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Dear Deligne,

Below are my responses (to both your letters).

Response to the letter dated 23 Feb 2016.

– "*Why use exponentials?...*". It did occur to me recently that the higher order terms (≥ 2) do not influence the MI (multiplicative integral). Still, I wonder if by replacing the function exp with any other analytic function ϕ that has the same first order Taylor expansion, we will not cause problems with the crossed module structure? Also, can we find such ϕ that is functorial in G, besides exp ?

– "*Your choice of regularity condition...*". You are most likely right, and a much weaker condition on the integrand would work, at least for defining the MI in Chapter 4 of the book. I do not think that the more delicate properties (such as Stokes in dimensions 2 and 3, and thus invariance under partitions) would still be true for an integrand that is not piecewise smooth.

Frankly, my knowledge of differential geometry and analysis is pretty weak, so I tried to be safe and make sufficiently strong assumptions, so as not to fall into errors. There is no good literature (as far as I know) for "modern" differential geometry (i.e. in a style that directly interfaces with algebraic geometry). And there is even less literature on piecewise smooth geometry, or on manifolds with corners. (Not to mention "manifolds with sharp corners", including arbitrary convex polyhedra, for which almost everything works. As I understand it, a manifolds with corners has the extra condition of having normal crossings at the corners, which is only important for some needs.) Therefore I had to work out the basics for myself, in Chapter 1 of the book. Working with piecewise C^{∞} differential forms, and with piecewise linear geometry for certain aspects, seemed to be the safest way to proceed.

– "*I expect that using G-torsors...*". This is a very interesting idea. It reminds me of something that Larry Breen suggested to me several years ago, in one of our meetings. Only he was not as precise as you are now.

If I understand correctly, you are saying that if we replace the rather rigid setup of groups (the crossed module) with the more flexible setup of torsors, and perform a suitable quotient torsor operation, then we get something similar to the twisted abelian integral that I have in Def 8.3.12 of the book?

This approach might lead not only to a simplified 2-dimensional MI, but also to an accessible treatment of higher dimensional MI (where the crossed module is replaced by a crossed complex, à la R. Brown).

[1] This is a redacted version, typed on 12 May 2016, of my original reply to Deligne. Several paragraphs (of a personal nature) we deleted. Some mathematical errors were fixed.

Which reminds me – did you see Kapranov's paper:

[Ka] M. Kapranov, Membranes and higher groupoids, arXiv:1502.06166.

Here is a quote from page 4:

> An approach to 2-dimensional holonomy somewhat different from that of J. Baez and U. Schreiber (and thus from the approach of the present paper) was developed by A. Yekutieli [48]. That approach is based on direct approximation by "Riemann products", not on iterated integrals. It can probably be applied to the p-dimensional holonomy (situation of Chapter 3) as well. An approach involving the holonomy of connections on the space of paths (and thus using iterated integrals, albeit implicitly) was developed by J. F. Martins and R. Picken [38].

Response to the letter dated 25 Feb 2016.

I agree that the approach taken in Chapter 5 of the book, namely quasi-crossed module with additive feedback, is awkward, and unnecassarily so.

I think at the time (the book was written in 2010) I was trying to accomodate the situation described in Sections 5.6-5.7. Namely the base "manifold" X is the set $\mathrm{MC}(\mathfrak{g} \otimes \mathfrak{m})$, that is part of the Deligne crossed groupoid (i.e. strict 2-groupoid) $\mathrm{Del}(\mathfrak{g} \otimes \mathfrak{m})$ associated to a quantum type DG Lie algebra $\mathfrak{g}$ and an ideal $\mathfrak{m}$ in an artinian parameter ring R. Here we work over a base field $\mathbb{K}$ of characteristic 0, and R is an artinian local $\mathbb{K}$-ring, with maximal ideal $\mathfrak{m}$ and residue field $R/\mathfrak{m} = \mathbb{K}$. A DG Lie algebra $\mathfrak{g} = \bigoplus_i \mathfrak{g}^i$ is of quantum type if $\mathfrak{g}^i = 0$ for all $i < -1$. In this case the groups G and H (1- and 2-morphisms) are not isomorphic at various $\omega \in \mathrm{MC}(\mathfrak{g} \otimes \mathfrak{m})$, so apparently there is no "bundle of crossed modules".

However, this was probably an oversight. This is because when we start with a cosimplicial DG Lie algebra, and pass to the Thom-Sullivan model, our "base manifold" becomes a simplex.

We now take $\mathbb{K} = \mathbb{R}$. The presence of a compatible connection α (Def 5.2.4 in the book) over a nice base manifold, such as $(X, x_0) = (\boldsymbol{\Delta}^2, v_0)$, seems to force local triviality. As done in Sec 5.4 of the book, for any $x_1 \in X$ and a path ρ from x_0 to x_1, there is an isomorphism of crossed modules $\Psi(g)$. It is Fig 23 on page 94 of the book. Of course this isomorphism $\Psi(g)$ depends on ρ (if α is a tame connection, then the change is an inner conjugation by a 2-morphism). But if we choose a rule for ρ, such as the linear path from x_0 to x_1 of speed 1, then $\Psi(g)$ will probably depend smoothly on x_1, thus giving rise to a C^∞ trivialization. So in the end we do have a bundle of crossed modules, as proposed in your letter.

Needless to say, if I were to write the book today, I would go by your proposal...

Question. I wonder if I can show your handwritten letters to other mathematicians, if they become relevant to future research discussions?

Further remarks.

In the end, I did not use this MI to glue stacks, in my paper

[Ye1] A. Yekutieli, Twisted Deformation Quantization of Algebraic Varieties, Advances in Mathamatics, Volume 268, 2 January 2015, Pages 241-305.

Instead I took a more direct (though less beautiful) route, using three ideas:

(1) Extending the Goldman-Millson Invariance Theorem from

[GM] W.M. Goldman, J.J. Millson, The deformation theory of representations of fundamental groups of compact Kähler manifolds, Publ. Math. IHES 67 (1988) 43-96.

(that I understand is actually your idea) to *pronilpotent coefficients*, and to *unbounded DG Lie algebras.*

By "pronilpotent coefficients" I mean that the parameter ring R is a complete noetherian local ring over $\mathbb{K}$, with maximal ideal $\mathfrak{m}$, and with residue field $R/\mathfrak{m} = \mathbb{K}$; e.g. $R = \mathbb{K}[[\hbar]]$. Recall that previous treatments only condsidered nilpotent coefficients (i.e. the ring R was artinian).

By "unbounded" I mean that $\mathfrak{g}$ can have nonzero components in any degree (not just in degree ≥ -1).

This was done in the the paper

[Ye2] A. Yekutieli, MC elements in pronilpotent DG Lie algebras, J. Pure Appl. Algebra 216 (2012) 2338-2360.

Here is an ouline of what was done in [Ye2]. For any DG Lie algebra $\mathfrak{g}$ (without restriction on boundedness), and any complete parameter ring R, we have the usual Deligne groupoid

$$\mathrm{Del}(\mathfrak{g}, R) = \mathrm{Del}(\mathfrak{m} \mathbin{\widehat{\otimes}} \mathfrak{g}).$$

The set of gauge classes of MC elements is

$$\overline{\mathrm{MC}}(\mathfrak{g}, R) = \boldsymbol{\pi}_0(\mathrm{Del}(\mathfrak{g}, R)).$$

Now it is possible to produce a "nonabelian smart truncation of $\mathfrak{g}$" at -1, and from that to obtain the *reduced Deligne groupoid* $\mathrm{Del}^{\mathrm{r}}(\mathfrak{g}, R)$. There is equality

$$\overline{\mathrm{MC}}(\mathfrak{g}, R) = \boldsymbol{\pi}_0(\mathrm{Del}^{\mathrm{r}}(\mathfrak{g}, R)).$$

(I later found out that Kontsevich was also aware, in 1994, of this reduced groupoid.)

The proof of invariance theorem in [GM] extends to this situation, yielding the next result. (The tiny printed letters can be magnified on the screen to a comfortable size.)

Theorem 4.2. *Let $(R, \mathfrak{m})$ be a parameter algebra over $\mathbb{K}$, and let $\phi : \mathfrak{g} \to \mathfrak{h}$ be a DG Lie algebra quasi-isomorphism over $\mathbb{K}$. Then the function*

$$\overline{\mathrm{MC}}(\mathbf{1}_R \otimes \phi) : \overline{\mathrm{MC}}(\mathfrak{m} \widehat{\otimes} \mathfrak{g}) \to \overline{\mathrm{MC}}(\mathfrak{m} \widehat{\otimes} \mathfrak{h})$$

is bijective. Moreover, the morphism of groupoids

$$\mathbf{Del}^{\mathrm{r}}(\phi, R) : \mathbf{Del}^{\mathrm{r}}(\mathfrak{g}, R) \to \mathbf{Del}^{\mathrm{r}}(\mathfrak{h}, R)$$

is an equivalence.

The concept of reduced groupoid allowed me to prove the following in [Ye2].

Theorem 0.6. *Let $\mathfrak{g}$ and $\mathfrak{h}$ be DG Lie algebras, let R be a parameter algebra, and let $\Phi : \mathfrak{g} \to \mathfrak{h}$ be an L_∞ quasi-isomorphism, all over the field $\mathbb{K}$. Then the function*

$$\overline{\mathrm{MC}}(\Phi, R) : \overline{\mathrm{MC}}(\mathfrak{g}, R) \to \overline{\mathrm{MC}}(\mathfrak{h}, R)$$

is bijective.

The proof uses the bar-cobar construction to "linearize" ϕ. Note that even if we start with quantum type DG Lie algebras $\mathfrak{g}$ and $\mathfrak{h}$, the bar-cobar construction produces unbounded DG Lie algebras!

I should mention that there was a mistake in the proof of the invariance theorem in the paper [GM], that went unnoticed for thirty years:

Proof. The proof is very similar to the proof of "Full" in [4, Subsection 2.11]. (Note however that there is a mistake in loc. cit. In our notation, what is done there is referring to the obstruction class $o_1(\omega, \omega')$, but this is not defined since $p(\omega) \neq p(\omega')$ in general.) The proof is illustrated in Fig. 2.

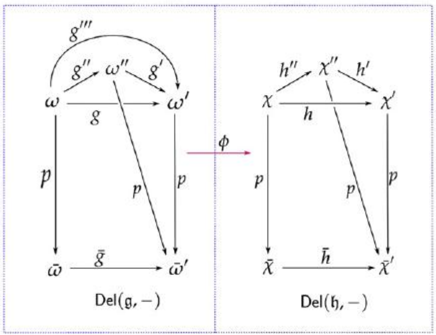

Fig. 2. Illustration for the proof of Lemma 3.11. Some of the arrows, like g and h, belong to the groupoid $\mathbf{Del}^{\mathrm{r}}(-,-)$. Other arrows, like g'' and h'', belong to the groupoid $\mathbf{Del}(-,-)$. The function ϕ sends $\omega \mapsto \chi$, $\bar{\omega} \mapsto \bar{\chi}$, $g \mapsto h$, etc. The whole diagram is commutative.

(2) Studying the *Deligne crossed groupoid* (i.e. 2-groupoid) for *quasi-quantum type DG Lie algebras* and for pronilpotent coefficients.

A DG Lie algebra $\tilde{\mathfrak{g}}$ is quasi-quantum type if there is a quasi-isomorphism $\mathfrak{g} \to \tilde{\mathfrak{g}}$ where $\mathfrak{g}$ is quantum type. (This is what happens in the bar-cobar construction.) In this situation (or if R is artinian) the Deligne crossed groupoid $\mathrm{Del}^2(\mathfrak{g}, R)$ exists, and

$$\boldsymbol{\pi}_0(\mathrm{Del}^2(\mathfrak{g}, R)) = \boldsymbol{\pi}_0(\mathrm{Del}(\mathfrak{g}, R)) = \overline{\mathrm{MC}}(\mathfrak{g}, R).$$

Using Theorem 0.6 of [Ye2] I proved this:

Theorem 6.13. *Let R be a parameter algebra, let $\mathfrak{g}$ and $\mathfrak{h}$ be DG Lie algebras, and let $\phi : \mathfrak{g} \to \mathfrak{h}$ be a DG Lie algebra quasi-isomorphism. Assume either of these two conditions holds:*

(i) *R is artinian.*
(ii) *$\mathfrak{g}$ and $\mathfrak{h}$ are of quasi quantum type.*

Then the morphism of 2-groupoids

$$\mathbf{Del}^2(\phi, R) : \mathbf{Del}^2(\mathfrak{g}, R) \to \mathbf{Del}^2(\mathfrak{h}, R)$$

is a weak equivalence.

(3) An equivalence theorem for cosimplicial crossed groupoids. A cosimplicial crossed groupoid G has "descent data", that are sort of nonabelian 1- and 2-cochains, combined together. They are just an abstraction of the familiar descent data for gerbes. Their gauge equivalence classes make up the the set $\overline{\mathrm{Desc}}(G)$ – that is a sort of nonabelian 1- and 2-cohomology.

In the paper

[Ye3] A. Yekutieli, Combinatorial Descent Data for Gerbes, J. Noncommutative Geometry, Volume 8, Issue 4, 2014, pp. 1083-1099.

I proved the following:

For a cosimplicial crossed groupoid $G = \{G^p\}_{p \in \mathbb{N}}$ we denote by $\overline{\mathrm{Desc}}(G)$ the set of gauge equivalence classes of descent data. The purpose of this note is to prove:

Theorem 1.1 (Equivalence). *Let $\Phi : G \to H$ be a weak equivalence between cosimplicial crossed groupoids. Then the function*

$$\overline{\mathrm{Desc}}(\Phi) : \overline{\mathrm{Desc}}(G) \to \overline{\mathrm{Desc}}(H)$$

is bijective.

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I have stopped working on deformation quantization and MI for the time being. But it would be great if someone else (a strong graduate student, or a young researcher) would take it upon him/herself to expand the nonabelian MI, perhaps along the lines of your ideas. And also to connect my work to that of Kapranov, Schreiber, and others. If you know or hear of such a person, I would be more than happy to assist him/her in their research on these matters.

Finally, I want to express again my delight that you took time to read my work and to comment on it.

Cordially Yours,

Amnon Yekutieli