philosophical comments on RG

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1 General comments on RG: the relation between the "old-school" and Wilsonian approaches to RG, scheme-dependence, etc.

blah blah. Might want to turn into its own mini document

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ethan: *the rescaling step: we change variables from x to xs, and then adjust the coupling constants in such a way that long-distance correlation functions at the fixed point (and hence the partition function if the deformations are not relevant) are unchanged up to powers of $1/(x\Lambda)$, where x is a long-distance scale that correlation functions are evaluated at. The step where we change variables $x \to xs$ is not just a trivial change in integration variables, since it affects the domain of integration. Said another way, unless we are exactly at the fixed point, the theory is scale-dependent (since things change when integrating out modes at short scales), and so the change in variables is nontrivial. However since we don't want to change the partition function, we need to do something else to correct for this, viz. to change the couplings. We are changing the couplings and not the operators (i.e. there is no "re-scaling map $\mathcal{R} : \mathcal{O}(x) \to s^{\Delta}\mathcal{O}(sx)$") since the operators are dynamical, while the couplings are "background fields" that we can tune by hand. (note: actually as usual both perspectives are possible. At least in normal CFTs where the RG proceeds by scaling spacetime we can act on the operators directly, since by assumption they all transform in representations of the conformal group)*

ethan: *Suppose we deform the fixed point action by a term $g\int\mathcal{O}$. one way to do RG is to compute correlation functions as a function of g, and then find a β_g such that $(d_{\ln\Lambda} + \beta_g\partial_g)C = 0$, where C is a long-distance correlation function. Therefore we are comitting to a given IR theory where the coupling g at a given scale μ is at some fixed nonzero value $g(\mu)$ which we take in from experiment. This can basically always be done. The nontrivial part about RG is that β_g is an autonomous function of g. This depends on the renormalizability of the field theory, and is where all the actual nontriviality of the RG framework lies. Anyway, this approach still isn't always desired, since sometimes the g-dependence of the C correlation functions is hard to calculate. For example*

consider $\mathcal{O} = \cos(\theta)$ in a compact boson theory. It's not that easy to compute $\langle e^{i\phi(x)} e^{-i\phi(0)} \rangle$ as a function of g. Therefore it may just be better to ask how one has to change g if they want to preserve correlation functions of $g \int \mathcal{O}$, at least at long distances. This then preserves the partition function and as such preserves correlation functions.

Contrast the two approaches: high energy people do experiments at low energy, and want to make predictions for high energies. Hence the high-energy slogan is "vary the UV couplings to keep the IR physics fixed", with the issue of (ir)relevance being one of sensitivity to initial conditions. Cond-mat people want to predict the IR behaviors of UV models. Hence there it is "integrate out modes and see how the coupling constants flow". (although this is what cmt people do too — the rescaling step is designed to preserve IR correln functions).

Why do we want to re-express dimensionless quantities in terms of the new couplings? Because dimensionful quantities only appear in correln functions when they are made dimensionless through the cutoff. It is only ever dimensionless things which contribute to physical stuff.

Wilsonian approach

One important point that I haven't seen in textbooks is that the procedure of integrating over the fast modes is *one-loop exact*, in that the flows of the couplings $\delta_{\ln \Lambda} g_\alpha$ in the effective action $S_{eff}[\phi_>]$ are computed only from diagrams that contain a single loop. This is simply because of phase space reasons: each independent momentum integral of the fast fields comes with a phase-space factor of $\delta\Lambda$, and so in the $\delta \ln \Lambda \to 0$ limit, only the diagrams with one momentum integration, viz. one-loop diagrams, survive. Therefore if S_I is the part of the action that mixes the high and low momentum modes, we have (in Euclidean signature)

$$S_{eff}[\phi_<] = S_0[\phi_<] + \frac{1}{2} \mathrm{Tr} \ln \left[1 + G_> \frac{\delta^2 S_I}{\delta \phi^2} \Big|_{\phi=\phi_<} \right] + \mathcal{O}((\delta \ln \Lambda)^2). \tag{1}$$

Therefore it is not that "when integrating out the fast modes, we restrict to one-loop since it's easier", but rather "when integrating out the fast modes, one-loop is all there is!", at least as far as getting the beta functions for the coupling constants goes.

The fact that the k-shell evolution of the coupling constants only involves digrams with a single internal line is what makes the functional RG program tractable.

Given that the differential changes in the couplings are determined by a one-loop answer, we run into a predicament: not all physical quantities show up at the 1-loop level! For example, consider the anomalous dimension in ϕ^4 theory. It is easily checked that there are no k-dependent contributions to $\Sigma(k)$ at one-loop, so that the $(\partial_x \phi)^2$ term in the action is unchanged after doing the integral over $\phi_>$. How then are we ever supposed to recover the $\eta \sim g^2$ anomalous dimension of the ϕ^4 field theory? The key is that the information about Σ is contained in *irrelevant* operators generated during the shell integration. For example, consider the two-loop answer for η, which is determined by the sunrise diagram. The anomalous dimension is computed to lowest order by $d_{\ln \Lambda} G_>(k) = -\eta$ (recall $G_<(k) = \Lambda^{-\eta} |k|^{2-\eta}$ (we are in four dimensions for now)). The terms contributing to this at two-loop will have one loop in the high momentum shell, and one propagator running over *all* momenta. The high momentum loop can be thought of as an effective 4-point vertex for

the $\phi_<$ fields, and so we can evaluate the sunrise diagram by evaluating a cactus diagram with the vertex given by the effective vertex after performing the shell integration. However, we know that a momentum-independent $\phi_<^4$ vertex doesn't give a cactus diagram with any k-dependence—therefore we need to keep the k-dependent part of the effective vertex, even though this goes as $k^{n\geq 2}\phi_<^4$, and would have naively been dropped due to irrelevance.

Schematically, write the effective action after doing the shell integration as (in what follows we will drop all numerical constants; equations should all be understood as having $\sim$s)

$$\delta S[\phi_<] \supset g^2 \delta \ln \Lambda \int_x \left[\phi_<^4 + \frac{1}{\Lambda^3}\phi_<^2(\partial\phi_<)^2\right] \tag{2}$$

Now when we compute $G_<$ using the new effective action for $\phi_<$, we see that there is now a momentum-dependent term:

$$\delta G_<(k) \supset -g^2\delta\Lambda \frac{1}{k^2}\int_q \left(G_{<,0}(q)\frac{k^2}{\Lambda^3}\right)\frac{1}{k^2} + \dots, \tag{3}$$

where the two $\frac{1}{k^2}$s are from the free propagators $G_{<,0}(k)$ on either side of the vertex and the $\dots$ are terms higher-order in k^2/Λ^2. Doing the integral (still not writing constants), we get

$$\delta G_<(k) \supset -g^2(\delta \ln \Lambda)\frac{1}{k^2} + \dots \tag{4}$$

Therefore the anomalous dimension to lowest order is[1]

$$\eta = -\frac{d\ln G}{d\ln\Lambda} \sim g^2. \tag{5}$$

The important point here is that in order to compute universal properties of the critical point like the anomalous dimension, it is *not* sufficient to do a computation which only involves an integration over a high-energy shell—one must compute diagrams where low energy modes are integrated over as well. Indeed, the RG flow of any quantity computed with a diagram at more than one loop will necessarily involve integrals over all energy ranges. This is actually very reasonable when one remembers that near critical points, modes on all scales are coupled to modes on all other scales—therefore it would be odd if one could compute universal properties of the critical point by only looking at what's going on within a small range of high energy dof. At the same time though, the RG is indeed calculating what happens when a shell of high energy dof get integrated out. The fact that the results of what happens when the high energy dof get integrated out depends on what the low energy dog are doing is a statement about how the different energy regimes are coupled to eachother. One potentially good slogan for the RG could then be "RG is the thing that tells you how different energy scales are coupled together".

Remarks on the rescaling procedure

Robert had the following nice pov: the goal in applications of QFT to most of physics is to compute correlation functions like $\langle\phi(\lambda x)\phi(\lambda y)\rangle$ in the limit $\lambda \to \infty$. The rescaling step of

[1] ethan: *explain why there's no β_g term*

the RG is designed to do this by

$$\langle \phi(\lambda x)\phi(\lambda y)\rangle_{S_0} = \langle \phi(x)\phi(y)\rangle_{S_\lambda}. \tag{6}$$

That is, the RG is a way of transferring the long distance depndence of correlation functions into the form of the coupling constants in the action. Looking at the action is only useful for computing UV observables, but not IR ones. So, if we can transfer problems about IR correlators in for one action to problems about UV correlators in another action, then since the UV correlators can be gleaned from the form of the action, we can more easily read off the values for the IR correlators. (this is the rescaling, but explain why this point of view is still just a trivial change in integration variables)

Some comments on the rescaling step of the RG. Surely if we just wanted to know about the correlation functions of low energy operators in some particular model, we should just do the path integral over the high energy modes and then compute in the effective action thereby generated. This is in line with the approach of the exact RG: we get expressions for how the couplings change as a function of the cutoff, but nowhere do we talk about re-scaling the cutoff back to its original value after integrating out the high energy modes. This is the type of RG procedure we usually do when working on a lattice — we decimate and get rid of some of the dof, and are then left with a new model defined on a coarse-grained version of the original lattice, with all the correlation functions in the new model on the new lattice matching the corresponding ones in the original model. This is just doing the path integral step by step. In particular by construction, all dimensionful quantities (e.g. correlation lengths) do not flow under RG. Therefore it is very misleading to say things like "the mass of the scalar flows to infinity under RG". The RG in this sense is not telling you how dimensionful quantities flow — they don't — it is rather telling you how to make simplifications in the way you describe the theory such that the dimensionful quantities computed by the theory are (approximately) unchanged. This way of doing RG is in keeping with the mantra appearing in hep, where one describes RG as "the way you change the coupling constants to keep the low energy physics fixed". Indeed, the hep approach doesn't involve any kind of rescaling like this (wavefunction renormalization is similar but not the same thing).

Why then do we usually include this extra re-scaling step when working with continuum field theories? Honestly, I'm not sure this step is actually that helpful. First, note that the rescaling step leaves all correlation functions invariant. In fact, sending $\phi(x) \mapsto \lambda^{-\Delta}\phi(\lambda x)$ is no more than a mere change of integration variables, and as such it really does nothing as far as physics is conserved — whether or not we include it is then purely a matter of whether or not doing so makes our description of the system nicer; it ultimately does not affect any physics (we could just as well do $\phi(x) \mapsto \lambda^{-42\Delta+7}\phi(\lambda x - 42)$; as long as we make this replacement everywhere no observables change[2]).

Instead, we do the rescaling because we want to make it easy to compare the theory before and after integrating out some high energy modes; the comparison, at least at the

[2]Although Wilsonian RG is formulated by starting close to a fixed point, we are not using the dilitation symmetry at the fixed point to do the rescaling. That would be e.g. acting on fields in a correlator with the dilitation, but not also acting on the fields in the action. Here we are acting on all instances of the fields, which is just a trivial change of variables.

level of actions, is then done most naturally if the two theories have the same cutoff.

The downside of doing this is that one has to be slightly more careful when matching correlation functions. Before the re-scaling, we have[3]

$$W_S^n(k_1, \ldots, k_n) = W_{S'}^n(k_1, \ldots, k_n), \tag{7}$$

where S' is the effective action obtained after integrating out the modes with $k \in [\Lambda - \delta\Lambda, \Lambda)$ (but without re-scaling), and where the above equality holds provided all the k_i are less than $\Lambda - \delta\Lambda$. After we do the re-scaling we have to re-write this as

$$W_S^n(k_1, \ldots, k_n) = Z^{-n/2} W_{S'}^n(k_1/\lambda, \ldots, k_n/\lambda), \tag{8}$$

where on the RHS we are using the re-scaled action this time, and where the restriction on the range of k_1 is as before. The reason that this is a little weird is that if high-momenta correlators computed with the S' theory correspond to low-momenta correlators in the original theory (the one whose correlation functions we want to know)! So after doing RG in this way, we are left with a new theory, defined with the same cutoff as the original one, but which is such that its correlation functions computed at scale λk are equivalent to the correlation functions in the original theory at scale k. Now since in the deep IR we will reach a fixed point where the correlators are just power laws, the only difference between the correlators at λk and those at k will be a trivial constant factor, so as long as we're only interested in the mega-deep IR then we don't have to remember this.

Anyway, the tl;dr of this rather rambling discussion is that the re-scaling thing is not anything physical; it's just a choice made to facilitate comparing effective actions as one integrates stuff out (it is biased towards comparing actions rather than correlation functions).

Or is this just another issue of being able to interpret things actively vs. passively? In the hep approach, the input to the theory is a set of observables at low energy. The goal is then to figure out how the dimensionless parameters in the action need to change in order to keep the IR dimensionful quantities (correlation lengths, masses, etc.) fixed.

On the other hand, we could also consider having the coupling constants in the action be the input to the theory, and examining how the resulting IR dimensionful quantities change. The distinction here is whether one wants to use field theory as a way of using observables at one IR scale to make predictions for observables at a different IR scale, or whether one wants to make predictions about the IR observables associated to a particular microscopic model.

$$\phi(x) \mapsto \phi'(x) = \lambda^{\Delta}\phi(\lambda x), \qquad \lambda < 1. \tag{9}$$

Note we are always taking the active point of view, where space is fixed. This transformation then corresponds to stretching out the fields in space, plus making them smaller by scaling them by a constant.

Why the β function is autonomous; the scaling hypothesis

maybe talk about the scaling theory of localization

[3] In practice, all equations like this will only be expected to hold up to powers in k_i/Λ.

Therefore in 1D the relation is rather obvious: here $g(L)$ is a monotonic function of L, and hence we can solve for $L(g)$; therefore any explicit L-dependence in β_g can be converted into $g(L)$-dependence. However, this monotonicity property is only true when there is no critical point, and in general we cannot invert and find an expression for $L(g)$. So why does autonomy hold in general?

Hep approach

The hep approach deals with examining how the effects of fluctuations change with the scale at which measurements are made. For example, electrons in QED are screened by clouds of virtual particles. When doing scattering experiments with electrons, the strength of the interaction will depend on the energy with which they are scattered, since if the particles to be scattered have higher energies, they will peer deeper into the screening cloud, and see a larger fraction of the bare charge. The hep RG lets us calculate the effective amount of screening that occurs at different energy scales (in terms of common field theory notation, this means calculating how the couplings change as a funtion of the RG scale μ).

One more old-school way to say what the hep approach does is to say that it provides a way of telling one how to write down a set of bare couplings which reproduce a fixed set of low-energy observables given a large UV cutoff, which then in turn provides a way of writing down the limiting form of the bare couplings in the limit $\Lambda \to \infty$, allowing one to construct (at least formally, and in the renormalizable case) a truly continuum QFT.

However I think describing things in this way kind of short-sells the hep approach. It is not only useful for defining the continuum limit, but rather is useful in that it provides a powerful way of relating observables at different low-energy scales to one another.

The crucial thing about the hep approach which makes it calculationally superior to the Wilsonian approach (at least for low orders in perturbation theory) is that it only ever involves a *finite* number of coupling constants.

The input to the hep RG program is a measurement of some correlation functions at low energies. For example, suppose we measure the Coulomb force law at a momentum p^2, and get an answer V_p. Then we define the electric charge $\alpha_p \equiv p^2 V_p$. This will be some finite (and in our universe, small) number, at least for values of p^2 accessible to humanity. We then want to formulate the theory using this input parameter α_p. In particular, we want a theory that takes in α_p as input, and then tells us $\alpha_{p'}(\alpha_p)$. Computing the relationship between the α_ps at different values of p essentially the whole job that the theory is constructed to accomplish. In order for the theory to be self-consistent, it should be able to accept as input α_p and return the same values of $\alpha_{p'}$ regardless of p. This is equivalent to requiring that the parameters in the Lagrangian be independent of μ.

Put another way, the couplings are infinite because the perturbative corrections to things like e^2 involve loop integrations over an infinite energy window, and so in order for loop-corrected quantities to be finite, the bare coupling constants must correspondingly diverge. More precisely, consider e.g.

Note that irrelevant terms never show up in the hep RG, which works essentially by projecting away all irrelevant terms at the cost of making adjustments to the relevant ones. For example, consider the 4-point vertex in ϕ^4 theory in 3+1D at an energy scale p^2. The

one-loop corrections to the vertex are not just proportional to $\ln(p^2/\Lambda^2)$, which diverges as $\Lambda \to \infty$. Instead, they contain additional finite contributions

How to organize fluctuations—by scale, not by number of loops!

One conceptual problem with the hep approach is that an expansion in the number of loops is *not* a physical way to organize an expansion in fluctuations. Indeed, the mere existence of autonomous RG equations (the CZ equations) for things like the n-point correlation functions $G^{(n)}$ means that the calculation of the 1-loop beta functions determines an *infinite* number of terms in the expression for $G^{(n)}$, at all orders in the couplings (i.e, at all loops).

For example, consider a marginal coupling g associated with some n-point Greens function $G_n(p)$. Let G_n be expanded as[4]

$$G_n(p) = g + \sum_{k=2} g^k \sum_{l=1}^{k-1} A_l^k t^l, \tag{10}$$

where the A_l^k are some numbers to be determined by doing Feynman diagrams, and where we have defined the RG time to the scale p as

$$t \equiv \ln(\Lambda/p), \tag{11}$$

with the sign chosen so that $t \to \infty$ in the IR. For visual clarity, we write this out longhand as

$$G_n(p) = g + g^2 A_1^1 t + g^3(A_2^2 t^2 + A_1^2 t) + g^4(A_3^3 t^3 + A_2^3 t^2 + A_1^3 t) + \dots \tag{12}$$

The terms in each group are cut off at $g^{k+1}t^k$ since a diagram with g^{k+1} vertices can have at most k totally independent momentum integrations (these diagrams are the ones of concatenated bubbles). The A_i^i terms are the leading logs, the A_{i-1}^i terms are the subleading logs, and so on.

Now write a similar expansion of the beta function as

$$\beta_g = \sum_{k=2} \beta_k g^k, \tag{13}$$

where we have started at g^2 since g is marginal. We then plug this into the equation $d_{\ln\Lambda} G_n(p) = 0$, and match coefficients in t. We find that

$$\beta_2 = -A_1^1, \qquad \beta_3 = -A_1^2, \qquad \beta_4 = -A_1^3, \qquad \dots \tag{14}$$

and

$$A_k^k = (A_1^1)^k, \qquad A_2^3 = A_1^1 A_1^2, \qquad \dots. \tag{15}$$

Therefore the right way to organize the expansion of G_n is in fact *not* in loops (number of powers of g), but rather by the degree of subleading-ness of the log:

$$G_n(p) = g + \left(A_1^1 g^2 t + A_2^2 g^3 t^2 + A_3^3 g^4 t^3 + \dots\right) + \left(A_1^2 g^3 t + A_2^3 g^4 t^2 + \dots\right) + \left(A_1^3 g^4 t + \dots\right) + \dots, \tag{16}$$

[4]This is the bare Greens function—we are going to be writing the β functions as the changes in the bare couplings with the cutoff, just so we don't have to deal with the anomalous dimensions from the renormalized fields in the renormalized Greens function.

where the first term in each group of parenthesis determines the coefficients of *all the subsequent terms* in the group. In particular, the whole n point function is determined *entirely* by the terms linear in t! This is not obvious at the level of diagrams, and provides a powerful way of summing up an infinite number of diagrams. This all really just comes from the existence of an autonomous RG equation—all the magic of the RG program lies in the existence of an appropriate CS equation! Also note that in addition, the β function for g is similarly entirely determined by the terms linear in t (as it had better be if a differential equation is to exist!). This is another explanation of why the momentum-shell RG works despite it only involving diagrams where only a single loop is in the high energy shell—these are the diagrams that produce the linear in t logs.

The above series expansions were seemingly very innocuous, and yet they let us derive constraints on the n point function valid to infinite order in perturbation theory—where was the magic? The magic was all in the fact that we wrote β as a function of g alone, and not of t. Writing $(\partial_{\ln\Lambda} + \beta_g(g,\Lambda)\partial_g)G_n(p) = 0$ is a triviality, and lets us conclude nothing. It is the fact that the β function is an *autonomous* ODE that is the crucial point. Given that β_g does not depend explicitly on $\ln\Lambda$, in the expression for $\partial_{\ln\Lambda}G_n(p)$, all the terms not linear in t will have t-dependence which must be canceled by the structure of β_g and the $\ln\Lambda$-dependence of g, and this gives rise to the dependence of all the non-linear-in-t terms on the linear ones. Now if $G_n(p)$ was a generic function of t and g, such a cancellation would not be possible. Furthermore it is *not* a priori obvious that the terms in $G_n(p)$ are related in the way we have demonstrated—that is, the explicit $\ln\Lambda$ independence of β_g is *not* obvious. The $\ln\Lambda$ independence is equivalent to renormalizability, which is generically a very non-trivial thing to prove. ethan: *but in the Wilsonian approach? Or is this just saying that the hep-th approach agrees with Wilsonian only in the case of renormalizable theories?*

Anyway, from the above relations on the coefficients in the expansion of $G_n(p)$, we see that it's really not best to organize perturbation theory by loops—rather, one should organize it by scale (i.e. by the power of $\ln(p^2/\mu^2)$ appearing in diagrams). For e.g. ϕ^4 theory this is somewhat obvious, at least for the leading logs: the fact that $A_k = A_1^k$ just comes from the fact that the leading log term t^k at a given loop order k comes from k bubbles concatenated together, so that the divergent part of the integral is simply the kth power of a single bubble. However at subleading orders and in more complicated theories these relations are less obvious.

From this point of view, should we be bothered by the fact that in theories like QED and ϕ^4 (both in four dimensions) the bare coupling constants appearing in the action are infinite? Not at all: the interactions in these theories are marginally irrelevant, and so in order for them to have a finite value in the IR, their bare values must diverge in the $\Lambda \to \infty$ limit. This is because the bare coupling constants are the effective interactions deep in the UV, at the scale Λ. Indeed, the typical one-loop relation between the bare and renormalized couplings is

$$g_B \sim g_\mu(1 - Cg_\mu \ln(\Lambda/\mu)), \tag{17}$$

where g_μ is the coupling at scale μ, i.e. the value of the four-point vertex (with the 1-loop corrections taken into account) when the external legs are set at $p^2 = \mu^2$. When we take the scale all the way to the UV by setting $\mu = \Lambda$ we get $g_B = g_\Lambda$, so that indeed the bare parameters in the Lagrangian are the UV couplings.

Note that in the hep approach, we are never integrating anything out—we are merely providing a way of relating correlation functions at different momenta.

This is why in the hep approach, it is in my opinion often conceptually clearer to avoid calculating flows by taking differentials with respect to $\ln \Lambda$. We view Λ and the bare couplings as fixed, and the effective couplings as changing as we change the scale at which they are measured.

Alternatively, if we really want to imagine changing the cutoff, then the RG provides us with a way of saying how the *bare* couplings need to change in order to keep the physical coupling at some low scale (the one we measure) fixed. The fact that we can do this is the "cutoff independence" of the theory: for a fixed input α_p at some p, the RG tells us how to construct a limit of theories with cutoff Λ and bare couplings $\alpha_B(\Lambda)$ which all produce the same effective coupling α_p at scale $p \ll \Lambda$ (again, this $p \ll \Lambda$ caveat is needed since the hep approach to RG only is able to hold α_p invariant under changes in Λ up to terms polynomial in p^2/Λ). This limit for $\Lambda \to \infty$ is what defines a continuum QFT.

One thing that is not really ever mentioned explicitly is that as we just mentioned, this whole construction works only up to order $\mathcal{O}(p^2/\Lambda^2)$. It is impossible to change Λ and adjust only the relevant / marginal terms while literally preserving correlation functions on scales below Λ. Likewise, the relation between α_p and $\alpha_{p'}$ cannot be determined as a function of the relevant / marginal couplings alone unless we permit ourselves to drop terms of order p^2/Λ^2.

In any case, powers of momenta are uninteresting in $\mathbb{R}$ space, since they are contact terms. The only things that are really relevant for $\mathbb{R}$ space physics are the logs.

ethan: *comment on IR behavior of superrenorm theories* Note that in renormalazable (but not super-renormalizable) theories, phenomena like nonzero anomalous dimensions are possible because the theory must be defined with the help of a UV regulator, since a UV fixed point doesn't exist. Thus non-universal information (Λ) shows up in things computed by the QFT, but the way in which it shows up is universal.

Relations between the two approaches; general comments

In the hep approach, you're figuring out how the coupling constants have to change in order to keep the low-energy physics fixed: the input to the RG program is IR universal quantities. In the Wilsonian approach, you're looking at how the IR physics changes as you integrate out high-energy modes: the input to the RG program is a UV Hamiltonian.

Now the hep approach is much more useful when doing calculations, since we only have a finite number of parameters to keep track of. However, the values of the coupling constants computed within the hep framework are also extremely fine-tuned: it involves projecting onto the *single* RG flow that connects the UV and IR fixed points (in the case when a UV fixed point exists). Almost all of the flows into the chosen IR fixed point will *not* lead to sensible well-defined continuum limits (only one out of an infinite number will, assuming again that a UV fixed point exists). Of course though, this is a fine thing to do as long as we only care about calculating universal quantities.

Reading qft books, one often gets the sense that things which are Λ-dependent are unphysical. This is totally untrue; things like T_c are completely physical quantities. Rather,

Λ-dependent things are non-universal.

Note: we are bothered about the light mass of the Higgs, since it is relevant. Should we also be bothered about the fact that $\Lambda_{QCD}/\Lambda_{Planck} \ll 1$, or is the marginal RG flow slow enough that the "UV value of g^2" doesn't have to be "un-naturally" small?

It is important that an RG flow, in the Wilsonian sense, is not something that one can do in the lab. Instead, a Wilsonian RG flow is merely an operation one can perform on partition functions to make certain correlation functions easier to calculate.

In the hep approach, the RG flow *is* something that we can "do" in the lab—we just measure correlation functions at different energy scales.

In the Wilsonian approach, the RG is a map between different theories (it is exact when we just integrate out modes in principle, but once we re-scale the momenta we are mapping between different theories with different actions), while in the hep approach there is only ever one theory, and the RG just provides a way to relate correlation functions at different scales within that one theory.

In the Wilsonian approach, we think of the RG map as modifying coefficients of terms in the action; in the hep approach the action is always fixed—it is the same for all RG scales (the bare terms are independent of the cutoff). The things that change under RG are rather correlation functions of operators as the energy scale associated with them is changed.

Scheme-dependence

Therefore talking about "*the* IR fixed point" of a system is a bit misleading—the fixed point one gets depends on the way in which one organizes the integration over fluctuations. Changing the scheme one uses to do the RG changes where on the critical surface the fixed point is. So really universality is a property of the critical surface itself, rather than of a single point on the critical surface. ethan: *really? or is it more that changing schemes corresponds to adding in irrelevant couplings?*

Scheme (in)dependence and the physicality of the beta functions

Consider a theory with a set of dimensionless couplings g_α. We can write a series expansion for the beta function for g_α about the critical point as

$$\beta_\alpha = \sum_{n \in \mathbb{Z}^>} \beta_{n\alpha}^{\gamma_1 \cdots \gamma_n} g_{\gamma_1} \cdots g_{\gamma_n}, \tag{18}$$

where the sum starts at 1 since wolog we are taking the couplings to be such that the critical point is at $g_\alpha = 0 \, \forall \, \alpha$. We want to know to what extent the $\beta_{n\alpha}$ are scheme-independent, that is, to what extent they remain unchanged upon making redefinitions of the coupling constants.

From our knowledge of critical phenomena, we know that because the scaling dimensions of the fields are determined by the eigenvalues of the first derivatives of the β functions linearized about the critical point, at least $\beta_{1\alpha}$ should be scheme independent. What about the higher-order coefficients? In what follows we will show that in fact unless the couplings are marginal only the first term (the scaling dimension) is scheme-independent. For the case

of marginal couplings, it actually turns out that the statement is slightly stronger: in this case the first *two* terms are actually scheme-independent.

Let f_α the coupling constants corresponding to some other way of describing the physics near the critical point (again defined so that $f_\alpha = 0 \,\forall\, \alpha$ is the critical point, so that the critical points in the f_α and g_α schemes are both at the origin of the critical manifold). For example, g_α might be the renormalized couplings one computes within dim reg, and f_α the ones computed with PV. Different choices for f_α give us different ways of parametrizing the vicinity of the critical point, and we would like to know to what extent these different parametrizations give different beta functions.

Near the critical point, both the gs and the fs will be small, and we can benefit from writing f_α in a power series

$$f_\alpha = \sum_{n>0} f_{n\alpha}^{\gamma_1 \cdots \gamma_n} g_{\gamma_1} \cdots g_{\gamma_n}. \tag{19}$$

Finally let $\widetilde{\beta}_\alpha$ be the beta function for f_α, and expand it too as a power series

$$\widetilde{\beta}_\alpha = \sum_{n>0} \widetilde{\beta}_{n\alpha}^{\gamma_1 \cdots \gamma_n} f_{\gamma_1} \cdots f_{\gamma_n}. \tag{20}$$

All of the $\beta_{n\alpha}, \widetilde{\beta}_{n\alpha}$, and $f_{n\alpha}$ will be taken to be symmetric tensors wolog.

Now a generic change of variables is generally not a useful thing to do, since we are only ever really interested in a set of variables which are scaling variables. Therefore in what follows we will take $\widetilde{\beta}_1$ and β_1 to be diagonal matrices, which following convention will be written as $\beta_{1\alpha}^\gamma = \delta_\alpha^\gamma y_\gamma$, $\widetilde{\beta}_{1\alpha}^\gamma = \delta_\alpha^\gamma \widetilde{y}_\gamma$ (no sum). Furthermore, in what follows we will wolog be working with parametrizations such that $f_\alpha = g_\alpha$ in the limit in which we approach the critical point, and hence we will set $f_1 = \mathbf{1}$ from now on.[5]

For the beta function of the reparametrized couplings, we have

$$\widetilde{\beta}_\alpha = \frac{df_\alpha}{dg_\lambda} \beta_\lambda. \tag{21}$$

We then replace everything in the above equation with their power series representations in terms of the g_αs and match coefficients. To quadratic order in the couplings, this yields the two equations

$$y_\alpha = \widetilde{y}_\alpha, \tag{22}$$

and (no sums on repeated indices)

$$y_\alpha f_{2\alpha}^{\rho\omega} + \widetilde{\beta}_{2\alpha}^{\rho\omega} = 2y_\rho f_{2\alpha}^{\rho\omega} + \beta_{2\alpha}^{\rho\omega}. \tag{23}$$

The former equation tells us that the scaling dimensions y_α are indeed scheme-independent, as expected. The latter tells us that unless the couplings are all marginal, the second-order terms in the beta functions are not scheme-independent, since the tensor $\widetilde{\beta}_{2\alpha}$ depends on the values of the $f_{2\alpha}$s. A similar analysis of the $\mathcal{O}(g^3)$ terms in the equation for $\widetilde{\beta}_\alpha$ shows that unless both the first and second-order coefficients $\beta_{2\alpha}$ vanish, the third-order coefficients are

[5] Said another way, we can always perform a trivial multiplicative re-scaling of f by $f_\alpha \mapsto \frac{1}{c_\alpha} f_\alpha$ (this of course doesn't affect the scaling dims), so that $f_1 = \mathbf{1}$.

similarly scheme dependent. This then generalizes to the statement we claimed above, viz. that only the first non-zero coefficient is scheme-independent.

ethan: *actually, it turns out that the marginal case is somewhat different. Here one can show that (at least for the case of a single coupling) the first two terms in the beta function are invariant under the change of variables. This seems to be somewhat of an accident.*

One consequence of the non-universality of the higher-order terms in the beta functions is that for non-marginal scaling variables, it is generically possible to make a redefinition of the coupling constants such that $\widetilde{\beta}_\alpha = y_\alpha f_\alpha$. This means that in the absence of marginal couplings, we can generically find a parametrization of the critical point such that the linear scaling law $f_\alpha \to s^{y_\alpha} f_\alpha$ holds even for f_α of order 1. For simplicity, consider the case of a single coupling with scaling dimension y. The condition that f scale linearly is, in obvious notation,

$$y(g + f_2 g^2 + f_3 g^3 + \cdots) = (1 + 2f_2 g + 3f_3 g^2 + \cdots)(yg + \beta_2 g^2 + \beta_3 g^3 + \cdots), \tag{24}$$

which means that f will scale linearly provided that we choose $f_2 = -\beta_2/y$, $f_3 = \beta_2^2/y^2 - \beta_3/2y$, and so on. Similarly, if g is marginal but $\beta_2 \neq 0$, we can work in a scheme where $\widetilde{\beta} = \beta_2 f^2$, without any higher-order terms: the condition for this at lowest order is $f_3 = -\beta_3/2\beta_2$.

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