RESEARCH PROGRAMME

MICHAEL K. KINYON

The projects described in the first two sections take up nearly all of my current research time.

Coquecigrues of Leibniz algebras and Related Topics

Introduction. A *Leibniz algebra* is a "non-anticommutative Lie algebra", that is, it is a nonassociative algebra in which the multiplication satisfies a Jacobi identity, but not necessarily skew-symmetry. These were first introduced over a dozen years ago by Loday, and it has since been observed that they arise in various parts of mathematics. As an example, let V be a vector space, and on $V \times \mathfrak{gl}(V)$, define a bilinear multiplication by $(x, A) \cdot (y, B) := (Ay, [A, B])$ for $A, B \in \mathfrak{gl}(V)$ and $x, y \in V$. This example, which Alan Weinstein and I called a hemisemidirect product, turns out to be canonical in the sense that every Leibniz algebra can be embedded in a hemisemidirect product.

Lie algebras, of course, are the algebraic structures arising from "differentiating" the conjugation operation of a Lie group. Conjugation in groups generalizes to the notion of a *rack*. A rack $(S, \circ)$ consists of a set S with a left distributive operation $\circ$ such that for any $a, b \in S$, the equation $a \circ x = b$ has a unique solution $x \in S$. For an example not given by group conjugation, consider $V \times GL(V)$ with the product $(x, A) \circ (y, B) = (Ay, ABA^{-1})$. It turns out that the tangent algebra structure to a *Lie rack* is exactly a Leibniz algebra. For instance, if $V = \mathbb{R}^n$, the tangent Leibniz algebra of the rack $V \times GL(V)$ is $V \times \mathfrak{gl}(V)$.

Lie's "Third Theorem" (and Cartan's global version) constructs for each Lie algebra a corresponding Lie group. One of the outstanding problems in the theory of Leibniz algebras is to extend Lie's third theorem to Leibniz algebras, and this is the problem on which I am most actively working. Loday has dubbed the mysterious group-like object corresponding to a Leibniz algebra a "coquecigrue" (a medieval mythical beast). From the considerations above, it is natural to conjecture that a coquecigrue has some sort of associated rack structure.

There are various motivations for seeking coquecigrues; the most meaningful to me is that Leibniz algebras, in the guise of Leibniz and Courant algebroids, arise in Poisson geometry. Courant algebroids have a natural Leibniz algebra structure on their sections, and so there might be a corresponding "coquecigroid" to go with them. If this is so, such an object will have important implications for mechanics, because Jerry Marsden has shown that Courant algebroids provide a natural framework for certain formulations of Lagrangian mechanics with symmetry.

Previous Work. In a paper that appeared in *Amer. J. Math.*, Weinstein and I made an attempt at constructing coquecigrues: to every Leibniz algebra, we associated a smooth loop (quasigroup with identity element, a kind of "nonassociative" group) satisfying certain properties. However, our loop turned out not to be the coquecigrue, because in the special case where the Leibniz algebra is a Lie algebra, the corresponding loop structure is not a Lie group. (Roughly speaking, our construction integrates the adjoint representation of a Lie algebra, not the Lie algebra itself.) Nevertheless, the loop turned out to be interesting on its own, and it illuminated the difficulties with the general program.

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Current Work. In August 2003, I introduced a new partial solution to the coquecigrue problem. The structures are called *Lie digroups*. A digroup is a set with two semigroup operations satisfying certain compatibility conditions. A digroup is a group if and only if the two operations coincide. It is easy to show that the tangent algebra structure for a Lie digroup is a Leibniz algebra. However, not all Leibniz algebras can be obtained this way, which is why digroups are only a partial solution to the coquecigrue problem. Roughly speaking, the Leibniz algebras that occur as tangent algebras for Lie digroups are those which split over their ideals generated by squares. Unfortunately, many of the Leibniz algebras arising in applied mathematics do not have this property. My latest paper on this topic, "Leibniz algebras, Lie racks, and digroups" has been submitted; a preprint is available on the arXiv.

Future Work. Whatever the correct notion of coquecigrue turns out to be, it should reduce to the notion of digroup in the split case. Here are a couple of other approaches to the general problem I am considering. A differential-geometric approach is to try to generalize the path-space proofs of Lie's Third Theorem, as explicated, for instance, in the recent textbook of Duistermaat and Kolk. The problem here is that the "natural" generalization constructs a Banach Lie group of paths, but then there is no obvious quotient structure. Alan Weinstein has suggested that perhaps one should replace based paths with based smooth solutions of algebra-valued Maurer-Cartan equations on connected, simply connected, pointed manifolds. Another possibility is try to push extension theory. As a vector space, a Leibniz algebra is isomorphic to the direct sum of two Lie algebras, one of which is abelian. The Leibniz product is reconstructible from this data and a type of cocycle. "Integrating" this might give a coquecigrue corresponding to the Leibniz algebra.

There are other issues here one should consider. Leibniz algebras form a quadratic operad in the sense of Ginzburg and Kapranov, and there is a functor between the category of twisted Lie algebras over a quadratic operad to the category of formal groups over that operad. Roughly speaking, the formal group structures lives in the operadic dual. The dual of a Leibniz algebra is a chronological algebra, and these structures have turned out to be very important in control theory. It isn't clear (to me, at least) if formal groups over operads are relevant to Leibniz algebras, but it would not surprise me if the specialization of the Ginzburg/Kapranov theory to the Leibniz algebra setting revealed something like a "formal coquecigrue" structure.

Despite not being the complete answer to the coquecigrue problem, both racks and digroups are still of independent interest. Manuel Ladra (Univ. Santiago de Compostela) and I are working on determining the cotriple cohomology theory for augmented racks and digroups.

Loops and Quasigroups

Introduction. A loop is essentially a "nonassociative group". There are natural examples such as the space of relativisitic velocity vectors with Einstein's velocity addition law. This is not a group if nonparallel velocities are considered, but it is a loop, that is, it has an identity element and the left and right translation mappings are bijective. Another well-known example is the set of nonzero octonians; these form a loop under multiplication.

I was not originally interested in loop theory intrinsically, but only for its applications to other areas, such as differential geometry and differential equations. But in the last half dozen years or so, I have also been working on algebraic questions as well. Much of my work has involved the use of computer software, especially the equational reasoning tools OTTER and Prover9 and the finite model builder Mace4, both developed by William McCune. Group theory is too mature a subject for tools like these to be of much use, but loop and quasigroup theory is relatively young (about 80 years old), and many unsolved problems are amenable to computer attacks. Most of my work in this has been joint with Ken Kunen, J.D. Phillips, Tomaš Kepka, and Petr Vojtěchovský.

Since even the basic terminology of loop and quasigroup theory is unfamiliar to most mathematicians, it would fill several pages to explain exactly what it is we have accomplished. While I will try to fill in some informal definitions along the way, others I will simply to have to leave as buzzwords.

Previous Work. Kunen, Phillips, and I affirmatively solved a conjecture first posed in 1956: every diassociative A-loop is Moufang. The most well-studied class of loops are Moufang loops, which satisfy the identity $(xy)(zx) = (x(yz))x$. These loops include, for instance, the nonzero octonions. As the statement of the problem we solved suggests, many problems in quasigroup and loop theory involve trying to show that other loop varieties are related somehow to Moufang loops. Here a loop is diassociative if any two elements generate a group; an A-loop is one in which all the identity-fixing permutations generated by left and right translations are automorphisms.

Kepka, Phillips and I also solved an open problem first posed by Belousov in 1965: every loop isotopic to an F-quasigroup is Moufang. F-quasigroups were introduced in the mid-1940s, and are defined by certain associative-like identities. The study of quasigroups is often "reduced" to the study of loops via isotopy, and thus the more highly structured such loops are, the more structure the associated quasigroup variety will have. Solving the isotopy problem for F-quasigroups essentially solved

Outside of computer-based investigations, Phillips and I also worked with Michael Aschbacher on one form of the outstanding problem in loop theory: to find a finite, simple Bol loop which is not Moufang, or to show that no such loop exists. Bol loops are defined by the equation $x(y \cdot xz) = (x \cdot yx)z$. Moufang loops are Bol loops, and Moufang loops are, as indicated, well understood. Another important subvariety of Bol loops are Bruck loops, which satisfy $(xy)^{-1} = x^{-1}y^{-1}$. The space of relativistic velocity vectors forms a Bruck loop. The finite, simple Bol loop problem is open even for Bruck loops, but using the classification of finite, simple groups, Aschbacher, Phillips, and I made substantial progress toward the conjecture that every finite Bruck loop is solvable. Essentially, we showed that a minimal counterexample must be a simple 2-loop satisfying various obstructions. At this point, we do not know if such a loop exists. Our paper will be appearing soon in *Trans. Amer. Math. Soc.*

In a series of papers (one with Phillips), Kunen and I have been exploring the structure of conjugacy-closed loops (CC-loops). Such loops are defined by the property that the left multiplication permutations are closed under conjugation, and likewise for the right multiplications. We showed that every finite, nonassociative Moufang CC-loop (extra loop) has nontrivial center and has order divisible by 16. We also established Sylow and Hall theorems. More recently, we showed that every finite, power-associative CC-loop has order divisible by 16 or by 27, and we have classified all loops of those minimal orders. (Power-associative means that every subloop generated by one element is a group.)

I joined a project of Phillips and Vojtěchovský in to find constructions of C-loops, inverse property loops with squares in the nucleus. Right now, we are finishing up a paper in which we give general constructions of Bol loops in which the commutant (the set of elements commuting with all elements) is not a subloop.

Future Work. The most significant project right now is the study of the structure of Osborn loops. This is a broad class of loops that includes Moufang and CC-loops as special cases. I am convinced that an important aspect of the future of loop theory as a speciality lies in these loops. Perhaps the biggest result is that in an Osborn loop satisfying $(xy)^{-1} = x^{-1}y^{-1}$, the mapping $x \mapsto x^3$ is a centralizing endomorphism. This is a sweeping generalization of one of the foundational results of the theory of commutative Moufang loops. It will be interesting to see if this generalization has a combinatorial interpretation, just as commutative Moufang loops are associated to Hall triple systems.

Kepka, Phillips, and I have found that many of the fundamental isotopy results for F-quasigroups generalize to classes of quasigroups which include, as special cases, many of the well-known varieties. We are trying to use the isotopic description to understand the structure of these new classes.

Kunen, Phillips, and I have also been active in generalizing Moufang's theorem (diassociativity of Moufang loops) to broader classes of loops. We have one published paper on this, but the obstruction to publishing others is the construction of nontrivial examples. This is a hard problem even for computers, because nonMoufang examples tend to be quite large. For instance, we have been sitting on one long paper for over two years simply because we have no examples of the loops in question. We are working on general methods for constructing examples.

We are also working on the structure of A-loops. A not-so-famous open problem is to find a finite, nonassociative, simple A-loop. We have recently shown that such a loop of odd order must have odd prime exponent. However, we have no nonassociative examples of such loops.

Besides the above, there is a well-known (within the loop/quasigroup theory community) list of open problems maintained by Vojtěchovský. The biggest of these is the (non)existence of finite, simple, nonMoufang Bol loops, and there are others I hope to tackle with my collaborators in the future.

Second Tier Projects

These are projects that are, for various reasons, on hold. In most cases, it is due to lack of time (either mine or my collaborators) to complete them.

Modules for Symmetric Spaces. This is a project with Terrell Hodge. A symmetric space can be characterized algebraically as a manifold with a multiplication $\cdot$ satisfying $x \cdot (y \cdot z) = (x \cdot y) \cdot (x \cdot z)$, $x \cdot x = x$, and $x \cdot (x \cdot y) = y$. There is one additional topological condition: for any a, the equation $x \cdot a = a$ has a unique solution x in some neighborhood of a. The problem we are looking at is to determine what the correct notion of representation or module is for a symmetric space. There are several potential applications here. For instance, it is well-known in the theory of integrable systems that there is a correspondence between symmetric spaces and certain types of K-P equations. Although this statement is quite vague at the moment, it seems clear that understanding the possible modules for a symmetric space will say something about the corresponding equations.

Hodge has done some important work recently in the representation of the infinitesimal objects corresponding to symmetric spaces, namely Lie triple systems. What we would like to do is to integrate this to find modules over symmetric spaces, generalizing modules over groups. Abstractly, this is certainly possible: one can define a module over an object in any category to be an abelian group in the comma category over the object. The difficulty here is to figure out what this means in this setting, that is, what are the exact conditions on a vector space and on the action of the symmetric space on the vector space. Put another way, we would like to characterize abelian group objects in such a way as to "de-categorify" the definition of module over a symmetric space. Besides giving us a definition easier to follow, it should also be one which obviously differentiates to modules over Lie triple systems.

We think we almost have the correct definition, except that one of the conditions is still too technical to be useful. Once we have broken it down into something better, there are many directions this research project can go, all of which are important. Obvious issues include relating modules over symmetric spaces to modules over the various groups related to symmetric spaces, for instance, in their realization as homogeneous spaces. And of course, a description of irreducible modules will be useful. In a more functional analytic direction, there should be a notion of "positive definite Hermitian" representation which is exactly analogous to the notion of unitary representation of a compact group. More generally, one would suspect that modules for Hermitian symmetric spaces (or even Riemannian) are more highly structured than for general semisimple symmetric spaces. In addition, it is known that many symmetric spaces correspond to Jordan triple systems, and it would be useful to clarify the relationship between the modules for those structures and the modules for symmetric spaces.

By the way, we are not restricted to the smooth category here. We expect that whatever conditions we obtain will transfer over to the algebraic (or even finite!) categories with appropriate modifications.

The Nahm Equations and Related Structures. My main collaborator in this project is A.A. Sagle. Given a Lie algebra $\mathfrak{g}$, the Nahm equations are $\dot{x} = [y, z]$, $\dot{y} = [z, x]$, $\dot{z} = [x, y]$. These equations arise from special symmetry reductions of the Yang-Mills equations; the physical interest is primarily in solutions with special finite time blow up (these solutions are in a one-to-one correspondence with the moduli space of $\mathfrak{g}$-monopoles). Sagle and I have been studying the Nahm equations from the point of view of nonassociative algebras. Here is a brief description of this perspective works.

Quadratic differential equations are systems of nonlinear (autonomous, ordinary) differential equations which, after a standard change of variables, can be written in the form $\dot{x} = q(x)$ where $x \in \mathbb{R}^n$ and where $q : \mathbb{R}^n \to \mathbb{R}^n$ is homogeneous quadratic, i.e., $q(\alpha x) = \alpha^2 q(x)$ for all $x \in \mathbb{R}^n$, $\alpha \in \mathbb{R}$. Many differential equations arising in applications can be cast in this form, e.g., the Lorentz equations, the Rossler equations, various models from genetics or evolutionary biology, systems describing second order chemical reactions, etc. If we define a bilinear mapping $* : \mathbb{R}^n \times \mathbb{R}^n \to \mathbb{R}^n$ by $x*y = \frac{1}{4}(q(x+y)-q(x-y))$, then $q(x) = x*x$. Thus $(\mathbb{R}^n, *)$ has the structure of a commutative, (usually) nonassociative algebra, and following an idea of L. Markus from the 1960s, we consider the equation $\dot{x} = x * x$ to occur in this algebra. It is natural to conjecture that the behavior of trajectories is at least partially described or influenced by the structure of the algebra. This point of view is a bilinear analog of what happens in the linear case: the dynamics of linear (autonomous, ordinary) differential equations are entirely encoded within the theory of vector spaces acted upon by a single linear transformation (the Jordan form, etc.). Although the algebraic perspective has some obvious limitations, there are a number interesting special classes of algebras which arise in practice. The structure of such algebras (e.g., the presence of idempotent elements, ideals, subalgebras, etc.) gives information about the trajectories of the differential system.

For the Nahm equations, there is an interesting interaction between the Lie algebra structure of $\mathfrak{g}$ and the corresponding structure of the *Nahm algebra* $\mathfrak{g}\times\mathfrak{g}\times\mathfrak{g}$, where the latter has the commutative product induced by the differential equations, namely $X_1 \cdot X_2 = \frac{1}{2}\begin{pmatrix} [x_1, x_2] + [y_2, z_3] + [z_2, y_3] \\ [y_1, y_2] + [y_3, z_1] + [z_3, y_1] \\ [z_1, z_2] + [y_1, z_2] + [z_1, y_2] \end{pmatrix}$, where $X_i = (x_i, y_i, z_i)^T$, $i = 1, 2$. The Nahm equations themselves can be rewritten as $\dot{X} = X \cdot X$ in this algebra. The general question here is this: What can one learn about the Nahm equations from studying the structure of the Nahm algebra?

Physically interesting solutions are those with simples poles at $t = 0$ and $t = 1$ such that the limiting behavior of the Lie algebra spanned by the three components is a copy of $su(2)$. We have a description of these in terms of the Nahm algebra structure, at least as far as that structure contributes to a Laurent series description. In any case, we have just about finished a manuscript with some solid results about the Nahm equations, but there is much more to be done here, especially in a differential geometric vein.

A famous theorem of Nomizu states that the invariant affine connections on reductive homogeneous spaces are in a one-to-one correspondence with the equivariant bilinear multiplications on the tangent space to the coset of the identity. (Weinstein and I used this fact in the Leibniz algebra project.) The Nahm algebra structure thus induces a “Nahm connection” on the Lie group $G \times G \times G$. (Without going into details, it also induces a connection on the space $(G \times G \times G)/G$ where the subgroup modded out is $G \approx \{(g, g, g) : g \in G\}$.) Within this interpretation, the Nahm equations are exactly the *geodesic* equations for the connection, suitably lifted to the tangent space. This does not seem to have been noticed in the literature on the Nahm equation, and I think the following question is of interest: What is the relevance of the “Nahm connection” on $G \times G \times G$

to the Nahm equations? What does it mean for the physically interesting solutions to be geodesics for the Nahm connection?

Some tantalizing hints have been given recently by Atiyah and Bielinski. Their point is this: while the complexification of a Lie algebra leads easily to the complexification of its Lie group, there is no corresponding "quaternionization" of a Lie group G from the "quaternionization" of its Lie algebra. They believe that the Nahm equations themselves play that role. This is an interesting idea, and it would be nice to see if the Nahm algebra structure is related to their point of view.

A couple of other points: there is another algebra structure here on $g \times g \times g$ given by $XY = X \cdot Y - (1/2)[X, Y]$ where $\cdot$ is the Nahm product and where $[\cdot, \cdot]$ is the Lie bracket on $g \times g \times g$. This is a "noncommutative Nahm algebra", which is obviously Lie-admissible. Clearly there will be some connection between this and connections (pun intended) on $G \times G \times G$. In addition, much of our work on Nahm algebras should extend without too much work from Lie algebras to Malcev algebras. In this case, the 21 dimensional Nahm algebra of the seven dimensional, central simple Malcev algebra should tell us something about connections on $S^7 \times S^7 \times S^7$, either as a direct product of Moufang loops or a direct product of homogeneous spaces.

Dynamic Equations on Time Scales

Dynamic equations are a generalization of differential and of difference equations. The idea is to generalize simultaneously the domains of differential equations (intervals on the real line) and the domains of difference equations (the integers) to arbitrary closed subsets (so-called time scales). In many cases this is too broad to get a good theory, but there are specializations which still include both differential and difference equations.

I am currently working with Calvin Ahbrandt on Prüfer transformations for linear Hamiltonian systems in the time scale setting. What we have found is that all of the various Prüfer transformations have a very natural interpretation in terms of the conjugate symplectic structure on $\mathbb{C}^n$ which underlies the Hamiltonian system. Thus Prüfer transformations are, from a geometric point of view, quite natural. However, dynamically we have found is that this is one case where the continuous and discrete cases behave quite differently. In particular, if one considers discrete Prüfer transformations in, say, $h\mathbb{Z}$, and then takes the limit as $h \to 0$, one does not seem to recover the standard transformation for differential equations. This suggests that the "correct" generalization to time scales may be more difficult than has been suggested in the literature.

In another direction, I have been trying to generalize some results in oscillation theory for linear matrix differential equations that I obtained with W.J. Coles several years ago. Those results generalized some classical results of Hartman. It turns out that my old results, which were based on a Riccati equation approach, generalize (nontrivially!) to the dynamic equation case. I gave one talk on this, only to find out that Lynn Erbe was working on the same problem. So my results really need to be merged in some way with his. I have not had much time to do this, and there is one other impediment: the lack of good examples. Oscillation theory for differential or difference equations is filled with papers with give minor improvements to various oscillation critera, only to leave out any examples to which the improvements might apply. I do not like publishing papers of that type, and this is what is really holding up this project.

Stabilization and Optimal Control. This project has slowed to a standstill in the last few years, principally because both Tony Bloch and I have been a bit overcommitted. I am keeping it here in my research programme because I still think it is important, and perhaps someone reading this will be inspired. Bloch, Sergey Drakunov and I studied the feedback stabilization problem for a Lie algebraic generalization of the nonholonomic integrator (a.k.a., the Heisenberg system). Let $\mathfrak{g}$ be a real semisimple Lie algebra with Cartan decomposition $\mathfrak{g} = \mathfrak{h} \oplus \mathfrak{m}$ where $\mathfrak{h}$ is a compactly imbedded subalgebra and $\mathfrak{m}$ is the orthogonal complement of $\mathfrak{g}$ relative to the Killing form. The system we considered is $\dot{x} = u$, $\dot{Y} = [u, x]$, where $x \in \mathfrak{m}$, $Y \in \mathfrak{h}$ are the state variables and $u \in \mathfrak{m}$ is the control variable. This is a natural generalization of several systems arising in mechanics, and we conjecture that this system is a (local) canonical form for certain classes of systems living on symmetric spaces. (Brockett showed that this is true if the dimensions of the control space $\mathfrak{m}$ and the state space $\mathfrak{g}$ match up in just the right way.)

Since the control variable u lives in a proper subspace $\mathfrak{m}$ of the state space, it follows from Brockett's famous "no-go" theorem that the system cannot be stabilized by smooth, or even continuous, stabilizing feedback. We found a natural discontinuous stabilizing strategy with several remarkable properties. Roughly speaking, while one state variable is decreasing in norm, only its largest eigenvalue decreases, and the other state variable maintains constant spectrum. The proof that our strategy does indeed stabilize the system involves some rather delicate inequality estimates which are intimately related to the structure of the Lie algebra itself. Several problems have suggested themselves: Why does our algorithm seem to be robust despite the fact that it depends on explicit computation of eigenvalues? Second, what is the relationship between the geometry of the system and the selection of eigenvectors in the common cases where the geometric multiplicity is greater than 1? Is there a canonical choice of an eigenvector?

In another phase of the same project, Bloch and I are considering the corresponding two-point optimal control problem for this system; this arises naturally from subRiemannian geometry. Much is already known, especially in light of work by Brockett, and later, Sussman and Liu, for the Lie algebra $so(n)$. There is no doubt that everything extends formally to the general semisimple case, but the big question here is whether that generalization is meaningful from the geometric point of view? Part of the problem seems to be this: the natural subRiemannian structure induced by a Cartan decomposition seems to be more closely related to the "nilpotentization" of the Lie algebra than to its semisimple structure. There is also a related class of subRiemannian control problems on symmetric spaces which was studied by Jurdjevic, and it would be nice to know how the two classes are related.

Finally, there is also the question of stabilization and control of the so-called "dynamic" problem $\ddot{x} = u$, $\ddot{Y} = [u, x]$. This is an abstract problem which includes (after changes of variables) dynamic Heisenberg systems. At the moment, we have some reformulations of the problem, but no real progress. The geometry should tell us something about it once we straighten that out.

History of Mathematics

In recent years I have been taking an active interest in the history of mathematics. I served as Editor of the *Proceedings for the Canadian Society for the History and Philosophy of Mathematics* for two years. Glen Van Brummelen and I co-edited a volume of papers from the *Proceedings*–the Kenneth O. May Lectures–which was published by Springer. With regarding to my scholarship in the history of mathematics, I have a half-written paper on the early history of quasigroups and loops, especially as developed by American mathematicians in the 30s and 40s. Also, John Glaus and I were writing a paper about a little known paper of Euler regarding paradoxes of integral calculus. Although I have given (generally well-received) talks on both topics, I have been a bit too busy in the past few years to finish the manuscripts.

Department of Mathematical Sciences, Indiana University South Bend, South Bend, IN 46634 USA
E-mail address: `mkinyon@iusb.edu`
URL: `http://mypages.iusb.edu/~mkinyon`