10-21-19 **Zhi-Xun Shen (Stanford): ARPES on Cuprates review**

Nice review on usage of ARPES on cuprates (mostly BSSCO). Pre-formed pairs advocated for b/c of the smooth behavior of the gap when moving through T_c. But now the thinking is that actually the pseudogap and the BCS gap are competing orders, rather than closely related things. Advocated for a more or less vertical line on the phase diagram separating the FL from the strange metal. Have the unusual scenario wherein the spectral features get *broader* at lower T.

QCP: when passing doping around 0.19 one (apparently) sees a sharp change of the gap Δ from something $O(3)$ times the BCS value to precisely the BCS value. Also the slope of $\Delta(\theta)$ measured wrt the nodes is constant across a wide range of underdoped samples w/ different T_cs, but the slope then starts to change (decreases) abruptly at the critical doping. The decreasing slope is what you expect beyond critical doping within BCS theory, since BCS says that the gap (everywhere in k space) should be proportional to T_c, which declines past critical doping.

- Why can we ascribe meaning to the $p \sim 0.19$ doping value as a qcp when we're looking at the system from deep in the SCing state? If we're deep in the SC, haven't we already flowed a long way away from the critical point?[1]

10-30-19 **Chong Wang (Perimeter): Approaching the Neel VBS DCP with the $2+\varepsilon$ expansion**

Very nice perspective on the complex fixed point adjacent to the putative DCP. The starting point is essentially an assumption about the emergent $SO(5)$ symmetry, which is on firmer numerical ground than any of the scaling dimension results. Since bootstrap tells us that the transition in 2+1 is weakly first order, we start from $d = 2$ and do an epsilon expansion. The perverse

[1] While we are saying that there is some critical point at doping of 0.19 ish, this critical point is not supposed to be the same as the one which we think of as controlling the phase diagram. But in any case, we really haven't flowed that far away from the putative important QCP by entering the superconducting region—we expect to have flowed away from the critical point by something like $\Delta_{SC}/\Delta_c < 1$, with Δ_c the temperature up to which the quantum critical fan extends.

thing about this is that we work with sigma model with $d+2$ components, which is non-integral.

The theory is

$$S = \frac{1}{4\pi g}\int_{X_d} (\nabla n)^2 + S_{WZW}. \tag{1}$$

When $d = 2$ there are two fixed points: the Gaussian, which is unstable in $d = 2$ but stable for small enough g for any finite ε, and the WZW CFT, which is attractive at small ε for all g above a threshold. As ε is increased up to 1, these two fixed points come together and merge for some $0 < \varepsilon_* < 1$, and for $\varepsilon > \varepsilon_*$ the two fixed points presumably bifrucate off into the complex plane, leaving behind the realm of unitarity. The assumption is that this point is close to $\varepsilon = 1$, thereby giving a complex CFT close to the weakly first order transitions seen in numerics in $\varepsilon = 1$.

At a technical level, one needs to do a large k expansion as well, where k is the WZW level. So the RG is controlled when all of $\varepsilon, g, 1/k$ are small.

As a miscellaneous aside, I also learned that the WF fixed point for $\varepsilon \notin \mathbb{Z}$ is not unitary, due to operators with imaginary scaling dimensions that disappear from the theory when $\varepsilon \in \mathbb{Z}$. These operators are of pretty high dimension, but eh, still interesting (see Rychkov's paper for more).

- Apparently the WZW term can actually be treated exactly up to two loops, even though naively it doesn't make sense. Why is this?
- What's the generic thing that happens when two fixed points merge after tuning a parameter? They can either change places or split off into the $\mathbb{C}$ plane; anything else? Question can be addressed by characterizing the fixed points by the scaling dimensions; then the problem is probably equivalent to looking at how eigenvalues of matrices move around as you change the matrices.

12-2-19 **Liang Fu (MIT): Supermetal**

Super awesome talk on what happens when a higher order VHS[2] is at the Fermi surface. The basic example was a theory where the dispersion is such that two pieces of the FS touch tangentially, with the dispersion being expanded as $E_k \sim k_x^2 - k_y^4$. At such a VHS, the DOS diverges as $E^{-\varepsilon}$, which is stronger than the usual logarithmic divergence of VHSs in two dimensions.

[2]Where not only $\partial_k E = 0$, but where the Hessian $\partial_{k_i}\partial_{k_j} E$ is degenerate as well.

As far as thermodynamics goes, one can get away with ignoring the rest of the FS and focusing on the higher VHS; this is done by pretending that the dispersion is literally $k_x^2 - k_y^4$, and letting $|k|$ range to infinity. Therefore the low energy theory is basically just a real space model with a single species of fermion and a funny dispersion. Note that since the dispersion is unbounded below, we still have a "FSea" (in fact it is infinitely large in k space) when we set $\mu = 0$. Turning on nonzero μ takes us between a hole-like or particle-like Fermi liquid. Therefore the $\mu = 0$ point is right at a Lifschitz transition and since the $E = 0$ contour goes off to $k \to \infty$ with there being no preference between electron- and hole-like FSs at this point, the $\mu = 0$ point is scale-invariant and no scale k_F exists.

One then proceeds by including interactions and looking for fixed points. At the mean field level, one writes the 4-fermion interaction as $n_\uparrow(E_\uparrow)n_\downarrow(E_\downarrow)$, where E_σ are energies of the two Fermi surfaces. The idea is to express the energy $E \sim \sum_k E_k + g n_\uparrow(E_\uparrow)n_\downarrow(E_\downarrow)$ as a function of the n_σs, which are then solved for self-consistently. The values of $n_\uparrow \pm n_\downarrow$ determine which phase we're in: AFM if $n_\uparrow - n_\downarrow$ gets a vev and Fermi liquid if $n_\downarrow + n_\uparrow$ gets a vev (hole-like or electron-like depending on the sign.[3] Liang and haruki find an AFM for all g in MFT at $\mu = 0$, and pockets of FLs when $\mu \gtrsim g$. The transitions to the AFM are first order.

Since the transition to an AFM at small μ happens at small g, fluctuations need to be taken into account to establish the nature of the phase transition. This is done by doing RG. The small parameter is the ε appearing in the singularity of the DOS—by going into fractional dimensions one can tune this ε to be arbitrarily small, and it controls the RG. At $T = 0$ they find an interacting fixed point that is very analogous to the WF fixed point for bosons. The asymmetry of the falloff of the DOS means that the fixed point is at nonzero μ, at least within the momentum shell scheme. They find that the critical point has diverging charge and spin susceptibilities but a finite pairing susceptibility.[4]

- The interacting point they find is a NFL, which they demonstrate by looking at the singular power-law nature of the frequency part of the self-energy. This requires going to two loops to see (sunrise diagram).[5] Is this a generic feature of interactions? What kind of interactions are needed to give $|\omega|^\eta$ parts of Σ?

[3] The n_σs are measured relative to the $\mu = 0$ state and can be negative.

[4] The KL mechanism doesn't exist here since $\nexists\, k_F$.

[5] The cool thing here is that unlike other NFLs, this one is *perturbatively close* to a free theory!!!

- The quasiparticles are only destroyed in the region close to the VHS. What exactly does this mean? How exactly do we think about the fixed point from a more traditional FL framework?
- How exactly do all these things go away when the VHS gets buried below the Fermi energy?

12-3-19 **Subir Sachdev (Harvard): Random Hamiltonians for the quantum critical point at $p \sim 0.19$ in the Cuprates**

The basic idea is to consider a random Heisenberg model $H = \sum_{i<j} J_{ij} S_i \cdot S_j$, with J_{ij} having zero mean and being Gaussianly distributed. This is just a spinful version of the Sherrington and Kirkpatrick model. For $S = 1/2$, the ground state is a spin glass with $\langle S_i(\tau) S_i(0) \rangle \sim$ const. But for larger spins, can consider a critical scaling for the correlator as some power of τ, e.g. $1/\tau^{d-1}$. The path integral for the spins looks like

$$S = i \int d\tau \, A(S) \partial_\tau S - \int d\tau \, d\tau' \, S(\tau) Q(\tau - \tau') S(\tau'), \tag{2}$$

with $Q(\tau) \sim \tau^{-(d-1)}$. Then you can impose self-consistency $\bar{Q} = Q$ with the bar denoting averaging over disorder to get a mean field solution to the problem and constrain d. Do this by eliminating the annoying WZW sympletic part above by fermionizing. The interaction SQS can be decoupled with a bosonic mode, as long as the bosonic mode lives in d dimensions, since then $\langle \phi(\tau, 0) \phi(0, 0) \rangle \sim 1/\tau^{2(D-2)/2} = 1/\tau^{d-1}$.

The above was done in the Sachdev and Ye paper. Now we want to introduce doping by having hopping electrons and spin exchange, so that

$$H \sim - \sum_{i<j} \left(t_{ij} c^\dagger_{\alpha i} c_{\alpha j} - J_{ij} S_i \cdot S_j \right). \tag{3}$$

The infinite U constraint of $\sum_\alpha c^\dagger_{i\alpha} c_{i\alpha} \leq 1$ is an annoying constriant to work with, since it's an inequality. Better would be a gauge constraint like Gauss's law. For this we do slave-particle by writing $c_\alpha = f_\alpha b^\dagger$. We think of this as acting on a reference vacuum state with no hole by creating a hole ($b^\dagger$) and destroying a spin (f_α). This introduces a $U(1)$ gauge redundancy under which both f and b are charged. If $\sum_\alpha f^\dagger_\alpha f_\alpha + b^\dagger b = 1$ then $\sum_\alpha c^\dagger_{i\alpha} c_{i\alpha} \leq 1$ (but not conversely?!). So when we implement this constraint with a Lagrange

multiplier s, we get free terms like $b^\dagger(\partial_\tau + s)b$ and so on. For spin 1/2, this gives us an $SU(1|2)$ algebra at the point where the chemical potentials for the bosons and fermions are symmetric.

We could also do slave boson via $c_\alpha = b_\alpha f^\dagger$—the statistics doesn't matter in the constrained subspace, and the same algebra is reproduced (since $SU(1|2) \cong SU(2|1)$). The spin-spin interaction can be similarly decoupled with a fermionic field with action $\int k^r dk\, \psi_k^\dagger \psi_k$.

Anyway at the "critical point" where both $SU(2|1)$ and $SU(1|2)$ formulations are equivalent, get $\langle c^\dagger(\tau)c(0)\rangle \sim \tau^{-1}$ like in a FL. We can move away from the critical point in two ways within MFT: condensing bosons in either the $SU(1|2), c_\alpha = f_\alpha b^\dagger$ description, or in the $SU(2|1), c_\alpha = b_\alpha f^\dagger$ description. In the former we get a Fermi liquid, with $c_\alpha \sim f_\alpha$ since the boson is condensed. Here $\langle S(\tau)S(0)\rangle \sim \tau^{-2}$ as expected in a FL. In the latter description, we have a spin glass $\langle S(\tau)S(0)\rangle \sim$ const, since the spin operators $S^a = \frac{1}{2}b^\dagger\sigma^a b$ are now just constants after the condensation. The critical point where the bosons aren't condensed and either description is valid is a deconfined critical point. Apparently reproduces decently well the specific heat divergence seen in experiment.

12-3-19 **Hong Liu (MIT): Voids in operator growth**

Hong defined a void in an operator as follows. Let $\mathcal{O}$ be an operator and evolve it in time with some random unitary $\mathcal{U}$ via $\mathcal{U}^\dagger\mathcal{O}\mathcal{U} = \mathcal{O}(t)$. We can decompose $\mathcal{O}$ in an operator basis as

$$\mathcal{O}(t) = \sum_I \alpha_I(t)\mathcal{O}_I, \tag{4}$$

where the basis operators $\mathcal{O}_I$ would run over e.g. $\sigma^{I_1} \otimes \sigma^{I_2} \otimes \ldots \otimes \sigma^{I_l}$, for $I_i \in 0, 1, 2, 3$. We then interpret the coefficients $|\alpha_I(t)|^2$ as the probabilities for finding the operator $\mathcal{O}_I$ at time t. This interpretation makes sense if $\mathcal{O} = \rho(t = 0)$ is the initial density matrix of the system, since then

$$|\alpha_I| = |\mathrm{Tr}[\rho(t)\mathcal{O}_I]| \tag{5}$$

A void is then defined as an operator $\mathcal{O}_I$ that has a bunch of consecutive **1**s in a row. An operator with a void in region A would be expected to show up with probability $q^{-|A|}$, where q is the number of generators in the onsite Hilbert space. This is because we expect generically each α_I to be $1/q^{L/2}$, and fixing $\mathcal{O}|_A = \mathbf{1}$ gives q^{L-A} choices for each of the operators that occurs with probability $(1/q^{L/2})^2$.

However, one can show that these operators with voids actually occur more often that one might think, and in fact are required to exist due to unitarity (i.e. due to $S(A) = S(\bar{A})$.

12-9-19 Robert Willett (Nokia Bell Labs): Interferometry in 5/2 FQHE

Today's chez pierre was on interferometry measurements of $\nu = 5/2$ QHE samples. Recall that for 5/2, we naively expect a Fermi surface: five electrons plus two flux quanta makes a fermion. Therefore we naively expect a Fermi surface. However we see in expts that the longitudinal resistance is indeed zero (i.e. there's a regular plateau), and so something must be going on to gap the FS (pfaffian / anti-pfaffian, etc.). Halperin, Lee + Read predict something with abelian charge 1/2 particles and non-ab charge 1/4 particles. The braiding of the 1/4 particles is accompanied by a change in the physical fermion parity.

Basic principle is to design the usual interferometry coral thing and look at resistance along the edge[6]. You always have oscillations coming from the electronic AB phase as they enclose mag flux in the coral, so you subtract these off to see what the braiding interference is.

The way you control the qps inside the coral is either by sweeping flux or by adjusting the side gate voltage on the coral. Qps here are fluxes without electrons or electrons without flux, so when you change the mag field in the coral, the additional / fewer flux quanta relative to the 5/2 filling with appear as qps. On the other hand, changing the side gate voltage lets you change the area of the coral[7] which also lets you tune the number of qps inside.

One interesting effect is the "even-odd effect": if 1/4 particles on the edge are moving around a 1/4 particle trapped in the coral, then by the braiding properties the physical fermion parity of the coral is changing rapidly—this results in a very rapid oscillation to the resistance that totally decoheres / washes out the oscillations from braiding. But if you also have a 1/2 (Abelian) particle in the coral, then the braiding phase with this will still survive.

Anyway Robert showed some data for the resistance oscillations as a function of magnetic field. The fourier transforms weren't all that convincing, but

[6] qps on the edge will either jump the first tunneling point or go around the coral and jump the second; these two processes interfere and lead to oscillations in the resistance as a function of the number of qps inside the coral.

[7] The coral is created electrostatically with electrodes placed above the 2DEG that capacatively control where the electrons are able to move.

he claimed to have pretty clear evidence for the existence of the non-Abelian qps.

12-9-19 ZX Shen (Stanford): marathon review of ARPES in the cuprates

Today we had the rare treat of getting an ≈ 4 hour long survey of photoemission techniques in the cuprates from the master himself.

Fermi pockets

In some multilayer compounds, Fermi pockets can be seen clearly on some of the layers. E.g. there's this 5-layer compound where the middle three layers all have some sort of Fermi pockets. However, this is likely due to the fact that the middle layer has AFM ordering, which spreads to the two adjacent layers.[8]

ZX doesn't seem too worried about the Fermi arcs—he bets that pockets will be found someday. It's very hard to see any kind of signal, so the fact that we don't see the backside of the pockets isn't worriying necessarily.

Comments on the spectra

Spectra[9] for $p < p_* \approx 0.19$ and $p > p_*$ look the same: basically just a vanilla SC.[10] But above T_c, the two spectra look totally different. For $p > p_*$, $A(\omega)$ has a broad peak at E_F.[11] As T is lowered, this peak splits into two with a big dip opening between them; this is what we expect for a gap forming at the FS. For $p < p_*$ and $T > T_c$, the spectrum is the opposite: there's a *dip* at E_F (the psgap). Therefore the two doping regimes offer very different pathways to the same SCing state. This also tells us that the phase boundary (if we assume that it exists) is basically vertical in the T-p plane.

Sticking to the UD case, the distinct nature of the two gaps can be illustrated by noting how they redistribute spectral weight. The psgap results in a broad transferring of spectral weight up to relatively high ω, while the SCing

[8] Apparently ARPES can see the first ~ 3 layers into the sample.

[9] Everything that follows for now is comments on spectra at the antinode.

[10] It's very important that the spectra for the UD and OD cases are essentially identical below T_c, *even at high energies*. The SCvity totally erases the psgap.

[11] They can see slightly above E_F in ARPES due to thermal activation of states above E_F.

gap shows the more typical transfer of lots of spectral weight around E_F to right below E_F. This means that the psgap and SCing gap operate at very different energy scales.

If the two gaps represented independent ordering tendencies, we would expect the SCing gap to "add" on top of the psgap, with the spectra weight just being reduced further after SCvity forms. But in fact this is *not* what we see—some of the spectral weight taken away by the psgap *comes back* after SCvity forms![12] Therefore the psgap and SC represent two competing ordering tendencies. The picture here is potentially that some degrees of freedom which are fractionalized above T_c for the UD case "recombine" after SCvity happens, while for the OD case they are never fractionalized in the first place.

The area of the peak in $A(\omega)$ right below E_F in the SCing state scales linearly with T_c. This would make sense within the purview of e.g. BCS theory if it was a peak in $\sigma(\omega)$, since the area of the peak is proportional to n_{SF} which is in turn proportional to T_c. But this is the single-particle electron spectral function, which a priori shouldn't mimick the behavior of $\sigma(\omega)$.

Phase diagrams

The part of the phase diagram we can explore is the p-T plane. The interesting thing about p in the cuprates is that it does not function as anything remotely like a chemical potential. Changing p doesn't lead to just a trivial change of a Fermi level or to a renormalization of the band structure or anything like that; it totally changes the physics.

The general organizing principle for the phase diagram is the observation of a critical something at $p_* \sim 0.19$. This can already be seen in the SCing state. If one looks at $\Delta(\theta)$, where θ is measured in k space with respect to the node, one finds that $\partial_\theta \Delta(\theta)$ is constant in UD'd cases, but then starts to fall as soon as one enters the OD'd regime. The falling is what we expect: T_c decreases past p_*, and in BCS $\Delta \sim T_c$, so that Δ should decrease with decreasing T_c. The mystery is then why this doesn't happen on the UD'd side.

Anyway, part of the point of this is that the "phase boundary" between the UD and OD regions is pretty much vertical in the T-p plane. This is corraborated by looking at the spectra, which are qualitatively different as one crosses p_*, for all $T > T_c$.

In e-doped cuprates, we have a big separation of scales—$\Delta_{SDW} \gg \Delta_{SC}$. In p-doped curpates all the various ordering scales are approximately the same—

[12] Note that it's kind of nontrivial that he can measure the absolute spectral weight across different samples.

this makes them more interesting.[13]

For e-doped cuprates there are actually qps at the antinodes, while it is the nodes that are less coherent (opposite to p-doped!).

Other experimental tools

One kind of cool probe ZX talked about was time-resolved ARPES: you do pump-probe by zapping the sample with a laser to get it in some excited state, and then you do follow-up ARPES in the usual way. They can actually see the band structure changing on a time scale of hundredths of picoseconds!

Anyway I think the point of this was to study the properties of the in-plane phonons: they're able to excite a particular in-plane mode relevant for superconductivity,

- Note that for two competing orders (psgap and SCvity), the general expectation would be a first order transition between them. This is because if we plot the free energies of the two "ordered" states as a function of e.g. T, we expect the two free energy curves to cross transversely. This leads to a discontinuity in the derivatives of the free energy across the transition, and hence a first order transition. So, what order is the SCing phase transition in the cuprates? Probably second order, but then how do we explain this competing order thing?

12-17-19 **Hao Zhang (Tsinghua): Majorana nanowires**

Awesome talk at TQC shenzhen from a former delft guy. Basically since 2012 they've had this massive effort to make sure they're doing all the measurements correctly. They grow the nanowires from this very cool vapor deposition process—a gold particle acts as a seed and then some gas of high SOC elements basically nucleate below the gold particle and grow it up into a big stalagmite shape. So you end up with this huge forest of nanowires, which can be grown in e.g. hashtag shapes as well. Then you have to do this comically ham-fisted approach where you break off the wire with a stm chip or something, and then drag the wires over to a sample and all this crazy shit. Even to see what

[13] Why is e doping different from h doping? The nn neighbor hopping model on the square lattice for the cuprates is PH symmetric. The thing that breaks the symmetry is the nnn hopping. Note: if nnn hopping is evidently so important for the phase diagram, why can we stop there?

they're doing, they need electron beam imagining cause everything is so small. Pretty cool.

Anyway then they were able to basically get rid of disorder and are now pretty confident (but weren't between 2012 and now) that they've got a majorana.

He was able to give me a good picture of where the electron goes when it tunnels into a wire that e.g. dave totally failed at: the electron *doesn't* delocalize. Even when you have a conductance peak corresponding to a single electron, what's really going on is that a single electron enters the wire on one end and then another is ejected from the other end—there is no sense in which one electron enters and then splits up into two delocalized parts. You're not actually changing the $(-1)^F$ of the wire. This is in the case where you have a charging energy—forget what happens in the other case.

The Alicea proposal of controlling where the topological regions are with gates is basically impossible because the whole thing is proximitized to a superconductor—you can't gate a superconductor. Basically you can't gate a metal in general, and definitely not a superconductor (graphene is okay because of the very low carrier density). So instead you've got to use measurement-based quantum computation.

12-17-19 Frank Verstrate (Ghent): quantum symmetries in tensor networks and stuff

Nice talk at TQC that finally made me understand some of the point behind the whole tensor network approach to topological phases.

Frank's whole motivation for getting into this was understanding measurement-based quantum computation. The idea here is that you can do quantum computation without needing to actually do entangling gates or anything like that: you just need a big resource (an entangled state) on which to do measurements. By proceeding and measuring the qubits of the system (with the types of measurements you do dependent on outcomes of earlier ones), you can actually do computations.

This motivated considering states built from tensor networks contracted from tensors with special symmetry properties. All of non-chiral TOs and stuff can be reconstructed in this framework. MPOs are tensors that can be pulled through the network topologically. Frank calls them symmetries since they commute with the Hamiltonian, but they aren't symmetries in the usual sense since they are not invertible in general.

Operators which commute with the MPOs must be marginal or irrelevant.

This is basically coming from the fact that the MPOs (alias string operators?), through their non-commutativity and linking and all the usual topological stuff, are encoding information about anomalies. In fact Frank called this statement about commuting operators not being relevant an LSM theorem: if you restrict to operators that commute with the MPOs then you can rule out a trivial gapped state (in the absence of symmetry breaking), and hence there can't be any relevant operators left over, since they would take you to such a trivial gapped state. Note that we can get some results about the existence and properties of critical states without ever talking about conformal transformations or even about the notion of distances / actual geometry!

One example of this is the MPO that implements KW duality in the Ising model—normally we don't think of this as being performed by an operator, but this approach is very useful. If this MPO is a symmetry then we must be at the self-dual point, i.e. MPO symmetry $\implies$ criticality.[14]

- How exactly does this measurement-based computation thing work? What about teleportation?
- How does this duality defect in the Ising example work?

1-9-20 Shaowen Chen (Columbia): more stuff on 2d twisted graphene systems

Another talk on the progress made on identifying the correlated insulators seen when stacking various types of graphene layers on top of one another. One thing they do differently is to gate the samples both above and below; this way you can control the carrier density and displacement field D independently so that you can e.g. look at transitions as a function of D.

First he talked about looking at the effects of pressure. They literally put the whole sample inside a bath of oil and then squeeze it. Pressure affects the extent to which the dirac cones on the two layers hybridize; this in turn affects the flatness of the center band; hence the extent to which a give angle is magic can be tuned by applying pressure.

One system he was looking at is 2+1 stacked graphene—two layers together and then one stacked on top. This has a semimetallic state at charge neutrality, with stuff changing when the displacement field is turned on. Depending on

[14] Again it's not a conventional symmetry since it fuses with itself to produce the identity and ψ.

the filling, there are metal-to-insulator transitions seen as a function of D. Also depending on the filling there are MITs seen as a function of an applied $B_\perp$; these transitions are not understood yet.

2-6-20 **??? (UIUC): dissipationful superconducting states and phase separation**

Pretty interesting colloquium on superconducting films. Studies of 2D SCs motivated by the fact that the relevance of disorder means that in 2D you can't really have metallic states.

The setup in her lab is an array of SCing islands fabricated in a grid with variable lattice constants. As T is changed, there are two transitions in the resistivity. One is (presumably) where the islands start SCing, and the other is where the whole sample (including the metallic substrate) starts to be SCing. However, one finds array sizes for which the extrapolation leads to a value of $T = 0$ for the second transition, giving a resistive dissipationless state even at $T = 0$—the stuff supressing SCtivity here evidently must be phase interactions between the different dots.

Also talked about vortices. Remember that phase slippage means that moving vortices produce a transverse current. Also the vortex motion is dissipative, giving resistance. Therefore if flux density is such that the vortices are commensurate and form a crystal, they will be self-pinned and give rise to a low resistance state. Hence the resistance as a function of B field is very non-monotonic, exhibiting minima whenever you are near a commensurate lattice flux density.

2-24-20 **Yahui: new parton construction for cuprates**

Canonical way to get a FS-size changing transition is $c = fb$, and then looking at the Higgs / confinement transition for the boson b. However this predicts a spinon fermi surface in the phase where b is confined; such surfaces don't show up in heavy fermions (small magnetically ordered FS to large FS) or in cuprates, so this idea is out.

Instead of fractionalizing and "reducing dof", try "adding new dof" via ancillas. Basically think of the backside of the Fermi arcs as coming from new dof that you add to the FS.

3-9-20 **Joe Cheneklsy (MIT): designing quantum toy models**

Graphite / phene has huuuuge diamagnetism! Roughly because formula for Lamour diamagnetism goes as $1/m$ and graphene disperses linearly. Graphite flakes levitate on magnets!

Philosophy is that toy models are useful becuase sometimes nature finds a way of realizing them anyway (graphene, Kitaev honeycomb, Kane-Mele carbon + SOC (carbon no-go since too light for SOC, but can put heavy elements on top of the graphene to provide the SOC effect).

For interesting physics we want flat bands to occur not because of the absense of hopping terms in the Hamiltonian (atomic insulator), but because interference effects lead to localized wavefunctions. E.g. in kagome materials you can have localized standing waves moving around the kagome hexagons.

Finally spin liquids are insulators (e.g. herbertsmithite is blue, hence can't be a metal), and so you need to use neutrons to study them, which in turn implies that you can't study 2d spin liquids since thin films don't provide big enough cross-sections for the neutrons. So could one then dope a spin liquid so that it becomes a metal, e.g. herbertsmithite? People tried (someone made black hrbrtsmte), but its basically impossible. Too bad, since otherwise we could use optical probes to study them.

3-30-20 **Doug Natelson (Rice): High T_c tunnel junctions**

Worth remembering why big elements like uranium can have their band structures analyzed by single-particle wavefunctions, even though they have tons of electrons. The key is the Pauli principle; subsequent electrons get added at higher kinetic energy, and the growth of the kinetic energy is larger than the increased Coulomb repulsion energy, allowing the system to be treated to first approximation in the non-interacting limit.

Johnson noise: basically fluctuation-dissipation theorem; there are fluctuations even in equilibrium. The noise here is white and linearly proportional to T. Thus the fluctuations in e.g. voltage are often used to measure T.

Shot noise: measures magnitude of tunneling charge. Want to tell the difference between a large number of small-charge tunneling events and a smaller number of large-charge tunneling events. Want to then do measurements of tunneling shot noise in cuprates to see what the charge carriers are. Do this by creating a junction of two doped high Tc materials with an undoped layer in between them.

Measure voltage fluctuations by putting voltage amplifiers on both sides of the junction. The noise caused by the tunneling with show up in both amplifiers while the noise intrinsic to the amplifiers themselves will be independent;

hence you can get the tunneling noise by cross-correlating the noise of the two voltage amplifiers.

Pairing seems to be more robust than phase-coherent superconductivity (look at the noise spectrum to see fraction of tunneling events that involve pairs).

Senthil: can we have anisotropy in momentum space? e.g. the antinodes are the regions which contribute the most to the c-axis conductivity. So the antinodes are where the pairs live; the nodal regions have no coherent quasiparticles.

8-3-20 **Debanjan and Cyprian: phonons, plasmons, and pairing in TBG**

- Bandwidth of the phonons exceeds the electronic band width — hence phonon interactions should be important.
- Umklapp processes (in mini BZ) are very small momentum compared to the original BZ, hence they will be important.
- T_c in the real system is determined by phase coherence rather than by where the pairing gap sets in.
- Eliashberg theory: basically BCS gap equation but treats phonon and electron interactions on the same footing. Get an equation like

$$\Delta(i\omega, k) \sim \int_{\omega', k'} K(\omega', k', \omega, k)\Delta(i\omega', k') \tag{6}$$

  for some kernel K. The diagrams here are the vertex corrections.

8-3-20 **Dominic Else: correlated fragile topology**

- property of ground state wavefunctions: can the wavefunction be deformed into an atomic insulator *without* changing the charge?
- requires spatial symmetries (probably)
- only makes sense for symmetries where you have $U(1)$ particle number conservation

- example from bose mott insulator on hexagonal lattice where the mott wavefunction is acted on by product of $\sum_{i\in\partial\text{hex}} b_i$ over all hexagons. Can show that resulting wavefunction is short-range-entangled and hence is presumably the ground state of a local Hamiltonian.

8-4-20 Sagar Vijay and Michael Gullans: non-unitary quantum circuit stuff

Sagar:

- Measurement outcomes are always recorded, so that the system always remains in a pure state. If we consider the mixed-state density matrix obtained by considering the superposition of measurement outcomes, evolving in time gives us a volume law, and nothing interesting happens.
- Alternative interpretation: purification transition for mixed initial states.
- the steady-state volume law phase (low measurement rate) is distinct from a random page state:

$$S_A = s_0|A| + \Delta \ln |A| + \dots \tag{7}$$

  A generic random page state only has the first term. The coefficient Δ is the same throughout the entire volume-law phase. (check out R. Fan + S. Vijay + Ashvin + You 2020)

- He considers few-body non-local unitary dynamics with proj. measurements. OP for the phase transition tests whether local projective measurements outside of two disjoint subsystems can increase their mutual information
- fully-entangled phase distinct from page state, which has no memory of any spatial structure
- When are two disjoint subsystems entangled? Ask whether if there's a partition of the system such that A is in one factor and B is in another. Look at

$$\min_{C\supseteq A} S_C \text{vs} \min[S_A, S_B]. \tag{8}$$

  Think in terms of Bell pairs: if no Bell pairs have one spin in A and one spin in B, then A and B are separable (even though A and B will still be entangled).

- Take a look at the "cluster state" on four qbits: shows you that you can increase mutual information between A and B by performing projective measurements *outside* of A and B.

- models for this phase transition: instantaneous quantum poly-time dynamics: initialize in product state in X basis; apply two-site control-phase gate in Z basis; with probability p measure a spin in Z basis, then rotate to X-basis.

- graphical interpretation of dynamics: control-phase gates draw lines between qubits, while projective measurements on a given qubit erase all lines emanating from it — phase transition is again a percolation transition

- steady-state wavefunction can be prepared from a product state with only $O(N)$ two-site unitary gates; contrast this with the page state which needs much more. As a consequence, measuring a finite fraction of the spins in the steady-state will disentangle the remaining spins (unmeasured spins will form a "separable state").

Michael: Threshold results for monitored random quantum circuits. Basic motivation: any kind of sustained memory / transmission / processing of quantum info will likely be a non-eq. process. Exists a notation of a phase transition between success and failure at sustained quantum info storage at long times and for large systems. (measurements are like errors)

- Does simulations with clifford unitaries; simulation done with the Gottesman-Knill theorem

- noisy coding theorem: exists a *sharp* critical error rate for a given code rate at which error correction ceases to be possible.

- exists a (second-order) transition at some critical error rate e_c near zero code rate where the amount of information able to be conveyed through a quantum channel turns on smoothly from zero

- order parameter for the phase transition: look at the entropy of a single entangled reference qubit (related to entanglement fidelity)

8-10-20 **John McGreevy: Entanglement basics**

- What do you mean by "characterize the physical corner of $\mathcal{H}$ by its entanglement properties? Given that one can saturate entanglement fairly quickly in quantum circuits, how can entanglement provide enough information to distinguish all the different states in Hilbert space?[15]

- When you said "all the states you'll ever see", do you mean "all the states you'll ever see as a theorist", or all the states that are in principle possible to create with physical time evolution? Is it not easy to create one of these "far away states" by e.g. acting on a strongly interacting electron system with a bunch of creation operators (e.g. like shining ARPES light on a material)? Certainly thermal states are states that I "see" on a regular basis.[16]

- Are phases with spontaneous symmetry breaking and gapless Goldstones codimension 1 in parameter space? If so how can we have gapless phases?

- You said for the gapless case, phases are distinguished by regions in which perturbation theory works. Do these regions of convergence fully cover parameter space (with the exception of a few higher-codimension things)? What phase is e.g. strongly coupled QCD in?[17]

- How does the quantum circuit definition for phase equivalence work for gapless phases, where the definition of phases was in terms of the convergence of perturbation theory? No adiabatic evolution is possible?[18]

- Why do we expect S_A to be area-law even at critical points in $d > 1$? Apparently it was just his hunch. Related to the fact that fluctuations in 1+1D are strong enough to prevent order?[19]

[15] IT's not entanglement, but complexity. This is just a counting argument; there's nothing information-theoretic about it. Also we are assuming that the only way to prepare states is to act with unitary time evolution. Acting with non-unitary time evolution or creation operators or something can create states that are far out in the "desert" of Hilbert space.

[16] These don't count; the argument is only for unitary time evolution.

[17] First, distinguish between radius of convergence for perturbation theory and existence of being able to analytically continue. Using radius of convergence is also problematic since it's zero at free field theories. Belief is that there are always things like the chiral lagrangian that we can use to tile parameter space with regions where some kind of perturbation theory is possible. …In any case, the better definition of a phase is a region in which expectation values of operators vary smoothly.

[18] Applying finite depth local quantum circuits should at least not make expectation values change dicontinuously, so any two wavefunctions connected by a finite depth quantum circuit are in the same phase. But is the converse true?

[19] His answer: intuition coming from self-similar tensor networks, viz. MERA. In 1+1D,

Going to focus on $T = 0$ ground states of quantum systems. Most basic thing to ask: how many dofs are there at $E \to 0$? (where by "dofs" we mean things that can be created by local operators).

Walls of gap closing are generically codimension 1: if we have n coupling constants in the Hamiltonian, then $\Delta = 0$ is 1 equation, and so we generically have a $(n-1)$-dimensional space of solutions. Showing that these gapless walls are closed though is more tricky — best way to do it is to find topological invariants associated with each phase. Note however that the wall of gaplessness does *not* have to correspond to continuous second order phase transitions: the gap closes at first order transitions as well, it just closes in that there is a level crossing between the ground states. This level crossing happens *below* the continuum of levels for a first order transition (it occurs right at the bottom of the continuum for a second order transition).

Code distance: the number of errors you can correct for. In topological phases, the code distance is proportional to some power of the system size (does it have to be proportional to the volume?).

Consider an interface between two phases A and B. By definition, the coupling constant that you tune to get between A and B has to pass through a "wall of gaplessness". Therefore if the Hamiltonian was equal to its value at the boundary between A and B everywhere in space, the phase would be gapless. This means that expecting the boundary between A and B to be gapless is pretty reasonable. In fact the existence of systems which are not in the same phase as the vacuum and which still have gapped boundaries (e.g. toric code, and ...?) are kind of cool / unexpected.

How do we then define phases when the whole system is gapless? Basically, a phase is a region in which perturbation theory works, in that expectation values of local operators and thermodynamics and stuff varies smoothly throughout the phase.[20]

If we require some symmetry group H, the allowed phases are obtained by slicing through some higher codimension subspace of parameter space. The way in which this slice intersects the walls of gaplessness and phase boundaries can be interesting.

doing the cut through the hyperbolic tensor network gives you a log divergence because of the structure of hyperbolic space. In more dimensions you don't get such a divergence.

[20] From McGreevy's updated notes: an annoying fact is that sometimes within a phase there are observables which vary non- analytically across a point where the thermodynamics and all local observables are perfectly smooth. A classic example is the roughening transition of Wilson loops in lattice gauge theory. So when we say 'physics varies smoothly' we really mean local observables. For non-local Hamiltonians, we do not even know how to define a notion of phase.

Remember that essentially everything we're interested in is captured by the ground state wavefunction. E.g. TEE cannot distinguish between many TOs, but the wavefunctions can. Can reconstruct fusion rules and braiding and everything from the wavefunction via "entanglement bootstrap".

Two phases are the same if they can be connected with a finite-depth quantum circuit. If there's a gap, the time for this circuit can be finite while preserving adiabaticity. Note that finite-depth quantum circuits map local Hamiltonians to local (but less local) Hamiltonians.

How is the entanglement different for a phase obtained from a finite-time local quantum circuit acting on a product state vs. a LRE state? One diagnostic is area vs perimeter law. The size of the region around the boundary which contributes to the entropy in the area law case is determined by the depth of the circuit which connects it to a product state.

SSA: $I(A:C|B) = S_{AB} + S_{BC} - S_B - S_{ABC} \geq 0$. For top phases in 2+1D, $I(A:C|B) = 2\gamma$.

Non-ground states: $S_A(\text{thermal states}) \sim R^d S_{th}(T)$, with $S_{th}(T)$ the thermal entropy density. This doesn't mean that these states are highly quantum, just that they have lots of entropy from being very mixed.

8-10-20 **Monika Schleier-Smith: Engineering quantum spin models with atoms + light**

Rydberg atoms: area goes as square of the principal quantum number — the states we're interested in for synthesizing e.g. spin chains have $n = 43$, super huge! This means they are very polarizable, which in turn makes them easy to trap with an optical dipole trap.

How to realize a TFIM in an AMO system like this? The X term can be implemented by using microwaves to induce transitions between $|\downarrow\rangle$ and $|\uparrow\rangle$ states — if we have a rapid shining of microwave pulses, this mimicks the effect of the X term (think about trotterizing the time evolution). Basically implementing the Hamiltonian by using light to implement H_{ZZ}, then H_X, then back to H_{ZZ}, and so on.

SYK-like fermion models are hard to make. But non-local spin models $J_{ij}S_iS_j$ are easy to make — interactions generated by photons can be arbitrarily long-ranged. All-to-all interactions are particularly well-suited for optical cavity setups. Can easily control ratio $J_\pm/J_z$ using a magnetic field oriented in a particular direction.

8-10-20 **Subir Sachdev: SYK criticality**

- What exactly goes on at $p < p_c$? How do you know there's spin glass? It's metallic since you have spinless fermions that are floating around—do these do anything, or is it just a decoupled stack of a spin glass and a metal?
- How does the duality between the two representations on either side of p_c relate to 2+1D fermion-boson dualities? (kinda cheap since they mention this in the paper but most people wouldn't have read it?)
- Does the spatial dimension of the bath have any consequences for transport / thermodynamics? Or is the bath purely a formal device?
- How do I couple theories like this to electromagnetic fields and stuff to compute responses, given that there is no dimensionality? What do you mean by resistivity?
- The critical point of the random $t = J$ model apparently has FL behavior with $\rho(0) \neq 0$. What does this mean since there are no dimensions,[21] and also is this related to S_0? Maybe we can compute ρ by arguing that it's related to the equilibration time, which can be well-defined (?) in 0+1D?
- What's the transition like between the metallic spin glass and the disordered FL at $U < \infty$?
- In what sense does the free random matrix model have quasiparticles? After all, the self-energy has a branch cut.

Quasiparticles are additive: total energy is polynomial in qp occupation numbers, and wavefunction is roughly a $\otimes$ of qp wavefunctions.

There's some kind of Mottness observed in essentially every type of observed system that has Plankian dissipation. SYK and its variants are missing this Mottness.

Note that in SYK the randomness is essential. Without the randomness we'd have to diagonalize a $2^N \times 2^N$ matrix, which is impossible.

Greens function going as $1/\sqrt{\tau}$ in the SYK model can effectively be derived just by looking at a $G(\tau) \sim \tau^{-\alpha}$ ansatz. This is distinct from the FL result,

[21] Looks like this resistivity was actually derived within the context of a model with uniform t_{ij} on a large-dimensional lattice.

which goes as $1/\tau$. However, a square root does not necessarily mean that there is not a quasiparticle description possible — it only means that the electron operator $c^\dagger$ does not create quasiparticles. It could be that the electron operator creates a complicated convolution of quasiparticles.

The time reparametrization invariance when $i\omega - \mu$ is neglected follows just from $\int_0^\beta d\tau' \, \Sigma(\tau, \tau') G(\tau', \tau'') = -\delta(\tau - \tau'')$.

Zero-temperature entropy means the ground state is "more thermal" than usual systems. This nonzero S_0 is *generic* for zero-dimensional theories without a quasiparticle description (but in higher dimensions there are of course counterexamples). Note that S_0 is computed in essentially the same way that it is computed in the multichannel Kondo problem (although in the Kondo problem the entropy is $O(1)$; here it is $O(N)$, and most other aspects of the Kondo problem aren't relevant).

Lattice SYK: for $t^2/U \ll T \ll U$ get an incoherent metal w/o quasiparticles. Also get linear resistivity. However, t^2/U is not such a low temperature, and below it you get normal fermi liquid behavior. Moreover, the resistivity is very high in these models — nowhere close to the Plankian limit. This is called a "bad metal". We want to look for something where we have a linear-in-T resistance with a small coefficient, which persists down to zero temperature. In these models you also never get any kind of insulating states. There is thus no Mottness — no parameters you can vary to get an insulator other than changing the chemical potential to deplete a band.

The things that the SYK island models are missing is on-site repulsion — without this you can never get Mottness.[22] One easy example of an insulator is a random J model, $H \propto \sum_{i,j} J_{ij} S_i S_j$ with random J_{ij}s. This can be solved by fermionizing the spins. Kinda like the SYK model, but the random couplings only have two indices.

8-11-20 **Chong Wang: Dualities I**

- What exactly is the reason why the ε expansion taken out to $\varepsilon = 1$ fails for the SC? The HLM paper seems to suggest that a continuous transition does exist at large enough n; does the duality mean that $n = 1$ gives a second order transition anyway?

Historical motivation: what's the nature of the finite-T transition for a 2+1D

[22] Just having all-to-all interactions isn't good enough for making a Mott insulator.

superconductor?

$$|d\phi - iqA|^2 - V(\phi) + \frac{1}{2e^2} dA^2. \tag{9}$$

In the $4-\varepsilon$ expansion, transition is always 1st order because of Coleman-Weinberg (have to remember that the gauge field is dynamical). However, if you put this theory on a lattice, can map it to

$$|\nabla\Phi|^2 + m|\Phi|^2 + \lambda|\Phi|^4 + \cdots \tag{10}$$

We know there's an $O(2)$ WF continuous transition for the second Lagrangian. Therefore we know that there must be some region of parameter space where the former theory has a second-order transition. This means that the $4-\varepsilon$ expansion must become unreliable at $\varepsilon = 1$. Also describes 2+1D quantum SF-insulator transition.

Rigorous KW duality is

$$(D_B\phi)^2 + r\phi^2 + \lambda\phi^4 \;\leftrightarrow\; (D_b\widetilde{\phi})^2 - r\widetilde{\phi}^2 + \lambda\widetilde{\phi}^4 + i\pi b \cup B, \tag{11}$$

with all gauge fields being $\mathbb{Z}_2$ fields. The role of the gauge fields is just to set boundary conditions.

Note that the boson-fermion duality can be written down exactly on the lattice, in that two Hamiltonians that look like they might have the appropriate continuum theories can show to be equivalent to eachother. Whether or not these lattice theories have the corresponding continuum limits appearing in the proposed duality is unknown though.

8-11-20 **Pablo: Moire stuff**

- What's universal about the dip in the SCing dome near $\nu = -2$? I thought this competing order stuff was rather non-universal in the cuprates. Is there any reason to understand why the dip in the dome to kind of occur off to the side of the dome, like it does in the cuprates?[23]

What has happened since 2018? Realized that things are very sensitive to θ; changes of 0.01deg are significant.

- Nematicity in SC states near $\nu = \pm 2$ (note that the nematicity changes with doping), as well in normal state above the SC domes

[23] Patrick says that this might be something non-universal (viz. not reproducible in a field theory analysis), and sensitive to the exact twist angle.

- Strange metals above the correlated insulators
- Competing / intertwined orders above SC domes. Responsible for the two-dome-like shape in the SCing regions (true in cuprates as well; the non-semicicular shape of the dome is due to suppression by some competing order). See a suppression in the hole-doped SCing dome as well.

Applying magnetic field: the dip region in the dome becomes a highly insulating state. See 2004.04148. Also measuring ρ_{xy} in zero field shows that the region coming down into the SCing dome has very high ρ_{xy}.

The SCing dome is nematic everywhere, but as soon as we go into the region near the depression in the dome, the nematicity direction changes abruptly. So maybe these two nematicities are distinct.

New expts measure $d\mu/dn$. See a sawtooth feature as a function of ν. Understand as follows. At $\nu = 0$, have four Dirac cones. As you increase ν, all flavors are being equally filled. Before you get to $\nu = 1$, get a spontaneous polarization where only one Dirac cone gets filled; the other three are emptied. This happens four times as ν is increased.

8-12-20 **Andreas Karch: Dualities II**

What's a duality? Nothing more than

$$\int_{\mathbb{R}} dx\, e^{-x^2/a} = \int_{\mathbb{R}} dy\, (1+2y) e^{-(y+y^2)^2/a}. \tag{12}$$

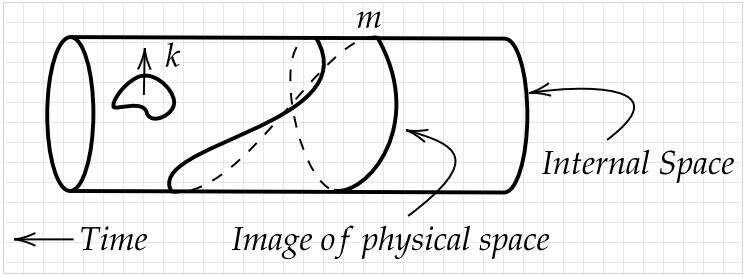
Wow! the RHS is super nasty and interacting! But secretly it's dual to a free theory! Amazing!

Compact scalar:

$$X = pt + wx + \ldots, \tag{13}$$

where $\ldots$ are oscillating terms. First two terms are zero modes, since they die under $\Box$. Note that X is *not* a direction in the space that the field X lives in — its the direction of the internal compact circle. p is the momentum of the field in the internal field direction, which has radius R. x is a center-of-mass coordinate on the internal circle; $x \sim x + 2\pi$ regardless of what spacetime the field X is placed on. p is the momentum of the field along the internal circle if the field (which can be thought of as a string if the spacetime X is on is a circle), in the limit where the image of physical space in the internal space is a point. w is the winding number of physical space in the internal space,

provided that physical space is a circle. Thus $p \in R\mathbb{Z}$, and $w \in \frac{1}{R}\mathbb{Z}$.



T-duality: too good to be interesting. Seiberg duality: actually interacting, but rigorously known. Work with SUSY $SU(N_c)$ gauge theory with N_f fermions / bosons. Old school particle theory people that go on and on about $SU(3) \times SU(2) \times U(1)$ are totally misguided: the gauge group is unphysical, and Seiberg duality gives us examples of theories where we can show that a $SU(N)$ gauge theory is exactly dual to an $SU(M \neq N)$ gauge theory.

Are IR dualities just examples of universality? Sort of, but normally when doing RG we fix a set of degrees of freedom, and look at how coupling constants change. Here we have totally different dof all along the way, and switch from strong to weak coupling. Somehow the confined phase of a gauge theory is equivalent to free non-interacting gluons of a different gauge group.

$$\text{seiberg duality} : SU(N_f - N_c) + N_f + \phi \leftrightarrow SU(N_c) + N_f, \tag{14}$$

where N_f means N_f fundamentals and ϕ is a singlet field in the N_f^2 of a $U(N_f)$ global symmetry group.

In SUSY, most symmetries treat bosons / fermions in a given multiplet equally. The $U(1)_R$ symmetry lets you give different phases to the bosons / fermions within a given multiplet.

8-12-20 **Michael Levin: partons**

Partons: generalized MFT.

Write down some simple fermion / boson Hamiltonian H_0, and find the ground state $|\psi\rangle$. Now $|\psi\rangle \notin \mathcal{H}_{spin}$, because we may not have $f^\dagger_{i\alpha} f_{i\alpha} = 1$ on each site i. However, can (essentially always) adjust the (site-dependent) chemical potential such that

$$\langle f^\dagger_{\alpha i} f_{i\alpha} \rangle_\psi = 1. \tag{15}$$

Therefore while $|\psi\rangle$ isn't in the spin $\mathcal{H}$ space, it is at least "close" to being in $\mathcal{H}$. Any H_0 with this property is called a *mean-field ansatz*. This produces a spin state $|\psi_{spin}\rangle$ and a low energy effective theory H_{eff}.

We hope that these states have the following properties:

- $|\psi_{spin}\rangle$ is the ground state of *some* local spin Hamiltonian.
- H_{eff} correctly describes the low-energy excitations of such a local spin Hamiltonian.

Construct $|\psi_{spin}\rangle$ by projecting $|\psi\rangle$ onto $\mathcal{H}_{spin}$,

$$|\psi_{spin}\rangle = P|\psi\rangle. \tag{16}$$

That is,[24]

$$\psi_{spin}(\alpha_1, \ldots, \alpha_m) = \langle 0|f_{1\alpha_1} \cdots f_{m\alpha_m}|\psi\rangle, \tag{17}$$

where the $\alpha_i = \uparrow / \downarrow$ and i runs over all the sites in the lattice. Although $|\psi\rangle$ is usually some boring mean-field $\otimes$ state, doing the projection can create lots of interesting entanglement and stuff.

Constructing the low energy theory H_{eff}: expect a gauge theory. First, find all onsite unitary transformations U such that $U^\dagger H_0 U = H_0$, and $U^\dagger \mathbf{S}_i U = \mathbf{S}_i$ for all i; these will form a gauge group G. Then couple to a G gauge field that is assumed to be weakly fluctuating.

Note that the Gauss law is *not* that the divergence of E vanishes, since there's a background charge:

$$\nabla \cdot \mathbf{E}_i = f^\dagger_{i\alpha} f_{i\alpha} - 1. \tag{18}$$

This is because the gauge transformation acts as

$$\prod_{ij} L^g_{ij}(e^{-i\theta^g_i} U^g_i)|\psi\rangle = |\psi\rangle, \tag{19}$$

where $U^g_i = e^{i\theta_i f^\dagger_{i\alpha} f_{i\alpha}}$, and where L^g_{ij} acts to shift the gauge field. Note that the physical spin Hilbert space is recovered when $\mathbf{E}_i = 0$ on each site; this is valid in the strongly confining phase (large h). The hope is that even though the non-confining weak-coupling phase has a different Hilbert space than the original model, since the anomalies match you can still get away with using the weak-coupling theory as a low-energy ansatz.

[24] The projection will vanish only if $|\psi\rangle$ has no weight on the singly-occupied subspace at a certain site i — but if this is the case, we cannot have $\langle f^\dagger_{\alpha i} f_{\alpha i}\rangle = 1$ at that site; hence the projection is non-zero.

The gauge-invariant Hilbert space can have either 0 or 2 fermions per site; hence $\mathcal{H}_{spin} \subset \mathcal{H}_{gauge-inv}$ (where 0 or 2 fermions are allowed as long as Gauss' law is obeyed). However, it will always have 1 fermion per site on average, since $\langle f^\dagger_{i\alpha} f_{i\alpha} \rangle - 1 = \langle \nabla \cdot \mathbf{E}_i \rangle = 0$ if we e.g. have translation symmetry and there is no $\mathbf{E}$ flux at ∞.

Note that the effective theory H_{eff} will *not* live on the original Hilbert space $\mathcal{H}_{spin}$! When the gauge coupling is infinite so that there is no magnetic flux, the gauge field is pure gauge and the Hilbert spaces are the same. But even though the gauge coupling is infinite when you literally introduce the gauge field to perform the constraint, this infinite-coupling limit isn't useful, and we essentially always assume that the dynamics of the system are such that the gauge coupling is weak. In this approximation, where we can have nonzero gauge flux, the Hilbert space of the effective theory is larger.

Note that $|\nabla \cdot \mathbf{E}|^2$ is an irrelevant operator — therefore even though to get to $\mathcal{H}_{spin}$ we'd need to add it with an infinite coefficient, we can still hope that we can scale as we would at the free fixed point. This means that the theory with $\nabla \cdot \mathbf{E} = 0$ everywhere and the unconstrained Maxwell theory should be the same in the IR.

The claim is not that the low energy states in $\mathcal{H}_{gauge-inv}$ also live in $\mathcal{H}_{spin}$. However, the expectation is that there exists some unitary map (or projector? The two don't have the same dimension) that the low-energy states in $\mathcal{H}_{gauge-inv}$ can be mapped to those in $\mathcal{H}_{spin}$.

The point is not to try to actually get the correct ground state wavefunction (we won't even have the right Hilbert space). The point is to write down something with the same kinematics, viz. the same anomalies. Then the two field theories have the potential to be "the same" for the IR questions we are interested in.

Ex: π flux state:

$$H_0 = \sum_{\langle ij \rangle} t_{ij} f^\dagger_{i\alpha} f_{j\alpha} + h.c, \tag{20}$$

with $t_{i,i+\hat{x}} = t$, $t_{i,i+\hat{y}} = -t$. Claim is that only gauge redundancy here is $U(1)$ (note that we don't regard the 0 and 2 occupancy states as being unphysical in the low energy theory). Gauge Hamiltonian assumed to be

$$H_g = h \sum_l E_l^2 - K \sum_\square \cos(B_\square), \tag{21}$$

where we assume $h \ll t \ll K$.

Properties of the π-flux state:

- $|\psi_{spin}\rangle$ invariant under spin rotation and lattice symmetries (modified because of the (-1) signs in the hoppings)
- Low energy theory is 4 2-component Dirac fermions coupled to a $U(1)$ gauge field — compact QED_3.

Part II:

Let $|\psi\rangle$ be ground state of some simple fermion Hmiltonian H_0, and $|\psi_{spin}\rangle = P|\psi\rangle$ with P the projector onto the spin Hilbert space. One way to perturb $|\psi_{spin}\rangle$: define

$$|\psi_{spin}^{\theta_{ij}}\rangle = P|\psi^{\theta_{ij}}\rangle, \tag{22}$$

with $|\psi^{\theta_{ij}}\rangle$ the ground state of $H_0^{\theta_{ij}}$, where the latter is H_0 but coupled to a gauge field $e^{i\theta_{ij}}$ on each link. The mapping $\theta_{ij} \to |\psi_{spin}^{\theta_{ij}}\rangle$ is of course gauge-redundant. To see this, note that

$$H_0^{\theta_{ij}+\chi_i-\chi_j} = V_\chi H_0^{\theta_{ij}} V_\chi^\dagger, \tag{23}$$

where

$$V_\chi = e^{i\sum_i \chi_i f_{i\alpha}^\dagger f_{i\alpha}}. \tag{24}$$

Then the ground states are related by

$$|\psi_0^{\theta_{ij}+\chi_i-\chi_j}\rangle = V_\chi |\psi_0^{\theta_{ij}}\rangle, \tag{25}$$

and so the projection is

$$P|\psi_0^{\theta_{ij}+\chi_i-\chi_j}\rangle = PV_\chi|\psi_0^{\theta_{ij}}\rangle = e^{i\sum_i \chi_i} P|\psi_0^{\theta_{ij}}\rangle, \tag{26}$$

since the projection sets the fermion number operator to be 1. So, the projected wavefunction differs only up to an irrelevant phase. This is how doing the projection creates a gauge invariance — even though the original ansatz Hamiltonian H_0 is not itself gauge invariant. The projection essentially creates the gauge field.

Ex: $\mathbb{Z}_2$ SL:

$$H_0 = \sum_{\langle ij\rangle} \left(t_{ij} f_{i\alpha}^\dagger f_{j\alpha} + \Delta_{ij} f_{i\alpha}^\dagger f_{j\alpha} + h.c. \right) + \sum_i \Delta_3 f_{i\uparrow}^\dagger f_{i\downarrow}^\dagger + h.c., \tag{27}$$

where

$$t_{i,i+\widehat{x}} = t_{i,i+\widehat{y}} = t, \qquad \Delta_{i,i+\widehat{x}+\widehat{y}} = \Delta_{i,i-\widehat{x}-\widehat{y}}^* = \Delta_1 + i\Delta_2, \tag{28}$$

where all all parameters are non-zero. The t is nn hopping, while the Δ_{ij} term is an off-diagonal nnn hoppings.

Symmetry transformations that preserve H_0 and $\mathbf{S}_i$:

$$f_{i\alpha} \to -f_{i\alpha} \qquad \text{(only one)} \tag{29}$$

Hence invariant gauge group is $G = \mathbb{Z}_2$. Introduce $\mathbb{Z}_2$ gauge fields on every link of the lattice — including off-diagonal nnn ones.

Low energy theory:[25]

$$H_{eff} = \sum_{\langle ij \rangle} \left(t_{ij} f^\dagger_{i\alpha} Z_{ij} f_{j\alpha} + \Delta_{ij} f^\dagger_{i\alpha} Z_{ij} f^\dagger_{j\alpha} + h.c. \right) + \sum_i \Delta_3 f^\dagger_{i\uparrow} f^\dagger_{i\downarrow} + h.c. + H_g, \tag{30}$$

where

$$H_g = -h \sum_{\langle ij \rangle} X_{ij} - K \sum_{\triangle} Z_{pq} Z_{qr} Z_{rp}. \tag{31}$$

We are assuming $h \ll t$ and $\Delta \ll K$. Gauge invariance constraint is modified just as in the $U(1)$ case to account for the background $\mathbb{Z}_2$ gauge charge at each site:

$$\prod_{l \in \partial i} X_l = (-1)^{f^\dagger_{i\alpha} f_{i\alpha} - 1}. \tag{32}$$

Properties: $|\psi_{spin}\rangle$ is invariant under rotation / lattice symmetries. Low energy theory consists of gapped spin-1/2 fermions with anyons.

Quantum Hall: We'll do the simplest example. QH system with N electrons and $3N$ flux quanta, so that $\nu = 1/3$.

To construct candidate state, consider a larger Hilbert space consisting of three species of fermions ("partons"). Call them f^1, f^2, f^3. Assume we have N particles from each species, and think of the electron as a composite of three partons; $c \sim f^1 f^2 f^3$. Next, need to choose a parton Hamiltonian H_0. Want H_0 to be gapped (assuming we want a gapped state). Want the *average* density of three species of partons to be equal in the ground state $|\psi_0\rangle$. (the partons are going to have charge 1/3, so having equal density for the partons and electrons means the charge densities are the same)

Given H_0, we can construct

[25] This is a "minimal" example which both breaks the gauge group down to $\mathbb{Z}_2$ and preserves all the lattice / rotation symmetries. Adding just normal pairing terms halfhasardly will generically not respect lattice symmetries. Also note that just the presence of the pairing term does not necessarily break things down to $U(1)$; there may be hidden $U(1)$s (found by e.g. using PH symmetry?). See XG's book.

- an electron state $|\psi_e\rangle$
- a low energy theory

Construction of electron state: $|\psi_e\rangle = P|\psi_0\rangle$, where the projection sets all the coordinates of the f^is to be equal:[26] if z_i is the coordinate of the ith electron, then

$$\psi_e(\{z_i\}) = \psi_0(z_i^1, z_i^2, z_i^3)|_{z_i^j = z_i \,\forall\, j}. \tag{33}$$

Example of such an H_0: (now p labels the flavor index)

$$H_0 = \sum_p \int f_p^\dagger(\mathbf{r}) \frac{(i\nabla + \mathbf{A}(\mathbf{r})/3)^2}{2m} f_p(\mathbf{r}) \tag{34}$$

Each parton species in in a $\nu = 1$ IQH state; automatically gapped. Recall that

$$\psi^{\nu=1}(z_1, \ldots, z_n) = \prod_{i<j}(z_i - z_j) \exp\left(-\sum_i \frac{|z_i|^2}{4 \cdot 3l^2}\right), \tag{35}$$

where l is the original magnetic length (remember that the partons see a field of $B/3$; hence the magnetic length square is 3 times as large ($l = 1/\sqrt{|B|}$).

Electron wavefunction is then

$$\psi_e(\{z_i\}) = \prod_{i<j}(z_i - z_j)^3 \exp\left(-\sum_i \frac{|z_i|^2}{4l^2}\right), \tag{36}$$

which is precisely the Laughlin state.

Low energy theory: first find symmetry transformations that preserve H_0 and the fermion operator c:

$$\begin{array}{lll} f_1 \to e^{i\chi} f_1, & f_2 \to e^{-i\chi} f_2, & f_3 \to f_3 \\ f_1 \to e^{i\chi} f_1, & f_2 \to f_2, & f_3 \to e^{-i\chi} f_3 \end{array} \tag{37}$$

Of course, the actual gauge redundancy is $SU(3)$; the above is just a $U(1)^2$ subgroup. If we however break the symmetry down to $U(1)^2$ by e.g. giving the f_i different masses, analysis becomes simpler.[27] Effective theory:

$$H_{eff} = \int f_p^\dagger(\mathbf{r}) \frac{(i\nabla + \mathbf{A}/3 + Q_{pq}\mathbf{a}^q)^2}{2m} f_p(\mathbf{r}) + \sum_{q=1}^{2} \left(h|\mathbf{e}^q|^2 + K(\nabla \times \mathbf{a}^q)^2\right). \tag{38}$$

[26] Setting the coordinates to be equal doesn't give zero since the fermions while anticommuting have different flavors.

[27] Although apparently if you do the full $SU(3)$ analysis you get the same results.

Here

$$Q_{pq} = \begin{pmatrix} 1 & 1 \\ -1 & 0 \\ 0 & -1 \end{pmatrix} \tag{39}$$

The gauge invariance constraint is now

$$\nabla \cdot \mathbf{e}^q = Q_{pq} f_p^\dagger f_p. \tag{40}$$

Integrating out some gauge fields ends up producing the usual expected level-3 CS theory.

Two issues with partons:

- Is the wavefunction correct?
- Is the low energy theory correct?

Wavefunctions: there exist counterexamples where $|\psi_{spin}\rangle$ seems very unlikely to be the ground state of a spin wavefunction, or is physically allowable but is in the wrong phase. Focusing on the wavefunction is therefore not super solid — let's instead focus on the low energy theory.

Conjecture 1: Every low energy theory obtained from the slave-fermion construction is physical, in the sense that it correctly describes the low-energy excitations of *some* spin Hamiltonian.

For example, let H_{eff} be the Hamiltonian defined in the $\mathbb{Z}_2$ spin liquid example.

Conjecture 2: There exists a symmetry-preserving gapped interpolation between H_{eff} and a Hamiltonian of the form

$$H = H_{spin} - \sum_{\langle ij \rangle} X_{ij} \tag{41}$$

where H_{spin} is a gapped spin Hamiltonian (the second term kills all the gauge fields). So, although we're working in this weird big Hilbert space which seems unphysical, we can smoothly interpolate to a spin Hamiltonian + some ancilla qubits that host the gauge degrees of freedom but otherwise do nothing. This interpolation quasi-obviously exists in the limit where H is very large (gauge field is confined, and you can do perturbation theory in t_{ij} with $K = 0$ — can show that you get a X_{ij} term plus a Heisenberg-type Hamiltonian for the spins. What about when h is small (deconfined)? Argue that we must have the same structure for anomaly reasons — since the anomalies are the same, expect (?) there to be a smooth interpolation to weak coupling.

Note: Kitaev's spin liquid is an example where the parton construction in terms of Majoranas is exact. But somehow some of the majoranas act as gauge degrees of freedom themselves?

8-13-20 **Sung-Sik Lee: NFLs**

- If we have nonzero Landau parameters, can forward scattering spread the hot spots around to the whole FS?
- Also — why is the patch theory valid in the presence of $O(1)$-strength forward scattering interactions?

If we have a FS coupled to a $\mathbf{q} = 0$ OP, the fermions can interact with the boson while staying near the FS, regardless of where they are on the FS. This is a hot FS, and the qps are destroyed everywhere on the FS. If the bosonic OP has momentum $\mathbf{q}$, only get a few hot spots.

Example: Ising-nematic:

$$\int d^2k\, \varepsilon(\mathbf{k}) c^\dagger_{\mathbf{k}} c_{\mathbf{k}} - V \int d^2q\, \Delta_{\mathbf{q}} \Delta_{-\mathbf{q}}, \tag{42}$$

where

$$\Delta_{\mathbf{q}} = \int d^2k\, \cos(k_x - k_y) c^\dagger_{\mathbf{k}+\mathbf{q}} c_{\mathbf{k}} \tag{43}$$

Depending on V, we will either get a FL with a circular FS or one with nematic deformation, with broken C_4. At the critical point, focus on long wavelength fluctuations of $\Delta(\mathbf{x})$.

Note that $\exists$ NFLs without SCing instabilities — these are easier to understand.

Note that the size of the patches Λ_y sometimes enter — for example, they enter in the pairing process. We only have sharp locality in momentum space (i.e. a good patch theory) if there is no pairing instability. The patch theory only works only when we can take $\Lambda_y \to \infty$ without getting anything singular.

In relativistic QFT, quantum fluctuations are weak in the $N \to \infty$ limit, and the $1/N$ expansion is organized in a perturbative series. This doesn't happen with a FS: the theory with large N flavors of fermions behaves instead like a large N matrix model — unlike a large N vector model this is not perturbatively solvable, due to the proliferation of planar graphs. Each single-line loop in the diagrams contribute a factor of N, due to the large phase

space. Therefore even in the large N limit there are infinitely many diagrams at each loop order in the large N limit.

The second index for the matrix theory comes from the angle on the FS. Vector fermion with FS in 2+1D is approximately a matrix fermion in 1+1D:

$$f_j(k_x, k_y) \sim f_{j\theta}(k). \tag{44}$$

For a one-patch theory (FL on the edge of some bulk), we have perturbative control, however.

Another approach: dynamical tuning (e.g. mross + mcgreevy + liu + senthil). Replace the boson dispersion with $|q|^{1+\varepsilon}$ instead of $|q|^2$. This reduces the density of states for the bosonic mode if $\varepsilon < 1$. When $\varepsilon = 0$ the boson-fermion coupling becomes marginal. Also preserves all symmetries. However, it breaks locality. One consequence is that the kinetic term is not renormalized perturbatively: if η is the anomalous dimension,

$$q^{1+\varepsilon-\eta}\Lambda^\eta = q^{1+\varepsilon}(1 - \eta \ln q/\Lambda). \tag{45}$$

This gives non-local corrections which are UV divergent. But short-wavelength fluctuations can't create non-local terms, and so no renormalization of the kinetic term can occur during RG. Hence this only works for theories without an anomalous dimension.

Yet another approach: dim reg. Preserves locality and symmetries. However, ambiguous: do you tune the dimension of space or the dimension of the FS? Unfortunately the size of the FS enters as a scale and causes UV / IR mixing. Also if the dim of the FS is greater than 1, any two points on the FS have tangent vectors which are parallel. To get around this, you might try doing dim reg only on the physical space, not the FS dim. This is nice because the size of the FS doesn't enter as a scale. Consider e.g. setting the dimension of space to 3, but keeping the FS dimension at 1. This is a line node, with the fermions gapped out everywhere along a sphere except for its equator. Turns out $d = 5/2$ is the upper critical dimension, so difference between dimension and $5/2$ is the control parameter.

Example of controllable case: chiral metal (balents + fisher 96). Stack of quantum Hall layers creates a two-dimensional chiral FS. Fermi surface on the edge is a line running around a cycle of the BZ (only one half of the BZ is filled). The important point is that this is chiral, so that a 1-patch theory works. With gaussian scaling, the coupling between the fermions and the boson is relevant. There's an interaction-driven scaling where it is marginal, however.

Since loop corrections generate $\Sigma \sim \omega^{1/z}$, the $\psi^\dagger \omega \psi$ term is always irrelevant in comparison: hence use the $\omega^{1/z}$ term to control the scaling.

$$G^{-1}(k) = (k_x + k_y^2) g\left(\frac{|\omega|^{2/3}}{k_x + k_y^2}, \frac{k_x + k_y^2}{\Lambda}\right). \tag{46}$$

In this theory (but not in general), the theory is finite when $\Lambda \to \infty$ — no correction to interaction-driven scaling. The scaling function $g(x)$ cannot be computed perturbatively, but the exponents are exact.

8-14-20 **Frank Pollmann: MPS**

Full diagonalization — can go up to $\sim$ 20 sites. Spare methods (only interested in e.g. the ground state) let us get up to ~ 40 or so sites.

Aim: reduction of variables $d^L \to Ld\chi^2$:

$$\psi_{j_1,\ldots,j_L} \approx \sum_{\alpha_1,\ldots,\alpha_{L-1}} A^{j_1}_{\alpha_1} A^{j_2}_{\alpha_1,\alpha_2} \cdots A^{j_l}_{\alpha_L} \tag{47}$$

Schmidt decomposition: decompose a $\otimes$ state into a superposition of product states.

$$|\psi\rangle = \sum_\alpha \Lambda_\alpha |\alpha\rangle_A \otimes |\alpha\rangle_{B'} \qquad S = -\sum_\alpha \Lambda_\alpha^2 \log \Lambda_\alpha^2 \tag{48}$$

When we do an SVD decomposition for a "physical" low-energy state, we only need to keep a small number of terms. For example, doing an SVD of an image for e.g. the Golden gate bridge,[28] you can only keep like 1/100th of the singular values; using just these can reconstruct the image well.

How well you reproduction works depends on the context. For a Mondrian painting, you can get away with only using *one* singular value. Mondrian paintings are area law, more detailed pictures are area law.

Note that each matrix in the MPS is not unique: can conjugate all the As by $A \mapsto X^{-1}AX$. Can use this ambiguity to bring things into a canonical form. Canonical form is in terms of $\Gamma^j_{\alpha\beta}$ and $\Lambda_{\alpha\beta} = \delta_{\alpha\beta}\Lambda_\alpha$ — here the Γ tensors change basis into the Schmidt basis, while the diagonal Λ tensors keep track of the singular values. Therefore in this representation cutting the MPS gives us the Schmidt values.

[28] Here we're doing $|\psi\rangle = \sum_{i,j} C_{i,j} |i\rangle|j\rangle$, with $C_{i,j}$ the pixels in some image. Compressing this by throwing out some of the singular values is equivalent to throwing out some of the basis vectors in this sum, saving memory.

Throwing out singular values in the SVD works for compression, but this approach requires you to first have the full wavefunction before compressing — this is of course impossible for big quantum problems though.

DMRG: generalization of MPS to the space of linear operators. Idea is to variationally minimize the energy; find ground state iteratively.

8-14-20 **Matthew Fisher: hybrid quantum circuits**

- Why do fermi liquids have $S_A(L) \sim L^{d-1} \ln(L)$? Matthew says that the area law comes from the system in question being local in $\mathbb{R}$ space.

Quantum quench in e.g. Ising Hamiltonian: entanglement spreads ballistically. This is much faster than the energy, which spreads *diffusively* (since we have a Hamiltonian).

Local measurements induce disentanglement (e.g. for A and B sharing a qubit and doing projective measurements onto $\otimes$ states).

Two kinds of open quantum systems:

- system coupled to a bath. Initial pure density matrix becomes mixed. The environment measures the system, but the results are lost. Get decoherence and system evolves with Lindblad equations.
- System is monitored by an observed. Initial pure state is measured and stays pure — there's an observer which keeps track of measurements. Dynamics described in terms of quantum trajectories.

1-qubit measurements:

$$\rho \to \frac{P_\pm \rho P_\pm}{\mathrm{Tr}[P_\pm \rho P_\pm]}. \tag{49}$$

Canonical model: random 2-qubit unitaires and single qubit measurements with probability p.

Direct simulation is very hard. However, can use stabilizers to encode "code word" quantum states. Evolve these with Clifford unitaries, and make measurements using the Z-component of spin. Gottesman-Knill theorem: such circuits with 500 ish qubits can be simulated on a laptop.

$|\psi\rangle$ is a "codeword" state if "stabilized" by L independent communiting Pauli string operators; $g_j|\psi\rangle = |\psi\rangle$. e.g. $|\psi\rangle = |0, \dots, 0\rangle$ is stabilized by $g_j = Z_j$. The state $|\psi\rangle = |00\rangle + |11\rangle$ is stabilized by $g_1 = Z_1 Z_2, g_2 = X_1 X_2$.

Clifford unitaries take Pauli string operators to other Pauli string operators.[29] Therefore we can follow the dynamics of the L stabilizers: if ψ stabilized by g then $U\psi$ stabilized by $UgU^\dagger$.

If Z_j anticommutes with g_1 and commutes with the rest, then the stabilizers are modified under measurement by replacing g_1 with Z_j.

Denote number of stabilizers starting in A and ending in A, B, C as n_a, n_b, n_c: then entanglement is

$$S_A = \frac{n_b + n_c}{2} \ln(2). \tag{50}$$

In some sense, Bell pairs are divided into two Bell pairs.

All single qubit measurements taken from Pauli group:

$$|\psi\rangle \to \frac{P_\pm|\psi\rangle}{\sqrt{p_\pm}}, \qquad P_\pm = \frac{1}{2}(1 \pm Z). \tag{51}$$

Mutual info $I_{AB} = S_A + S_B - S_{AB}$ locates the transition, for A and B two antipodal regions on a ring. When in area law phase, expect to get essentially zero for the mutual information. For volume law also expect to get zero: the areas all cancel. But at the critical point, get something else. Peak in the mutual information as a function of p gets sharper as system size gets larger — indicates that the transition is in fact second order. Indeed, see a very good scaling collapse. Also get log scaling of S_A at criticality. This is what one gets in the *ground state* of a CFT in 1+1D. Interesting since there's no energy or anything here. One can show that the mutual info in a CFT on a ring only depends on the cross ratios.

The volume law phase is interesting too: looks like the entang. entropy actually goes as $S_A \sim b_p \log(L_A) + a_p L_A$, so that the log is sort of already there. Does not expect a background log in e.g. medium-energy eigenstates of local Hamiltonians.

Mapping to a stat mech model: use random Haar unitaries and generalized measurements. Can map this to a generalized Potts model on 1+1D spacetime.[30] Volume law phase is ordered phase; area law is the disordered phase.

What if we start in a maximally mixed state, and just measure the entropy of the full system? (again, keeping track of the measurement outcomes). Measurements "increase the number of stabilizers, thereby increasing the entropy".

[29] Conjugating a Pauli string by a generic unitary produces a sum of Pauli strings. Conjugating by Clifford unitaries sends a Pauli string to a single different Pauli strings.

[30] See Jian, you, vasseur, ludwig and bao, choi, altman.

The transition which occurs is a *purification transition*.[31] When $p < p_c \sim 0.16$, have a non-zero entropy density even after evolving for a time long in the system size. When $p > p_c$, evolve to a pure state. Claim is that the transition is the same — i.e. purification transition $\sim$ entanglement transition. Only difference is the initial states: maximally mixed here, and pure in Matthew's examples.

Shor's error correcting code has 9 physical qubits, 1 logical qubit, and 8 stabilizers. Code space is

$$|\widetilde{0}/\widetilde{1}\rangle \sim (|000\rangle \pm |111\rangle)^{\otimes 3}. \tag{52}$$

Code distance, i.e. 'shortest logical operator' is $d = 3$.

Challenges: to extract S_A (or any other observables) requires multiple copies of the same wavefunction $|\psi\rangle$. To reproduce $|\psi\rangle$ requires post-selection on $O(Lt)$ measurement outcomes — this is unfeasible. Apparently there are ways to try and get around this.

Future questions:

- Universality class of transition?
- role of randomness? (randomness in outcome of measurements, positions of measurements, etc. — can get rid of some of these without loosing the phenomenology)
- Higher dimensions?
- Feedback? (i.e. determine what unitaries to evolve with based on measurement outcomes)
- Phases of QECC's? Phases of quantum computers?
- other dynamical phases in monitored open systems?

8-18-20 **Ruben Verresen: intrinsically gapless topological phases**

Within this talk, "topological phase" is in the sense SPTs. Note that you can actually have stable SPTs even when the gap is closed.

Different question: are there SPT phases that can *only* exist if the system is gapless? Yes; "intrinsically gapless phase".

[31] Gullans and Huse.

Seiberg points out that if you have a local operator acting on the edge which takes you between different ground states, it's not correct to say that the edge has symmetry breaking.

Example of an anomaly (not an SPT):

$$H = \sum_n (Z_n - X_{n=1} Z_n X_{n+1}), \tag{53}$$

has a $\mathbb{Z}_2$ symmetry

$$U = \prod_n Z_n \prod_n \left(\frac{1 + i X_n X_{n+1}}{\sqrt{2}} \right). \tag{54}$$

Note that $U^2 = \mathbf{1}$ on all local operators. Let $\mathcal{S}_n$ be the half-infinite string of U (i.e. the product over all $k < n$ for some n. Can show that

$$U^2 \mathcal{S}_n (U^2)^\dagger = -\mathcal{S}_n. \tag{55}$$

Therefore U acts as $\mathbb{Z}_2$ on local operators but as $\mathbb{Z}_4$ on string operators. (Levin + Gu 2012) If you could gauge the symmetry, only the endpoint of $\mathcal{S}_n$ would be physical; hence it would become a local operator and U would have to act as a $\mathbb{Z}_2$ on it.

Gapless phases with onsite symmetries can exhibit two equivalent things:

- "impossible" string order parameters and edge modes (gapped phases can never host such string order parameters)
- emergent anomalies in the low-energy theory (total system not anomalous, but low energy theory is)

Ising-hubbard chain:

$$H = H_I + H_H, \tag{56}$$

where

$$H_I = \sum_n (J_z Z_n Z_{n+1} + h_x X_n), \tag{57}$$

H_H is the Hubbard Hamiltonian at chemical potential μ, and where the X, Zs are built from bilinears of electron operators. Have an ising symmetry

$$R_x = \exp\left(i\pi \sum_n X_n \right), \qquad R_x^2 = P = (-1)^F. \tag{58}$$

When U is large and we're at half-filling, for $J_z \gg h_x$ have AF ordering. LRO is lost when doped away from half-filling because holes mean a given site is just as likely to be $\uparrow$ as $\downarrow$. However there is still order, because if we get rid of all the holes we get an AFM. Therefore get order for the operator

$$\langle R \rangle = \langle ZP \cdots PZ \rangle \neq 0, \tag{59}$$

where P is fermion parity on a site, and makes up for the presence of holes. Have LRO in the above string order parameter.

Dope the trivial Mott insulator: get a trivial Luttinger liquid. Dope the Ising Mott insulator: get a topological Luttinger liquid with the above string operator.

However, the string operator is "impossible". The edge of $\mathcal{S}_n^P$ is a Z operator, which doesn't commute with the string operators themselves.

Fermion parity is a gapped symmetry, in that any operator charged under it is gapped. Low energy symmetry is therefore $\mathbb{Z}_4/\mathbb{Z}_2 = \mathbb{Z}_2$. Low energy remaining symmetry has an anomaly.

Note that this cannot be viewed as a gapless thing stacked on top of a gapped SPT (can't remove the anomaly by stacking with gapped things).

Field theory comes from two luttinger liquids; the low energy theory has an anomaly which is the same as the $\mathbb{Z}_2$ anomaly in the single-component luttinger liquid. Same field theory as the one for the $Z - XZX$ chain.

The general statement is that

$$Z[A] = Z_{low}[A_{low}] e^{2\pi i \int \omega(A)}. \tag{60}$$

This is an intrinsically gapless topological phase iff these two factors are individually not gauge invariant. Neither of these phase factors can be associated to a gapped SPT (you can't trivialize by stacking with gapped SPTs). The $\omega(A)$ cannot depend only on A_{low}.

Can argue that there have to be edge modes at the boundary of such a system (the gapped degrees of freedom have to come down to trivialize the system so that you can go into the vacuum). Stable as long as you don't bring the gapped dof down to be gapless.

2+1D example: deconfined quantum crit. by embedding $SO(3) \times U(1)$ in a fermionic Hilbert space (e.g. Grover / Senthil).

General prescription:

- take a D+1-dim SPT with some anomaly at edge
- add gapped dof to trivialize SPT, e.g. by adding fermions (more generally, do a symmetry extension)

- get an emergent anomaly at the edge — edge theory will itself have edge modes when realized as a D-diml system. The presence of these edge modes is required only because of the gapped degrees of freedom.

Robust as long as the added dof are kept gapped.

Why do the gapped dof even matter for the low energy theory? Essentially because the string operators see the symmetry associated with the gapped dof.

Emergent anomaly is not because we have an emergent symmetry group, but rather that we have an emergent quotient. Full symmetry not anomalous; quotient symmetry is.

The gapless dof at the edge carry some quantum number that can't be screened by the gapless stuff in the bulk, i.e. the gapless modes at the boundary are sharply well-defined. The symmetry breaking on the two edges is correlated by the bulk gapless dof — just like how in a CFT different types of boundary conditions can be split in energy. This is why we have a $\mathbb{Z}_2$ degeneracy at the edge rather than $\mathbb{Z}_4$.

This is kind of like a Weyl semimetal where translation symmetry has been replaced by something onsite.

8-24-20 **Senthil's group meeting: Dan on strain-induced pseudo-magnetic fields and Adrian on flat topological bands in Kagome materials**

Dan:

Periodic patterns develop when you cool graphene; formed in the regions between wrinkles. Forms a triangular superlattice of very large lattice constant — this rules out the pattern as being formed from Moire physics wrt the substrate.

Take a sheet of plastic and stretch it along x: this leads to a compression along y, which is releived by the materical buckling out into the z direction. In graphene aligned with certain substrates, you can get wrinkles along two linearly independent directions. These wrinkles can trap two-dimensional buckling patterns.

If strain is only at long wavelengths and is of small magnitude, we can encorporate it is as a perturbation around the K, K' points. Have

$$\sigma \cdot v_F \mathbf{p} \mapsto \sigma \cdot (v_F \mathbf{p} + \mathbf{A}),, \tag{61}$$

where

$$A_x - iA_y \propto (\partial_x + i\partial_y)(u_x + iu_y), \tag{62}$$

where u is the displacement vector. Note that $\mathbf{A}$ is odd under T. Claim is that in this situation you get a largeish pseudo-magnetic field coming from buckling patterns with height of 10ish angstroms.

Basic point: allows us to get a Chern band with non-uniform Berry curvature and a relatively flat band. Maybe interesting things will happen.

Adrian:

Material is CoSn. Famous paper on kagome flat bands: Bergman, Wu, Balents. Can construct operators

$$W^\dagger = \sum_{i \in \partial \hexagon} (-1)^i c_i^\dagger \tag{63}$$

which are eigenstates of the usual tight-binding model:

$$HW^\dagger|0\rangle = 2tW^\dagger|0\rangle. \tag{64}$$

The gound state is a superposition over $W_i^\dagger$ acting on $|0\rangle$ for all i. The sum actually vanishes because of the canceling signs. This means the $W_i^\dagger$ operators are not all linearly independent. Can check that the $(\pi, 0)$ states have inversion symmetry with eigenvalue -1. In fact at the Γ point you have $+1$ inversion eigenvalue, while you have -1 eigenvalues at all the M points on the BZ edge. Therefore as long as you can gap out the Γ point using e.g. SOC you expect to get some kind of quantum spin hall.

Complications: real kagomes are just nearest neighbor, they do not just have one orbital per site, they aren't isolated 2D layers, and they do not have completely real hopping elements with SOC.

Some materials:

- Fe_3Sn_2 (lin+ PRL 2018). Iron sites form the Kagome, while the tin sites sit out of plane and sit in the hexagon centers. The band structure looks like an absolute mess and Adrian is rather skeptical.

- FeSn (Kang+ comm. nat. mat. 19) Sees some flat band in ARPES which can't be explained with DFT.

- Change iron to colbalt: the one extra electron might help to suppress magnetism. Arpes done at 60K; DFT sees relatively flat bands within the 2D BZ. However, some of the bands have significant out-of-plane dispersion, so only really flat in the $k_z = 0$ plane. However this is much

better than any other material looked at so far. However the flat band is 0.2eV away from the Fermi level which I guess is troublingly large. Also hard to say that you have a band which is completely flat across the 2d BZ: need to have a way of tracking the band index across the BZ, which is hard since there are many overlapping bands.

Outlook for CoSn: need to get the flat band up to the fermi level. Can do this by substituting iron in for Cobalt: doable since they are very similar chemically (although the iron might add some unwanted magnetism). The reason why this works is that the number of electrons for iron is different, this then changes the number of electrons per unit cell.

The relevant thing to look at is not necessarily the magnitude of the energy difference between the flat band and E_F, but rather the number of bands between the flat band and where E_F is; this tells you how much you need to change the filling of electrons before you get the flat band up to E_F.

8-18-20 Max Metlitski: boundary criticality in the $O(N)$ model in $d = 3$ revisited

note: style is making lots of assumptions, but stating them carefully. Always do the easiest thing first, even if it requries making lots of assumptions.

Imagine we have a critical CFT defined on a manifold with boundary. In general, the BCFT is not unique (can exist multiple fixed points on the boundary with RG flows between them). Scaling dimensions on the boundary will generically be different from those in the bulk.

Modern interest in boundary CFTs: want to know what happens if you take an SPT and drive a phase transition in the bulk (e.g. grover + vishwanath 1206.1332).

Discuss classical $O(N)$ model:

$$\beta H = -\sum_{\langle ij \rangle} K_{ij} \mathbf{S}_i \cdot \mathbf{S}_j. \tag{65}$$

Consider model with a boundary, with the spins on the boundary having the coupling K_1.

Suppose $d > 3$. Phase transition as a function of $\kappa \equiv K_1/K$. If κ is small, boundary orders at the same temperature as the bulk ("ordinary boundary criticality"). However if κ is large enough, the surface will order before the bulk orders. There are then two phase transitions: first the boundary orders (regular transition in $d-1$ $O(N)$ model), and then the bulk orders in the

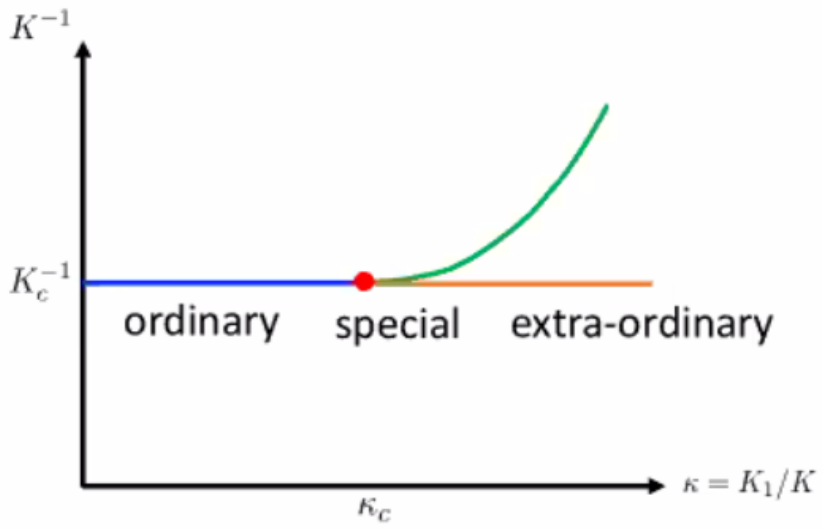
presence of an already ordered boundary (and the transition is different to than the ordinary one). Have an interesting tricritical point at κ_c. There is only one universality class if there is a boundary magnetic field. If the bulk has $d > 3$, adding a field on the boundary makes the transition equivalent to the extra-ordinary one.

Now look at $d = 3$. If bulk is ordered, by Mermin-Wagner cannot have long-range order on the boundary, only QLRO. Get KT-type transition on the boundary at $N = 2$.

Now look at $d = 3, N > 2$. Two possibilities: could just have an ordinary transition for all κ. Or, could also have ordinary for small κ and extra-ordinary at large κ. Max claims that get correlators like

$$\langle \mathbf{S}(x) \cdot \mathbf{S}(0) \rangle \sim \frac{1}{\log(x)^{q(N)}} \tag{66}$$

for small enough N. Can show that for $N = \infty$ in $d = 3$ there is only one fixed point.



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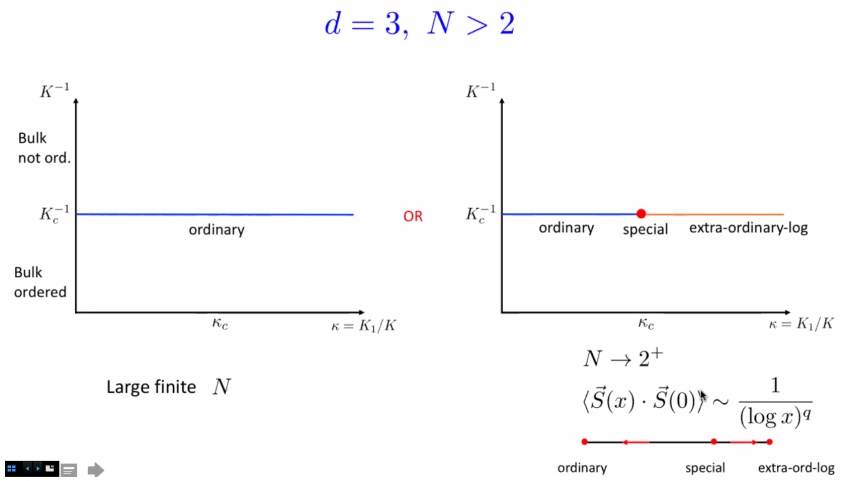
$d > 3$



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Sketch of RG in $d = 3$: suppose we are at large K_1. Describe the boundary with a NLSM for the boundary spin $\mathbf{n}$. Polyakov taught us that if we perturb about the ordered state, we have

$$d_t g = \frac{N-2}{2\pi} g^2, \tag{69}$$

where g is the coupling on the boundary. Coupling to bulk gives an extra contribution: (holds true for $N = 2$ as well)

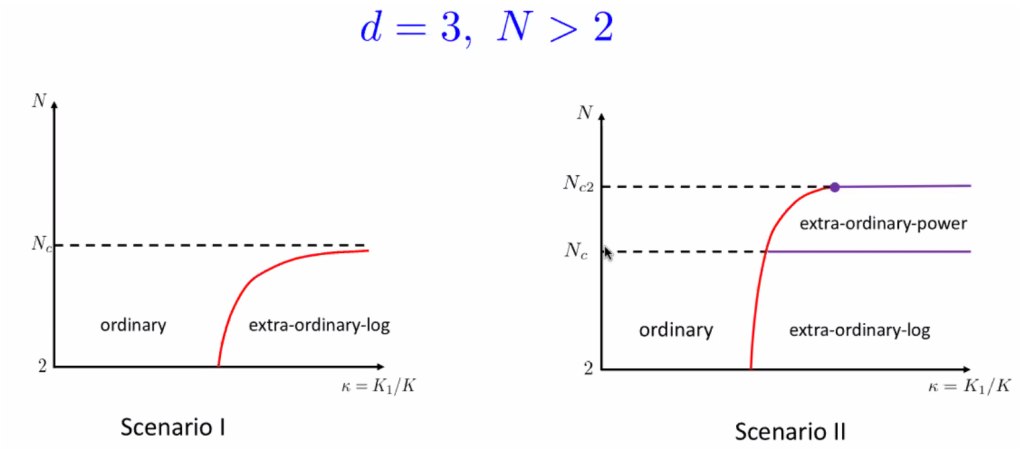
$$d_t g =_\alpha g^2 \equiv \left(\frac{N-2}{2\pi} - \frac{\pi}{2} s^2 \right) g^2, \tag{70}$$

where s is determined by some universal N-dependent number determined by the physics of the bulk fixed point. If $\alpha > 0$ then $g \to 0$ and you get the extraordinary log fixed point: $\mathbf{n}$ correlators go as a power of the log of their separation. If $\alpha < 0$ then unstable.

What is the sign of α? When $N = 2$, $\alpha > 0$ and get extraordinary log fixed point. When $N \to \infty$ can show that $\alpha < 0$. Hence there exists at least one N_c where α changes sign. Large N naively gives $N_c \approx 4$; bootstrap data gives $N_c \approx 3$.

What happens near N_c? Depends on details of bulk contribution to RG

which is unknown at present.



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9-2-20 **Subir Sachdev: cuprate theory review**

$\mathbb{R}$-space picture for doped Mott insulating ferromagnet. At small doping p, the number of charge carriers is clearly p. At large doping and thinking in momentum space, it's clear from band theory that instead the number of charge carriers is $1 + p$. Hence in the two limits, the number of carriers is either p or $1 + p \implies$ something interesting has to happen at intermediate p. Large FS regime: ARPES clearly shows that the density of carriers is $1 + p$. In the Fermi arc regime, if you imagine that the electrons occupy the Fermi pockets, the area is roughly consistent with having the density of carriers be $\sim p$.

Angle-dependent magnetoresistance allows you to map out the fermi surface (2004.01725). Find that in the low p regime, a large FS and an electron-pocket FS are both totally inconsistent with experiment. However, a FS reconstructed by a (π, π) wavevector *is* consistent with the data. Note: why don't we see the backside of the arcs in ARPES? Becuase ARPES response depends on the quasiparticle weight, while the magnetoresistance doesn't, so a consistent interpretation is that the qp weight on the backside of the pockets is very low.

We need to think about metal-metal phase transitions. One which is well understood is the metal-metal transition in the Kondo lattice.

$$H \sim J_K \sum_i (c_i^\dagger \boldsymbol{\sigma} c_i) \cdot \mathbf{S}_{fi}, \tag{72}$$

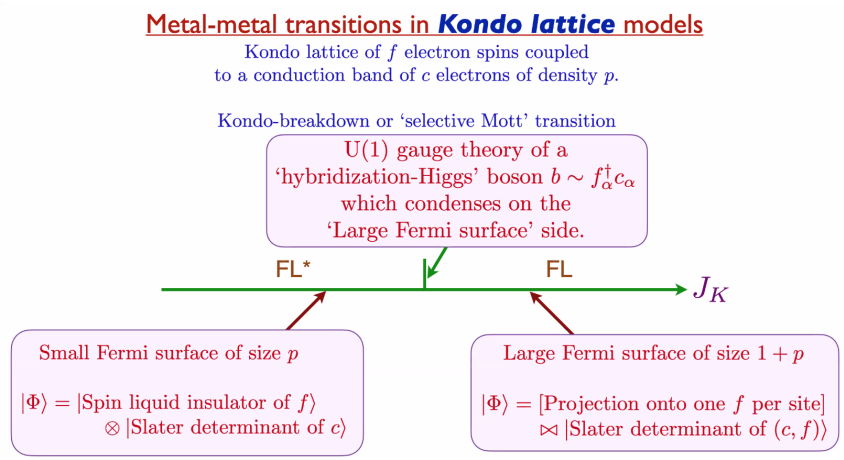
where the f electrons are localized, and the c operators are in the conduction band.

Kondo effect: J_K becomes larger and larger at lower temperatures, until eventually the localized moments become part of the conduction sea, since they get locked into the conduction electrons by strong entanglement. Thus the f electrons dissolve into the Fermi sea at large coupling; get a Fermi surface of size $1 + p$ (p from the conduction electrons, 1 from the dissolved local moments). In this phase the f electrons can venture out off their site since they get screened — hence in this phase

$$\langle c^\dagger f \rangle \neq 0. \tag{73}$$

Modern way of understanding this: Kondo lattice model has a gauge symmetry $f \to e^{i\theta} f$, since the f electrons appear in H only in the form $\mathbf{S}_{fi}$. Then in the large coupling regime where the local moments dissolve into the Fermi sea, the gauge field is Higgsed by virtue of the above expectation value.

When J_K is small, have 'Kondo-breakdown' (the f electrons are no longer screened by the c electrons, but rather by each other), or 'selective Mott' transition. In this phase the f electrons form an insulating Mott insulator: the f electrons go into a spin liquid (all pair up into singlets). Hence the Fermi surface now only has size p; it is called FL* and is allowed by Luttinger's theorem since you have topological order via the spin liquid for the f electrons. This is the deconfined phase of the gauge theory.



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Can this approach be applied to the cuprate problem? No:

- Only works for a particular spin liquid in the f band, viz. the spinon FS with a trivial PSG; this is likely not applicable to the square lattice.

- No natural extension to the case where the non-FL state has magnetic order.
- Most importantly: no simple extension to the one-band model (we had two separate bands in the Kondo problem; in the cuprates there is no such separation). With only one band, it's unnatural to say 'these electrons are localized and these electrons are not'; hence changing charge density from p to $1+p$ can't be done naturally.

There is a way of getting an FL* phase in a one-band model though: dope a spin liquid in which spinons and holons (not holes; they don't have spin) bind to form 'electrons', which form a small FS (this is the Wen-Lee proposal). This is only able to be concretely described in the 'BEC limit', where the holons and spinons get bound together (but this is not sufficient — what we need is the analogue of the BCS theory, which treats pairing without assuming that the pairs are in the BEC limit; rather they are large and mutually overlapping). This approach also doesn't yield a theory of the transition to the FL state.

Yahui's ancilla model: two layers of ancillas. In large FS phase, the two ancillas are just in singlets, decoupled from physical layer. In small FS phase, the physical layer and one hidden layer couple and hybridize as in the Kondo screening phase.

Problem with the ancilla model: predicts ghost fermions which contribute to κ but not σ. Hence predicts a violation of the WF law. This is a problem because 1805.04589 shows that the WF law is obeyed across all dopings, with the normal-state thermal conductivity jumping at p_* together with the conductivity.

9-22-20 Hart Goldman: dirty quantum critical points

Dirty boson problem: ϕ^4 boson with a mass $\mathcal{L}_{dis} = R(\mathbf{x})|\phi|^2$ (disorder only in space), with $R(\mathbf{x})$ having zero mean and $\Delta\delta(\mathbf{x})$ Gaussian correlations.

Disorder and interactions have different upper critical dimensions, which we'll always be below. Might be relevant in some intermediate glassy phase between superfluid and mott insulator in presence of disorder.

Initial approach: double ε expansion in both space and time dimensions so that $(\varepsilon, \varepsilon_t) = (0, 0)$ gives you upper critical dimension for both disorder and interactions. Problem with this is that disorder and interactions have same scaling value and so they want to 'mix' and go off into the $\mathbb{C}$ plane, giving $\mathbb{C}$ RG eigenvalues.

Better starting point: start directly from WF fixed point, and take into account η_{ϕ^2} when figuring out the Harris criterion. Turns out that disorder is marginal at the WF fixed point as $N \to \infty$.

Decouple ϕ^4 with HS field σ, then do replica trick and integrate out disorder. This produces $\int d^2x\, d\tau\, d\tau'\, \sigma(\tau)\sigma(\tau')$ type of couplings. Turns out that the disorder is screened non-perturbatively as $N \to \infty$. Large N action is

$$\mathcal{L} \sim \sigma \frac{1}{\sqrt{-\partial^2}} \sigma + \int d\tau'\, \sigma(\tau)\sigma(\tau'). \tag{75}$$

9-24-20 **Ashvin: skyrmions in TBG**

The band / sublattice dof in TBG is crucial — many other Moire materials (e.g. twisted double bilayer graphene) don't have this, and it only takes 4 electrons to fill up the flat bands.

$U(1)_v$ symmetry comes from graphene lattice translation being realized as an internal symmetry at the scale of the mini BZ.

The $K_\pm$ points of the mini BZ for each valley index descend from the same K point of monolayer graphene — so they have the same chirality, unlike in monolayer graphene. Scale for magic angle set as

$$1 \sim \frac{w_{0,1}}{2 v_F k_D \sin(\theta/2)}, \tag{76}$$

where k_D is the distance between the $K_\pm$ points in the mini BZ and $w_{0,1}$ is the tunneling strength. Thus magic angle when time for tunneling in between sheets $\sim$ time to traverse the Moire pattern.

Wavefunctions, not just eigenvalues, are very important for this problem. Replace the flat bands with some other types of wavefunctions, things change.

Tight binding doesn't work because of fragile topology — not as strong as an obstruction due to a nonzero Chern number, but similar.

The parameters $w_{0,1}$ are hopping strengths between the layers which are diagonal / off-diagonal in the graphene sublattice index. The fact that they aren't equal is due to the AB regions being energetically favorable. Chiral model just has off-diagonal hopping: then if σ^z is Z in graphene sublattice space, have $\{\sigma^z \otimes \mathbf{1}, H\} = 0$. The single particle Hamiltonian for each valley produces nearly flat bands. Linear combinations of these flat bands are both a) sublattice-localized and b) carry $C = \pm 1$.

With just graphene: strain acts as a magnetic field pointing in opposite directions for the two valleys. TBG is like two copies of this.

Usually the density operator does not split as a sum over band indices; in fact usually it's a very off-diagonal mess. But it turns out that in the chiral limit one finds

$$\rho \approx \rho_{C=+1} + \rho_{C=-1}. \tag{77}$$

Not allowed to mix the wavefunctions in the two Chern sectors into one another: the density operator isn't invariant under this and hence the flat-band Hamiltonian isn't either.

Provable ground state at $\nu = 2$ in chiral flat band limit: put half the electrons in $C = +1$, half in $C = -1$. Ground state since these states are all annihilated by $\delta\rho_q = \rho_q - \bar{\rho}_q$. This is because the density operators respect Chern number. Then acting on these states with $\delta\rho_q$ takes out an electron from a given band and adds an electron at a different momentum to the same band; since the bands are filled this then vanishes by Pauli exclusion.

Which ordered state is selected out? A CDW would be $\Delta_R\tau^x + \Delta_I\tau^y$, with τ^i acting in valley space. This turns out to be ruled out. Instead of just having the electrons living on different sites, you want the hopping to be modified as well. If you change the amplitude of the hopping you get T-IVC order, viz. $\sigma^x(\Delta_R\tau^x + \Delta_I\tau^y)$, while changing the phase gives K-IVC order, viz. $\sigma^y(\Delta_R\tau^x + \Delta_I\tau^y)$.

Dispersion gives an AF coupling between two Chern sectors, essentially by momentum-space superexchange. Favors KIVC. Going away from chiral limit also favors KIVC.

Get a pattern of circulating currents, but these circulating currents are *not* really tied to the graphene lattice.

Note that some experiments see superconductors but not proximate insulators — this doesn't rule out this skyrmion idea: this idea just needs order, it doesn't need insulators.

9-25-20 **Andrea young: pomeranchuk effect and orbital magnetism**

Align with hBN; generate a big gap between the sublattices. Then the insulator at $\nu = 0$ gets very strong and the stuff at $\nu = \pm 2$ goes away. At $\nu = 3$ find an anomalous Hall magnet. This is because you have four $C = \pm 1$ bands, and thinking about quantum Hall ferromagnetism, the natural expectation is that removing one electron will give you a net Chern number of ± 1. This kind of magnetism comes completely from the valleys, i.e. it is purely orbital, not magnetic (very little SOC).

In Andrea's experiments, magnetization density in the above systems is much larger than $1\mu_B$/Moire cell. Therefore the magnetism is *not* coming

from spins — it's mostly coming from orbital components.

◇ ◇ ◇

In pure TBG, generally see no orbital magnetism / QAH / anything between $-2 < \nu < 2$. But: orbital insulators are not far away in energy: adding a magnetic field can bring them out.

In andrea's experiments, adding a magnetic field induces a transition to a new state *independent* of the direction of the field. Therefore the transition is due to the Zeeman energy and is presumably just into a spin+valley polarized phase.

Look at the h transition when $T = 0$: get $\Delta E = E_Z$. Look at the T-transition when $h = 0$: get $\Delta S T_c = \Delta E$. Therefore

$$\Delta S \approx \frac{E_Z}{T_c}. \tag{78}$$

Why is the linear resistivity everywhere? Basically because ferromagnetism is very close by at all places in the phase diagram.

Phase transition into this new phase in the field / at high T does *not* occur at $\nu = 1$; in fact the exact filling at which it occurs is rather complicated to figure out. However, when you add a field out of plane, the Landau levels which develop are associated with $\nu = 1$; i.e. the Landau fans all emanate from $\nu = 1$.

Andrea's intuition: it happened to be that in their device near $\nu = -1$, they saw a transition between distinct phases upon increasing the temperature / field: goes from unpolarized phase at low T to a fluctuating but polarized phase at high T. But if you do the same experiment near $\nu = -3$, you don't see anything: it just disorders. You don't necessarily change from one type of isospin order to another type of order which can fluctuate more and hence is entropically favorable.

Why doesn't this happen at other filling fractions? Because at e.g. $\nu = 2.5$ the ground state is already the broken symmetry state, unlike at $\nu = 1$. But this is just details, and will change from sample to sample.

At 40K you have a feature in the resistivity that is just as big / on equal footing to the one which happens at $\nu = 2$. Hence the mechanism for symmetry breaking is likely the same; what happens at $T = 0$ is just details.

9-28-20 **Senthil: band topology meets strong correlations**

Main conceptual problem of TBG: interacting electrons in partially filled topological band.

Typical correlated solid: interaction strength $\ll$ gap to remote bands. Project interaction to active band to obtain effective model in Bloch basis. Often turn this into a lattice model by going from the Bloch to Wannier basis; then get tight-binding model with interactions. Fails for topological bands: in k space there is no global gauge for the wavefunctions, hence they are not smooth, hence they are not localizable in $\mathbb{R}$ space.

How to deal with a correlated Chern band which is *not* a Landau level is laregly an open problem; this is precisely what we need to know for TBG. Can we approach things from the perspective of Landau levels?

Differences between Moire bands and LLs: Moire bands are time reversal symmetric (roughly two coupled Landau levels), don't have uniform berry curvature, and bandwidth isn't zero. More basic problem is that dealing analytically with a model restricted to a single Landau level is hard.

Bosons at $\nu = 1$ can also form a composite fermi liquid. EFT is still FS + $u(1)$ gauge field + CS term. HLR not well-suited to LLL projection, though. Mean field effective mass is bare electron mass — LLL is obtained by taking $\omega_c \to \infty$, but then $m \to 0$; what happens?

Basic problem: projected density operators satisfy GMP algebra:

$$[\rho(\mathbf{q}), \rho(\mathbf{q}')] = 2i \sin\left(\frac{(\mathbf{q}\times\mathbf{q}')l_B^2}{2}\right)\rho(\mathbf{q}+\mathbf{q}'). \tag{79}$$

Together with $H = \frac{1}{2}\int U(\mathbf{q})\rho(\mathbf{q})\rho(-\mathbf{q})$ completely defines the LLL quantum Hall problem. No parameter to perturb in. ethan: *If it's just interactions, how do we couple to background gauge fields?*

Pasquier-Haldane approach: representation in terms of composite fermions; get gauge redundancy. Constraint implemented by operators which themselves satisfy the GMP algebra (but with opposite sign). Nick read: W_∞ gauge theory broken to $U(1)$ by mean field composite FL solution.

Represent states in many body boson $\mathcal{H}$ by

$$|m_1, \ldots, m_N\rangle = \varepsilon^{n_1 n_2 \ldots n_N} c^\dagger_{n_1 m_1} \ldots c^\dagger_{n_N m_N}|0\rangle. \tag{80}$$

Have two density operators $\rho_{L/R}$ which satisfy the (anti)GMP algebra. Constraint is $\rho_R(\mathbf{q})\mathcal{H}_{phys} = 0$.

Way to go beyond mean field: best approach is

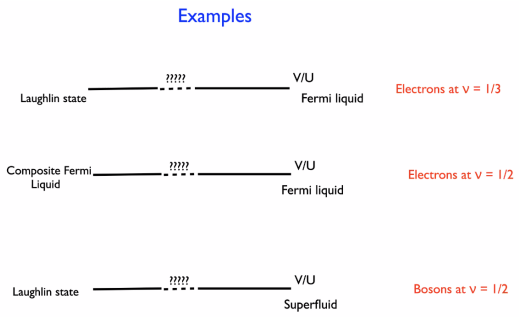
$$c(\mathbf{R}, \tau) = \int \frac{d^2k}{(2\pi)^2} e^{i\mathbf{k}\cdot\mathbf{R}} c_{\mathbf{k},\tau}, \tag{81}$$

where $\mathbf{R}$ is a translation operator in the LLL (non-commutative!). Things become simple in $\mathbb{R}$ space so long as we use star products for every product. Can either use non-commutative coordinates with regular product, or regular coordinates with $*$ product.

Assume gauge fields (but not matter) varies slowly on the scale of the magnetic length.

Introducing bandwidth: square lattice with flux through unit cell being 2π. Get LLL + periodic potential + interactions. Still have GMP algebra.

Want to know these examples:



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Spinful bosons/fermions at $\nu = 1$ is an interesting problem. For small kinetic energy, get a QAH insulator. Large kinetic energy, get a Fermi liquid. How do these evolve into one another?

9-30-20 Patrick Lee: imagining spinons in a QSL

Can have Kondo effect with a spinon FS: doesn't matter if the FS is electronic or spinonic, can still screen the local moments.

Back in the 70s: 1T-TaS_2 a layered TMD; well known that you have star-of-david charge ordering below 180K (stars form a triangular lattice pattern). Only 2D CDW with an insulating ground state (others become better metals because when you make a CDW you remove some Fermi surface and hence have less scattering); since number of electrons per (star of david) unit cell is odd, must be a Mott insulator. Problem in bulk system: strong interlayer coupling can give you a unit cell with an even number of electrons, which would give a trivial band insulator. In multilayer systems maybe some layers form MIs and others pair to form band insulators. See $\kappa \propto T$ but coefficient is very small.

Hubbard periodic-anderson model: hopping to the metal, onsite U, and hopping between the orbitals. Kondo: singlets between spins and conduction

electrons. Spin liquid: singlets between spins. There's a competition here. Normal Kondo: have flat band. Spin liquid: already have some dispersion.

For Kondo lattice, should see *two* peaks; the single tunneling peak of the single-site Kondo problem gets split in two. Temperature scale of splitting should be the same as T_K, which in this system is 50ish K. So there's a problem in the experiment.

Would be great to grow this on bN so that you can gate it; other parameters are hard to tune.

Don't see $2k_F$ singularities, but do see singularities at sums of two $2k_F$ vectors. Maybe there's some $2k_F$ order? Can't be charge order since STM would've seen it. Spin density wave or spinon pair density wave?

NV centers can be used to sense magnetic field environment? May be able to see potential SDW order?

10-2-20 **Seth: spin liquids II**

Kitaev model: flux operators commute with Hamtiltonian. Moreover, the operator which moves one flux around another is equivalent to the operator which counts the former's magnetic flux; hence the fluxes are mutual semions.

Split the spins into four Majoranas: one lives at the vertex center, while the other three live halfway down each of the legs. Can work in constrained Hilbert space where the quartic terms reduce to simple majorana bilinears on links.

Adding a magnetic field gaps out the gapless Majorana bands; they turn out to have $C = \pm 1$ (calculation of the Chern number only depends on wavefunctions, not on Hilbert space).

Turns out generically get a stripey kind of spin order as $T \to 0$ in experiments. People had the idea to add a polarizing magnetic field to kill the order; apparently for medium strength fields there can be an intermediate regime where you still get a spin liquid. Matsuda claimed to see the 1/2 thermal hall conductance but PN Ong did the experiment and found magneto oscillations in κ; this suggests some sort of neutral FS.

10-13-20 **Yahui: fractional fermi liquids**

Two scenarios for the PG: fluctuating symmetry breaking order, or fractional metal with deconfined gauge field. Main problem: not sharply distinguished at finite temperature.

Conventional tJ model. numerics:

- 1D: conventional Luttinger liquid
- fermi liquid; superconductor; no evidence for exotic metal, even on frustrated lattice — seems that doped Mott insulator is conventional. Gauge field always higgsed at $T = 0$ because of Bose condensation; hence ground state is conventional.

Goal: suppress slave boson condensation. Can try to dope with doublons, i.e. spin triplets. Hilbert space is 5 dimensional: $\uparrow, \downarrow$, and then x, y, z — the doublon gets three states since its a triplet.

When doing partons, can assign arbitrary fractional charge under non-dynamical background fields; quantization doesn't matter for background fields.

Ancilla qubit: not fractionalizing a physical operator—fractionalizing the vacuum!

10-15-20 **Louis taiffalier: more cuprates**

ADMR: basically poor man's quantum oscillations. You're not changing the strength of the field; rather you're changing its angle. Although PL points out: the experiments are done at a point where electrons are not actually enclosing a full orbit, i.e. $\omega_c \tau < 1$: the scattering time is too short to have the electrons move around the FS. So really ADMR is (in this case) just a local probe of v_F.

To reproduce ADMR data in Nd-LSCO, need a particularly-shaped Fermi surface, as well as a scattering rate $1/\tau$ which is dependent on angle. The angle-dependent part is essentially independent of T; the isotropic part is T-linear. Can basically perfectly reproduce the data.

Fermi surface in Nd-LSCO is almost square—gives Vh singularities near the square corners. For TI2201 have $1/\tau_{inelastic}$ which is very anisotropic; for Nd-LSCO the inelastic part of the scattering rate is isotropic. (elastic and inelastic parts are separated out by looking at the temperature dependence—inelastic part is T-indep (?)).

Other examples of $\ln(g)$ critical points: iron-pnictide SCs, e.g. $BaFe_2(As_{1-x}P_x)_2$. Find $C/T \sim m^* \sim -\ln|g - g_c|$ (where here g is doping). Someone chimes in to say that the coefficient of the log in C/T is same as the coefficient in linear T resistivity. PL chimes in to say that there could be subtleties related to the Vh singularity, but LT says ADMR is so good that you can really accurately model the Vh singularity.

Present measurements: $\kappa_{ky}/T \sim \sigma_{xy}$ above p_*. But below p_*, find that κ_{xy}/T goes *negative* at low T, while σ_{xy} stays positive. Find that drop in κ_{xy}/T is still there and very large even at very small p_* where the system is an insulator. Thus this κ_{xy} can't come from electrons or magnons — maybe from phonons?

Problem with universal dissipation: sometimes get m^* from specific heat C/T, which itself is T-dependent — hence m^* is undefined. Things are better if you can get m^* from q oscillations or ADMR.

PL points out: don't actually need quasiparticles (in the Landau sense of inverse lifetime vs energy) for the Boltzmann equation to work. This just requires the self-energy to be frequency dependent but not momentum dependent. This means that quantum oscillations (derived using the Boltzmann equation, not by assuming quasiparticles) can exist even when the system is very non-quasiparticle, e.g. when we have this amazing $\rho \sim T$ from 40mK to 40K.

10-19-20 **Dominic: ersatz FLs as strange metals**

Experimental signatures of T or R breaking orders are rather controversial. In some cuprates the orbital currents (which are at the scale of a single unit cell) cancel and are hard to observe at larger distances, while in others there is predicted to be a net moment. In this case one can perhaps make progress by doing muon spin resonance or similar. But—can have diverging susceptibility without any ordering. Goal would be to directly measure the susceptibility at $T > T_c$ (anywhere in the linear-in-T regime is fine).

One way of measuring inversion symmetry breaking is to look for a nonlinear optical response, i.e. E^2 terms in the conductivity (in frequency space this comes from a response at 2ω)—if they exist there is inversion or T breaking. However, one can also have $E \cdot \nabla E$ terms, which are normally small but apparently are large in some cuprates.

10-19-20 **Dimitri Efetov: TBG**

Screening properties of the graphene back gate are a function which crosses over when the thickness of the hBN insulating sandwich layer is the same as the size of the Moire unit cell.

Graphene is not sheets of paper — it's more like saran wrap. Very hard to get the krinkles out.

Sees Chern insulators extending down to almost zero field.

Is neutrality insulating? In Pablo's group they always see it be metallic, which is a problem for Ashvin + Bernevig theory.

10-28-20 **Patrick Lee: cuprates**

Moderate pair fluctuation above T_c (from transport) but *robust* pairing (much!) above T_c (interlayer Josephson plasmon, diamagnetism, etc.).

Is PG due to charge order? Claims that this is unlikely. Unless we want to get really exotic, PG should come from some kind of fluctuating pair density wave. Should this be d-wave pairing or what?

PDW is nice since it naturally gaps out part of the FS, explaining the Fermi arc and pseudo-gap. Have strong PDW fluctuations, but weak dSC fluctuations.

Note: pseudo-gap actually predicted by RVB mean-field — this shouldn't be forgotten.

However, spin-charge separation fails at low T since bosons at finite density basically always condense — low T states within RVB mean field are all conventional.

Focus on the *low-energy* physics, where the spinons and holons are recombined. Still think of antinodal gap as a spinon gap, but work with electrons.

In the high-doping FL, have $\sigma \sim p\tau/m$, but $1/R_H \sim 1+p$. Is this consistent? Yes, because of Landau parameters: $j = ev/(1+F_{1s})$. Say then that $1/(1+F_{1s}) = p$. Find $\sigma_{xx} \sim 1/(1+F_{1s})$ but $\sigma_{xy} \sim 1/(1+F_{1s})^2$ (current appears squared in expression for σ_{xy}). Therefore conductivity can *vary smoothly* across p_*, even though R_H jumps across p_*.

How to satisfy Luttinger for $p < p_*$? Break translation, have ghost spectral weight, volume of p, or have an FL* with volume of p and pockets. Latter requires ghosts and topological stuff. Challenge for theories with ghosts: they still contribute to specific heat and stuff, and hence should in principle be visible in experiments.

Problems with CDW interpretation: gap exists at fixed momentum. (gap for CDW always comes down from above, but not what is seen in arpes). Conventional pairing also doesn't work for some reason.

Radical idea: try pairing on the *same side* of the Fermi surface. Amperian pairing: particles moving in the same direction attract by exchanging gauge magnetic field. Since the fine structure constant for the emergent gauge field is of order 1, the magnetic and electric couplings will be similar, and the Amerian attraction will be large. Pair carries nonzero total momentum. Distinct from

FFLO since FFLO still pairs electrons on opposite sides of FS (and has pairs with relatively small total momentum). Unlike BCS, PDW naturally gaps out only part of the FS.

Motivation for PDW can come from CDW, which naturally gives you nodes in the pairing gap. Unlike BCS, CDW has intrinsic particle-hole asymmetry. Also predicts a Fermi pocket whose backside is hole-like, and hence cannot be seen in ARPES.

11-12-20 **Chandra Varma: cuprate stuff**

Look at the magnitude of the free energy at various different points in the phase diagram. The AF free energy is ~ 10 to ~ 100 times larger than that of the superconductor. The PG is roughly of order of the SCing gap, while the other orders (charge density wave, spin glass, etc.) are 10 to 100 times smaller than the SC. So the idea is that these other orders can be ignored.

Getting the resistivity right essentially requires a spectrum which is constant in ω for $\omega > T$ and mostly independent of momentum.

For chemistry reasons Chandra claims that one needs to account for three orbitals (d, p_x, p_y) per unit cell — trying to reduce to a one-band model means you assume the difference between Cu and O energies is small; Chandra thinks this is not true.

11-20-20 **Senthil: EFLs**

Residual resistivity looks like it is zero in nice samples; hence disorder can't really be playing a big role in the cuprates. Also there's this old irradiation experiment.

Is the critical point compressible? Likely so, since taiffailer's group showed that there's at least one material where you can tune the critical doping with pressure.

Do lattice translations in the UV act internally in the IR? Maybe not if the IR involves spatial coarse-graining.

$LU(1) = G_{IR}$. Both translation and $U(1)$ charge conservation get embedded into $LU(1)$. So the anomaly is to be thought of as a self-anomaly for $LU(1)$.

The Hamiltonian need not be local in the θ direction (and in the FL it isn't), but the claim is that the kinematics (anomaly polynomial and stuff) is indeed local.

To mix with the current, you need T odd, vector under rotations, etc.

Current order is controversial experimentally, and not clear why it helps stuff like the opening of the PG. Here we have a totally different rationale.

Schamit: couple FS to x. This will generically break the infinite dimensional symmetry group. But the claim is that the endpoint of the RG flow will still have an infinite dim symmetry group (although the charges for the new IR symmetry will be different from those in the UV theory).

12-1-20 **Sid something something: entanglement dynamics stuff**

Rydberg atoms: two consecutive up-spins interact with a VdW potential going as n^{11}, where n is the principal quantum number. For Rydberg atoms, this energy is very high. So for all intents and purposes one can project onto the state where there are no consecutive up-spins. Initial states and Hamiltonians can be boring, but with strong constraints like this you can get interesting dynamics.

Entanglement spectrum of ρ_A for a bipartition $A \cup B$ determines all other bipartite entanglement quantities.

Entanglement spectrum of random pure state: write $|\psi\rangle$ as a matrix (with indices in A and B), so that

$$\rho_A = \psi\psi^\dagger. \tag{83}$$

If ψ is a random matrix, then the entanglement spectrum (the spectrum of $\rho_A \ln \rho_A$) can be shown to behave as $\sqrt{\varepsilon}$, where ε is the eigenvalue (Schmidt value). This square root singularity might be a universal property of ergodic systems. But: can change in constrained systems.

How to distinguish between a random pure state and an infinite-temperature mixed state? Unless you can measure things greater than half the system size, it's essentially impossible to tell (Page). This changes in constrained systems.

How does the Hilbert space grow as you add sites in a constrained system? For the Rydberg blocked chain, the H. space dimension turns out to grow as L^ϕ, with ϕ the Golden ratio. Same as Fibbonacci chain!

For a small relative H. space dimension, the entanglement spectrum is gapped — there are lots of zeros in the reduced density matrix. For relative dimension greater than 1, one gets the $\sqrt{\varepsilon}$ law. There is a critical point at 1 where the scaling is as $\varepsilon^{2/3}$.

This will hold in generic highly excited states, regardless of the choice of Hamiltonian.

12-4-20 **Zhihuan — DMRG**

Quantum monte carlo: nice, and the fact that you can do things in parallel means that you can get large ($\sim 50 \times 50$ sites) systems.

Both exact diagonalization and QMC dodge the problem of having a large Hilbert space: exact diagonalization you just don't worry about it, while with QMC you take a smart sampling of the Hilbert space.

For DMRG, we also try to focus on a particular low-energy subspace (determined by some ansatz).

iDMRG: start from a small system, and grow it using RG. Eventually converges into an infinitely long chain.

Small blocks $\rightarrow$ add dof $\rightarrow$ discard nonuniversal stuff and reduce $\mathcal{H}$ space dimension. The initial steps are all done exactly. So you don't start reducing the $\mathcal{H}$ dimension until you get to moderately large systems where you already have some universal information in the ground state wavefunction.

Truncation: goal is to maximize the fidelity of the reduced density matrix.

Decay of schmidt eigenvalues: goes as a power law in 1+1D, but becomes *constant* in 2+1D.

1-19-21 **Shubhayu — DMRG on bilayer QH systems**

Binding of skyrmions, and the fact that the skrymion pairs are energetically favored over single electrons, can be argued analytically at zero doping. To see what happens at finite doping, numerics are required — there are many other competing orders that you have to worry about. E.g. generic expectation is that at any finite doping you get a Wigner crystal (at least for unscreened Coulomb interactions).

Skyrmion size and mass are both affected by density of skyrmions. Nonuniform berry curvature leads the skyrmion energy to depend on its size, and increases the spin stiffness.

ρ_s increases with C^2 — hence people usually don't think about skyrmion pheno in any quantum hall systems with $C > 1$ (including stuff not in the LLL).

DMRG: biases towards low entangled states. FL has very large entanglement, much larger than e.g. a SC or CDW. So DMRG biased against these states.

Best definition of effective Skyrmion mass just comes from $\rho_{SF} = e^2/m$.

Note that $\rho_s \sim 1$mev means that $8\pi\rho_s \sim 250$K!

1-28-21 **Chong — stiefel liquids**

examples: non-coplanar magnetic order competing with valence bond

definition: quantum criticality with WZW terms on $SO(N)/SO(4)$.

Normal WZW term in 2+1D for DQCP: well-defined since $\pi_4(S^4) = \mathbb{Z}$ and $\pi_{n<4}(S^4) = 0$.

Note: for well-definedness of WZW, do we need π_{d+1} or H_{d+1}?

In 2+1d, the weak-coupling fixed point for the sigma model is stable. This is unlike what happens in 1+1d. Hope for regular DCQC is that there's another attractive strong-coupling fixed point on the other side of the conventional symmetry-breaking fixed point.

Renormalizability: non-renormalizable doesn't mean not useful. But if both the IR and UV are strongly coupled, then we basically can't do anything. QED and gravity are both non-renormalizable, but they are weakly coupled in the IR, and hence we don't care. But for the sigma model it is non-renormalizable and also strongly couped in the IR. (note: non-renormalizable $= \not\exists$ UV fixed point?)

Competition between coplanar magnet and VBS: Dirac spin liquid. Four Dirac cones: two from spin, two from band structure. Coupled to dynamical $U(1)$ gauge field. $N_f = 4$ QED.

$N_f = 4$ QED_3: add chiral symm-breaking mass field: $mP_{\alpha\beta}\bar{\psi}_\alpha\psi_\beta$. Then

$$P \in U(4)/(U(2) \times U(2)) = SO(6)/(SO(4) \times SO(2)). \tag{84}$$

Get a sigma model in this target space after integrating out the fermions. Looks like we can then get a sigma model on $SO(6)/SO(4)$, coupled to a $U(1)$ gauge field. This is basically just another way of saying that we can do a parton decomposition on the field P.

General idea: sigma model on one manifold with skyrmions forbidden by hand $\rightarrow$ different (simpler) sigma model.

Most obvious worry: the repulsive symm-breaking fixed point and the putative attractive strong-coupling fixed point could collide (as in deconfined criticality). Apparently can show that the symm-breaking FP moves towards small coupling at $N \rightarrow \infty$, but that the two FPs stay well-separated in this limit. (the fact that the repulsive FP goes towards weak coupling isn't that useful since it's a matrix theory and you need to sum over all planar diagrams which is hard)

Dirac spin liquid + single monopole $\rightarrow$ regular deconfined QCP. Claim is that this is part of the Stiefel liquid cascade.

$$S_{LSM} = \pi \int (w_2^{SO(3)} + t^2) \cup (xy + c^2 + r(x + c)), \tag{85}$$

where t is time reversal, x, y are lattice rotations, c is inversion, and r is reflection. First term: spins have spin 1/2, and are kramers doublets. Second term: where the spins live. e.g. xy says that each unit cell has one spin 1/2.

A lattice relaization means a homom:

$$\phi : G_{UV} \to G_{IR}. \tag{86}$$

Anomaly matching:

$$w(G_{UV}) = \phi^* w(G_{IR}). \tag{87}$$

Previous action written above should be viewed as $w(G_{UV})$. The IR anomaly would be just in terms of the sigma model fields and stuff.

3-1-21 **Brian Skinner — Semarium Hexaboride: netural fermions?**

Band structure: d band and f band meet at the chemical potential, but one is hole like and the other is particle like. So have a mixed valence metal.

Experiments: metal at high T, then goes insulating, but then the resistance saturates at low temperature. The Hall coefficient $R_H = 1/ne$ also saturates at low temperature.

Coleman: actually get a topological Kondo insulator. Kondo coupling between f and d electrons: this opens up a gap between the f and d bands, but the gap is topological and there are surface states. The surface states mean that the resistivity doesn't go to infinity at $T = 0$.

Appears to have a Fermi surface by quantum oscillations, but is an insulator. On one hand, could be FS of majorana fermions. This still responds to magnetic field by its kinetic energy. On the other (senthil), could be FS of composite excitons.

Other metallic features: (very large) $C/T \to$ const at $T \to 0$, and large optical conductivity at frequencies (far) below the gap.

No quantum oscillations seen in $\rho(T)$, but maybe seen in other similar compounds.

What needs to be explained:

- Good bulk insulating state, gap of order 3 meV
- Large C/T at low T

- large linear in ω optical conductivity
- Q oscillations with large extremal area

Why is SmB6 an insulator at all? There is a significant number of substitutional impurities. Each impurity: hydrogen atom. Once the hydrogen atom wavefunctions at each impurity overlap, you get a transition to a metal.

In SmB6: dielectric constant very large since electronic gap very small. At the known level of impurities, looks like it should definitely be a metal.

But: here we don't have a hydrogen atom. The minimum of the dispersion for the valence band is actually at a surface: $\varepsilon_{\mathbf{k}} = (|\mathbf{k}| - \rho_0)^2$. Solving the Schrodinger equation for this kind of energy, get $\psi(x) \sim \sin(x)x^{-1}e^{-x}$. Ionization energy is like 3 meV. The $\sin(x)$ oscillations mean that the hopping integrals between two nearby impurities is very small, and so it's very hard to turn SmB6 into a metal by doping.

Claim is that optical conductivity can come from the impurity states because of the oscillating properties of their wavefunctions.

Specific heat: thermodynamic, so doesn't care whether the contribution is coming from localized impurity states or delocalized band ones. Experiments see that C depends on impurity concentration, so claim is that it's actually impurity-driven.

Impurities can be very dense in the insulating state! Can have rather large interactions between the impurities.

Oscillations: can also get quantum oscillations if you have a sharp kink in the dispersion; as LLs pass this kind the magnetization oscillates. This is quantum oscillations without a Fermi surface!

To distinguish between actual metallic delocalized states and stuff coming from a localized impurity band, need to do something which cares about the delocalized nature of electrons: thus need to measure transport. Apparently $\kappa/T \to 0$, which doesn't look good for neutral FS.

3-22-21 **Johnpierre Paglione — SC in** UTe_2

- re-entrant superconductivity: ferromagnetic SC at low T and low B. Pressure kills SC. Large enough field kills SC, and then larger fields eventually *restore* the SC. Apparently some sort of field-induced quantum critical point where fluctuations are strong.
- UTe_2 has superconductivity and is very close to having ferromagnetic order.

- anisotropy is very strong. non-monotonic behavior of T_c with B. Re-entrant SC above 40T. Lives *inside* of the field-polarized phase.
- thermal transport goes to zero with T in a way consistent with d-wave point nodes. However, still see peak in C/T at low T. Contribution to this peak can't be itinerant, and is weakly field-dependent. Persists above H_{c2}.
- microwave sees large quasiparticle conductivity, and STM only sees a weak gap in the density of states. However, no bulk thermal conductivity. Hence probably some exotic surface fluid?

4-8-21 **Anton — thermal response**

Only invariants of gapped 2d materials: κ_{xy} and σ_{xy}. How to prove nothing else? In particular why no mixed invariants?

Consider a slab of material infinite in one direction, and with thermal and chemical potential gradients in the other (finite) direction. Can write number and energy currents as

$$\begin{aligned} I^N &= -\sigma_{xy}\Delta\mu - \nu_{xy}\Delta T \\ I^E &= -\eta_{xy}\Delta\mu - \kappa_{xy}\Delta T. \end{aligned} \tag{88}$$

Can we have

$$\lim_{T\to 0}\nu_{xy} \neq 0, \qquad \lim_{T\to 0}\frac{\eta_{xy}}{T} \neq 0? \tag{89}$$

Can show that would both be topological invariants if nonzero.

Bulk gapped. Can write currents as a sum of edge currents on if the two limits above are equal. Can we then get a phase where $\nu_{xy}(0) \neq 0$?

Theorems: no nonzero current in any conserved quantity in a 1d system in equilibrium (needed a proof b/c originally people thought that currents in superconductors was an equilibrium situation). *Can* have nonzero equilibrium current if the system is the edge of an anomalous 2d bulk.

Handwaving argument for $\nu = 0$: if have nonzero equilibirum current then need a 2d bulk topological action. Only actions you have are AdA, $\mathrm{Tr}[\omega d\omega + 2\omega^3/3]$. No mixed A, ω actions $\implies$ mixed coefficient should be zero.

Complaints: no one knows how to couple gravity to a lattice. Also no one knows how to relate topological action to thermal properties.

Also, exist mixed $U(1)$-thermal anomalies in non-unitary CFTs. However, can show using OPEs that no mixed response exists in any unitary chiral CFT. $\langle j^N \rangle = (k_L - k_R)\mu/2\pi$ has no T dependence.

However, must the edge be described by a CFT? Maybe the edge is intrinsically non-relativistic or something.

Third law of thermo: "nernst unattainability principle": *impossible to lower the entropy of a body to its $T = 0$ value within a finite time* (assuming $S|_{T=0}$ is finite).

This implies $\nu_{xy}(0) = 0$. Entropy current is

$$j^s_k = \frac{\eta_{kj} E_j}{T} - \frac{\kappa_{kj}}{T} \partial_j T. \tag{90}$$

Can hence use an electric field to create an entropy pump. By third law, net entropy current must vanish at $T \to 0$. This implies that symmetric part of η/T must vanish. Let antisymmetric part be ν^A. Streda formula:

$$\nu^A = \frac{\partial \mu}{\partial T} = \frac{\partial S}{\partial B}. \tag{91}$$

Nernst heat theorem: $\lim_{T\to 0} S(T)$ *is indep of parameters of Hamiltonian.*

Nernst unattainability theorem: $\nexists$ entropy analogue of Thouless pump.

Currents on a lattice: for sites labeled by p, q, get (RHS is analogue of divergence)

$$\frac{dQ_q}{dt} = -\sum_p J^N_{pq} \qquad \frac{dH_q}{dt} = -\sum_p J^E_{pq}, \tag{92}$$

with

$$J^E_{pq} = -i[H_p, H_q], \qquad J^N_{pq} = i[H_q, Q_p] - i[H_p, Q_q]. \tag{93}$$

Like in field theory, currents are ambiguous:

$$J_{pq} \mapsto J_{pq} + \sum_r N_{pqr} \tag{94}$$

with N_{pqr} skew-symm.

7-28-21 **Anaelle Legros — cyclotron mass in cuprates**

- measured mass $m_c \sim 4.9 m_e$ in LSCO
- no detectable signs of field-driven Fermi surface reconstructions

- note that Kohn's theorem says that interactions do not affect m_c in an electron gas (assumes Galilean invariance)
- m_c agrees with specific heat mass
- carrier density goes like p instead of $1 + p$

7-28-21 **Avishkar Patel — cuprate stuff**

- Point is that the 2d sheet resistivity smoothly crosses over from values less than the Ioffe-regel value of h/e^2 to values greater than it, while retaining the same linear-T slope
- surprising that there's a smooth crossover from the good metal to the bad metal — most numerical things and theoretical models have a change in slope of $\rho(T)$ in the crossover from good to bad metal regimes. One exception is the SYK island lattice model

7-28-21 **Dominic — strange metals**
Central dogmas:

- strange metal physics not intrinsically related to disorder. Residual resistivity *is* dependent on disorder strength, but slope of $\rho(T)$ isn't
- compressible (critical point occurs at different dopings in different cuprates; also it can be tuned by pressure)
- conductivity scaling:

$$\sigma = \frac{1}{T}\Sigma(\omega/T) \tag{95}$$

Coherent part of conductivity is related to almost conserved quantities,

$$\mathrm{Re}\,[\sigma_{coh}] \sim \frac{(\chi_{JM})^2}{\chi_{MM}} \frac{\gamma_M}{\omega^2 + \gamma_M^2}, \tag{96}$$

where γ_M is the relaxation rate for the almost conserved quantity M. Thus Lorentzians $\leftrightarrow$ almost conserved quantities. Say that M is almost conserved if $\gamma_M \to 0$ with $T \to 0$ faster than T (low-energy theory has M as emergent

symmetry). If γ_M is nearly conserved, any conductivity satisfying scaling must be incoherent!

Possiblities: 1) there are no nearly-conserved quantities that overlap with the current — then get no coherent part. 2) actually this is probably not possible — hence require "critical drag", where $\chi_{MM} \to \infty$.

Coherent part wins outside the critical fan; incoherent wins in the critical fan. Since these have two different functional forms, there shouldn't be a scaling collapse between the two (as a function of e.g. pressure or doping).

Claim: for any clean compressible system, there is some conserved quantity M such that $\chi_{JM} \neq 0$. Coherent conductivity $\leftrightarrow$ emergent symmetries w/ t'Hooft anomaly. χ_{JM} is not dynamical, it's fixed by the t'Hooft anomaly.

7-28-21 Rick Green — electron doped curpates

Electron doped cuprates have a fermi surface reconstruction (as seen by jump in Hall coefficient) at doping slightly beyond optimal. Interesting since long-range AF order ends before the SC starts. So FS reconstruction due to short-range AF order?

T-linear resistivity in field-induced state after killing the SC. Normal state has T^2.

7-29-21 Jake Ayres — magnetoresistance across p_* in the cuprates

Magneto resistance is

$$\rho(H,T) = f(T) + \sqrt{\alpha T^2 + \beta H^2}. \tag{97}$$

Appears to be indep. of impurity scattering.

Slope of magneto resistance at high field increases linearly with underdoping. Low fields are more complicated. Goes sharply from H/T at OD to H/T^2 for UD.

Takeaway: nature of scaling with H, T changes at p_*. T^2 scaling at low fields usually. Presence of H-linear term means that the Fermi arcs must present some kind of boundary to cyclotron orbits.

7-30-21 Pablo — strange metals in tbg

- $\rho \sim T$ down to lowest T near the SCing domes

- phonons can't work: off by order of magnitude, also Moire phonons are gapped, and in any case should be a kink in the slope
- $\rho^{-1} = e^2 \chi D$, χ the compressibility. So $\rho \propto T$ can be due to e.g. $\chi^{-1} \propto T, D \sim const$ or $\chi^{-1} \sim$ const., $D^{-1} \propto T$. Try to resolve with chemical potential measurements

8-9-21 **Yazdani group — nodal SC in TBG**

Large screening (from graphite gates): big SC domes, but no correlated insulators. SC is more robust with more screening. Maybe some phase which competes with the SC, which is stronger with stronger interactions.

Gap is around 1-1.5 meV. Even using strong quasiparticle broadening, hard to get nodeless fit.

Pseudogap regime persists to more than 10x the highest B_c in transport.

Looks like T_c is caused by quasiparticle poisoning?

8-9-21 **OSU guy — Hall angle in TBG**

Find $\cot \theta_H \sim T^2$, while $\rho_{xx} \sim T$. This is the same as in cuprates!

Magintude of ρ_{xx} is fairly well below h/e^2.

Apparently there's an insulator at $\nu = 5$ due to the higher band being flatish?

10-4-21 **Brad ramshaw — Plankian scattering in strange metals**

Note that ADMR is different from quantum oscillations, in that it doesn't need landau level quantization to exist. For ADMR, we just use Boltzmann transport to look at the magnetic field dependence of $v_{F,z}$ when computing e.g. $1/\rho_{zz}$. Just need semiclassical things moving around the Fermi surface; don't need LLs.

Something qualitative changes going through x_*. Goes from $\rho = aT + bT^2$ to just aT. Fit ADMR data by using a $\mathbf{k}$-indep $1/\tau$ and a $\mathbf{k}$-indep $1/\tau$ (the latter being associated with forward scattering from out-of-plane dopant impurities).

In some materials e.g. Nd-LSCO, there are vhss close to the zone boundary. These regions have high scattering and don't really contribute to the conductivity.

10-7-21 **Josh Fredman (Chicago) — cosmology colloquium**

Local probes (cephieds to boostrap to SN in the Hubble flow) measure H_0 to be large. Structure probes (DES, Plank, BAO, BBN) measure H_0 to be small. Tip of the red giant branch methods are in between. Note that the local probes are a direct measurement, while e.g. stuff from the CMB requires that one make an *assumption* that ΛCDM is correct.

Cosmology is nice but has so much semiquantitative imprecise data-focused reasoning that I would maybe get sick of it after a while.

Currently unknown whether the equation of state of dark energy has $w = -1$ or not. He proposed some sort of very slow inflaton thing to be responsible for accelerated expansion.

3-15-22 **Nick Bultinck — kekule spirals in tbg**

- nontrivial topology of tbg made manifest by going into sublattice basis. This is obviously done when hbn is aligned generating a σ^z mass, but convenient to do in general
- moire patterns act like a magnifying glass for strain
- heterostrain of magnitude 0.1 or 0.7 percent. Wouldn't worry about this in monolayer graphene, but more important when there's a moire pattern
- to lowest order, strain acts in MLG as applied vector potential. Here $\mathbf{A} = (\varepsilon_{xx} - \varepsilon_{yy}, -2\varepsilon_{xy})$. Has fairly significant impact on the flat bands
- kekule figured out how benzene works. Same pattern in iks.
- iks: spontaneously breaks translation and valley $U(1)$; preserves a combination of the two
- parts of the BZ are completely valley polarized — may be good for phenomenology?
- IKS = boosted semi-metal + IVC
- put minimum of K band on maximum of K band, then partially valley polarize.

- stiffness of IKS about 0.4meV. KT temperature claimed to very large, about 7K!
- changing magnitude of strain induces first order transition between IKS and uniform insulating orders
- strain: gives semimetal at neutrality, and IKS at all other integer fillings
- strain will be less important for semiconductors, basically because there is no magic angle. twist angle and strain are basically two sides of the same coin

3-15-22 **Senthil — APS talk**

made a shoutout to experimentalists — thank you for keeping this field interesting

- ferromagnetic insulators are RARE things in insulators. When they occur, they are usually very weak.

  this fails when the bands cannot admit localized wannier functions. this is the situation in tbg, but is very rare in nature
- in systems without C_2T symmetry: flat bands in two valleys have equal and opposite chern number C. This happens in systems with e.g. hbn alignment. These systems are easier to think about.
- HF gives strong ferromagnet, at energy scale set by coulomb interaction. HF usually overestimates FMs.
- spin-valley polarized ferromagnetic insulator seen in tbg aligned with hbn. Goldhaber-Gordon and Young. Good evidence for valley polarization, but not so good evidence for spin polarization. Too early to declare victory. No actual evidence for spin polarization.

3-15-22 **Leo — coulomb screening and superconductivity**

- net increase of chemical potential across filling: about 40 meV. Not direct reflection of band width, can just be tied to coulomb interaction.

- leo's expts don't completely obviously mean that strong interactions is bad for the SC — could be that you're just screening out disorder
- away from magic angle, the bandwidth as measured by $\Delta\mu$ is actually *higher* away from the magic angle! Makes sense if $\Delta\mu$ is just measuring strength of Coulomb interactions.
- DO see SC at lower twist angles! May have some evidence that T_c is nonzero at small twist angles, goes to zero, and then is nonzero again near the magic angle!
- small twist SC has very sharp transition in $R(T)$.
- at magic angle, SC directly related to flavor polarization. In small twist angle regime, no clear Hall density reset.
- at magic angle, pomeranchuk effect is operative. Not at small twist angles.
- at magic angle, get linear in T resistance. Not so at small twist angles.
- coulomb screening has no effect on the small twist angle SC.
- small twist angle SC phase: SC diode effect, implies breaking of TRS
- linear in T resistivity is correlated with flavor polarization: hence probably not due to phonons!

6-25-22 **GRC day 1**

Ludwig Holleis — Andrea Young ABC nice because reproducibility is easier. Electric field polarizes charge carriers onto a single layer. Get flat band.

Bernal bilayer graphene (only stable 2-layer stacking pattern). Get diverging density of states as k^2. Displacement field opens up band gap. Several isospin transitions as function of D and n_e. Very low T_c of 30mK. SC not killed by any of the fields they have. Suggests that spin polarized since they know spin polarization happens nearby?

Put WSe_2 (tungsten diselenide) to add proximity induced SOC. Using displacement field can turn off and on SOC. Claim that this enhances T_c by factor of 4. Critical $H_{c\perp}$ gets smaller? Still no bound on $B_\parallel$. SC does not seem to

appear at phase transition. Can measure Ising SOC from looking at how Landau levels get filled — $\sigma^z s^z$, leads to sublattice splitting in the Landau levels. Get strength of $O(1)$ mev? Note that with the electric field, you polarize the electrons onto one layer — thus an in-plane field doesn't do anything — the electrons don't hop between layers, so no way for them to see flux when they hop.

why does $B_\perp$ get smaller with SOC? and how are you estimating pauli violation?

Frank Scheindler — Princeton Trion — 2 particles + 1 hole. Project into flat bands and have $H = \sum_q V_q O_q^\dagger O_q$, where $O_q = \sum_k \langle u_k | u_{k+q} \rangle n_{k,q}$. This is remarkable — I'm allowed to find it remarkable, since this isn't my work. Trions: just do $c^\dagger c^\dagger c$ on the ground state. Almost built from quasiparticles (gapped, dispersive) and goldstone (almost gapless).

Other talks Talk Omar Mehio on spectroscopic stuff in mott insulators. Looking at exciton recombination and stuff. How can doublons and holes relax? Can't just emit photons, because of momentum conservation — which limits radiative decay to a tiny region around $\mathbf{k} = 0$. So the physics of this recombination process is interesting; can be probed by pump-probe stuff. Look for the bound state energies in spectroscopy.

Talk by kevin nuckolls on tbg stuff. Interesting that PCS spectroscopy has big dig.

Talk by David Barbalas on energy and momentum transport in LSCO. Energy relaxes more slowly than momentum.

Talk by Tamaghna Hazra on the kondo problem, where triplet moments hybridize with the sea and lead to triplet pairing.

Talk by Hyeok Yoon doing andreev spectroscopy on UTe2 (uranium telluride). Basically doing btk stuff and looking for nodes. Sees bound states, so probably gapless. Interesting reentrant phase and stuff.

$$\sum_i s_i^+ s_{i+1}^- s_{i+2}^- s_{i+3}^+ + h.c. \quad \text{(spin half)}, \qquad \sum_i S_i^+ (S_{i+1}^-)^2 S_{i+2}^+ + h.c. \quad \text{(spin one)} \tag{98}$$