Physics II

1. Phases and colors

This is just an account of the existing theory of physics, using words and examples to try to make it seem more familiar.

Let's suppose we imagine a light wave somewhere, and we try to specify the color c of the light at each point of space.

It is observed that the frequency of an observed wave can depend, according to the Doppler shift, upon the motion of the observer himself.

We might initially think that at each point of space, the light might be considered to have a phase, and the color is the rate of change of the phase as the wave moves.

According to Schroedinger, Hamilton had considered that light should have a moving wavefront, and the speed of motion of the wavefront is faster than the speed of light, by the same proportion that the particle represented is slower than the speed of light.

But let's start with something very easy, an actually motionless wave in space

$$\phi(x) = cos(2\pi x)$$

and we allow the observation point to move, and then we would like to say that the color or frequency is just then the time derivative of x, that is, the speed of motion of the observer, as *he* passes the wave which is stationary and fixed in space

$$\text{color} = dx/dt$$

We can make more general wave fronts and phase patterns if we allow linear combinations of the coordinates, so in two coordinates we might say that the color observed is

$$f(x,y)dx/dt + g(x,y)dy/dt$$

and we might say that this is the color of the light at each point (x, y), if still the wave is not moving, and the observer is moving with velocity vector $(dx/dt, dy/dt)$.

Slightly more generally, it makes sense to work on the tangent bundle of whatever configuration manifold one is considering, and we continue to view functions like dx/dt and dy/dt as functions on the original manifold, but it is strictly more general to allow things like

$$fdx/dt + gdy/dt$$

where f and g are functions whose domain of definition is the tangent bundle. This includes the earlier case because we are allowed to let f and g be constant along the fibers.

But it remains true that x and y here have nothing to do with the tangent bundle, they are ordinary functions on the manifold. So we are still only describing what

we might think of as phase angles as functions on the manifold, and as we are working on the tangent bundle, we allow our function coefficients to be functions on the tangent bundle when we take linear combinations. A reason for doing it this way is that we might say that there is a vector field δ on the tangent bundle which describes the 'action.' I sort of use that word wrong but I'll explain a bit later why it is not so wrong to call this an 'action.' This is a derivation which we can call d/dt although we are not required to say what t is, or we allow that t might be different for different observers, or t might be undefined, but still the derivation acts on functions on the tangent bundle. Functions like x and y are allowed as we can think of them as constant on the fibers, and for any function x on the manifold, $\delta(x)$ makes sense. In fact if we want the derivation to be compatible with the tangent bundle structure, we can require

$$\delta(x) = dx$$

when x is a function on the manifold, viewed as a function on the tangent bundle constant on fibers.

Thus we have

$$\delta : x \mapsto dx \mapsto ?$$

This is incidentally the same as saying that if we ever have a one form, mapping it in the natural way to a one form relative to the base, sending this via the isomorphism to sections of the pullback from the base, and finally contracting against δ, all of this is the same as contracting against the Euler derivation. In Classical Mechanics primer this is the formula $i_\delta \eta = j$.

The slight generalization of the notion that color c should be a linear combination of $\delta(x_i)$ for x_i coordinate functions is that its differential $d'c$ should be the Lie derivative of a global section of the pullback of one forms from the base.

If we like we can start with just an arbitrary one form on the tangent bundle, and I do not specify where this should come from (it is the cotangent bundle which has a natural one form, not the tangent bundle, yet if we can identify the two via a Legendre transform not necessarily respecting zero sections, we can obtain a one form that way). We make no requirement about where the one form comes from.

A general one form on the tangent bundle just locally looks like

$$\sum_i r_i d' q_i + p_i d' dq_i$$

where q_i are required to be constant on fibers.

When we map to relative sections, we just go modulo

terms like the first term, and consider only

$$\sum_i p_i d' dq_i$$

When we apply the operator which identifies relative forms with sections of the pullback of one forms on the original base manifold, this just becomes

$$\sum_i p_i d' q_i$$

This is nothing but a slight formalization of what we had earlier when we considered the q_i to describe phase functions, the q_i are still our functions on the manifold, the p_i functions on the tangent bundle, but instead of directly applying δ we have just formally put d' there instead. So this is an expression that *contracts* to what we had before, under the action of δ.

That is, if we apply i_δ to the above expression we get back

$$\sum_i p_i \delta(q_i).$$

If any particular observer wants to say that he has a notion of time t so that $\delta = d/dt$ then he can write this contraction along δ as

$$\sum_i p_i d/dt q_i$$

and recover the earlier expression.

What is sometimes called the 'Lagrangian' condition is here something really easy. We wanted to say that our original one form was a Lie derivative of something like this, something in the pullback from the base. What actually happens when we take the Lie derivative of the one-form above, then?

We get

$$\sum_i \delta(p_i)d'q_i + p_i d'\delta(q_i)$$

$$= \sum_i \delta(p_i)d'q_i + p_i d'dq_i$$

And this is identical to the one form we started with, except the coefficients r_i are forced now to be $\delta(p_i)$.

That is, the condition that our original one-form was the Lie derivative of an unspecified section of the pullback of one forms from the base, is no different than saying that it was the Lie derivative of its own image, under the composite of projecting to relative forms and identifying relative forms with sections of the pullback from the base.

So we could have started with any one-form we believe to somehow be natural, and modified it by projecting it to a relative form, then mapping to a section of the pullback via the isomorphism, and finally taking the Lie derivative.

Or we could have equivalently just insisted that $r_i = \delta(p_i)$ for our choice of ω, or just replaced r_i by $\delta(p_i)$, noting that this change does not therefore depend on any choice of coordinates.

To summarize the two assumptions we've made: the only assumption we made about δ and ω is that whenever x is a function constant on tangent bundle fibers we must have

$$\delta : x \mapsto dx \mapsto ?$$

and that for our ω, we have $r_i = \delta(p_i)$.

The Euler contraction of ω is

$$\sum_i p_i dq_i$$

viewed as a function with domain the tangent bundle.

And now, our requirement that $\delta(q_i) = dq_i$ implies that this is the same as $\sum_i p_i \delta(q_i)$, the same expression which initially we used to describe a linear combination of phase angles, and which we derived in a different way subsequently.

This same expression is independent now of the choice of vector field δ. If the dq_i are suitably linearly independent and ω satisfies our hypothesis, the expression is determined by and determines the one-form ω itsself. There is no need to specify anything but the sequence of functions p_i locally, and since $\delta(q_i)$ is just the universal (=deRham) differential applied to q_i, we may think of ourselves when we choose the p_i as describing phase fronts, but our basis is no longer any absolute rates of change dq_i/dt, but rather the universal and invariant deRham differentials of the q_i.

Our rule is now describing the one-form $d'c$ not the function c.

It is exactly

$$d'c = \delta(\sum_i p_i d'q_i).$$

Instead of applying δ just to the q_i, as we did before, we are going to apply it as a Lie derivative to the whole one-form.

Writing

$$\delta = i_\delta d' + d' i_\delta$$

this is

$$d'c = i_\delta(\sum d'p_i \wedge d'q_i)$$
$$+d'(\sum p_i \delta(q_i))$$

If we wish to integrate this along a path, say, we have

$$c = \int i_\delta(\sum d'p_i \wedge d'q_i)$$

$$+ \sum p_i \delta(q_i).$$

The second term was our first attempt at a definition of color, it was path independent. But now our basic and reasonable assumptions force the first term – the correction to path independence if you like – to be the path integral of the contraction of a symplectic form.

We can work out this function

$$i_\delta(\sum_i d'p_i \wedge d'q_i)$$

$$= \sum_i \delta(p_i) d'q_i - p_i d'\delta(q_i)$$

$$= \sum_i \delta(p_i) d'q_i - p_i d'dq_i.$$

The second term is automatic, but note that from the equation before that this is also d' of a difference of two functions, and if we write it as $d'H$ then the term which is not automatically specified is $\partial/\partial q_i H = \delta(p_i)$.

Whenever the p_i are general enough that they can be used as local coordinates in the fiber direction, complementing the manifold structure of the base, we can figure out the dynamics of our moving point by that equation. We can determine our moving point by the rule that the q_i partial derivative of the difference between the color and our original definition of the color is the rate of change of p_i.

That same function is also the contraction of a symplectic form along our action vector field δ.

To determine the color change along a path, we integrate $d'c$ and this expression has two terms, the path integral of the contraction of the symplectic form along δ, and the change in our original approximation or attempt, that is, the change in the earlier formula.

We could also, if we wished, deduce orbits of the vector field δ from the formula for c, as c is in fact what is sometimes called the 'Lagrangian.'[1]

[1]In calling c the Lagrangian we are implicitly saying that H plays the role of the Hamiltonian. These ideas are currently unconnected with Dirac's notion that in quantum mechanics the Hamiltonian should act by Poisson bracket. A first inkling of what this would mean in relation to the vector field δ which we call the 'action' would be if we make Poisson brackets with respect to the two-form $\phi = d'\eta\omega = \sum d'p_i \wedge d'q_i$. Then the action of Poisson bracket against $d'H = i_\delta\phi$ is just the restriction of the Lie action of δ to closed forms $d'\Psi$. Thus what is sometimes called $\{H, \Psi\}$ or what we here would call $\{d'H, d'\Psi\}$, sends $d'\Psi$ to $d'\delta(\Psi)$, and it induces the action of δ itself on functions. In that sense, Dirac's notion of replacing multiplication by H with the Poisson action of H brings us back to our essentially arbitrary vector field δ. We then have to worry about the fact that for connecting ideas with Schroedinger's equation, $\{d'H, d'(\ -\ )\}$ is a derivation while the Laplacian is not quite a derivation, due to the presence of a cross term. Moreover, if wave functions Ψ are taken to be functions of the space coordinates (constant on fibers of the tangent bundle) then also δ does not preserve this space of functions, rather sends it into the disjoint space of $d\Psi$. The corollary that Dirac's formulation would require wave functions to have tangential dependence is *not at all* unconnected to issues of spin coordinates, and the three difficulties might be expected to mutually cancel each other out in a single correct understanding of this.

In conventional definitions of kinetic energy, our first approximation of the color corresonds to twice the kinetic energy (or twice its negative depending on conventions) while the function H corresponds to the total energy, and the color change along the path, with colors as viewed from the moving point, is interpreted as the difference between potential and kinetic energy.

The Feynman interpretation is that a three dimensional wave is comprised of a disjunction of such paths as this, and also as occur in a particle acceleration experiments, where the input is an accelerator, and the output is an observation such as a spectrograph, that is, input and output are one dimensional waves, and the instantiation of a three dimensional wave is in its existence as a complex probability distribution. It is possible to reformulate three dimensional theories of waves like this, by using appropriate probability distributions.

Note, however, that probabilities depend on what *you* know, and what *you* do not know. So that Feynman diagrams and probability distributions are uniquely tailored to particle acceleration experiments.

In the case of electromagnetic waves, we see that the formulation depends only on an assumption of a global involutive (sending x to dx) vector field on the tangent bundle, and any choice of one form on the tangent bundle – where we are allowed to ignore all but its projection to the relative one forms, or that it suffices to start with a relative one-form.

I have tended to call the vector field δ an 'action;' the component of δ which is quadratic in dx is a global symmetric connection on the tangent bundle; and the more correct terminology is to call the relativistic color c including Doppler shift the 'action.'

The orbits of δ minimize the path integral of this with respect to variations, this is the etymology of the word 'action' in this context. That is, if one wished to do so, one could define the dynamics of the observer using variational calculus starting with the form $d'c$.

It is really only a notion of color including doppler shift.

In Feynman's attempt to generate three dimensional wave functions, he allows an infinite superposition of functions

$$e^{2\pi ict}$$

where c is the frequency, and these are defined along paths.

Schroedinger's equation is a calculation of the eigenfunctions of the sum of the Laplacian plus a potential.[2]

[2]It is not quite rigorous to say that Schroedinger's equation deals with eigenfunctions of the Laplacian plus a potential term, as in his analysis it is really twice the mass times a potential term that occurs. Note though that twice the mass can be subsumed into the square of Planck's constant if one wishes to think of it as an actual potential.

Regarding special relativity, although light speed is considered to be absolutely bounded above, this does not affect experiment to the extent one might believe, due to unbounded time dilation. That is, special relativity does not contradict travelling to a star one light year away, while your clock records less than one year. Also the notion of an invariant Lorentzian metric on space-time appears to be mainly a convenience.

A weakness, however, in this existing formulation using the tangent bundle involves the artificial special role of the zero section, and the fact that electron spin really requires something more like a projectivized vector bundle.

The reason two quantum numbers are needed in ordinary electromagnetism is because the complexification of spherical coordinates is not the same as rectangular, one ends up needing to set two Riemann spheres side-by-side, and consider as algebraic wave functions differential forms on an infinitesimal neighbourhood of the diagonal. Existing theories can be improved numerically by considering that electron repulsion has an effect not only on one tensor factor.

However, rather than interpolating between two complexifications of Euclidean space, it might be better to find something analogous to the tangent bundle which would serve as a system of coordinates that physicists could use.

An algebraic fluid mechanics, on the other hand, requires a contractible divisor at infinity and it seems from Jeremy Gray's math history book that Poincare and his friends were looking for such things.

It is not quite right that people in spectroscopy add extra terms to the Hamiltonian, while people in theoretical physics ignore these extra terms, yet claim that they understand one of the terms and there is nothing else needing explanation. A similar issue arises in the way the 'complete' Hilbert space of Euclidean space is assumed even locally to be complete in a statistical sense, wnen even to get consistency between two types of quantum numbers seems to require asserting that there are two orthogonal classes of electromagnetic waves. That is, when one talks about probability in a particle accelerator experiment, there is a notion of completeness having to do with counting events, and assuming a complete set of possibilities. That is different than asserting more generally that all things can be deduced from understanding one or another interpretation of electromagnetism, or even asserting that partial theories now existing can be consistently adjoined to each other.

The notion that Feynman diagrams can calculate the Lamb shift is obviously an a posteriori calculation and has no statistical validity. It has political validity surely, as the governments had been expecting something like an improvement in thermonuclear reactions, and received instead an improvement in the smallest measurable energy level of Hydrogen. This is not to say that something which is politically admirable should be devalued conceptually, as intuition may become more subtle the more it is understood.

2. The classical Schroedinger equation

Regarding the 'classical' Schroedinger equation, it does seem that it can be repaired by allowing the electrostatic effect to affect 'spin' coordinates, and the fine stucture constant is not needed, nor is Planck's constant needed for the fine structure, only the permittivity of free space as a change of electromagnetic units.

An issue is that where one might have expected to apply a constant coefficient to the electrostatic perturbation matrix, and this does work well for the higher energy levels, yet the energy levels close to the ground level are the ones of spectrographic interest. Yet, the eigenvalues of Laplacian minus the central potential are expanded relative to each other near the ground level, by exactly the reciprocal of this coefficient for some reason.

The phenomenon is like when making an electronic amplifier using a high gain amplifier with a negative feedback circuit: the size of the feedback resistor is proportional to the gain.

If one wanted to approximate the highest energy levels, one might sensibly work relative to an energy level in which all electrons are somehow separated from the nucleus. But just for relevance, it is the supposed 'ground' level one wishes to approximate.

Here is an example of the main philosophical difficulty: if I were to suddenly switch off the electrostatic repulsion in an atom, does this mean the absolute total energy has increased, or decreased? We'd see the radius of the atom decrease, but how does one understand the change of total energy?

When one performs a perturbation analysis, the rate of change of the eigenvalues is all one knows about, but it is not clear what numerical scale is meant to compare this change with the energy levels, if the situation when the electrostatic repulsion is zero but changing is assumed.

One finds that by setting the proton charge such that the higher levels look least like a step function (minimizing second successive differences along sequences of constant l quantum number), the lower levels fall into place relative to the ground level.

To do this requires an iterative process, as the ordering of the energy levels, the correspondence between levels and term symbols, is unknown, and during the change, levels permute. Then the relevant levels to use are those which are higher after the permutation permutation – and the sequences of constant l are defined in reference to the term symbols, so are conjugated by the permutation – and so the calculation needs to be repeated iteratively before it converges to the correct value.

The result is that the ultraviolet spectrum is nearly correct, the visible also nearly correct, but shifted towards the ultraviolet range, and some infrared lines shifted downards and to the visible level. This reflects that the ideal proton charge to compensate for the perturbation approximation should have been less for higher energy levels but we make no attempt to do anything but choose a single value.

3. Alternatives to tangent bundle

Thurston noticed that if you take just the underlying real manifold of a compact smooth algebraic curve, you can still do intersection theory with particular (2 sided etc) smooth curves, the analog of the Neron Severi group only exists in its real projectivication and it is a circle bundle over the projectivication of the global sections of the line bundle associated to twice the canonical divisor.

If you just use the space of global one forms, then conformal automorphisms with respect to a metric act by projective linear automorphisms on the ambient projective space.

If you try this for twice the canonical divisor, this fails. That is, the smooth automorphisms act on a circle bundle over the projectiviation of the tensor square of holomorphic one forms, but this seems not to come from any linear representation at all. One cannot for instance use Hodge theory to calculate cohomology of $\Omega^{\otimes 2}$ without a connection on Ω (on the functions on the tangent bundle). What is missing is exactly that hypothesis

$$\delta : x \mapsto dx \mapsto ?$$

of the physics section!

There is some confusion between real and complex theories at this juncture though. For solutions of Schroedinger's equation, atomic spectra, all that was needed was $\mathbb{P}^1 \times \mathbb{P}^1$. Each factor is defined over the reals and represents a different element of the Brauer group.

Here, one might see the base restriction of an algebriac curve as having a two sheeted branched cover by the pair of complex conjugate copies $S \times S'$ somehow, and there is also confusion between infinitesimal neighbourhoods of the diagonal and tangent sheaves etc.

That is, it is not clear what mathematical models or theories would fit to make a link between Maths and Physics.

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