RESEARCH STATEMENT

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My mathematical research mostly revolves around these two subjects:

(i) *computational algebra*, with a strong focus on computational group theory (CGT), including p-groups, small finite groups, and polycyclic groups; and

(ii) *algebraic Lie theory*, specifically concerning reductive algebraic and Kac–Moody groups, twin buildings, symmetric spaces and arithmetic groups.

In addition to these, a third major mathematical activity of mine, albeit one that is less concerned with generating papers, is

(iii) maintaining and improving the GAP computer algebra system [9].

I will elaborate on each of these three points in the following three sections

1. Computational algebra

In a nutshell, computational algebra deals with modelling algebraic objects in a computer, and developing *effective* algorithms to work with these models, which then are applied to resolve concrete mathematical problems. (As opposed to purely theoretical algorithms, which often are completely impractical for applications.)

In my Diplom thesis, I employed methods from computational group theory (by modelling certain amalgams as finitely presented groups, and then performing Todd-Coxeter coset enumeration to prove properties about these) to resolve the final open cases of the so-called Phan-type theorem for $\mathrm{Sp}(2n, q)$, a family of finite groups of Lie-type. This and later work lead to [H1, H2, H3, H4], all relying at least in parts on computational results. Since then, using computational methods for proofs or experiments (mostly using GAP, but at times also other tools like Mathematica, Singular, Sage, Magma, . . .) has been a key theme to my research, including that in algebraic Lie theory. More on that in the next section. Another example is [H12], which combines purely theoretical methods to deal with non-embeddable polar spaces, with computational methods to deal with “small” cases: the problem is again translated to questions about certain group amalgams, and these are settled by two different methods: via coset enumeration on the one hand; and via nilpotent quotients (using the GAP package NQ) and confluent rewriting system (using the GAP package kbmag) on the other hand.

Besides experimentation, I also work on computational theory and methods themselves. Initially this was done with a focus on polycyclic groups. These groups admit finite subnormal series with cyclic factors (hence the name), and allow for an effective solution to the word problem, and have many other interesting and nice properties, which make them quite amenable for computational methods, yet rich in a wide diversity of natural examples.

On the one end of the spectrum, we have finite polycyclic groups; this class coincides with the class of finite solvable groups. Among these, the p-groups, p a prime, play a special role. For a fixed prime p, the category of p-groups of class less than p is equivalent to a category of finite p-Lie rings; this is known as the Lazard correspondence (ultimately due to the BCH formula). Finite p-groups of order p^n have been classified for $n \leq 7$. They can be described by a finite number of families, parametrized by the prime p and in some cases a few additional parameters.

In work with Eick and Zandi [H8, H10], we studied the Schur multiplier of Lie rings, and how it relates to that of the corresponding p-groups. While implementing this, it turned out that most of the computations can be performed almost independently of the prime p, *i.e.*, a single finite computation can produce results about an infinite family of p-groups resp. p-Lie rings, for almost all primes p. This leads to:

Problem A. Devise a theory of "symbolic" p-groups resp. p-Lie rings, where p is not regarded as a prime in $\mathbb{N}$ anymore, but instead a free variable (hence "symbolic"), possibly along with several other parameters $x_1, \dots, x_k$. To formalize this properly, move from the category of Lie rings (= Lie $\mathbb{Z}$-algebras) to the category of Lie R-algebras, where R is a suitable parameter ring (such as $\mathbb{Z}[p, x_1, \dots, x_k]$, or $\mathbb{Q}(x_1, \dots, x_k)[p]$). Using this, provide a category theoretical underpinning for the above observation, and derive further algorithms for computing in families of p-groups resp. p-Lie rings without specifying the prime p. *E.g.* computations of central series, Schur multipliers, automorphism groups, and of descendants.

This in turn is motivated by the following:

Definition B. A function f from a set P of integers to the natural numbers is *PORC* (polynomial on residue classes), if there exists a natural number m such that $f(x)$ is a polynomial when restricted to each residue class modulo m, *i.e.*, to $\{a \in P \mid a \in k + m\mathbb{Z}\}$ for $0 \le k < m$.

Conjecture C (Higman)**.** Let $f_n(p)$ denote the number of groups of order p^n. Then f_n is PORC.

This is known to be true for $n \le 7$. This problem has been studied for a long time by many people, and I do not propose to solve it with computational methods. Still, I hope to be able to contribute to it in some ways. A major project I plan to tackle during the next few years, is to use symbolic methods to verify by (almost) purely computational means the classification of groups of order p^n for $n \le 7$, which so far consists of hundreds upon hundreds of pages of manual computations. If this proves successful, a natural course of action will be to extend this to $n = 8$; however, this is considerably harder.

Turning to infinite polycyclic groups, finitely generated torsion free nilpotent groups are polycyclic. Here, the Mal'cev correspondence takes the role of the Lazard correspondence (again, ultimately due to the BCH formula), providing a close connection to integral lattices in finite-dimensional rational nilpotent Lie algebras. This useful "linearization" trick can be used to prove the well-known fact that automorphism groups of polycyclic groups are arithmetic groups. While theoretical algorithms exist to compute them, they fall into the category of "mostly useless in practice". It is therefore a highly interesting problem to explicitly and effectively compute these automorphism groups, viewed as arithmetic groups. In [8], the isomorphism problem for torsion free nilpotent groups of Hirsch length at most 5 has been solved, which yields a very nice and explicit description of their automorphism groups. Together with Bettina Eick, I am now working on extending this result to Hirsch length 6, and at least in certain special cases to arbitrary Hirsch length. This is an important stepping stone towards the following very difficult overarching problem (which is not necessarily a goal of its own, but it informs the general direction of the research):

Problem D. Determine the automorphism group of an arbitrary polycyclic group. Failing that, at least determine a group in the same commensurability class.

Lastly, together with Bettina Eick and Alexander Hulpke, I have been working on extending the classification of small groups (see *e.g.* [2]) from groups of order ≤ 2 000 up to order ≤ 20 000 and beyond, *cf.* [H9, H16]. Currently, I am preparing two GAP packages based on this work; one with the resulting database of groups; and one providing implementations of methods for counting and constructing non-solvable groups described in the (sadly unpublished) PhD thesis [1].

2. Algebraic Lie theory

Classic Lie theory deals with Lie groups, viewed as manifolds; it relies heavily on analytical methods and differential geometry. In algebraic Lie theory, these are replaced by algebraic tools; in place of Lie groups (as manifolds), one studies algebraic groups (as varieties or schemes), Hecke algebras and other algebraic structures. Kac–Moody groups then arise as "infinite dimensional" generalizations of semisimple algebraic groups over arbitrary fields. One major motivation for studying them is the interest of theoretical physicists in Kac–Moody algebras and groups, *e.g.* for unifying quantum theory and gravitation via M-theory. More broadly speaking, I also count objects derived from or closely related to the aforementioned objects as being part of algebraic Lie theory; this includes *e.g.* geometrical ones like symmetric spaces and Moufang buildings, but also finite groups of Lie types and (in the widest sense) Coxeter groups.

As with computational algebra, my involvement with this subject can be traced back to my Diplom thesis and the resulting publications. As in that thesis, there are many examples where I actually combined my expertise in the two different areas, even though this is not always visible anymore in the final paper. To give a concrete example, consider the following result, jointly obtained with Matthias Grüninger and Bernhard Mühlherr in [H14]:

Theorem E. *The unipotent horocyclic group of a Moufang twin tree of prime order is nilpotent of class at most 2.*

We would never have dared to hope this might be true in such wide generality, much less attempted to prove it, had not my computer experiments suggested so, and given us an idea how to tackle it.[1] These experiments involved devising and implementing a custom variation of the polycyclic collection process, albeit on infinitely many generators and with "symbolic" presentations (*i.e.*, depending on various generic parameters which are not specified, but rather treated as variables, leading to polynomial expressions for the exponents in normal forms), coupled with Gröbner basis computations over $\mathbb{F}_2$, which were done in Singular. This is a paper were I am both proud of the computational aspects (even though they do not appear in the paper), as well as the purely theoretical results, and I think it clearly shows the value of having strong computational methods available.

Currently, we are working on a manuscript in which we provide a classification of sorts for trivalent Moufang twin trees (*i.e.*, whose prime order is 2, hence the valency is 3). We then proceed to prove that there are uncountably many such trees whose automorphism groups are virtually simple infinite groups.

Besides all the computational work, a substantial amount of my research still has a strong theoretical flavor involving only pen-and-paper calculations. Most of my PhD thesis and the publications [H5, H6] resulting from it, as well as several later works such as [H13, H15, H17], do not involve computational methods, not even for experiments. I plan to keep working on this line of research, which I deeply enjoy.

That said, the lack of experiments in these papers is not much caused by philosophical musing, but rather caused by a lack of suitable tools. As such, I am highly interested in developing computational tools which can be employed to study Kac–Moody groups, as well as the associated twin buildings and symmetric spaces. To this end, I propose the following:

Problem F. Develop and implement effective methods to perform computations in split Kac-Moody groups of non-spherical non-affine type over finite fields (or even arbitrary computable fields such as $\mathbb{Q}$). In particular, provide normal form computations with respect to the Bruhat decomposition, or at least almost-normal forms together with an algorithm for deciding equality.

This extends (at least in spirit) prior work by Cohen, Haller, Murray and Taylor, *cf.* [6, 7]. The key insight is that a split Kac-Moody group admits a refined Bruhat decomposition

$$G = BNB = UNU \cong U \times T \times W \times U,$$

[1]The reason for this being that there is a closely related open problem, which has been studied by many people, on the root groups of Moufang building of rank 1: All known examples there are indeed nilpotent, but proving this in general seems hard. Also, most known examples are nilpotent of class at most 2 as well, though eventually examples with class 3 were discovered.

where U is a the "unipotent radical" of a Borel subgroup B, T a maximal torus in B with normalizer N, and $W \cong N/T$ a Coxeter group. In the "classical" setting W is finite; here we are primarily interested in the case where W is infinite, *e.g.* hyperbolic. The action of T and W on U is well-known and can be effectively computed. Hence, computing normal forms in G can be reduced to computing normal forms in U, T and W.

- The torus T is abelian, and so computing normal forms is easy.
- For W, Brink and Howlett [3] showed that Coxeter groups are automatic, which implies that the word problem has a quadratic solution. Together with work by Casselman [4, 5], this allows for an effective solution of the word problem in infinite Coxeter groups. However, this is not currently implemented in a publicly available package for GAP and hence this will have to be implemented as part of the project.
- If $|W| < \infty$, then U is a nilpotent group, and over finite fields is even a p-group; in any case, collection provides an efficient way to compute normal forms in that case. The situation is more complicated when $|W| = \infty$, as then U is never nilpotent (it contains non-abelian free subgroups), but is a direct limit (or amalgam) of nilpotent groups. This is by far the hardest part of this project, and whether the desired normal form can be achieved is not yet clear. Barring that, however, for many applications, it is sufficient to have an algorithm which decides equality of two given words in U, without using a normal form; or equivalently, whether a given word describes the identity. This is considerably easier. One approach here is to exploit the action of U on the twin building $\Delta = (\Delta_+, \Delta_-)$ associated to G, as U stabilizes a chamber c in Δ_+, but acts sharply transitively on the set of chambers in Δ_- opposite to c; we thus can decide whether an element is the identity by studying the orbit of a single chamber opposite c. While Δ_- is infinite, in order to study the action of U on it, it suffices to construct suitable finite subsets of Δ_-. By using a clever parametrization of the needed parts of the building, this "local action" then is sufficient to decide whether a given word describes the identity or not.

3. Work on mathematical software

A third major mathematical activity of mine, albeit one that is less concerned with generating papers, is maintaining and extending the GAP computer algebra system [9]. This is on the one hand a service for the wider community, as all researchers in the area of computational algebra who rely on GAP benefit from this. On the other hand, it is also foundational for my actual research in CGT, as this relies heavily on GAP. As such, there is an active interplay between my research in algebraic Lie theory, in computational algebra, and my work on GAP and the wider GAP ecosystem (*e.g.* GAP packages).

One of my major projects in 2017 with respect to GAP in 2017 was the integration of HPC-GAP back into GAP (joint work with Alexander Konovalov, Chris Jefferson and Markus Pfeiffer). HPC-GAP is a fork of GAP for "high performance computing" (bringing to GAP support for multithreading, cluster computing etc.), and was originally developed in St Andrews for, but as a fork of GAP it was not easily accessible for the majority of users and developers of GAP alike. With the imminent release of GAP 4.9 to the public, this integration work will be completed, and HPC-GAP will be available to a wider audience. However, it will still be marked as beta, and more work to make it a hassle free HPC solution for everybody will be required, which is one of the GAP-related activities I plan to pursue in the future.

Another is integration with other computer algebra systems: While GAP excels at computations related to group theory, it is often behind the state of the art when it comes to problems from other areas of mathematics, such as number theory, which quite frequently pop up naturally in the course of working on group theoretical problems (and of course the converse also frequently happens). This is not a big surprise, given that GAP is developed primarily by group theory experts, who, even if they had the expertise, only do number theory as a side aspect of their central research interests.

Hence, a major interest of mine in this regard is to improve the integration and collaboration between GAP and other computer algebra systems, such as SINGULAR, NORMALIZ and others, so that everybody can benefit from the expertise of others in the relevant fields. As such, I am one of the principle authors of interfaces for the two aforementioned software projects in GAP, both of which were supported by SPP 1489.

More recently, I have also been active within the OSCAR project, which is highly interesting to me, as it pursues exactly the goals of integration I described above; in addition, there are various other potential benefits for GAP, such as the hope of making it possible to use the programming language JULIA for extending GAP, enabling GAP developers to implement speed sensitive core parts of algorithms in another high level language (instead of C or C++, as is currently the case), without having to give up on the rich ecosystem of GAP and GAP packages. Thus, I plan to continue the collaboration with the OSCAR project and other activities in the SFB-TRR 195, and *e.g.*will continue to join workshops and developer sprints for it.

For the past few months, I have also been supervising a part of the OSCAR project which attempts to integrate the JULIA garbage collector into GAP which is a key requirement to writing a highly efficient bidirectional interface between GAP and JULIA. We recently made good progress on that, and I look forward to further work on GAP related aspects of OSCAR.

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