11 BEFORE THE FIREWALL, 2007–2011

11.1 QUANTUM GRAVITY: WORMHOLES, BLACK HOLE MODELS, BUBBLES OF NOTHING, LOOPS

Having spent most of the last few years on cosmic strings, AdS/QCD, integrability, and other odds and ends, I wanted to focus more on the fundamental question, "What is quantum gravity?" Even with the anthropic principle looming, the problem of finding the theory of quantum gravity remained one that needed to be solved. Solving this might lead to any number of wonders. Moreover, it was the kind of problem that might be solved by theoretical reasoning alone. And we had this remarkable tool, AdS/CFT, or more generally gauge/gravity duality, which we had certainly not applied to its fullest.

The first order of business was sorting out the old confusion about the Euclidean wormholes of Coleman: do they appear in the path integral for quantum gravity? This came up when Nima Arkani-Hamed was visiting, and he was interested in the possibility that such wormholes might allow us to study other parts of the string landscape. With grad student Jacopo Orgera, we first found Euclidean wormholes in spacetimes with known field theory duals; this was really the hard part. Then, extending an argument by Rey, wormholes would violate cluster decomposition in a way that was inconsistent with the dual field theory. (Several people suggested that this might be corrected by adding nonlocal operators to the dual field theory, but that would only produce nonlocality at the boundary.) So our conclusion was that these solutions did not appear in the path integral for the evolution in quantum gravity. This was supported by the observation that all the solutions we could find had actions that were *less* than the BPS action.[1,2]

According to INSPIRE, it was at this point more than ten years since I had written a paper on black holes and the information problem. Like many of those who had worked on this, I regarded it as essentially solved by gauge/gravity duality, in the BFSS matrix form and in the AdS/CFT form. Of the three options—information loss, information emission, and remnants—only emission was consistent with duality to gauge theory. There remained the question, how does the information escape? But this seemed to fit nicely with the principle of black hole complementarity, enunciated by Susskind, Preskill, and 't Hooft: the information could be both inside the black hole and outside, as long as no single observer could see both copies. And various thought experiments supported this.

Still, our understanding seemed to be incomplete. For example, we only had a nonperturbative construction of the CFT side of the duality. We could in principle calculate the black hole S-matrix by making a duality to the CFT and solving numerically. But in the bulk regime, where the black hole radius was large compared to the Planck length, it seemed that there should be a nonperturbative construction in the bulk. So even though I was not actively working on the problem, I was often thinking about it. The 2001 paper by Maldacena, recasting the problem in terms of the long-range two-point function, struck me as a particularly clear way to formulate it.

THE INFORMATION PARADOX IN ADS/CFT

The formation and evaporation of a black hole in AdS has a dual picture in terms of the boundary CFT. It is described by an initial state dual to the collapsing matter that evolves into a final state dual to the remaining Hawking radiation once the black hole has disappeared. Since the boundary evolution is manifestly unitary—providing a one-to-one map between initial and final states while maintaining purity at all times—the bulk evolution must also be unitary by the statement of the duality. Thus, information cannot be lost.

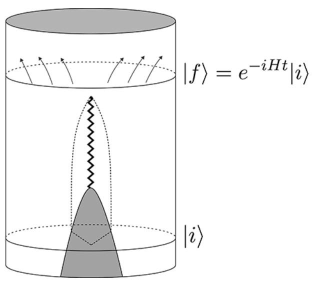

Figure 11.0a

A simpler version of the information paradox arises by dropping a particle into a black hole and seeing if it ever makes it back out again. The probability amplitude for such a process is captured by the *two-point function*. From standard bulk physics, the particle is never expected to emerge once it falls through the event horizon, leading to an exponentially decreasing two-point function for all time. However, this is paradoxical because general quantum mechanical principles require that it approach a finite nonzero value at late times.

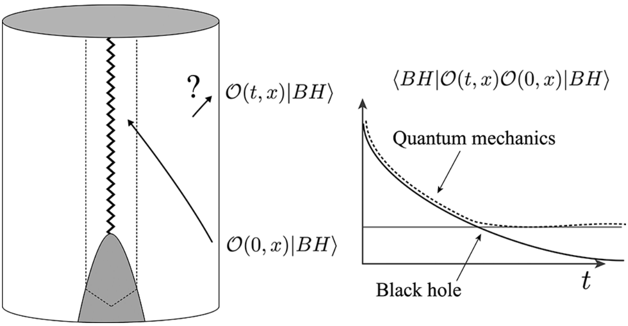

Figure 11.0b

Thus, a nice paper by Festuccia and Liu (FL) caught my eye. They wanted to make a toy model of the CFT which captured what seemed to be the main feature of the black hole as discussed by Maldacena. That is, at infinite *N* the long-term behavior of the black hole two-point function falls off exponentially forever, but at finite *N* there is a minimum, past which the two-point function is disordered. FL argued that this behavior could be seen even in the weakly coupled limit on the CFT side.

FL's argument was based on truncation to a simple subset of graphs. With postdoc Norihiro Iizuka, we wanted to find a solvable model that exhibited this behavior. After some experimentation, we found a matrix model that worked. In particular, we showed that at $N \to \infty$, there was a range of parameters for which the asymptotic two-point function fell exponentially, as with a black hole, while the finite-N correlators had to exhibit exponential decay and then disorder.

Perhaps the most notable thing about this paper is that it is the only time that I have used Mathematica, in this case to solve a nonlinear recursion equation.[3] This is aside from a few simple integrals, and even there I preferred Gradshteyn and Ryzhik. I guess I'm a Luddite (Dorothy, who is my IT manager as well as my wife, would agree), but until recently I have always been more accurate than the students and postdocs with which I was working. Up until now, I was able to find problems that did not need more.

In a follow-up, with postdoc Takuya Okuda, we found a larger set of models, including a simple one that could be solved analytically at large N. We were also able to obtain the $1/N^2$ correction, each of us doing it a different way: Nori by direct Feynman sum, Takuya by sum over Young tableaux, and me using loop equations. Unfortunately, this was complicated enough that we could not see getting the general term, or summing for an exact expression.

One other paper from this period dealt with the stability of nonsupersymmetric orbifolds in AdS. With Horowitz and Orgera, we addressed the question of whether any nonsupersymmetric vacua could be stable.[4] We focused on nonsupersymmetric orbifolds (the same as Adams, Silverstein, and I had looked at in unwarped spaces). These had tachyons at weak coupling but not at strong, but we suspected that there would still be some instability. Indeed, it was the Witten bubble of nothing, now wrapped around the twisted direction of the orbifold.

One final note in this section is a comment on loop quantum gravity. It was widely believed by those working in that field that it predicted violations of Lorentz invariance at high energy. But for those familiar with quantum field theory, this did not make sense: renormalization would spread the symmetry breaking to all operators allowed by dimensional analysis, and these included relevant operators visible at low energy. I had thought about writing this argument out, as had several others, but did not care to get caught up in it. Fortunately, Collins, Perez, Sudarsky, Urrutia, and Vucetich (CPSUV) did write out the argument, which I think had a large impact. So I was happy.

Gambini and Pullin, two of the original authors of the loop Lorentz-breaking idea, wrote a paper with Rastgoo proposing two ways to evade the CPSUV argument. So I studied the paper, and found that it failed to do what it intended. One of the models depended on being on a Euclidean lattice, and the other depended on the Lorentz symmetry being weakly broken at all scales. I did realize that there was a way to make it work, though: supersymmetry! (I then looked around and found that Nibbelink, Pospelov, Jain, and Ralston had already noted this.) So if Lorentz violation is found, we can say that SUSY is predicted, though not the reverse.

11.2 UNDERSTANDING ADS/CFT

Any AdS string theory will have at least three scales: the Planck scale l_p, the string scale l_s, and the AdS scale l. In order to have a spacetime that is smooth on the string and Planck scales, l must be much larger than the others. In terms of the dual CFT, these correspond to a large number of fields N and large dimension for all nontrivial operators of spin three or more (since these spins cannot appear in the low energy field theory). It seemed plausible that the reverse was true as well: any CFT with a large gap above two in its operator spectrum, and a large number of fields, would have a spacetime dual.

I had a chance to clarify this when Joao Penedones came to the KITP as a postdoc. He first worked with Giddings and student Michael (Mirah) Gary to expand on my work on the flat space limit of AdS scattering. This was very nicely done, and it seemed to me that it could be applied to a proof of the sufficiency of large *N* and a large gap of dimensions in generating spacetime. So, with my students Idse Heemskerk and Jamie Sully, we investigated the simplest CFT model and the simplest nontrivial observable, the four-point function.

We solved for the most general CFT with the given spectrum of states by solving the bootstrap equation. On the AdS side, we then found the most general Hamiltonian, with given spins and dimensions. There was a one-to-one match between the possible bulk actions and the possible CFTs: there were no CFTs with large *N* and a gap that did not have a candidate bulk dual. Here, Penedones's calculational abilities were essential, the first (but not the last) exception to my record of beating the computers. We would have liked to take this further, but our approach of counting was clumsy and difficult to generalize. But we could say we had proven one nontrivial aspect of AdS/CFT.

I had thought that the idea that many fields and a large gap were sufficient to give a large spacetime was general lore, and I thought that it came from Tom Banks, who generally contributes such deep insights. But I checked with him and he denied it. So it seems that this idea was immaculately conceived.

DERIVING ADS/CFT

A convenient regime of AdS/CFT is one where the bulk theory is well described by an effective local QFT interacting weakly with gravity in AdS. This limit is where Newton's gravitational constant and the string length are both taken to be very small, which translates on the boundary CFT to a large number of fields and *large gap*—large mass for fields of spin greater than two. The question in trying to derive AdS/CFT is whether imposing these conditions on a general CFT, outside known examples of AdS/CFT, is sufficient to guarantee a dual gravitational bulk description.

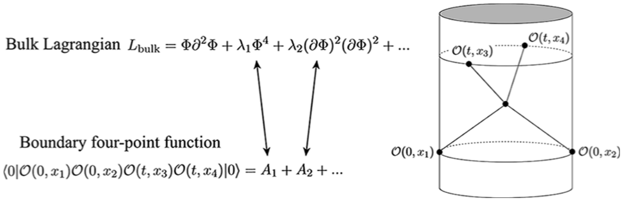

Figure 11.0c

The structure of a putative gravitational dual to a general CFT can be probed by the four-point function. If such a dual exists, then the four-point function would correspond to the scattering of two bulk particles, and hence would be sensitive to the interactions appearing in the Lagrangian of the bulk QFT. Indeed, in the relevant limit of a general CFT, it neatly decomposes into a sum of terms in one-to-one correspondence with the interaction vertices of an effective bulk Lagrangian.

With Heemskerk we looked at other ways to understand AdS/CFT. We figured that the scale-radius relation should be interpreted in a Wilson renormalization group form, so the bulk fields should be integrated out a radius at a time. There were other papers with an RG interpretation for the radius, but I think we were different than most (Faulkner, Liu, and Rangamani's paper was very similar) in being more faithfully Wilsonian. Our formalism reflected the fact that double-trace operators arise necessarily even in the planar limit—a surprise to us.[5]

In the end, our paper struck me as a new formalism, but not a new insight into the nature of AdS/CFT. However, it turned out to be useful in AdS/condensed matter (AdS/CM) studies. Indeed, almost all of my work at this time was based on AdS/CFT, and this connected all of physics, from black holes to condensed matter to conformal field theory. So in writing this, I have to separate the subjects for clarity, but sometimes the right separation is not clear.

ADS/CFT AND WILSONIAN RG

There is a sense in which the energy scale of the CFT corresponds to the emergent holographic bulk radial direction that is orthogonal to the boundary. The UV/IR property of AdS/CFT gives a relation between them that identifies the high energy regime of the boundary theory with the bulk region near the AdS boundary. When defined with a UV cutoff, the boundary theory resides on a finite radius surface representing that cutoff, instead of the asymptotic boundary. Further lowering of the cutoff via Wilsonian RG translates in AdS to integrating out the fields between the corresponding radii.

Holographic Wilsonian RG

$$\int \prod_{r=0}^{K_1} d\Phi(r) \prod_{r=K_1}^{K_0} d\Phi(r)\; e^{-S_{K_0}} = \int \prod_{r=0}^{K_1} d\Phi(k)\; e^{-S^{\text{eff}}_{K_1}}$$

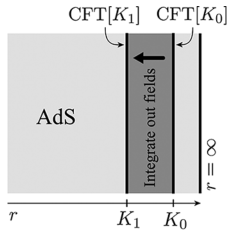

Figure 11.od

11.3 ADS/CM

I had some small history in condensed matter physics, with my interpretation of Fermi liquids, my dabbling in non-Fermi liquids, and my brief collaboration with Charlie Kane and Matthew Fisher.[6] With AdS/CFT, I thought about ways that it might produce a non-Fermi liquid. Around 2003, I recall that Matthew Fisher announced that high T_c was about to be solved. He had his own new idea in mind, but I responded, “Yes, and AdS/CFT will solve it.” But so far neither of our approaches has succeeded.

It was Subir Sachdev, together with Christopher Herzog, Pavel Kovtun, Dam Son, Sean Hartnoll, and Markus Muller, who first found a useful role for AdS/CM. Sachdev was the world expert on quantum critical phenomena, critical points that sit at zero temperature, but with an interesting approach to that zero. AdS gave one of the few tools for studying such a strongly coupled fixed point, and moreover high-T_c seemed to lie close to such a point. But, having tried AdS/CM before, I was happy to sit back and let the large group of excited young people go with it.

But, as we have seen, being at KITP means that lively visitors were always pulling me in new directions. In this case, it was a monthlong *miniprogram* on AdS/CM in the summer of 2009. This program was designed and run by Sean Hartnoll. When he took it to the KITP advisory board in early 2008, there were only around six papers on the subject, mostly by him, but it was clearly a good thing to run, and by the time it ran a year later it was one of our most oversubscribed programs. Sachdev and I signed on as co-organizers to give the program some heft, since Hartnoll was just a postdoc. But because I was running a five-month string theory program just before then, I got Hartnoll to agree to do all the work. I think that he was peeved when I stuck to my word, especially as Sachdev seemed to have the same deal.

But it was an outstanding program, and it pulled me back into the subject. First, with Hartnoll, Silverstein, and David Tong, we studied backgrounds with Lifshitz symmetry, in a probe limit for the charged fields in a thermal background. I was skeptical that a probe approximation could capture high-T_c, but it was notable that for Lifshitz dimension $z = 2$ one obtained the correct anomalous dimension for the conductivity. I think my main focus in the project was the interesting RG flows.

A different approach to high-T_c /CFT, due to Thomas Faulkner, Hong Liu, John McGreevy, and David Vegh, was based on an $AdS_2 \times R_{d-1}$ black hole. In trying to understand their construction, I realized that it separated into a short-distance part that was universal and a long-distance part that was not. Faulkner, who had just joined KITP as a postdoc after getting his PhD from MIT, was thinking along similar lines. We realized that there was a simple way to extract the universal behavior, which one could think of as an IR AdS_4 space (the dimension relevant to high T_c) coupled to a $d = 3$ UV field theory that had no bulk dual. So we called it *semi-holographic*, since only part of the CFT had a bulk dual. Working this out led to a lot of interesting issues both for renormalization and for condensed matter.[7]

Yet another approach, with Kristan Jensen, Kachru, Andreas Karch, and Silverstein, looked at models with branes moving in two directions, corresponding to a lattice of fixed charges coupled to itinerant charges. This simulated the marginal Fermi liquid phenomenology of high-T_c. As with many other AdS/CM attempts, the effect of backreaction was not fully controlled. Much work on AdS/CM was *phenomenological*, meaning that one postulated a bulk theory without a known CFT dual. We preferred *top-down* constructions, with a known dual theory. But there was a downside: such theories had extra fields, in particular, scalars, that were prone to instabilities. For example, there was what Silverstein called *Fermi seasickness*, where scalars that were supposed to remain at the origin became tachyonic and developed expectation values.

With my latest grad student Ahmed Almheiri,[8] I looked at an alternative approach to stable top-down Fermi and non-Fermi liquids. Working on AdS/CM seemed to dismay Almheiri (he wanted to work on quantum gravity), but I told him it was a good project. After all, everything was dual. The idea was simply to take a familiar duality like $AdS_5 \times S^5$ or $AdS_4 \times S^7$ and turn on magnetic fields carrying S^5 or S^7 charges. Using a magnetic field for the symmetry breaking tended to be more stable than breaking by scalars or electric fields. This idea originated from d'Hoker and Kraus, who studied one example; we looked at the general case in the search for stability. It was a fun system to work out. We found that in a neighborhood near the space of supersymmetric values of the charge, there were stable solutions (modulo a possible fix for the dilaton). In our first draft we missed one instability, so this was completed by Donos, Gauntlett, and Pantelidou; this reduced but did not eliminate the region of stability.

One last condensed matter–motivated idea, with Silverstein, was to reinterpret a vacuum state as a finite density theory in higher dimensions. For example, the F1 + NS5 system is normally interpreted as a vacuum of a field theory in two dimensions. Instead, the F1 strings could be interpreted as excitations in the NS5 vacuum, so the state would be six-dimensional. Of course, by *T*-dualities and compactifications one could vary the dimension. Our main goal was to find holographic systems with the kind of "$2k_F$" singularities that arose in Fermi and non-Fermi liquids. These had not been seen in holographic models previously, but they were here.

11.4 MORE ODDS AND ENDS

11.4.1 AdS Hierarchies

In $AdS_5 \times S^5$, the AdS_5 and S^5 lengths are equal. More generally, in all simple examples, the *AdS* and compactification radii are of the same order. But in the landscape, there should be a dense spectrum of compactifications with positive and negative cosmological constants. The latter would include some with AdS radius much larger than their compactification radii. Silverstein and I set out to find such solutions. After discussing general constraints, the strategy we hit upon was to add 7-branes to the compactification, because they add a negative term to the energy density. We found some ansatzes of the desired form, but I do not know if we really succeeded: there were some singularities that went beyond my limited understanding of F-theory. Silverstein was optimistic, but I was by nature more skeptical. It may be that the solutions we sought were more sporadic, disordered sums of positive and negative energies.

11.4.2 Wilson Loops

Ray, Yee, and Maldacena had shown that a Wilson loop in the CFT was dual to a string worldsheet ending on the boundary loop. More precisely, this was true for a BPS Wilson loop, which has a scalar piece as well as the vector potential. I had mused over the fact that the ordinary Wilson loop, with vector potential only, was a perfectly good operator, and so should be calculable on the AdS side as well. Perhaps in connection with AdS/CM, I pursued this with my student Sully. It was fairly easy to figure out what was going on. The string for the BPS loop satisfied Dirichlet conditions, the position being fixed by the direction of the scalar on the loop. It was then easy to guess that the loop without the scalar was dual to a string with Neumann boundary conditions. Indeed, that fit with all the symmetries. As a further check, we considered loops that interpolated between the two limits, and showed that there was a nice flow between the simple loop operator in the UV and the BPS loop in the IR.

Unfortunately, when we put our short paper on the arXiv, we learned that Alday and Maldacena had noted this some time before. The renormalization part was new, however. So we rewrote the paper, expanding the RG part (adding in strings that were Dirichlet in some directions and Neumann in others) and resubmitted. But I did not have any application in mind for this, and indeed the paper has only received four citations. But I liked it, as a new application of the RG, and a new corner of AdS/CFT.

11.4.3 Scale and Conformal

Almost twenty-five years earlier, I had built on Zamolodchikov's 1 + 1 RG irreversibility theorem to prove that under broad conditions, scale symmetry would imply conformal symmetry in 1 + 1 dimensions as well. In the time since, people had occasionally tried, without success, to generalize Zamolodchikov's result to four dimensions. But around this time, there was a renewed focus on quantum field theory, and Komargodski and Schwimmer (KS) succeeded in proving the 3 + 1-dimensional irreversibility theorem. So it was natural to ask whether in 3 + 1 dimensions scale invariance again implied conformal invariance. Fortunately, there were two outstanding quantum field theorists at KITP for an LHC workshop: Markus Luty and Riccardo Rattazzi. We first spent some time understanding the KS derivation, which was much more intricate than the 1 + 1 Zamolodchikov theorem. We then examined how the KS theorem might be used to generalize my 1 + 1 argument.

It was an enjoyable project, with various twists and turns and with all three of us contributing key insights. In the end, we did obtain a theorem, but it was not quite as general as in 1 + 1: it held for perturbative theories, but for nonperturbative theories a technical assumption was needed, though it seemed plausible. A bit of excitement arose when another group at the same time announced a counterexample, a perturbative theory that was scale invariant but not conformal invariant. After some time, it was recognized that their theory was actually conformal. It was an impressive calculation, though, and in sorting this out we much improved our own analysis. Also, the two groups jointly managed to understand a classic paper by Osborn, which turned out to have gotten to many of the key results long before.[9]

Notes

1. At an event where we were both a bit inebriated, Andy Strominger told me that he thought this paper was a negative contribution to physics. I found this hilarious, and remind him of it at every opportunity, but it reflected Andy's very positive point of view, that every wormhole must be good for something.

2. [Recent work has shown that wormholes are good for ensuring that the entropy of Hawking radiation is consistent with unitarity.—Ed.]

3. Prior to that, I used Fortran to get a mass spectrum on one paper with Wise. He was impressed.

4. Orgera had started out as Gross's student, but I took over when Gross became busy with the Nobel, and we wrote two nice papers together. After his PhD he returned to the private sector in Italy.

5. Some time later, with another student Eric Mintun, we applied this to higher-spin theories.

6. To date I have not collaborated with any Nobel laureate, even though I have been a colleague for twenty-eight years combined with Weinberg and Gross. I guess our styles are a bit different, though our goals are much the same. But this drought is likely to be broken soon, when Kane receives the prize for topological insulators.

7. I had never thought that I would write a paper about spin-orbit couplings. I always thought that they were an annoying breaking of symmetry, not knowing that they had become the key to topological insulators.

8. His first two papers he signed, *Almuhairi*.

9. [This dispute earned Joe a bottle of wine from a bet with Grinstein, one of the authors of the counterexample.—Ed.]

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