Afterword

I was a student of Jean-Marie Souriau, working on my doctoral dissertation, when he introduced « diffeology ». I remember well, we used to gather for a seminar at that time — the beginning of the 1980s — every Tuesday, at the Center for Theoretical Physics, at Marseille's Luminy campus. Jean-Marie was trying to generalize his quantization procedure to a certain kind of coadjoint orbits of infinite dimensional groups of diffeomorphisms. He wanted to regard these groups of diffeomorphisms as Lie groups, like everybody, but he also wanted to avoid topological finessing, feeling that that was not essential for this goal. He invented then a lighter « differentiable » structure on groups of diffeomorphisms. These groups quickly became autonomous objects. I mean, he gave up groups of diffeomorphisms for abstract groups, equipped with an abstract differential structure. He called them « *groupes différentiels* », this was the first name for the future diffeological groups.

Differential spaces **are born**. Listening to Jean-Marie talking about his differential groups, I had the feeling that these structures, the axiomatics of differential groups, could be easily extended to any set, not necessarily groups, and I remember a particularly hot discussion about this question in the Luminy campus cafeteria. It was during a break in our seminar. We were there, the whole group: JMS (as we call him), Jimmy Elhadad, Christian Duval, Paul Donato, Henry-Hugues Fliche, Roland Triay, and myself. Souriau denied the interest of considering anything other than orbits of differential groups (Souriau was really, but really, « group-oriented »), and I decided when I had the time — I was working on the classification of SO(3)-symplectic manifolds which has nothing to do with diffeology — to generalize his axiomatics for any sets. But I never got the opportunity to do it. Sometime later, days or weeks, I don't remember exactly, he outlined the general theory of « *espaces différentiels* » as he called them. I would have liked to do it, anyway... I must say that, at that time, these constructions appeared to us, his students, as a fine construction, but so general that it could not turn out into great results, it could give at most some intellectual satisfaction. We were dubious. I decided to forget differential spaces and stay focused on « real maths », the classification of SO(3)-symplectic manifolds. I went to Moscow, spent a year there, and came back with a complete classification in dimension 4 and some general results in any dimension. This work represented for me a probable doctoral thesis. It was the first global classification theorem in symplectic geometry after the homogeneous case, the famous Kirillov-Kostant-Souriau theorem, which states that any homogeneous symplectic manifold is a covering of some coadjoint orbit. But Jean-Marie didn't pay any attention to my work, looking away from it, as he was completely absorbed by his « differential spaces ». I was really disappointed, I thought that this work deserved to become my doctorate. At the same time, Paul Donato gave a general construction of the universal covering for any quotient of « differential groups », that is, the universal covering of any homogeneous « differential space ». This construction became his doctoral thesis. I decided then to give up, for a moment, symplectic geometry and to get into the world of differential spaces, since it was the only subject about which JMS was able, or willing, to talk at that time.

The coming of the irrational torus. It was the year 1984, we were taking part in a conference about symplectic geometry, in Lyon, when we decided, together with Paul, to test diffeology on the *irrational torus*, the quotient of the 2-torus by an irrational line. This quotient is not a manifold but remains a diffeological space, moreover a diffeological group. We decided to call it T_α, where α is the slope of the line. The interest for this example came, of course, from the Denjoy-Poincaré flow about which we heard so much during this conference. What had diffeology to say about this group, for which topology is completely dry? We used the techniques worked out by Paul and computed its homotopy groups, we found $\mathbf{Z} + \alpha\mathbf{Z} \subset \mathbf{R}$ for the fundamental group and zero for the higher ones. The real line $\mathbf{R}$ itself appeared as the universal covering of T_α. I remember how we were excited by this computation, as we didn't believe really in the capabilities of diffeology for saying anything serious about such « singular » spaces or groups. Don't forget that differential spaces had been introduced for studying infinite dimensional groups and not singular quotients. We continued to explore this group and found that, as diffeological space, T_α is characterized by α, up to a conjugation by $\mathrm{GL}(2, \mathbf{Z})$, and we found that the components of the group of diffeomorphisms of T_α distinguish the cases where α is quadratic or not. It became clear that diffeology was not such a trivial theory and deserved to be more developed. At the same time, Alain Connes introduced the first elements of noncommutative geometry and applied them to the irrational flow on the torus — our favorite example — and his techniques didn't give anything more (in fact less) than the diffeological approach, which we considered more in the spirit of ordinary geometry. We were in a good position to know the application of Connes' theory on irrational flows as he had many fans, in the Center for Theoretical Physics at that time, developing his ideas.

All in all, this example convinced me that diffeology was a good tool, not as weak as it seemed to be. And I decided to continue to explore this path. The result of the computation of the homotopy group of T_α made me think that everything was as if the irrational flow was a true fibration of the 2-torus: the fiber $\mathbf{R}$ being contractible the homotopy of the quotient T_α had to be the same as the total space T^2, and one should avoid Paul's group specific techniques to get it. But, of course, T_α being topologically trivial it could not be an ordinary locally trivial fibration. I decided to investigate this question and, finally, gave a definition of diffeological fiber bundles, which are not locally trivial, but locally trivial along the plots — the smooth parametrizations defining the diffeology. It showed two important things for me: The first one was that the quotient of a diffeological group by any subgroup is a diffeological fibration, and thus $T^2 \to T_\alpha$. The second point was that diffeological fibrations satisfy the exact homotopy sequence. I was done, I understood why the homotopy of T_α, computed with the techniques elaborated by Paul, gave the homotopy of T^2, because of the exact homotopy sequence. I spent one year on this job, and I returned to Jean-Marie with that and some examples. He agreed to listen to me and decided that it could be my dissertation. I defended it in November 1985, and became since then completely involved in the diffeology adventure.

Differential, differentiable, or diffeological spaces? The choice of the wording « differential spaces » or « differential groups » was not very happy, because « differential » is already used in maths and has some kind of usage, especially « differential groups » which are groups with an operation of derivation. This was quoted often to us. I remember Daniel Kastler insisting that JMS change this name. From time to time we tried to find something else, without success. Finally, it was during the defense of Paul's thesis, if memory serves me right, when Van Est suggested the word « *difféologie* » like « *topologie* » as a replacement for « *différentiel* ». We found the word accurate and we decided to use it, and « *espaces différentiels* » became « *espaces difféologiques* ». There was a damper, however, « *différentiel* » as well as « *topologique* » have four spoken syllables when « *difféologique* » has five. Anyway, I used and abused this new denomination, many friends laughed at me, and one of them once told me, Your « *dix fées au logis* » — which means "ten fairies at home" — since then, there is no time when I say diffeology without thinking of these ten fairies waiting at home... Later, Daniel Bennequin pointed out to me that Kuo-Tsai Chen, in his work, *Iterated path integrals* [**Che77**] in the 1970s, defined « differentiable spaces » which looked a lot like « diffeological spaces ». I got to the library, read Chen's paper and drew a rapid (but unfounded) conclusion that our « diffeological spaces » were just equivalent to Chen's « differentiable spaces », with a slight difference in the definition. I was very disappointed, I was working on a subject I thought really new and it appeared to be known and already worked out. I decided to drop « diffeology » for « differentiable » and to give honor to Chen, but my attempt to use Chen's vocabulary was aborted — the word « diffeology » had already moved into practice, having myself helped to popularize it. However, it is good to notice that, although Chen's and Souriau's axiomatics look alike, Souriau's choice is better adapted to the geometrical point of view. Defining plots on open domains, rather than on standard simplices or convex subsets, changes dramatically the scope of the theory.

Last word? I would add some words about the use or misuse of diffeology. Some friends have expressed their skepticism about diffeology, and told me that they are waiting for diffeology to prove something great. Well, I don't know any theory proving anything, but I know mathematicians proving theorems. Let me put it differently: number theory doesn't prove any theorem, mathematicians solve problems raised by number theory. A theory is just a framework to express questions and pose problems, it is a playground. The solutions of these problems depend on the skill of the mathematicians who are interested in them. As a framework for formulating questions in differential geometry, I think diffeology is a very good one, it offers good tools, simple axioms, simple vocabulary, simple but rich objects, it is a stable category, and it opens a wide field of research. Now, I understand my friends, there are so many attempts to extend the usual category of differential geometry, and so many expectations, that it is legitimate to be doubtful. Nevertheless, I think that we now have enough convincing examples, simple or more elaborate, for which diffeology brings concrete and formal results. And this is an encouragement to persist on this path, to develop new diffeological tools, and perhaps to prove some day, some great theorem :).

At the time I began this book, Jean-Marie Souriau was alive and well. He asked me frequently about my progress. He was eager to know if people were buying his theory, and he was happy when I could say sometimes that, yes, some people in Tel Aviv or *in Texas mentioned it in some paper or discussed it on some web forum. Now, as I'm finishing this book and writing the last sentences, Jean-Marie is no longer with us. He will not see the book published and complete. It is sad,* diffeology *was his last program, in which he had strong expectations regarding geometric quantization. I am not sure if diffeology will fulfill his expectations, but I am sure that it is now a mature theory, and I dedicate this work to his memory. Whether it is the right framework to achieve Souriau's quantization program is still an open question.*