The following is not particularly brief and somewhat of a core dump. I have tried to divide things into three main sections.

(1) The issue of the definition of the quark-gluon plasma,
(2) The list of PHENIX published results and my current understanding (I tried to use Mike Tannenbaum's numbering convention),
(3) Some general remarks on the drafting of the white paper.

Sincerely,

Jamie

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(1) PLASMA DEFINITION SECTION:

Although in an ideal world (see Feynman's article Cargo Cult Science) we might be able to ignore this issue, the term is a landmark in our field despite the lack of any clear definition. I liked Paul's different levels of quark-gluon plasma, but I believe that if one cannot be concise, one can come across as too lawyerly. I note that the post-CERN press release discussion of quark-gluon matter versus quark-gluon plasma made me want to quit the field.

I think we need to be careful not to just repeat the mantra of the field without further clarification.

Having never taken a plasma physics course or research project, I decided to do some reading and had a few conversations with the plasma researchers at the University of Colorado. Below I include some interesting quotes from three relatively introductory books on the subject. All the quotes are enclosed by "=====" marks so you can skip them if desired.

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"Introduction to Plasma Physics", R.J. Goldston and P.H. Rutherford

"What is a plasma? First and foremost, a plasma is an ionized gas. When a gas is heated enough that the atoms collide with each other and knock their electrons off in the process, a plasma in formed: the so-called 'fourth state of matter.' Exactly when the transition between a 'very weakly ionized gas' and a 'plasma' occurs is largely a matter of nomenclature. The important point is that an ionized gas has unique properties. In most materials the dynamics of motion are determined by forces between near-neighbor regions of the material. In a plasma, charge separation between ions and electrons gives rise to electric fields, and charged particle flow give rise to currents and magnetic fields. These fields result in 'action at a distance' and a range of phenomena of startling complexity."

"Maxwell-Boltzmann statistics arise repeatedly in plasma physics, and the next example (Debye Shielding) is fundamental to the very definition of a plasma."

"Basic Principles of Plasma Physics: A Statistical Approach", S. Ichimaru

"Plasma physics is concerned with the equilibrium and non-equilibrium properties of a statistical system containing many charged particles. The forces of interaction betweeen the particles are electromagnetic, extending themselves over wide ranges. The system is characterized by an enormous number of microscopic degrees of freedom arising from the motion of individual paticles."

"A plasma may be defined as any statistical system containing mobile charged particles. Vague as it may sound, the foregoing statement is sufficient to define what is known as plasma in physics and engineering."

"A plasma is a collection of charged particles. The Coulomb force with which the charged particles interact is well know to be a long-range force. As a consequence, the physical properties of a plasma exhibit remarkable differences from those of an ordinary gas."

"The Debye length and plasma frequency are the basic quantities characterizing a classical plasma."

"Introduction to Plasma Physics and Controlled Fusion: Volume I: Plasma Physics", F.F. Chen

"Definition of Plasma: Any ionized gas cannot be called a plasma, of course; there is always some small degree of ionization in any gas. A useful definition is as follows: A plasma is a quasineutral gas of charged and neutral particles which exhibits collective behavior."

"In an ordinary gas ... since the molecule is neutral, there is no net electromagnetic force on it. The molecule moves undisturbed until it makes a collision with another molecule, and these collisions control the particle's motion. The situation is totally different in a plasma, which has charged particles. As these chargeds move around, they can generate local concentrations of positive and negative charge, which give rise to electric fields. Motion of charges also generates currents, and hence magnetic fields. These fields affect the motion of other charged particles far away."

"A fundamental characteristic of the behavior of a plasma is its ability to shiled out electric potentials that are applied to it [Debye shileding]."

"Criteria for Plasmas: A third condition [for a plasma] has to do with collisions. The weakly ionized gas in a jet exhaust, for example, does not qualify as a plasma because the charged particles collide so frequently with neutral atoms that their motion is controlled by ordinary hydrodynamics forces rather than by electromagnetic forces. If w is the frequency of typical plasma oscillations and t is the mean time between collisions with neutral atoms, we require wt > 1 for the gas to behave like a plasma rather than a neutral gas."

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The clearest thing to me is that we are not creating a plasma. A plasma is an ionized gas of electrically charged particles. The key characteristics seem to be the long-range Coulomb force giving rise to interesting electric and magnetic fields that dominate the described motion. Debye screening due to mobile electric charges is another key characteristic. Notice that no one requires equilibration in their definition. In fact, the electrons and ions are often not in equilibrium.

What we might be creating is an object of particles interacting dominantly via the strong force that is a "good" analog of the traditional plasma (as defined above). It is a good analog in the simplest possible sense. In a gas of atoms, the electric charges are paired, thus suppressing electromagnetic interactions (down to dipole moments etc.) and allowing atomic collisions to dominate the dynamics. In nuclear matter quarks/gluons are grouped into color singlet hadrons, thus suppressing strong interactions (down to residual pion exchange). If one frees the quarks and gluons, there is a basic analogy to ionizing a gas.

It seems that this may be the end of the analogy. Since the strong force is not long range, much of the key behaviour of plasmas due to long-range Coulomb interactions (important electric and magnetic fields) do not apply in the same way. Do we expect large scale color separation in the QGP giving rise to large color electric and magnetic fields? There is a paper by B.Mueller on color chaos, but I do not remember the key points.

In my mind, given the above definitions of a traditional plasma, it is up to us to define the quark-gluon plasma and make clear it is only a very limited analog of the traditional plasma. In addition, one key in the definitions below, is that "new" and "unique" behaviours are observed that distinguish a gas from a plasma. We need to define what are the QGP's unique characteristics.

This reminds me of color glass condensate (CGC). Originally Larry McLerran coined this phrase because he thought it had properties of the spin glass. Then when that turned out not to be true, the analog had to do with the separation of time scales in the evolution of glass materials. This is a little dangerous, but the point is that CGC is not a glass. If one can identify the key property that is analogous and make that clear, that is sufficient for the analogy.

One other note, many people want to say you have to have "free" quarks and gluons, quarks and gluons as the fundamental degrees of freedom, liberated quarks and gluons. At high enough temperatures (much higher than what we achieve at RHIC), the partons will achieve asymptotic freedom (which is a well defined type of freedom). In this case, it seems clear that one has a "weakly" strong-force interacting gas of quarks and gluons. It also seems clear that one had this form of matter in the early universe far before one got anywhere near what we label as the phase boundary to hadronic matter. In some ways, I like this definition of a quark-gluon plasma. Unique characteristics from our "hadron-gas".

However, this definition automatically means we do not create quark-gluon plasma at RHIC (or LHC for that matter). This does not really bother me. Perhaps there is another type of partonic plasma created at RHIC. Remember that in a traditional plasma, the quasi-particles need not be single electrons and ions. Many electrons can form bizarre configurations (fractional quantum hall effect as a solid state example). That may well be what we have at RHIC (other quasi-particles composed of quarks and gluons, but not free vacuum hadrons), though I think we do not have sufficient experimental evidence of this to date. If the new quasi-particles are really just the same as hadrons but with modified spectral functions, that does not sound like a good plasma analogy.

My other example/question here is if a partonic-plasma is simply a new form of matter characterized by new combinations of partons (not hadrons), then the color superconducting matter at high density, low temperature is a partonic-plasma of sorts. The place where it breaks down is if you want it to have long range electric and magnetic fields, then that does not work so well (but also not for our QGP either).

I am leaning towards the idea of defining the QGP as the asymptotically free limit which we do not make, and defining a new partonic-plasma phase at RHIC (yet to be confirmed experimentally). We know it is not a hadron gas, and not a QGP, so some other state.

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(2) PHENIX PUBLICATIONS AND WHAT THEY MEAN?

Here I have used the numbering from Mike Tannenbaum's email (perhaps that will help).

[1] Multiplicity and ET at mid-rapidity

Not much to add relative to Mike.

One point is that if we quote the Bjorken energy density, we should discuss how to put a systematic error bar on the point. We always are asking theorists to do this. Since the Bjorken scaling assumption strongly disagrees with data from Brahms and Phobos, how much do we expect this might change the value. Also, I have made a plot which I often show in talks of Bjorken energy density versus time (with the characteristic 1/t) drop off for the longitudinally expanding cylinder. We also need to mention the factor of 2 error in the Bjorken paper. It should be noted that a well-known theorist used the wrong equation (as in the paper) at the CERN press release presentation. Some PHENIX people mentioned at the plenary session discussion, that all models roughly agree on the energy density. What other models? Is there an energy density we can extract from a Landau model? The fact that Bjorken couples spatial slices with momentum (very clever) is what really allows dET/dy to map onto spatial energy density.

This brings up a basic point (which will be very important in the high pT suppression discussion), does everyone agree that the energy density must fall somewhere between 1/t and 1/t^(4/3)? Most all of our theoretical calculations we compare to have to do some modeling of the time evolution. Many use the Bjorken scenario (which we know not to be correct). Much of our ability to rely on them depends on our agreement about this fall off. I think Paul mad e the excellent point that the energy density starts at almost 1 TeV (complete spatial overlap of the Lorentz contracted nuclei in the CM frame), and then it must fall off from there.

Even though this white paper is focused on QGP issues, are we also to discuss CGC. It seems like we need to say something since it contributes to many of the initial condition arguments. Mike's point about CGC and ET is a good one. In particular they have often argued that longitudinal work can reduce the ET they calculate (a factor of 3 higher than we measure). However, they get the pseudorapidity distribution of Nch correct with Phobos. How can you have lots of longitudinal work and not move particles around in rapidity. I should note that after I was not so nice to CGC models at the Quark Matter 2004 student session, Raju made me aware of an erratum on part of this (hep-ph/0305112).

I think all our early conclusions on scaling of charged particles as getting at the mix of hard and soft processes (a perfect factorization ala X.N. Wang in HIJING) were too naive and I would not emphasize that point.

[2] Multiplicity, Charge and <pt> Fluctuations

The charge particle ratio fluctuations paper states the extreme result that we see fluctuations with D ~ 4 (or choose some other variable), which is expected in a pion gas, and that theory predicts D ~ 0.75 for a quark-gluon gas. I have been studying this in some detail and chatting with Tom DeGrand at Colorado and emailing the relevant theorists. I will post these finding under a different link. My conclusion is that even if we have a quark-gluon gas with reduced charged particle ratio fluctuations, and it follows the Bjorken scenario (as used by Jeon and Koch) such that we measure particles from a single spatial region (corresponding to rapidity +/-0.35), because our acceptance samples less than 25% of the pions that finally come out from that region, it substantially Poissonizes the fluctuations to the same numerical result as the pion gas limit (D ~ 4). Additionally, even in the theory papers they quote a number for pion rescattering of sigma-y ~ 0.5 units. This would have a dramatic Poissonizing of our observed fluctuations. Again, see this separate note. Even STAR with +/-1 unit of rapidity coverage and full azimuth (except spokes) only sees a small reduction in the D value. The STAR balance function result may be the most interesting on this topic.

The <pt> fluctuations, the main conclusion in the latest paper is that you can have fluctuations due to jets. These fluctuations are then reduced in agreement with the suppressed high pT observation. The main conclusion here is that (as with the back-to-back correlations), it provides evidence that these high pT objects are from jet pairs, and the suppression is on this level.

[3] v2 and HBT

As a small historical reference, at one time people in the field would show pt spectra and could not dis-entangle "temperature" from transverse velocity "flow". One interesting example is the paper "Transverse Baryon Flow as Possible Evidence for a Quark-Gluon-Plasma Phase", P. Levai and B. Muller, PRL 67, 12 (1991). Note that this paper is talking about proton-(anti)proton reactions.

Then people (U. Heinz included) worked out that the pT dependence of HBT radii also gives similar information. Thus, experiments such as NA44 and others then could separate the T and v parameters.

Now with the very large values of v2 at RHIC, it is realized that you have another observable that is sensitive to T and velocity flow. This is the best of situations, where the problem is over-constrained. And now that hydrodynamic calculations do not agree with HBT when they are matched with transverse momentum spectra and v2, people want to ignore the extra constraint.

People have argued that v2 is the most sensitive to the early stages (and self quenching), transverse flow to later stages, and HBT to the latest stage. Thus, some people like Miklos would argue that the disagreement with HBT should not bother us too much. People on the STAR experiment (see Mike Lisa's QM talk) like to show that the blast wave parameterization of the freeze-out hypersurface can describe all three. The comment I have heard (not tested myself certainly), is that there is no hydrodynamics calculation that produces this hypersurface. It would be nice to confirm this.

The large v2 measured by PHENIX indicate strong collectivity. However, the key is whether this "large" collectivity follows a hydrodynamic description and/or a description at the microscopic level (parton cascade). My understanding of the results from Denes Molnar's Ph.D. thesis is that he calculates parton-parton scattering using only the perturbative cross sections (assuming asymptotic freedom?). I have always assumed this is a microscopic description of what a "weakly" interacting gas of quarks and gluons would be like. When he finishes the calculation, he finds that v2 for the partons is much less than what is measured for the various hadrons. All of my assumptions about this calculation need to be confirmed.

It is this result that and the agreement of the hadrons (at low pt - pt<1 GeV for pions, pt<2.5 for protons) with hydrodynamic calculations that leads many people to say we have a "strongly" interacting system as opposed to the "weakly" interacting QGP. Note the title of the proposed Lee/Samios journal issue "Strongly Coupled QGP". Shuryak and others talk about parton-parton correlations or quasi-particles that imply not "free" quarks and gluons, and a much stronger coupling.

One key issue that needs to be resolved involves the recombination models.

People have found a rough scaling of v2/n versus pt/n where n is the number of valence quarks. I like Mike's comment on this "Let me only say on this issue that I think the famous v2/n vs pT/n plot doesn't prove anything." One main confusion on my part is that hydrodynamic calculations do not specific the quasi-particles (they just use an equation of state). At the end of the calculation, one can map the final hypersurface onto particles. When you map onto particles as hadrons with their full vacuum masses, we find reasonable agreement with pi, K, p, lambda (STAR) v2 data at low pT. The valence quark scaling at higher pT has led some to say that it shows that the hydrodynamic flow is for the quarks - hence QGP! (see Fries, Bass, Muller paper). This kind of comment really confuses me. How can the hydrodynamic fluid map onto free vacuum mass hadrons at low pT and quarks with constituent masses at higher pT? In addition, does the scaling with constituent mass imply no chiral symmetry restoration? If we had only neutral current masses, wouldn't we expect the strange quarks to show a big difference? Charm v2 will add something to this picture as well, for which Run-4 data should give us a real measure with statistics.

Denes also now says that if you coalescence the partons, it increases the v2 for hadrons. Thus, he claimed recently (though not in a paper that I have found) that this then reconciles his calculation (see above) with the magnitude of v2 for hadrons. If this is true, do we have the "weakly" interacting QGP? Shuryak disagreed with Denes at QM. I would like to understand this much better.

One partial explanation I got from Scott Pratt is to imagine intermediate pT quarks are attached to something else in the medium via a color string. He claims that these quarks have only a 40% chance to find an antiquark or diquark partner near in phase space to coalesce with and form a meson or baryon. If it does not find a partner, the string stretches further and pops out another q-qbar or diquark-antidiquark pair. Then you can form a hadron via traditional fragmentation. This results in a dropping down in pT and you fall into the low pT region. Thus, the scaling of v2/n at intermediate pT is not hydrodynamics. It seems too early to make a very strong conclusion on this point. Clearly we need to understand the models better.

Also, on the hydrodynamic calculations, there are a few inputs. Initial conditions, when to start the hydro, when to end the hydro and equation of state. It would be good to quantify from the literature what the sensitivity is to all of these inputs. One last point is that even though our data is at midrapidity, data at more forward rapidities show large disagreement with hydro. How do we treat this part? Again, using the Bjorken modeling.

[4] High PT Suppression

We have clearly established the suppression of high pt particles, that these particles come from jets, and that they show a large away side modification.

On the correlation side, we have a lot of unpublished data that I have not fully digested. We have to be very careful about using this data to draw strong conclusions. Pushing on those publications is crucial.

I have two items that I think are important.

One is whether the energy loss is partonic and the second is what we learn if the energy loss is partonic. My feeling is that a large component of the effect is induced gluon radiation in medium, but that other parts of the picture are missing. Remember that all the calculations predicted almost no broadening of the away side jet. Perhaps the radiated gluons keep re-interacting in medium (which Vitev et al do not include yet). X.N. Wang's paper on why the loss is partonic (not hadronic) has some good points, but some are weak. One point is just lorentz factors. This tells me that free vacuum hadrons cannot be re-scattering, but does not exclude some object building up a hadronic correlation. Lots of models out there with cross sections that grow linearly with time at the q-qbar separate to the pion wavefunction size. Another point is the lack of suppression at CERN. This is a tricky issue - see the recent paper by David d-Enterria. I must say that I generally do not like papers that go back and re-fit, plot, select, etc. older data to find new conclusions. You may fix some old errors, but you also don't know the details of the analysis and other possible issues. I am always somewhat suspect of the statement of no loss at CERN. Pt is always pretty low (maybe into the hydro range - see Peitzmann hydro fits of pizero). Terry Awes mentioned to me that even though X.N. Wang matches the central data, the trend with centrality of the WA98 data does not follow pQCD with just increasing Cronin. Any comment Terry? I have some more comments on how to deal with lower energies in the last section of this text. Another comment is similar to Mike's about the correlations. This is a strong point, and we need published results to back it up.

Another point that is good is that with formation time and hadron re-interactions you would expect the suppression to decrease with pT. We do not see this. However, I should point out that all models of partonic energy loss also said that suppression should decrease with pt. Only after the first data, with a balance of Cronin, shadowing, and energy loss does Vitev et al. get it to be flat.

One thing that still sticks out as odd is the HERMES data. I like X.N Wang's calculation of energy loss in a cold nucleus that agrees with the modified fragmentation functions from HERMES. However, HERMES now published results for different final state hadron species. They see big differences in the suppression, and the order seems to scale with the hadron-nucleon cross section. X.N. predicts no hadron dependence since the parton escapes the nucleus before fragmenting in vacuum into the leading hadron. Does some type of hadronic wavefunction begin developing in the nucleus that interacts? I emailed X.N. and he suggested that maybe struck anti-quarks that lead to antiprotons, etc. might have a larger annihilation probability in the nucleus and this might explain things. No paper or calculation yet.

Okay, putting these caveats aside, what do we learn? A long time ago Miklos explained to me that the radiation is only sensitive to the color charge density and time profile, not whether the color charges are deconfined or confined in hadrons. I have seen no strong arguement against this. Therefore, all we can learn is about the gluon density and time profile.

Many people mis-quote X.N. Wang's paper and say that the energy loss is an order of magnitude larger than in a cold nucleus. Actually, the energy loss observed in HERMES (implied in Wang's calculation) in a cold nucleus is about the same as the energy loss at RHIC! However, we know (and do we all agree) that the density is dropping at 1/t at RHIC. So if you correct for this 1/t, it implies that there would be an order of magnitude larger energy loss at RHIC if it were static. Thus, the conclusion about very large energy loss critically depends on our belief in the time evolution (1/t or 1/t^4/3 or otherwise).

So we can learn that the gluon density is very high. Miklos and others argue that since this gluon density agrees with Bjorken density and with requiring initial density for hydro and color glass and ... this starts to put together a consistent picture. I agree with this point, but the question is how quantitative. Note that gluon radiation is infrared divergent. Al Mueller points out this large sensitivity to the cutoff. GLV resolves this by having the cutoff as the plasmon frequency - resulting in calculations with no systematic error and no sensitivity to the cutoff scale (really?).

Again, I think the main point we have learned in that assuming our initial overlapping nucleons release a large number of gluons, we can calculate the suppression of jets - my guess is at the level of factor of 2 at pt < 6 GeV and 1.5 for pt > 6 GeV.

[5] Anomalous Baryon Enhancement 2-4 GeV/c

See the above discussion under v2.

[6] Charm Results (Single Electrons)

I quote Mike's conclusions below, since I disagree with them :)

From Mike = "6) Charm non-suppression, another major discovery. This proves that the gluon structure function isn't shadowed. ...Is this the predicted QCD dead cone effect, or should charm flow. Is the charm pointlike scaling like the proton pointlike scaling for 2 < pT <4.5 GeV/c, i.e. will it vanish at higher pT."

Our published results from 130 GeV, the charm cross section scales with binary collisions within 50% systematic errors. Although since we have no 130 GeV p-p results, the scaling is from PYTHIA, or just minimum bias to central Au-Au events. Thus, from the 130 GeV data I do not think we have much constraint on shadowing. From the 200 GeV data with p-p and Au-Au, the constraint is better, but needs to be quantified. I am not sure we have ruled out 20% gluon shadowing yet. Forward muon results will be very nice.

The issue of high pT charm and the "dead-cone" effect versus hydrodynamics etc is not yet resolved. Even with the 200 GeV Au-Au results shown by Sean Kelly at QM, the statistics are quite limited. Maybe at 2.5 GeV one can say that there is not a factor of 5 suppression, but this is not quantitative enough. Again, a lot more data from run-4 already on tape.

[7] J/Psi Results

I think we are all working hard on the run-4 results. My guess is that the suppression pattern (no wiggles) will look like the NA50 result. Remember above some point, the psi' disappears, and then the j/psi starts going away. The jpsi is down in NA50 almost to a surface emission point. My guess is even with our hotter system, we will go to the surface limit as well. What we will really need are the upsilon states to say even more.

[8] Missing Results

Direct Photons at low pT. Low mass dileptons. I am not sure we can measure direct photons at low pt with enough accuracy to ever make strong conclusions. Maybe we should be looking at Jack Sandweiss' idea of 2 photon HBT. The low mass dileptons are crucial. They measure the parton correlations in the medium. The phi -> ee is not enough because the lifetime is just too long. In all the above discussion there is lots of speculation on new quasi-particle configuration, but no measurements. So we need the low mass region.

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(3) OTHER REMARKS AND STAYING TRUE TO SCIENCE

There may be a large desire to define the QGP so that we have discovered it at RHIC, but it was not there at SPS or AGS. We should stay true to the science and avoid this desire.

My feeling is that it is hard to say at lower energies. What is J/psi suppression at the AGS (Tom and others never got to try this measurement)? What does a struck parton do when propagating through AGS matter? We have no idea. I think it is fair to say that many probes are only available at RHIC and that makes it a unique place to measure the properties of the medium.

The same might even be said of comparing to proton-proton. We may form QGP in a small volume in p-pbar (remember Bjorken's baked alaska picture). However, it is too small to probe with many of our tools. Also, we want to see collectivity. We should keep an open mind, and make positive statements about what we see at RHIC, and not make too many negative statements about what is not seen in other systems.

One key items is whether various models have a consistent and correct space-time picture. That is where real strength can be drawn in putting together hydro, energy loss, etc. But we need to evaluate whether this is a reasonable space-time picture and whether the models are really using it consistently.