The Dixmier-Moeglin equivalence for D-groups

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What is the Dixmier-Moeglin equivalence?

The Dixmier-Moeglin equivalence deals with giving a characterization of the primitive ideals in a ring R.

But what are primitive ideals?

A two-sided ideal P of a ring R is called *primitive* if it is the annihilator of a simple left R-module.

For example, if we take $R = M_2(\mathbb{C})$ and we take the left module M to be all column vectors $\mathbb{C}^2$, then M is a simple left R-module and we can see that the annihilator of M is zero, because if A is a 2×2 matrix such that $Av = 0$ for every $v \in M$ then $A = 0$. This means (0) is a primitive ideal of R.

In general, primitive ideals are always prime ideals but prime ideals need not be prime. For example, in the polynomial ring $\mathbb{C}[x]$ the ideal (0) is not primitive. I'll leave that as an exercise if you haven't seen this.

Warning: I'm cheating by defining primitive ideals to be annihilators of simple left modules. Technically, these are called *left primitive* ideals and right primitive ideals can be defined analogously. George Bergman, while he was still an undergraduate, gave an example of a ring in which the zero ideal is left primitive but not right primitive.

Bergman's example, it should be noted, is somewhat strange and pathological. For most noetherian rings one encounters in practice left primitivity and right primitivity coincide, and so we shall not make any distinction.

Facts about primitive ideals

- Every ring with 1 has at least one primitive ideal.
- Primitive ideals are always prime ideals.
- In a simple ring (0) is the only primitive ideal.
- In a commutative ring primitive ideals are precisely the maximal ideals.
- (Jacobson density theorem) If P is a primitive ideal of a ring R then R/P embeds "densely" in a ring of linear endomorphisms $\mathrm{End}_D(V)$ of a left D-vector space over a division D.

So why should I care about the primitive ideals of a ring R?

I'll give you two reasons.

The first reason is that one often understands a ring via its irreducible representations (i.e., the simple modules). For example, this is an important part of finite group theory with the representation theory of finite groups and many local-global principles for commutative rings can be interpreted in this framework.

This works well when dealing with finite groups but with noncommutative rings it is often an intractable problem to understand the simple modules, so Dixmier proposed that one should instead try to obtain a coarser understanding by understanding the primitive ideals (the annihilators of simple modules) instead.

So we can think of finding the primitive ideals of a ring as giving a "coarser" version of what we do when we find all the irreducible representations of a finite group. One can also hope that this information will then help us with structure theory problems about the ring.

To give a slightly better picture, a big part of Jacobson's way of studying many ring theory problems dealt with reducing problems to the primitive case.

What he'd do is that he'd try to argue that one could assume that the Jacobson radical is trivial. For rings with trivial Jacobson radical, we know something strong: R embeds in a direct product of rings of the form R/P with P a primitive ideal; then by using projections one can often reduce to just understanding the rings R/P.

Finally, he could often use the Jacobson density theory to reduce questions about R/P to linear algebra. (Admittedly, infinite-dimensional linear algebra and over division rings, but nothing's perfect ....)

The second reason is on somewhat aesthetic grounds, but I believe it is a valid reason.

Recall that the primitive ideals in a commutative ring are precisely the maximal ideals. Algebraic geometry tell us that if k is an algebraically closed field and A is a finitely generated commutative k-algebra then the maximal ideals correspond to the points of an affine variety X in a natural way.

Just as in the commutative case, the set of prime ideals, $\mathrm{Spec}(R)$, of a ring can be endowed with a topology (the Zariski topology), and the primitive ideals form a distinguished subset of this space.

So we can think of the collection of primitive ideals (the primitive spectrum) of a ring R as being a topological space in the same way and it is often the case that interesting classes of rings have interesting primitive spectra, just as in the case with commutative rings.

Example

If we take the ring $\mathbb{C}\{x, y\}$ with relation $xy = 2yx$ then one can show that the primitive ideals are the zero ideal along with the ideals $(x, y - \alpha)$, $\alpha \in \mathbb{C}^*$, $(x - \beta, y)$, $\beta \in \mathbb{C}^*$, and (x, y).

If we instead used the relation $xy = yx$, we'd get the polynomial ring in two variables and the Nullstellensatz tells us that the primitive ideals correspond to points on the plane via the correspondence $(x - \alpha, y - \beta) \mapsto (\alpha, \beta)$. In our first case we are just getting the points $(0, \alpha)$ and $(\beta, 0)$, i.e., the x- and y-axes, along with a "dense" point (0).

So hopefully I've convinced you that understanding the primitive ideals is worthwhile. The next question is: How do we identify the primitive ideals?

We know the primitive ideals are all prime ideals, so the question becomes understanding which elements of $\mathrm{Spec}(R)$, the prime ideals of R, are primitive.

The Dixmier-Moeglin equivalence

The Dixmier-Moeglin equivalence is a result (obviously due to Dixmier and Moeglin), which characterizes the primitive ideals in $\mathrm{Spec}(R)$ when R is the enveloping algebra of a finite-dimensional complex Lie algebra. It gives two equivalent conditions to being primitive, one topological and one algebraic.

Theorem: (Dixmier-Moeglin) Let R be the enveloping algebra of a finite-dimensional complex Lie algebra. Then for a prime ideal P of R the following are equivalent:

- P is primitive;
- $\{P\}$ is locally closed in $\mathrm{Spec}(R)$;
- P is *rational*.

What is locally closed?

You might know what it means for a set in a topological space to be locally closed: it just means that it's an intersection of a closed set and an open set. Well, $\mathrm{Spec}(R)$ is a topological space with the Zariski topology so for $\{P\}$ to be locally closed it means that the set of all primes properly containing P has to be closed.

In practice, this just means that there is some element $a \in R \setminus P$ such that a is in *every* prime ideal that properly contains P. For example, in the ring $\mathbb{C}\{x, y\}$ with relation $xy = 2yx$ you can check that every prime ideal properly containing (0) contains the element xy.

What is rational?

Rational is a bit more fun. We all know that a commutative domain has a field of fractions. As it turns out, there is a more general fact due to Alfred Goldie:

If S is a prime noetherian ring then S has a "noncommutative field of fractions," $\mathrm{Frac}(S)$, which we obtain by inverting the nonzero divisors in R. This ring is obviously not a field but it is the next best thing: it's a simple Artinian ring, so it is isomorphic to $M_n(D)$ for some $n \geq 1$ and some division ring D.

If R is a algebra over the complex numbers then we say that P is rational if $\mathrm{Frac}(R/P)$ has centre equal to the complex numbers (the base field of R).

For the rest of this talk, $k = \mathbb{C}$, and R will be a finitely generated associative (but not necessarily commutative) noetherian k-algebra. In this context we always have the following implications:

$$P \text{ locally closed } \implies P \text{ primitive } \implies P \text{ rational.}$$

So the interesting direction is whether rational prime ideals are locally closed.

When this final implication holds, we say that the ring R satisfies the *Dixmier-Moeglin equivalence*, DME for short.

So what happened after the work of Dixmier and Moeglin?

People started to notice that the equivalence of Dixmier and Moeglin holds more generally. It was shown that the DME holds for:

- finitely generated algebras satisfying a polynomial identity;
- (Zalesskii) group algebras of finitely generated nilpotent-by-finite groups;
- (Ken Goodearl and Ed Letzter) many quantum algebras.

We should point out that many of these examples are examples of Hopf algebras. Since Hopf algebras have additional structure, it is natural to ask whether the Dixmier-Moeglin equivalence holds for finitely generated noetherian Hopf algebras.

Lorenz's example

Martin Lorenz found an example of a noetherian Hopf algebra for which the DME does not hold. It's not hard to explain either. We start with $H = \mathbb{Z}^2$. Now let $A \in \mathrm{SL}_2(\mathbb{Z})$ be a matrix that has an eigenvalue > 1 in modulus. Then A gives us an automorphism σ of H. Then we take a semidirect product $G := H \rtimes_\sigma \mathbb{Z}$.

Then $\mathbb{C}[G]$ does not satisfy the Dixmier-Mogelin equivalence. In fact, (0) is a rational ideal (and primitive) and it is not locally closed!

What's going on with Lorenz's example?

We have $\mathbb{C}[H] \cong \mathbb{C}[x^{\pm 1}, y^{\pm 1}]$. The automorphism σ corresponds to an automorphism of $(\mathbb{C}^*)^2$. Now if we look at subvarieties that are invariant under σ, we see that because the matrix A has an eigenvalue > 1 in modulus, we get periodic points of arbitrarily large order and these points are Zariski dense. These periodic points give rise to an infinite set of prime ideals above (0) whose intersection is (0).

Let's compare Zalesskii's result and Lorenz's counterexample.

Zalesskii's result says that $\mathbb{C}[G]$ satisfies DME when G is nilpotent-by-finite and finitely generated.

Lorenz's example is $\mathbb{C}[G]$ where G is polycyclic-by-finite and finitely generated.

So what is the difference?

Growth!

A famous theorem of Gromov says that if G is a finitely generated group of polynomially bounded growth (don't worry! I'll tell you what growth is more or less right now) then G is nilpotent-by-finite. Lorenz's group has exponential growth while Zalesskii's result applies to groups of polynomially bounded growth.

There is an analogue of polynomially bounded growth for rings, too. To learn more about it, you can look up Gelfand-Kirillov dimension.

It's interesting to note that all the results from before of algebras satisfying the Dixmier-Moeglin equivalence all have polynomially bounded growth while Lorenz's counterexample has exponential growth (super-polynomial).

In light of this example, it's natural to ask the following question to exclude Lorenz's counterexamples.

Motivating Question: Let R be a finitely generated complex noetherian algebra of polynomially bounded growth. Does R satisfy the Dixmier-Moeglin equivalence?

Answer: No!

Somewhat surprisingly the answer to this question was open till about 2015. It is less surprising when one considers that this question wasn't actually asked by anyone—at least not in the literature.

In joint work with Stéphane Launois, Omar León-Sánchez, and Rahim Moosa, we found a commutative $\mathbb{C}$-algebra $R = \mathbb{C}[x_1, x_2, x_3, x_4]/I$ on four generators.

We showed that this ring R had an interesting derivation $\delta : R \to R$ (that is δ is $\mathbb{C}$-linear and satisfies $\delta(ab) = a\delta(b) + \delta(a)b$ for $a, b \in R$) so that if you form the polynomial ring $R[t]$ and deform the multiplication so that

$$t \cdot r = r \cdot t + \delta(r)$$

for $r \in R$ then one obtains a ring of polynomially bounded growth (growth like a degree four polynomial, in fact) that doesn't satisfy the Dixmier-Moeglin equivalence.

The story behind this collaboration is interesting: I was on Omar's PhD committee (Rahim was the supervisor) and I noticed that his thesis, a thesis in model theory, had some interesting connections to Poisson algebras. I reformulated the question of the DME for Poisson algebras in model theoretic terms. A few weeks later they told me they had reformulated my reformulation into model theoretic terms. Oh well.

A few weeks after that, they had what they thought was a counterexample. But more on this and how model theory is involved later.

What our example shows (and Lorenz's counterexample, too!) is that even just for commutative polynomial rings $R[t]$ when one deforms the multiplication rule, it is very difficult to know whether the Dixmier-Moeglin equivalence holds.

So how does one deform the multiplication of a commutative polynomial ring $R[t]$?

This is where one gets into so-called skew polynomial rings.

The rings $R[t;\sigma,\delta]$

If R is a commutative noetherian ring and t is an indeterminate, then one can produce a noncommutative (skew) polynomial ring $R[t;\sigma,\delta]$ by giving:

- a ring automorphism $\sigma : R \to R$;
- a σ-derivation $\delta : R \to R$ (i.e., δ now satisfies $\delta(ab) = \sigma(a)\delta(b) + \delta(a)b$).

One can then deform the multiplication by keeping the same multiplication as before on R and declaring that

$$t \cdot r = \sigma(r)t + \delta(r).$$

One can check this does indeed give us a ring! In the case that $\delta = 0$ we just write $R[t;\sigma]$ and when σ is the identity, δ becomes an ordinary derivation and we write $R[t;\delta]$.

Even in this very special situation, many interesting geometric problems arise when looking at the Dixmier-Moeglin equivalence.

To give an idea, I'll look at what happens in two special cases: $R[t;\sigma]$ and $R[t;\delta]$.

If one looks at $R[t;\sigma]$ with R a finitely generated commutative $\mathbb{C}$-algebra, one can look at this geometrically by looking at the scheme $X = \mathrm{Spec}(R)$ and the induced automorphism $\phi := \sigma^*$ given by $\sigma^*(P) = \sigma^{-1}(P)$. The Dixmier-Moeglin equivalence can be reformulated purely geometrically as follows.

GEOMETRIC DME: automorphism case

The Dixmier-Moeglin equivalence holds for $R[t;\sigma]$ if and only if the following equivalences hold for ϕ-invariant subvarieties of Y of X:

- (primitivity) there is a point $y \in Y$ that has a Zariski dense orbit under ϕ;
- (locally closed) the union of proper ϕ-invariant subvarieties of Y is a proper Zariski closed subset of Y;
- (rationality) there does not exist a dominant rational map $f : Y \to \mathbb{A}^1$ such that $f \circ \phi = f$.

That last condition is saying we can't have a commuting diagram

$$\begin{array}{ccc} X & \xrightarrow{\phi} & X \\ \downarrow & & \downarrow \\ \mathbb{A}^1 & \xrightarrow{\mathrm{id}} & \mathbb{A}^1, \end{array}$$

where the map from X to $\mathbb{A}^1$ has infinite image.

This does not hold in general, but if one imposes the polynomially bounded growth hypothesis on $R[t;\sigma]$ the result is unknown. Polynomially bounded growth in this setting corresponds to the automorphism ϕ having what is called *null entropy*—it's a technical condition but not hard to define.

With Dan Rogalski and Sue Sierra, we proved that this "geometric" DME holds for projective surfaces when the automorphism has null entropy. Lorenz's example shows that if you do not have the null entropy hypothesis then it does not hold for surfaces.

What about $R[t;\delta]$?

Here it is a bit harder to explain. But if one has a finitely generated commutative $\mathbb{C}$-algebra R then a derivation $\delta : R \to R$ corresponds, geometrically, to a section $s : X \to TX$, where $X = \mathrm{Spec}(R)$.

This is something that's not hard to explain, but it's best done on a blackboard. If one has a map $f : Z \to Y$, it induces a map $df : TZ \to TY$, so if Z is a subvariety of Y then we can of TZ as sitting inside TY.

GEOMETRIC DME: derivation case

The Dixmier-Moeglin equivalence holds for $R[t;\delta]$ if and only if the following equivalences hold for irreducible subvarieties Y of X with $s(Y) \subseteq TY$:

- (primitivity) there is a point $y \in Y$ that does not lie on any proper subvariety Z of Y with the property that $s(Z) \subseteq TZ$;
- (locally closed) the union of proper subvarieties Z of Y satisfying $s(Z) \subseteq TZ$ is a proper Zariski closed subset of Y;
- (rationality) there does not exist a non-constant rational map $f : Y \to \mathbb{A}^1$ such that $df \circ s = ds \circ 0$, where 0 is the zero section from $\mathbb{A}^1$ to its tangent space.

So even for rings of the form $R[t;\sigma]$ and $R[t;\delta]$—rings that our some of our most basic examples of **non**-commutative rings—understanding the Dixmier-Moeglin equivalence reduces to non-trivial (and interesting!) geometric problems.

The work with Launois, León-Sánchez, and Moosa shows that the geometric DME does not hold in the derivation case (the polynomial growth hypothesis comes for free in this case too!).

Modifying our original motivating question

Near the beginning of the lecture we asked whether algebras of polynomially bounded growth satisfy the Dixmier-Moeglin equivalence and we showed that the answer was 'no'. If one looks at the cases where the Dixmier-Moeglin equivalence had first been proved—enveloping algebras, certain group algebras, certain quantum groups—one notices that these are all special types of algebras: they are Hopf algebras!

With the additional Hopf structure, it's natural to ask about a strengthening of our original question.

Question: Does the Dixmier-Moeglin equivalence hold for finitely generated complex noetherian Hopf algebras of polynomially bounded growth?

This is still open unlike the original question, which had a negative answer. This question also exists in the literature. I wrote a paper with Joseph Leung where we made this conjecture and proved the conjecture in the cocommutative case.

The cocommutative case gives a nice story to tell if you give a talk on it because the main examples of cocommutative Hopf algebras are enveloping algebras and group algebras and so our theorem unifies Zalesskii's result on DME for group algebras of nilpotent groups along with the original result of Dixmier and Moeglin on enveloping algebras.

That makes our result sound pretty good, right? Well, I should point out that complex cocommutative Hopf algebras can really be built up in a natural way from a group G and an enveloping algebra U, so really we use Dixmier and Moeglin's work as a base case to deal with U and then use Gromov's theorem to assert that G is nilpotent-by-finite and then argue by induction on the Hirsch number of a large nilpotent normal subgroup of our group G, so the argument is not so hard if one uses some big theorems.

For me, a big part of the paper was an excuse to raise the question from the preceding slide.

Hopf Ore extensions

One time I went to Poland and I saw Ken Brown give a talk on what he called Hopf Ore extensions. To be fair, it might not have been in Poland, but it was definitely Ken Brown.

The idea was simple. We start with a Hopf algebra H and we ask when we can deform the multiplication on $H[t]$ (so we are looking at a skew polynomial ring $H[t; \sigma, \delta]$) in such a way that the resulting ring has a Hopf structure that extends the Hopf structure on H.

Ken was talking about his joint work with O'Hagen, Zhuong, and Zhang (let's call them BOZZ!) and they called their constructions iterated Hopf Ore extensions. Under mild hypotheses, they completely described what they had to look like. Interestingly, they made the remark in their paper that if one starts with a Hopf algebra of polynomially bounded growth then the Hopf Ore extension also has polynomially bounded growth.

Thus these families are a natural place to study the question about DME for Hopf algebras.

For me, someone who had been interested in the Dixmier-Moeglin equivalence for algebras of the form $R[t;\sigma,\delta]$ with R commutative, it was especially interesting to look at what happens when we assert, in addition, that this ring is a Hopf algebra.

How does that change things?

Just in case you don't know, I should tell you that if R is a finitely generated commutative $\mathbb{C}$-algebra that is an integral domain that is *also* a Hopf algebra then R is very nice. The ring R is just the coordinate ring of an affine connected complex algebraic group G (so a Zariski closed subgroup of some $\mathrm{GL}_n(\mathbb{C})$.)

If one asks that $R[t;\sigma]$ (automorphism case) or $R[t;\delta]$ (derivation case) be a Hopf algebra the conditions of BOZZ show that σ has to be an automorphism of R that is induced by translation by a central element of G. It shows that δ has to correspond to a section $s : G \to TG$ from G to its tangent bundle that commutes with multiplication $\mu : G \times G \to G$ in the following sense. The section s induces a section $s' : G \times G \to T(G \times G)$ and we require that

$$s \circ \mu = d\mu \circ s'.$$

Picture

$$\begin{array}{ccc} T(G \times G) & \stackrel{d\mu}{\rightarrow} & TG \\ \uparrow & & \uparrow \\ G \times G & \stackrel{\mu}{\rightarrow} & G \end{array}$$

Question:

Does the Geometric DME hold if we assume that our (σ, δ) pair gives rise to a Hopf structure on $H[t; \sigma, \delta]$?

The answer is 'yes'.

In joint work with Omar León-Sánchez and Rahim Moosa, we showed that any algebraic group G with a section $s : G \to TG$ that commutes with the multiplication map in the sense described before, we have that the geometric DME holds for (G, s).

Using this result along with work of Ken Goodearl, we were then able to use this result to give us the following result.

Theorem (B-LS-M) Let R be a finitely generated complex commutative Hopf algebra and suppose that $R[t;\sigma,\delta]$ has a Hopf structure extending that of R. Then $R[t;\sigma,\delta]$ satisfies the Dixmier-Moeglin equivalence.

One loose end

Early on, I mentioned that my collaboration with Rahim and Omar started when I was serving on Omar's thesis committee. I also mentioned that Omar and Rahim (his advisor) are model theorists. So there's the natural question: where does model theory enter?

Do we really need model theory or can this be done algebraically?

I'd like to say it could be done algebraically but it appears that model theory is used in an essential way via Hrushovski's trichotomy theorem.

Maybe that doesn't tie up the last loose end, but in any case I'll say...

Thanks!

P.S. Happy Birthday, Ellen and Martin and also November 4–5, 2017, Riverside there's an AMS special session for Lance's 75th birthday.