From Larry McLerran:

One of my fondest memories of Mike is from 1979. I was a postdoctoral fellow at SLAC, and I was attending the Gordon Conference. I met Mike, who I knew from a previous visit I made to BNL some years earlier. (I had been at one of his roast suckling pig barbecues at his home.) Mike was excited because of his recent work using the Metropolis algorithm to compute properties of QCD. When he explained to me what he had done, I also got really excited. I understood how important it was. *At that time, attempts to understand QCD on the lattice were very confused. Wilson discouraged attempts to numerically attack QCD, because he felt that one needed huge lattices to say anything.* At SLAC, Marvin Weinstein and Helen Quinn were trying to use lattice renormalization group procedures in the hope of being able to solve the theory semi-analytically. Lenny Susskind and John Kogut were pushing strong coupling methods to their limit. *Mike simply stepped in, did the not so obvious, but simple, computation, and opened up a new era of understanding strong interactions.*

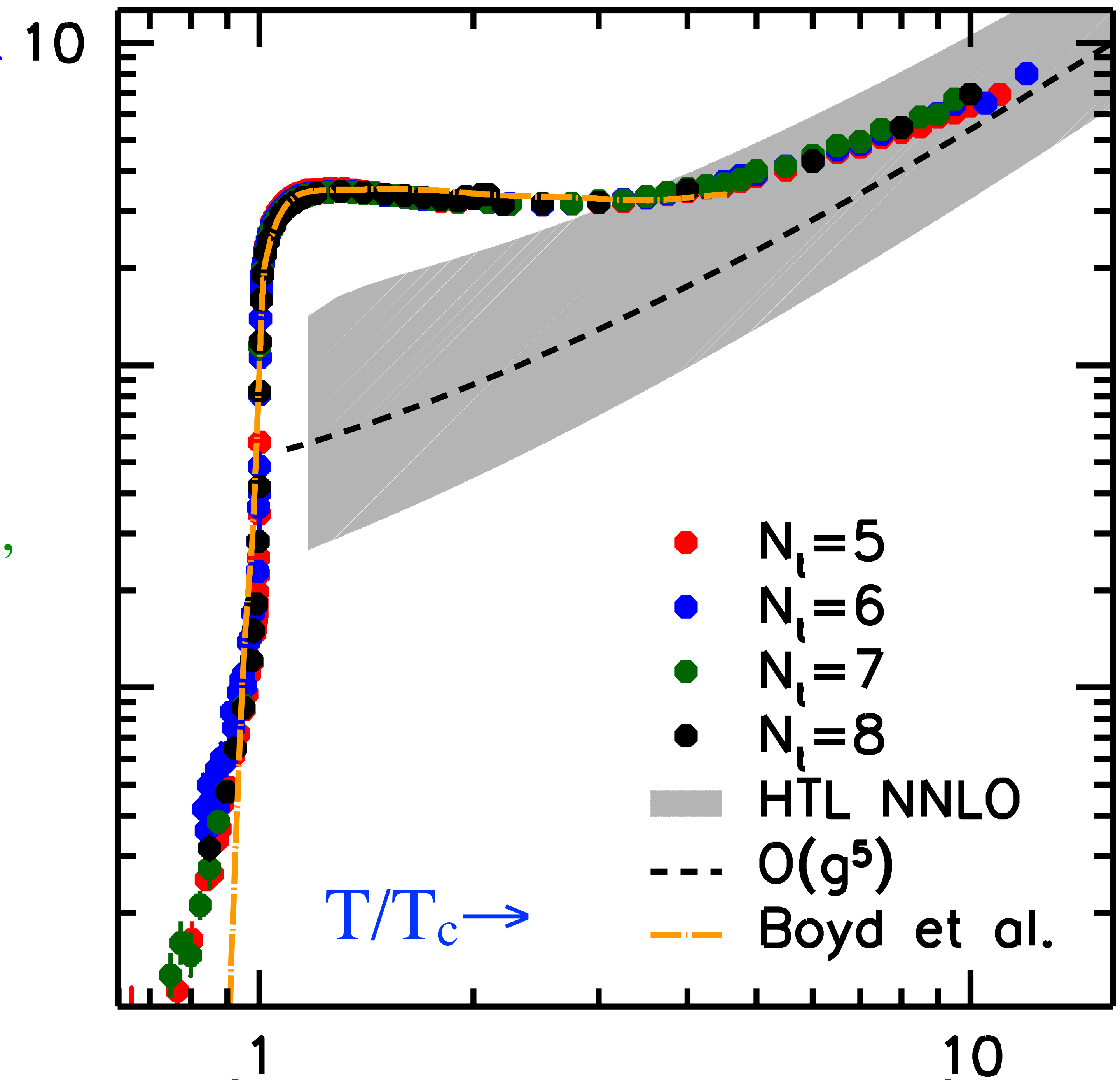
Mike was carrying around card deck copies of his program, and he gave me one. I took it back to SLAC and began work with Ben Svetitisky. We, and independently the group Kuti, Polonyi and Szlachanyi, were the first to find numerical evidence of a de-confinement transition. Although for awhile we were multiplying matrices together with the wrong indices, and it also took us a long time to find a good parameter, the Wilson line, which gave us a strong signal for the transition. Of course we got good answers for the de-confinement temperature, but for the wrong theory, SU(2), on a very small lattice.

I also was an evangelist for Mike's computations. At first the folks at SLAC did not appreciate what Mike had done. Mike came out, gave a seminar, and talked with people. As scientists, people became very excited, but also as scientists, they were skeptical. It was really an exciting time.

Lattice: “hidden” scaling, *pure* SU(3), *no* quarks

Pressure(Temp.), $T_c \rightarrow 4\ T_c$: leading corrections to ideality, T^4, are T^2 (*not* T^0)

$$\frac{e-3p}{T^4}\ \frac{T^2}{T_c^2} \uparrow$$



$\uparrow T_c$ $\qquad$ $10\ T_c \uparrow$

$$p(T) \approx \#\ (T^4 - T_c^2\ T^2)$$

Borsanyi, Endrodi, Fodor, Katz & Szabo, 1204.6184 True for N = 3 to 8.

In 2+1 dimensions, N= 2, 3, 4 & 5:

$$p(T) \approx \#\ (T^3 - T_c\ T^2)$$

Caselle + … 1111.0580 *Not* a mass term

Strings *above* T_c

Strings: good effective theory *below* T_c. What about *above* T_c?

Atick and Witten, '88: as $T \to \infty$, free energy of strings $\sim T^2$ in d = 10 and 26 dim.'s

$$p(T) \approx +T^2$$

Lattice: with*out* quarks, T^2 term with *negative* sign above T_c. In *d* dimensions:

$$p(T) \approx \# \, T^d (1 - T^2/T_c^2)$$

Lattice: *with* quarks, *no* such simple parametrization.
Borsanyi, Fodor, Hoelbling, Katz, Kreig, Szabo, 1309.5258
Bhattacharya + … 1402.5175

Why strings above T_c with*out* quarks, but not *with* quarks?