thus the only parameter to make up powers of mass is the temperature $T$. While mathematically trivial, this does have physical consequences.

At high temperature, fluctuations in $A_0$ can be neglected, so $\ell_0 \to 1$. With $b_3 = 0$, this requires that $b_2 \to 1$ as $T \to \infty$. At the minimum of the potential, when $b_2 < 0$, $\mathcal{V} = -b_2^2 b_4 T^4$; when $b_2 > 0$, $\mathcal{V} = 0$. Since the pressure $p = -\mathcal{V}$, we obtain a relation between the pressure and $\ell_0$ in the deconfined, or broken symmetric phase, $p \sim \ell_0^4 T^4$. At high temperature, $b_4 \to n_\infty$, to obtain the ideal gas result. As the temperature is lowered, certainly $b_2$ changes, and probably $b_4$ as well. Around the critical temperature, in the spirit of mean field theory I assume that one can neglect the variation in $b_4$, and only allow $b_2$ to vary.
The relationship between the pressure and $\ell_0(T)$ can in principle be tested on the Lattice. Qualitatively, one views $p/T^4$ as being approximately constant from $T = \infty$ down to $\kappa T_c$ because $\ell_0$ is approximately constant. From the mean field theory alone, one cannot tell if $p/T^4$ is 80% of the ideal gas value because $b_4$ is 80% of $n_\infty$, or because $\ell_0$ is .95 (or, equivalently, because $b_2 \sim .9$). Indeed, the best way of defining $\kappa$ is not merely from the free energy, but where $\ell_0$ differs significantly from unity. Below $\kappa T_c$, the sharp drop in the pressure is then viewed as the result of a sharp drop in the expectation value of the $Z(3)$ Polyakov loop.

In principle, this can be tested by measuring $\ell_0$ on the Lattice. For example, it is not obvious that the simple quartic potential in $V$ is necessarily adequate to fit the pressure; perhaps terms $\sim (|\ell|^2)^2$ have to be added. If only quartic terms enter, then one has a qualitative prediction: $p/(\ell_0^4 T^4)$ should be constant, at all temperatures. Of course near $T_c$ this may break down because of nearly critical fluctuations. This is certainly true for two colors.

Two natural questions arise, which have a related answer. First, why in the mean field theory is there a contribution from the condensate, but not from fluctuations in the condensate? Second, is this type of mean field theory for the pressure useful for any gauge theory at nonzero temperature?

To answer the first question, I appeal to the large $N$ expansion. At large $N$, the free energy itself is a natural order parameter: it is of order one in the confined phase, and of order $\sim N^2$ in the deconfined phase. The elementary explanation for this is of course that gluons are deconfined, and they contribute $\sim N^2$ to the free energy. However, there is a quandary: we should be able to describe the free energy, which is a physical quantity, exclusively in terms of gauge invariant quantities. So what is the term in the free energy $\sim N^2$ due to? I suggest that the expectation value of the $Z(N)$ Polyakov loop, $\ell_0$, is the only quantity which can do so. The potential is like $V$ in Eq. (3), except that there is no $\ell^3$ term, only terms $\sim |\ell|^2$ and $(|\ell|^2)^2$. With the normalization of the $Z(N)$ Polyakov line in Eq. (1), $\ell_0$ is of order one (not $\sim N^2$) at all temperatures. So one simply adjusts the overall coefficient, $b_4$, to be $\sim N^2$, as it must be to match the ideal gas value. There are then other contributions to the free energy: from fluctuations in $\ell$, for example. But $\ell$ only has two degrees of freedom, and so can be ignored relative to the terms $\sim N^2$. There are also contributions to the free energy from glueballs, either electric or magnetic. Again, however, any glueball state is necessarily a color singlet, and so can only contribute $\sim 1$ to the free energy.

The $Z(N)$ Polyakov loop does not always dominate the pressure. Consider a generalized (or “quarky”) large $N$ limit, in which the number of flavors, $N_f$, goes to infinity along with $N$. Since there are $\sim N_f^2$ hadrons, the free energy
is then nonzero, on a scale of $\sim N_f^2 \sim N_f N \sim N^2$, at all temperatures. Alternately, the expectation value of the $Z(N)$ Polyakov loop, $\ell_0$, is always nonzero: $\sim N_f / N$ at low temperatures, and $\sim 1$ at high temperatures. One might still imagine that there is a phase transition (for massless quarks, there is a chiral phase transition), but one does not necessarily expect a large change in the free energy, nor in $\ell_0$. One could still write down a mean field theory for the pressure, in terms of a potential like $V$, but it is not obvious if it will be of any use.

Indeed, this gives one a qualitative guide to when the mean field theory for $\ell$ should be of use. If the pressure in the low temperature phase is small, then presumably that is because $\ell_0$ is small. One can then model the increase in the free energy by a change in the Polyakov loop. Fortunately, this appears to be the case in QCD: from present lattice simulations, the pressure in the low temperature phase is small. Of course, in present simulations, the pions are relatively heavy, at least as heavy as physical kaons. This is where the present theory could fail to apply to QCD: if better simulations, with lighter pions, find that the free energy is much larger in the low temperature phase than at present, then using a mean field theory for $\ell$ to calculate the pressure would become increasingly dubious.

For the time being, let me assume that this is not the case. Now it is well known that the order of the phase transition depends extremely sensitively on the presence of dynamical quarks; it appears that the weakly first order transition in the pure glue theory is completely washed out by the effect of quarks. I suggest that this is only possible because the transition is so weakly first order. I suggest, however, that the nearly second order transition persists. In the pure glue theory, correlation lengths associated with $\ell$ grow by a factor of ten near the deconfining phase transition. With dynamical quarks, it is no longer a deconfining phase transition, but a chiral transition, or just crossover. Thus one does not expect correlation lengths associated with $\ell$ to grow as much; even within the model, they cannot, since then there is measurable pressure in the low temperature phase. Perhaps the correlation length associated with $\ell$ might grow not be a factor of ten, but five. This factor of five is sheer guesswork.

What is dramatic is that this field may dominate the free energy in QCD. In an ordinary critical point, there is both a regular and a singular part. The critical field affects the singular part, but there is always a regular part which remains constant about the critical temperature. In QCD, it might be that the regular part — associated with fields other than $\ell$, or with fluctuations in $\ell$ — are much smaller to those from the potential for $\ell$. Then it would be meaningful to speak of the transition in QCD as being “near” the critical point.
for two colors in the quenched theory. It would be preposterous to suggest this without data from the Lattice; with such data, it is a testable proposition.

6 Qualms

In this section I mention several outstanding problems which I swept under the rug.

Like any other operator in a quantum field theory, the Polyakov loop must be renormalized. It is necessary to separate this ultraviolet renormalization from the physical behavior of \( \ell \), as a \( Z(N) \) spin. For example, when \( \ell_0 \) is measured on the Lattice, it typically is much less than unity, even far away from \( T_c \). This is due to a renormalization on the Lattice, which acts to decrease the bare \( \ell_0 \).

To separate the effects of ultraviolet renormalization, it is probably not sufficient to measure just \( \ell \). One possibility is the following. Calculate not just the trace, but the full eigenvalue distribution for the matrix values, \( SU(N) \) Polyakov loop. By definition, one ignores an overall constant factor, which includes any ultraviolet renormalization. The trace of the Polyakov loop can then be computed from the distribution of eigenvalues. By construction, this gives an \( \ell_0 \) which approaches unity at high temperature. This may or may not be a practical procedure; regardless, since one has a renormalization condition with which to fix \( \ell_0 \) — such as \( \ell_0 \to 1 \) as \( T \to \infty \), the problem should be manageable.

Recently, the role of the magnetic \( Z(N) \) symmetry has been stressed by several authors. This is an interesting approach; however, the magnetic \( Z(N) \) symmetry is broken in the low temperature phase, and restored in the high temperature phase. But then a magnetic condensate cannot be used to develop a mean field theory for the free energy in a gauge theory, since that is only large at high temperatures.

More generally, several authors, most cogently Smilga, have argued that the Polyakov loop it is entirely a figment of imaginary time. (Indeed, as was clear in the discussion section, this feeling appeared to be shared by many participants at the conference.) The problems which Smilga raises are important, and I cannot solve them at present. However, I believe that it is possible to analytically continue the Polyakov loop to processes in real time. (I made one, incomplete, attempt.) Indeed, as is obvious from the original derivation by Taylor and Wong, hard thermal loops generalize the Debye mass term, \( A_0^2 \to A_0^2 \) to real time. But the Debye mass term is the leading term in an expansion for an effective lagrangian of Polyakov loops.

More generally, perhaps this can reconcile my approach with that of mas-
sive quasiparticles. The Polyakov loop is an operator involving $A_0$, while the quasiparticles involve the transverse degrees of freedom, the $A_i$'s. Maybe near the transition, where $\ell_0 \to 0$, the $A_i$ propagator is more involved than just a simple mass getting heavy: maybe it is that the wave function renormalization becomes small. After all, $\ell$ is something like the wave function for an infinitely heavy test quark.

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