Book review "Covariant Loop Quantum Gravity" by Carlo Rovelli and Francesca Vidotto.

Johan Noldus*

March 23, 2016

Abstract

Out of interest, I took up the book of Rovelli and Vidotto to see how far covariant Loop Quantum gravity had evolved over the years and how its foundations are framed. I hoped that the authors had filled in the necessary technical details which was somehow the promise of the book, that it was an easy accesible, thorough introduction to the subject. This report is my impression of the first 180 out of 250 pages, by then I encountered several difficulties and issues with the theory which require further explanation.

1 Introduction.

My experience with the Loop Quantum Gravity programme is that of a critical observer and sideline participant and I have spent some amount of time on the canonical quantization programme some 15 years ago. By then, I concluded that this programme had no chance of producing any meaningful result due to the problem of time which I shall briefly explain later on: I was sure that quantum theory would have to be *modified* in order to take into account gravitational phenomena. More precisely, the novel language of quantum theory should be one of spacetime and not one of space; this could be accomplished by suitably modifying the path integral for example; hence my interest in causal set theory, decoherence functionals and causal dynamical triangulations where such modifications all take place. At first sight, the "loopies" did not seem to be very willing to think in such direction and were therefore stuck in a programme that certainly appeared to lead to nowhere, at least in my opinion. But times change, people also change to some extend, which was the main reason for me to take up this book and try to get a sense of its merit. Being someone who is willing to look into the details and speak positively about a programme as long as the details are fine even if I would suspect the overall enterprise to be somewhat misguided or falling short to closer scrutiny, I thought that I would certainly not be dissapointed. At least the book raised that hope to me when the authors were talking about transition amplitudes associated to a *spacetime* boundary formulation with boundary conditions at spacelike infinity if necessary so that the boundary closes. This is certainly a way to revive "time" so I

*email: johan.noldus@gmail.com, Relativity group, departement of mathematical analysis, University of Gent, Belgium.

thought they would start to speak about spacetime geometry, "timelike" spin-networks, $SL(2,\mathbb{C})$ holonomies attached to general paths and so on. That, of course, is not accomplished in the book and the reader gets that feeling very early on in chapter 7 when one explains the four dimensional spin foam theory. But nevertheless, this is not a technical error, but something which one would suspect to cause serious trouble in the theory later on as it smells like a violation of Lorentz covariance. I have tried to enter in a personal communication about these issues with the author, but for some reason things did not go very well. So, I will proceed in the best way possible and highlight those things I find important; these may or may not be precisely the issues other people have in mind. The subsequent book review is exclusively technical and not philosophical, given that I do somewhat sympathize with the conceptual framework: the comments range from matters of presentation to issues which do need a more substantial treatment.

2 Technical issues.

I will review some technical issues in the order in which they appear in the book and still find unsatisfying regardless of some exchanges I have had about them before. Some of these issues have been worked out in detail by me, others are treated on a more general level indicating that the authors should do their best to explain them properly: I leave it up to the reader to judge by himself which comments he finds substantiated and which are merely cosmetical.

2.1 The magical circle.

Here, at page eleven, one tries to give an answer to the question of how to quantize dynamical systems defined on a (group) manifold, this is certainly an interesting question in its own right which did not receive much attention in the literature. This book also does not develop a general theory but rather discusses the example of a particle on a circle. The circle can be seen as a one dimensional manifold with a nontrivial homotopy group: in such a case, standard wisdom says one needs to quantize the system on the universal cover of the circle, which is the real line and look at the eigenspaces of the translation operator over 2π. Given an eigenvalue e^{iq} of this operator, the corresponding "eigenvectors" are given by

$$\Psi(x) = e^{i\frac{qx}{2\pi}}\Phi(x)$$

with $\Phi(x+2\pi) = \Phi(x)$. This makes all sense since

$$|\Psi(x)|^2 = |\Phi(x)|^2$$

in either a well defined function on the circle and the Heisenberg relations hold as usual, that is

$$\left[i\frac{d}{dx}, x\right] = i1.$$

So, at least for the circle, we have a well defined procedure and q may in principle be an observable quantity; now, this does not answer the question for how to quantize on more general, simply connected, manifolds albeit there is a natural prescription to do so: cover your manifold by coordinate charts $(x^\alpha)_{i\in I}$, replace all your momenta by *covariant* derivatives ∇^i_μ and work with covariant Hamiltonians. To interpret the standard Heisenberg commutation relations, you introduce a partition of the unity f_i attached to this atlas and define the action on a global function g by means of the standard action on the local representatives $f_i g$. So, somehow, the question of defining a particle dynamics on an arbitrary manifold is connected to making quantum mechanics generally covariant. The authors of this book do not consider this most natural answer and prefer to keep a global coordinate θ: in that case, they note that at $\theta = 0, 2\pi$ the Heisenberg commutation relation is *not* satisfied since the differential produces a $\delta(\theta)$ function. They propose to remedy this by considering functions which are periodic in 2π, that is to quantize the relationship

$$\{p, e^{i\theta}\} = -ie^{i\theta}.$$

Now, for the circle, this can be done without any ambiguity whatsoever, simply replace $e^{i\theta}$ by the unitary multiplication operator $U\Psi(\theta) = e^{i\theta}\Psi(\theta)$. The reason why this procedure is unique is because $U(1)$ has precisely one irreducible (unitary) representation which is, moreover, one dimensional. For higher dimensional *compact* groups, one can set up many inequivalent Schrodinger pictures in this way giving rise to a quantization ambiguity. Note that the above quantization deliniates a deep distinction with the procedure for non-compact groups, such as the additive group $(\mathbb{R}, +)$ where the standard Poisson brackets hold

$$\{p, g\} = -1$$

where $g \in \mathbb{R}$ and therefore a real number. All this shows there is something fishy about this nonconventional quantization as we will flesh out in more detail later on.

2.2 The Holst action.

Here, we return to page to page 65 and clarify some claim; one works in the Cartan formalism where one considers the vierbein $e^a_\mu dx^\mu$ which may be seen as a one form valued local Lorentz vector and the one form valued Lorentz antisymmetric connection $\omega^{IJ}_\mu dx^\mu$ where indices are raised and lowered with respect to the flat Minkowski metric. The torsion tensor is defined as a two form valued Lorentz vector

$$T^I = de^I + \omega^I{}_J \wedge e^J$$

and the curvature two form F as

$$F^I{}_J = d\omega^I{}_J + \omega^I{}_K \wedge \omega^K{}_J.$$

The Holst action then is defined as

$$I = \int e^I \wedge e^J \wedge \left(\frac{1}{2}\epsilon_{IJKL}\, F^{KL} + \frac{1}{\gamma} F_{IJ} \right)$$

where γ accompanies the so called Holst term. The claim now is that this theory does not depend upon γ as variation with respect to the connection produces the torsionless condition. Closer inspection reveals that variation with respect to ω^{IJ} gives

$$\delta I = \int e^I \wedge e^J \wedge \left(\frac{1}{2}\epsilon_{IJKL} D\delta\omega^{KL} + \frac{1}{\gamma} D\delta\omega_{IJ} \right)$$

where

$$D\delta\omega^{KL} = d\delta\omega_{KL} + \omega^K_{\ J} \wedge \delta\omega^{JL} + \delta\omega^{KJ} \wedge \omega_J^{\ L}$$

is the covariant derivative of the Lorentz antisymmetric, one form valued, tensor $\delta\omega^{IJ}$. Reshuffling some terms and using that the variations on the boundary variables vanish so that the integral of $De^I \wedge e^J \wedge \left(\frac{1}{2}\epsilon_{IJKL}\delta\omega^{KL} + \frac{1}{\gamma}\delta\omega_{IJ} \right)$ dissapears, we get the equation

$$D(\frac{1}{2}\epsilon_{IJKL} e^K \wedge e^L + \frac{1}{\gamma} e^I \wedge e^J) = 0$$

which can be rewritten as

$$T^{[I} \wedge e^{J]} + \gamma \epsilon^{IJ}_{\ \ KL} T^{[K} \wedge e^{L]} = 0.$$

Now, one can show that for all values of $\gamma \neq \pm\frac{i}{2}$ that there are 24 independent equations and hence torsion vanishes; for the critical values of γ, torsion exists since one only has 12 conditions instead of 24. This is a slight refinement of the statement in the book, as I mentioned to Rovelli, and certainly no criticism. The following two points however are somewhat more serious.

2.3 Generators of symmetries.

Now we come to one of my two main objections to the construction in the book: from the Holst action above, it is easy to derive the Noether three form $C^{IJ}(x)$ associated to the local Lorentz transformations. Under an infinitesimal Lorentz transformation $\Lambda^K_L = \delta^K_L + \frac{1}{2}\alpha_{IJ}(\mathcal{J}^{IJ})^K_L$ they are defined by the following Lorentz valued *three* forms

$$C^{IJ}(x) = \left(\epsilon_{KLRS} e^K \wedge e^L \wedge \omega^{M[S} + \frac{2}{\gamma} e_R \wedge e_S \wedge \omega^{M[S} \right) (\mathcal{J}^{IJ})^{R]}_M$$

and the reader may easily verify that this expression is not gauge covariant as is the case for the Noether currents in non abelian gauge theory. Now, one should not confuse the Noether currents of a theory with the first class constraints associated to the symmetry transformations, the former generate a representation of the global gauge group which does not behave nicely under local gauge transformations while the latter generate the *local* gauge group and do transform covariantly. Also, it can be that the global generators are trivial: for example in vacuum electromagnetism, the charge attached to the conserved current vanishes exactly and provides a trivial representation of $U(1)$. At this point, since the Holst action is the starting point of the story, I would have expected a detailed symplectic analysis of it, or at least a summary of the main results; none of this appears in the book. There is no constraint analysis, no representation of the diffeomorphism group, no local Lorentz transformations and no study of the conserved Noether currents at all; this used to be the main work of Ashtekar when going from the ADM variables to the dreibein or triad. Indeed, here one obtains nice, gauge covariant, expressions for the Gauss constraint densities, the local generators of $SU(2)$ transformations. Just to give an impression to the reader that a constraint analysis will involve first and second class constraints, let us start by counting that there are 40 local variables e^I_a and ω^{IJ}_b. The symplectic transformation is maximally degenerate meaning all momenta determine constraint equations; indeed

$$\pi^a_I = 0 = \pi^0_{IJ}, \; \pi^i_{IJ} = \left(\frac{1}{2}\epsilon_{IJKL} e^K_{[a} e^L_{b]} + \frac{1}{\gamma} e^I_{[a} e^J_{b]} \right) dx^a \wedge dx^b \wedge dx^i$$

are all primary constraints. The equations of motion provide for the secondary constraints:

$$0 = \epsilon_{IJKL} e^J_{[b} F^{KL}_{cd]} + \frac{2}{\gamma} e^J_{[b} F_{|IJ|cd]}$$

giving the equations for e^I_a and

$$0 = -\frac{1}{2}\epsilon_{IJKL} d\left(e^K \wedge e^L\right) \wedge dt - \gamma d\left(e_I \wedge e_J\right) \wedge dt - 2\epsilon_{[I|SKL|} e^K \wedge e^L \wedge \omega_{J]}^{\;S} \wedge dt + 4\gamma e_L \wedge e_{[I} \wedge \omega_{J]}^{\;L} \wedge dt$$

for ω^{IJ}_0. The reader is invited to work out the remaining equations for ω^{IJ}_i and we know upfront we have only 10 secondary, first class, constraints (and therefore 20 in general) associated to the Lorentz and diffeomorphism gauge symmetries leaving plenty of second class constraints. Hence, we need to perform a Dirac bracket quantization which could be rather involving: nevertheless, one should just remind that one is going to find Lorentz covariant Gauss constraints which are again *spatial* densities. I do think a detailed treatment of these issues would certainly benefit the book so that we would understand why the following non-local observables satisfy a local algebra, it must be because one takes integrated versions of the Gauss constraints and this is not made clear at all. To see what I mean, let us first return to the Euclidean 3-dimensional theory and let us think a bit about formula (3.102) on page 78 before we jump to page 121 where part of the Poisson bracket group algebra brackets are "proven". The Poisson brackets (3.102) are *not* the standard brackets but reveal properties one would expect from the Dirac bracket; to my taste, the authors should be more precise in effectively showing that the momentum constraint on the connection is indeed of second class. The claim is indeed that one has found non-local expressions L^i_l which should satisfy the local $SU(2)$ Lie algebra structure and which are defined by one forms instead of two forms which one would expect from general considerations. Actually, the authors never provide an accurate definition of L^i_l and claim it is fine to compute Poisson brackets in a gauge. Now, I do not know what it means to calculate Poisson brackets in a gauge as the latter usually do not leave a gauge invariant, something which we will illustrate now. So let us take the "naive" definition, then it is indeed rather easy to derive, as the authors do that,

$$\{U_l, L^i_{l'}\} = 8\pi G \delta_{ll'} U_{l_+} \tau^i U_{l_-}$$

on which they impose the "gauge condition" that $U_{l_-} = 1$ which gives

$$\{U_l, L^i_{l'}\} = 8\pi G \delta_{ll'} U_l \tau^i$$

and gives the impression that the L^i_l are going to generate the Lie algebra indeed. However, both these formulae are *not* the same from the point of view of the

Poisson brackets, one can only make such evaluation *after* all brackets have been computed. Indeed, the very definition of $L^i_l = \int_{s_l} e^i$ would immediately suggest that

$$\{L^i_l, L^j_{l'}\} = 0$$

which is indeed consistent with our first formula since

$$\{\{U_l, L^i_l\}, L^j_l\} = 64\pi^2 G^2 U_{l_+}\{\tau^j, \tau^i\}U_{l_-}$$

which implies that

$$\{\{U_l, L^i_l\}, L^j_l\} - \{\{U_l, L^j_l\}, L^i_l\} = \{U_l, \{L^i_l, L^j_l\}\} = 0$$

where $\{\tau^j, \tau^i\}$ is the anticommutator of τ^i with τ^j. It is however not commensurable with $\{U_l, L^i_{l'}\} = 8\pi G\delta_{ll'}U_l\tau^i$ since applying the Jacobi relations there would need a nontrivial bracket for $\{L^i_l, L^j_{l'}\}$. Now, as is common for the constraints, one really needs a two dimensional integral instead of a one dimensional one to generate the algebra: this suggests one to give the following proper definition,

$$L^i_l = \int_{s_l} U^i_j e^j$$

where the U^i_j is the $SO(3)$ matrix associated to the $SU(2)$ holonomy defined by well chosen paths from a vertex v, where the local $SU(2)$ transformation "lives", to the point on the segment s_l. A quick calculation reveals that

$$\{U_l, L^i_l\} = 8\pi G U^i_j U_{l_+}\tau^j U_{l_-} = 8\pi G U_l \tau^i$$

since $U^i_j\tau^j = U_{l_-}\tau^i U^\dagger_{l_-}$. The Poisson bracket

$$\{L^i_l, L^j_l\} = 8\pi G\epsilon^{ijk}L^k_l$$

indeed as one can verify by relying upon the formula

$$\tau_i U^i_j v^j = U^\dagger(v^j\tau_j)U$$

and the fact that the commutator $[\tau_i, \tau_j]$ defines an infinitesimal rotation on the Lie algebra. Therefore, with some more care, everything works out as it should; again deepening the presentation along the lines suggested here would greatly benifit the book. The reader must wonder where my objection is staying since the above points are chiefly a matter of presentation and mathematical rigor; the problem resides in what one intends to do with the linear simplicity constraint explained on pages 67 and 68. The authors have a rather similar thing in mind as happened for $SU(2)$ and construct operators based upon two forms, which as we have seen for $SU(2)$ should be a three dimensional integration involving "pull backs" to the reference point. The formula they arrive at is that their boost vector K^i and angular momentum vector L^i, where i is a restricted Lorentz index running from one to three, should satisfy

$$K^i = \gamma L^i.$$

What I claim is that this conclusion would *not* hold if one were to use the correct formula for the boost and angular momentum generators; indeed, it is easily seen that the constraint cannot hold between generators of the Lorentz algebra since γ would need to be $\pm i$ (and the representation should be non-unitary) which is forbidden. Therefore, I do not know what the K^i and L^i represent and strongly feel that a detailed analysis would be in place. Hence, the construction of the Y_γ map on page 141 seems like an ultimate attempt to save what can be saved from this constraint and as "loopies" often do, they tend to implement them weakly instead of strongly. The way I came to the presentation of the above actually occured in that order, I quickly noticed that K and L could not be generators because of the simplicity constraint, and therefore had to work the $SU(2)$ construction through to see if nothing went wrong there already. I have some other, more general, remarks about chapter 7 too, but for now, we will go back and continue the discussion started at the beginning and comment further on group quantization.

2.4 Group quantization, generalizing the magic circle.

We refer to the defining Poisson brackets constituting the basis for group quantization at page 97 which are

$$\{U_l, U_{l'}\} = 0,\ \{U_l, L^i_{l'}\} = 8\pi G \delta_{ll'} U_l \tau^i,\ \ \{L^i_l, L^j_{l'}\} = 8\pi G \delta_{ll'} \epsilon^{ijk} L^k_l.$$

As I mentioned before, one could entertain the possibility of further making quantum theory more covariant, what this would mean regarding Dirac quantization is currently unknown and a research topic of this author. The authors still want to take Dirac quantization rather literally and propose to "straightforwardly" quantize this algebra, But what do these Poisson brackets mean, how should we read them? For example, if we literally want to keep U_l as an $SU(2)$ variable and L^i_l as a generator of the Lie algebra, then it might be natural to go over to $SU(2)$ valued wave functions such as Ψ^v which is a vector in the defining representation attached to a vertex v; if v were to be the initial vertex of U_l then the representation used is the defining one, if it were the final vertex, then one uses the adjoint representation, if it were none of those, the representation used is the zero representation of the Lie algebra. In this way, U_l is seen to define a unitary multiplication operator with respect to the scalar product

$$\langle \Psi | \Phi \rangle = \sum_v \langle \Psi^v | \Phi^v \rangle$$

and the reader may have fun in identifying the complex vector representation with the real quaternion algebra. The advantage of this construction is that the multiplication operator leaves the space of gauge *covariant* states invariant (where the covariance resides in the vector index) while the method in the book does not leave the space of gauge invariant states invariant. There is nothing unnatural about this construction and one may try to extend it to higher tensors (be careful that only one endpoint of each edge may appear): if one is liberal about one's quantization procedure why not think in this direction? The construction alluded to in the book, since it is never explicitely given, does not respect the wholeness of U_l and goes over to normal, nonunitary representations of the components of U_l in some spin j representation. More specifically, the wave function

$$\Psi(U_{l_1}, \ldots, U_{l_L}) = \psi^{a_1 b_1 \ldots a_L b_L}_{j_1 \cdots j_L} D^{j_1}_{a_1 b_1}(U_{l_1}) \ldots D^{j_L}_{a_L b_L}(U_{l_L})$$

gets mapped to

$$D^{j}_{AB}(U_l)\Psi(U_{l_1}, \ldots U_{l_L})$$

which in components reads

$$D^{j}_{AB}(U)D^{j'}_{ab}(U) = \sum_{k,c,d} \{j'jk; aAc\}\{j'jk; bBd\}D^{k}_{cd}(U)$$

where $\{j'jk; aAc\}$ is the standard 3-j Wigner symbol. This gives rise to an infinite number of inequivalent representations of the multiplication operator depending upon the spin j, all of which may be seen to represent normal operators (since the delta functions on the group constitute their eigenvectors) which do not leave the space of gauge invariant spin networks invariant. In short, we have an infinite number of inequivalent Schrodinger representations where the ambiguity resides in the multiplication operator only and ambiguities in the differential operator should still be accounted for. Given these features and the lack of motivation to prefer one picture over another, in either why not a quaternion representation, I remain very unsatisfied about this method of group quantization; it should be studied in more detail and alternative constructions should be mentioned.

3 More general comments.

I have no further technical comments up till page 180 and hold the opinion that the above comments on group quantization and identifying the correct Lorentz generators constitute serious enough objections which should be further answered. Perhaps, all those answers do exist but I cannot trace them back in the book and I do not think it is the obligation of the reader to make extensive computations or to research the literature. There is no reason why, in a four dimensional theory, the projection on a three dimensional surface should break the Lorentz group; actually, all group generators are spatial densities which should be integrated over a spacelike three surface to give the correct result. This is so for the full diffeomorphism algebra and is likewise the case for the Gauss constraint; also, in the three dimensional theory does a two dimensional integration not break $SU(2)$ to $U(1)$. One would expect the holonomies attached to edges to be general $SL(2,\mathbb{C})$ holonomies and this is unfortunately not the case which is an indicator that Lorentz covariance is broken. By not giving exact definitions of the algebra generators, the reader may get the wrong impression that the choice of paths in calculating the holonomy do not matter while it is so that the latter is very special indeed and one gets the wrong answer if the paths do not match. There is another issue which I did not comprehend that well, which is why the limit of large quantum numbers (spin in this case) should have something to do with the classical limit. This appears to be outright nonsense, it would be the same as saying that the limit of large momenta would determine the classical limit of QED or QCD; given that it is, moreover, not very clear what the physical significance of the spin network quantum numbers is, one cannot just confuse the limit $\hbar \to 0$ with the limit of $j \to \infty$. As always, I am willing to refine my views if further substantial evidence is given that the construction turns out the be all right but that is just not clear at all at this point.

References

[1] C. Rovelli and F. Vidotto, Covariant Loop Quantum Gravity, An elementary introduction to Quantum Gravity and Spinfoam theory, homepage C. Rovelli.