Registration form (basic details)

1a. Details of applicant

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1b. Title of research proposal

From Arithmetic Geometry to Non-commutative Riemannian Geometry, and back

1c. Summary of research proposal (289 words, excl. keywords)

Can one say anything about solutions of polynomial equations using principles from mathematical physics? This proposal says "yes".
Around 1960 in Paris, two groundbreaking schools emerge, one in algebraic geometry (Grothendieck) and one in operator algebras (Connes). Each goes its own way.
Algebraic geometry led to Arithmetic Geometry (AG), embodying the century old study of solutions of polynomial equations, and solves many famous problems in number theory (Weil's and Mordell's conjectures, Fermat's Last Theorem) and string theory.
The theory of operator algebras becomes Noncommutative Geometry, a mathematically correct theory for various aspects of quantum theory and differential geometry.
But do these two subjects really live on different planets? Around the start of the new millennium, some become convinced that links exist that may enable us to tackle old arithmetic problems using methods from non-commutative geometry, as the method of automorphic forms (the "Langlands Program") achieved earlier.
This proposal lays the foundations for and explores the first applications of such a link. From an object in arithmetic geometry, one constructs a noncommutative "Riemannian" geometry (NCRG). These NCRG's come with a novel type of zeta function. This zeta function will be shown to be so powerful that it knows everything about the original object. The NCRG-zeta function would thus be in stark contrast to the usual zeta functions that are around in arithmetic geometry: in general, the latter do not determine the original object at all.
This project is the starting point of a whole new branch of research: our intuition from Riemannian geometry can be applied to arithmetic geometry. With its firm tradition in arithmetic geometry and its emerging school in noncommutative geometry, The Netherlands are an ideal place to take part in this project for the 21st century.

KEYWORDS: arithmetic geometry, noncommutative geometry, zeta function

1d. Previous applications

no

1e. NWO Council area

EW

1f. Host institution

Universiteit Utrecht, Departement Wiskunde, Mathematisch Instituut

Research proposal

2. Description of the proposed research (5317 words, excl. references)

A general remark at the start: this is a pure mathematics proposal and as such motivated by internal questions of mathematics proper. However, at designated places we will indicate why these questions are timely and relevant to neighbouring fields such as physics and information science, with which the project is indeed very much interwoven.

2a. Research topic

This section is meant to be a general outline. Unavoidably, there will be some technical definitions, but the section is so designed that these can be skipped without losing the overall thread.

The general aim of the project is to construct links between algebraic geometry and noncommutative Riemannian geometry, and to exploit those in answering questions on both sides. Before turning to the specific technical aspects of the project, I will introduce four sample problems to give an impression of what it aims at.

(1) <u>Better zeta functions</u>

In 1859, Riemann introduced his famous zeta function, originally conceived to study the prime number distribution. This was the start of a whole collection of formalisms of zeta functions, such as in algebraic number theory (prime ideal distribution), algebraic geometry (rational point distribution) and differential geometry (geodesic length distribution). Its success lies in the fact that the zeta function contains many meaningful invariants (such as the class number, etc.). It is thus natural to ask to what extent the zeta function of a structure determines this structure *completely.*

The answer is subtle: if the structure is a general number field, the answer is “no” (due to Gassmann in the 1920s), but “yes” for special ones (number fields that are Galois over **Q**, a theorem of Bauer) [1].

For general Riemannian manifolds, the answer is also no: this is the famous construction of isospectral, non-isometric manifolds (of Vignéras and Sunada). The problem can be given a particularly appealing formulation for planar domains: the question is “whether you can hear the shape of a drum”, i.e., given a drum in the form of a given planar domain, can you deduce the shape of that domain by knowing the spectrum of the drum’s vibrations? The answer is again negative (a theorem of Gordon, Webb and Wolpert).
In some cases, such as that of compact Riemann surfaces (which are Riemannian manifolds with a hyperbolic atlas, so have constant curvature -1), one can prove that there are only finitely many objects with the same zeta function as a given one (Buser). But in arithmetic geometry, one even encounters formalisms where infinitely many objects have the same zeta function. This is, for example, the case for so-called Mumford

curves, a *p*-adic analogue of Riemann surfaces [2].

From the above, the following question is immediate:

Question A: *Do these objects admit another type of zeta function that* does *encode its isomorphism type?*

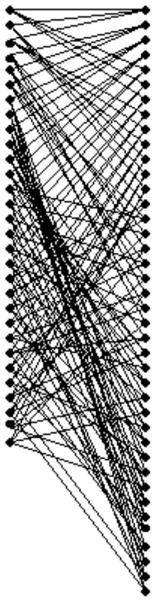
In this project, we will associate to Riemannian manifolds and Mumford curves, and maybe even number fields, such a new zeta function via Noncommutative Riemannian Geometry (NCRG).

Before we continue, a short interlude with some general words about what NCRG embodies: *noncommutative geometry* is Alain Connes' generalization of differential geometry, in which the ring of functions on a space is not necessarily commutative: it is a C*-algebra (acting on a Hilbert space) [3]. Later, Connes observed that distance in classical *Riemannian geometry* (familiar from general relativity) has an analogue in noncommutative geometry by means of a "Dirac operator". Thus arises the notion of a *noncommutative Riemannian geometry* as a triple *(A,H,D)* of an operator algebra *A* acting on a Hilbert space *H* and an unbounded Dirac operator *D* acting on *H* and satisfying some technical compatibility condition with the action of *A*.

A natural notion of zeta function is also present in theory of NCRG. The zeta function for a spectral triple *(A,H,D),* indexed by a fixed operator $a \in A$ is defined as $\zeta_a(s) := \mathrm{tr}(a|D|^s)$. This is indeed another generalisation of the Riemann zeta function: that itself also arises for a specific choice of *(A,H,D).*

In answering Question A, both the construction of the NCRG, and proving that its zeta function has the desired property of determining the object is entirely new; this sometimes also involves proving a new ergodic rigidity theorem. It might give us some clues as to the resolution of the old problem in arithmetic geometry that the usual zeta function does not determine a number field uniquely.

(2) Better symbolic coding



We start this subsection with an apparently completely different question. What is the best way to encode a binary signal with mechanical constraints (such as: between any two one's, at least two and at most ten zeros occur)? This problem was traditionally approached by Frequency Modulation, but more efficient methods use coding by shifts from symbolic dynamics [4]. "Efficient" means storing as much information in as little space as possible (given the constraints), and this is reached by minimizing a characteristic quantity called entropy.

Question B: *Construct efficient shifts for coding.*

According to the current project, the answer to this question is, rather unexpectedly, related to Question A (and Question D, cf. infra): we will construct shifts from noncommutative geometries associated to graphs of modular curves. It is not so strange to look for minimal entropy there, since we see a similar phenomenon in coding theory and graph expansion. Indeed, the asymptotically best error-correcting algebro-geometrical codes and the best expanding graphs also arise in that way: technically speaking, one starts with a modular curve and a prime of totally degenerate reduction and considers the intersection dual graph of that reduction. I have drawn such a graph in the picture (it corresponds to a reduction of a Drinfeld modular curve). This is a graph whose "Laplace operator" has particularly appealing spectrum, and the entropy is very closely related to that spectrum [4]. Here we see an example of intuition from arithmetic geometry flowing back to noncommutative geometry, and ultimately to its applications in coding.

The project also takes the practical question of minimizing entropy in coding devices as a motivation for studying in more detail the combinatorial structure behind certain NCRG's, and this leads us back to Question A, but now for such discrete structures. In the application to coding theory, only the operator algebra plays a decisive role; the Riemannian aspect is at the background. In the discretized version of Question A, however, the metric becomes important. We now discuss this briefly.

Given a graph, one can construct a classical zeta function (in the sense of Ihara and Bass): this zeta function of the graph encodes lengths of paths. Once we have associated an NCRG to such a graph, the NCRG zeta function comes with it for free. What is the relation between the two zeta function formalisms? Can the operator algebra point of view contribute something to our understanding of the classical zeta function? The exercise in [14] shows that, indeed, the pole order at 1 of the Ihara zeta function of a graph can be determined via the computation of a certain *K*-group of an operator algebra (a Cuntz-Krieger algebra, also known as a topological Markov chain) associated to the graph. We will also investigate what happens for buildings of higher dimensions, following work of Guyan Robertson et. al.

(3) <u>Another bridge from algebraic geometry to noncommutative geometry</u>

To state it somewhat crudely: the more analytical world of noncommutative geometry is better suited for a study of continuous invariants than the world of arithmetic geometry, which is essentially the study of finitely generated algebras. However, the latter field does have a few key problems that involve, or seem to involve, continuously varying parameters. One should think of deformation problems, uniformization problems, the accessory parameter problem, etc. One may expect that by passing from arithmetic geometry to noncommutative geometry, some continuously varying parameters will get a natural geometrical meaning as invariants on the noncommutative side.

There have been various attempts to associate operator algebras to finitely generated algebras (the basic objects of algebraic geometry). To name a recent spectacular one: Ralf Meyer found C*-algebras whose derived category of "bornological" modules is equivalent to that of modules over the original algebra. However, the derived category rarely determines the isomorphism type of the algebra (although this is true in favorable cases, such as initiated by the work of Orlov in the 1990s) [6].

But still the question remains:

Question C: *Develop an(other) operator-theoretical/non-commutative geometrical viewpoint on the objects of classical (possibly non-commutative) algebraic geometry.*

In this project, we will not only look for a C*-algebra, but enhance it by a metrical aspect, i.e., consider noncommutative *Riemannian* geometries. The moral of the project is to a large extent that the algebraic-geometrical structure leads to natural candidates for *metrical* structures (viz., Dirac operators) on the noncommutative side. For some examples of this, cf. infra.

I believe the actual emerging picture will be that one can look at classical geometry through noncommutative glasses in various distinct ways, each capturing different aspects of the original object. Apart from the approach of Meyer, we have the conformal "quantized calculus" of Connes-Donaldson-Sullivan-Teleman, elaborated by John Lott. There is recent work of Igor Nikolaev on relations between commutative tori (elliptic curves) and noncommutative tori in the sense of Rieffel. Also, Christian Bär has constructed real NCRG's for conformal structures [7].

From our point of view, the higher characteristic classes of the NCRG's that we construct are a natural place to look for important invariants, that have a standard geometrical meaning on the NCRG side. What makes our approach markedly different from previous methods is that we insist on *finite summability* of the NCRG's.

"Characteristic classes" are one of the main gadgets of modern geometry; they are at the basis of many theories of classification. It would take us too far to define them exactly, but in noncommutative geometry, special values of zeta functions at integers are a kind of characteristic classes, namely, 0-Hochshild homology, and hence the characteristic classes fit in very well with our previous discussion of zeta functions. In the finitely summable case (a technical term from NCRG theory) they become computable via a so-called "local index formula" (due to Connes and Moscovici [18]).

Consider the example of Riemann surfaces. On the moduli space of compact Riemann surfaces of a given genus (= the set of all such Riemann surfaces up to conformal/anti-conformal isomorphism), the association of the classical "Laplace spectral" zeta function to a Riemann surface is a map with finite fibers of varying cardinality, and the variation of this cardinality is not well-understood; an explicit upper bound on the cardinality has been given by Buser [2]. The NCRG-zeta function, however, will be in one-one correspondence with points in that space (cf. [11]). The net result is that *the noncommutative zeta function formalism can serve as a coordinate on the moduli space of Riemann surfaces, and special values of these zeta functions are themselves characteristic classes on the noncommutative side.*

(4) <u>The conformal aspect of noncommutative geometry</u>

The general concept of an NCRG is only about fifteen years old. Alain Connes introduced it in the 1990s under the name of "spectral triple". Two typical applications are a mathematical description of the standard model of elementary particles (in which the Higgs mass can be calculated rather than having to be prescribed), and the noncommutative version of classical Riemannian spin manifolds. In recent years, some textbooks have been published on the subject, but they all stress the presence of an *even, real* structure [8].

As a technical interlude: an *even real* structure means enhancing the spectral triple (A,H,D) by various extra data, namely a $\mathbf{Z}/2$-grading operator and an anti-linear isometry on H. A recent deep theorem of Rennie and Várilly says that usual Riemannian spin manifolds have associated NCRG's that allow one to reconstruct the original spin manifold: the (then commutative) NCRG's should be endowed with such a real even structure [9].

But our principle is that we want to apply NCRG's to algebraic geometry, which is much closer to the world of complex analysis on manifolds than to real analysis on spin manifolds. We should therefore leave out all reference to a real structure and study "bare" spectral triples *(A,H,D)*.

Such general bare spectral triples do occur "in nature", as we know since the groundbreaking work of Consani and Marcolli on NCRG's in arithmetic geometry [10]. It actually appears that these general NCRG's might be everywhere, and that the mere concept of a bare NCRG deserves to be more thoroughly investigated: it is an attractive mathematical structure with a simple set of axioms, and maybe it is as ubiquitous in mathematics as, say, "groups", but we just didn't look enough where this abstract structure underlies and unifies existing notions. Thus we ask:

Question D: *Develop the theory of not necessarily real NCRG's by using examples constructed from complex geometry and arithmetic geometry. What is a good categorification of an NCRG? What are basic structure theorems for NCRG's?*

This question cannot be entirely separated from Question C; the aim of Question D, however, is rather to use input from arithmetic geometry as a tool in noncommutative geometry, whereas Question C goes the other way.

Our approach to this question has two sides.

One is very direct and involves computing with actual NCRG's that are associated to certain basic objects that occur in arithmetical geometry, namely, *modular curves*. We consider modular curves because they are very central to modern number theory, with their role as "moduli spaces" of elliptic curves (and as such played a key role in the solution of Fermat's Last Theorem - they are extremely ubiquitous anyhow).

To these modular curves, considered over the complex or *p*-adic numbers, we shall associate a novel NCRG and compute its typical geometrical features, namely, its characteristic classes, cyclic homology etc. What are these? Which information of the modular curve is encoded in the NCRG?

The calculations are also meant to resolve the lack of examples of NCRG's that don't have a real structure, and our work is "NCRG-botany" in the sense that we make a catalogue of such examples for future use.

The other aspect of our solution to the question is rather on the theoretical side: what is the "correct" notion of a *category* of NCRG's, so what is the correct notion of "morphism" of NCRG's? In down to earth terms: we have a fairly good understanding of what an NCRG is, but we don't understand completely how to compare two of them. There are various options, such a strict isomorphism (which is probably not very informative), Morita equivalence (but with connections in the Dirac operators), … There is some technical literature dealing with this problem, but it is not well enough understood to make general claims. For example, one desired feature is that the association of an NCRG to a Riemann surface is such that a map of Riemann surfaces induces a map between the corresponding NCRG's. We don't know how to do this a present.

To conclude, I repeat the general aim of the project: to construct links between algebraic geometry and NCRG, and to exploit those in answering questions on both sides. The answers we will provide to the four questions above are manifest examples of this kind of interaction.

2b. Approach

In this section I will give a technical presentation of the various subprojects and what investigators these subprojects should be assigned to, but first I will outline the general method. The main principles are:

- to see algebraic geometry as a part of noncommutative geometry, but then of *Riemannian* noncommutative geometry: algebraic geometry should involve metrical notions on the noncommutative side.
- a noncommutative zeta function can be more powerful than its "commutative" analogue, in the sense that this function alone allows one to reconstruct the original object in algebraic geometry.

An example of the methodology

A bridge can be constructed between algebraic geometry and noncommutative Riemannian geometry using the fundamental idea of *uniformization* in algebraic geometry. As an example, I will explain briefly how this is done in my recent work with Marcolli [11].

We start in the world of arithmetic geometry with a (smooth projective) curve X, given as solution set $\{(x,y,z)\}$ in complex numbers of an equation $F(x,y,z)=0$ where F is a homogeneous polynomial (for example, the Klein quartic $F(x,y)=x^3y+y^3z+x\,z^3$). Left is a plot of the homogenenous surface $F(x,y,z)=0$ in real three-space.

Since Riemann, we know that the complex solution set can be seen as a complex manifold, that is: the neighbourhood of every point of X looks locally like the complex plane.

This complex one-dimensional manifold can be globally uniformized: it can be written as the quotient $X=\Gamma\backslash\mathfrak{h}$ of the complex unit disc $\mathfrak{h}$ for the action of a discrete group Γ of Möbius transformations. Left is a picture of the disk tessellated by fundamental domains (here, triangles) for such a uniformization of the Klein quartic. The main point is that this introduces a *dynamical* aspect. For our purposes, it is even better to use global Schottky uniformization: this means writing X as a quotient of $\Omega=\mathbf{P}^1(\mathbf{C})-\Lambda$ by a free discrete subgroup Γ of PGL(2,**C**) with limit set Λ.

In noncommutative geometry, one "unfolds" this quotient, or rather the algebra $C(\Gamma\backslash\Omega)$ of smooth functions on it into a crossed product algebra A that captures the topological structure, but the complex structure is somehow lost. This is remedied by enhancing A into an NCRG. In our case, H can be the L^2-functions on the limit set of Γ (as in the work of Marcolli and Consani in [10] and elsewhere), but this leads to a non-finitely summable spectral triple. Instead, we choose H to be a GNS-representation: it is the Hilbert space completion for the inproduct $(a,b)=\tau(b^*a)$ where $\tau : A \to \mathbf{C}$ is a certain state on the algebra, that, on characteristic functions of subsets U of the limit set takes as value the *Patterson-Sullivan measure* of that set U. This choice will turn out to play an important role in the proof of the fact that the zeta function detects the conformal geometry, since the conformal geometry is captured by that measure. The Dirac operator D is formed as an alternating product of projectors onto the pieces of a decomposition of a canonical subalgebra of A by finite-dimensional algebras. In the given reference [11], we actually restrict the algebra to the commutative subalgebra $C(\Lambda)$. Denote the resulting spectral triple by $\mathcal{S}_X$. The gist is that *the dynamical aspect of the group action on hyperbolic space* *can be captured by this abstract "quantum-mechanical" system* (with observables seen as operators acting in a Hilbert space). Now the noncommutative space has in some sense topologically "one dimension less" and is thus easier than the original curve (that is complex one-dimensional, so real two-dimensional), but since it is non-commutative, it comes with a fancy dimension spectrum that still captures something of the complex structure.

Why this is so is our main theorem, that we can rephrase in terms of zeta functions. As a Riemannian manifold, the Riemann surface has a dynamical zeta function associated to the spectrum of its Laplace operator. However, this zeta function doesn't necessarily capture the isomorphism type of the Riemann surface [2]. But recall that an NCRG (A,H,D), also has zeta functions, indexed by a fixed operator $a \in A$ and defined by $\zeta_a(s):=\mathrm{tr}(a|D|^s)$. For these new zeta functions, we have:

Theorem [11] *Let X denote a compact Riemann surface of genus g>1, and let $\mathcal{S}_X$ denote the above constructed finitely summable spectral triple. Then $\mathcal{S}_X$ determines X up to conformal/anti-conformal isomorphism. More precisely, the zeta functions of the spectral triple $\mathcal{S}_X$ determine X already (up to such isomorphism).*

This is in stark contrast with the classical "commutative" zeta functions!

A few words about the proof will help to clarify the method. The spectral triple encodes the behaviour of the uniformizing Schottky group w.r.t. the boundary measure of Patterson-Sullivan.

> step 1: one deduces, by some explicit computations in the Hilbert space, that equality of zeta functions of two Riemann surfaces implies the corresponding boundary map to be absolutely continuous: this is a computation reminiscent of certain basic number theoretical theorems on Dirichlet series.
>
> step 2: the conclusion follows from an *ergodic rigidity theorem* that we deduce from a general statement of Chengbo Yue, similar to classical Mostow rigidity [12]: a method from the theory of dynamical systems (the theorem is termed "ergodic" because its proof uses analogues of E. Hopf's ergodicity theorem for the geodesic flow on the Riemann surface).

<u>Projects and subprojects</u>

There are four main parts to the project. The starting point is the *construction* of NCRG's associated to objects in arithmetic geometry. This automatically leads to some interesting combinatorial problems, and to some questions in ergodic rigidity and the study of NCRG-zeta functions. In the final part, we compute the NCRG-aspects of classical modular curves. We will put the first three questions from section 2a into the perspective of these subprojects, and observe without further mention that question D from section 2a permeates the whole project.

Subproject I. The construction of new NCRG's

Consani and Marcolli have associated NCRG's to arithmetical curves [8]. A drawback is that they are not finitely summable. This was remedied by Cornelissen et. al. for Mumford curves using a construction based on a GNS-representation in [13], but that construction is so generic that it doesn't seem to capture much information about the original object. See also very recent work of Christensen-Ivan-Lapidus [14].

Goal: To construct *finitely summable* NCRG's associated to uniformizations of varieties in arithmetical geometry (such as Riemann surfaces, Mumford curves, cf. Question A), and of graphs and buildings, that allow the reconstruction of the boundary metrical space. We believe this can be done by a non-generic version of the construction in [13], namely by encoding the Patterson-Sullivan measure in the state that is used to construct the GNS-representation.

Investigators: PhD student 1 will investigate the case of graphs. PhD student 2 will investigate Fuchsian uniformization/higher dimensions (allowing cusps, as is the case for open modular curves).

Attainability: PI + Marcolli have done this for Schottky uniformization of compact Riemann surfaces, and that work provides a solid starting point for continuing the investigations.

Speculative aspects: we will speculate on how to construct such NCRG's when no uniformization is visible, such as for p-adic curves whose reduction is not completely split (maybe using Mochizuki's theory of indigenous bundles), or for global fields. To "noncommutativize" such aspects as l-adic cohomology of algebraic varieties (such as the Tate module) is a deep problem that we do want to spend some time thinking about.

Subproject II. Combinatorial aspects of NCRG's

For p-adic uniformization, the operator algebra in the NCRG can be constructed combinatorially from the reduction complex. For graphs, Cornelissen et. al. [15] classified the isomorphism type of the algebras in graph theoretical terms: this is essentially an exercise in graph theory.

Goal: To extend this to some higher dimensional buildings. To compute characteristics (like entropy) of the shifts associated to these C*-algebras, in particular answer Question B from the introduction. This is a very concrete question with potential applications on the technological side of symbolic coding. To understand better the interaction between classical (Ihara-Bass) zeta functions of graphs and noncommutative zeta functions.

Investigators: PhD-student 1; Postdoc 1.

Attainability: One can build on earlier work by Robertson and Vdovina [16], and in particular, some of the conjectures outlined there. Postdoc 1 should be an expert on C*-algebras, shifts and/or symbolic dynamics.

Speculative aspects: understanding the combinatorics better can provide a handle on some questions in the K-theory of C^*-algebras on buildings that are currently out of reach, but for this, we first need a better understanding of the precise link.

Subproject III. New zeta functions and ergodic theorems

This relates to Question A and C.

Goal: An NCRG comes with a zeta function. To prove that for the NCRG's constructed in part I, such as for Fuchsian groups, p-adic uniformization, graphs, and higher dimensional varieties, these determine the objects in arithmetical geometry (almost) uniquely. Also study natural questions about the classical case of "hearing the shape of a drum" by "listening" to variations of the Laplace spectrum.

Investigators: PI+Marcolli; partly PhD Student 1 + Ph D Student 2.

Attainability: Again, this was done for Schottky uniformization of Riemann surfaces in the work of the PI with Marcolli; the Fuchsian case should be similar. For Mumford curves, this becomes very speculative: new ergodic rigidity theorems should be proven; we currently only have such a theorem for graphs, due to Coornaert [17]. It is rather speculative whether Mostow rigidity holds in any form for Mumford curves, but this constitutes a question that should be answered. We have a possible approach in mind (involving p-adic valued pseudo-measures). The case of graphs should be done by PhD Student 1 and the (easier) higher dimensional case is left to PhD Student 2.

Speculative aspects: In accordance with the first section, can one make new zeta functions for global fields? Can one enhance the construction of zeta functions of varieties over finite fields by noncommutative ideas? It is only by working out the above mentioned cases - where we do have a handle to hold onto - that we will ever be able to find such new structures.

Subproject IV. Higher characteristic classes for NCRG's associated to modular curves and deformations; categorification

This relates to Question B and C.

Goal: To compute invariants of the NCRG associated to concrete objects in arithmetical geometry such as modular curves; to families of such objects, and their deformations; in particular, focus on characteristic classes of the NCRG; to study some basic properties of the category of NCRG's. Can one "categorify" the main theorem from [11] into a functor from a category of Riemann surfaces into a category of spectral triples?

Investigators: PI; Postdoc 2; PhD Student 3.

Attainability: Work out the local index formula for these NRCG's [18], and compute with it. Postdoc 2 is supposed to be an expert on modular curves or spectral triples.

Speculative aspects: Is it possible that deformation parameters in arithmetical geometry get some meaning in relation to noncommutative characteristic classes? Can we say anything on the accessory parameter problem using this idea?

2c. Innovation

(1) We connect, in a rather new way, two distinct fields of mathematics. That the two separate fields do interconnect became clear over the past ten years, and this project adds significantly to that trend. To name some examples [19]:

- Manin and Marcolli consider the noncommutative boundary of modular curves;
- Manin, Polishchuk and others develop the visionary noncommutative real multiplication program for classical number theoretical problems;
- Consani and Marcolli recast the Arakelov geometry used in the proof of Mordell's conjecture in terms of noncommutative spectral geometry;
- Connes and Marcolli reformulate explicit class field theory in a noncommutative thermodynamical formalism.

One observes how these examples each nicely hook noncommutative geometry to one of the significant trends in modern number theory. However, all of these existing links are at the same time markedly different from our methodology.

Some further signs of interaction:

- The densely packed room at the workshop "Arithmetic Geometry and High Energy Physics" (Lorentz Center, Leiden, 29 Aug 2005 through 2 Sep 2005), organized by Cornelissen, Marcolli and Waldron [20].
- At the closing symposium of the GBE/FOM Programme on Mathematical Physics (9 March 2007), Sir Michael Atiyah suggests that the "fortress of number theory" will be the next to be captured by mathematical physics.
- The 2007 founding of a new journal on Number Theory and Physics (Comm. Numb. Th. Phys.), and of a journal exclusively dedicated to Noncommutative Geometry (J. Noncommut. Geom.).

(2) We connect the strong existing Dutch school in Arithmetic and Algebraic Geometry (Edixhoven, van der Geer, Looijenga, Moonen, Oort, van der Put, Steenbrink, Top, …) and Number Theory (Beukers, Lenstra, Stevenhagen, de Smit, …) to the equally impressive one in Mathematical Physics, in particular its emerging subsection in noncommutative geometry (Crainic, Landsman, Moerdijk, Müger, van Suijlekom). These two schools are represented by the NWO Dutch national research clusters on "Geometry and Quantum Theory" and "Discrete, Interactive, Algorithmic Mathematics, Algebra and Number Theory", and the current project is a novel bridge between those clusters.

Concretely,

- using NCRG zeta functions in this way is new; their general features have not been considered enough since their conception about 15 years ago, probably due to a lack of examples.
- looking at isospectrality problems from noncommutative geometry has never been considered prior to [11].
- using reduction graphs of modular curves in symbolic coding has not been considered before.
- our approach to a noncommutative view on conformal geometry is entirely new.

2d. Plan of work

<u>Composition of the research team</u>

- **PI**, salary partially paid by project to alleviate teaching load. I allocated 75% of my research time to this project to allow for the continuation of various other smaller research efforts.

- **3 PhD-candidates**, one of which partially paid by the project, two completely paid by the project. The project caters for the students to travel around and invite some visitors.

- **2 Post-docs**, again with travel grants and money for visitors and laptops, and with the explicit possibility to be engaged in teaching (mainly to master students). Postdocs should be given the freedom to pursue their own ideas within the context of the project.

<u>Why me?</u>

I will first say something about my background and why I believe to be suitable to lead this project. For this project, one has to be able to stand on two legs: one in algebraic geometry and one in noncommutative geometry. It should be clear from the PI's c.v. that he is a relative novice to the field of spectral triples (which itself is rather young), and his background is in number theory, automorphic forms, and algebraic geometry (in particular also *p*-adic methods). But he has already joined forces with the established school of M. Marcolli (MPIM-Bonn) in noncommutative geometry, and as a matter of fact, the past year has already produced various starting results, proving to my opinion that "an (unbiased!) algebraic geometer that comes to noncommutative geometry" is a very natural, fruitful and productive concept.

<u>Interaction</u>

Nevertheless, our emerging working group will have to learn a lot. We expect to have an intensive program of visitors and visits. I will first mention the direct collaborations:

International collaboration: We interact a lot with Marcolli (Bonn) and her school. For *p*-adic analysis, I will continue to collaborate with F. Kato (Kyoto). For combinatorial aspects, I have contact with the school of Vdovina-Robertson (Newcastle, UK) that I want to intensify and might build contacts with the Australian school of Carey-Pask-…(Newcastle, AUS). I want to contact Consani (Baltimore) for feedback and potential collaboration. For spectral triples, I plan to make contact with Christensen (Copenhagen), Ivan (Hannover).

Apart from this, I have reserved funds for two larger workshops, and for shorter "master class" visits of experts to teach a class of, say, one month.

National input: as mentioned in section 2c, at the national level, the project fits within the GQT research cluster (all three research directions: Langlands program, quantization and moduli spaces) and some arithmetical aspects match with the DIAMANT-cluster (esp. about zeta-functions). There is even some overlap with (symbolic) dynamics from the NDNS+-cluster. A related MRI-masterclass around these topics is conceived for the academic year 2009-2010.

Local input: Utrecht is a strong department in number theory, algebraic geometry and mathematical physics. This project can benefit from discussions with Frits Beukers (zeta functions, uniformization), Frans Oort (modular curves), Eduard Looijenga (complex geometry), Marius Crainic (noncommutative geometry), Karma Dajani (ergodic theory), as well as a host of post-docs and visitors in these fields.

<u>Timeline for research</u>

The global timeline is clear from combining the budget plan with the detailed outline of the project in section 2b.

2e. Literature references

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[2] The formulation "Can you hear the shape of a drum?" is due to Lipman Bers, and also the title of a famous article by Marc Kac in *Amer. Math. Monthly* **73**(4), pp.1–23. 1966. The negative solution is in C. Gordon, D. Webb, and S. Wolpert, Isospectral plane domains and surfaces via Riemannian orbifolds. *Invent. Math*., **110**(1):1–22, 1992. For compact manifolds, the question was apparently first posed by I. Gelfand, cf. Marie-France Vignéras, Variétés riemanniennes isospectrales et non isométriques. *Ann. of Math. (2),* **112**(1):21–32, 1980 and Toshikazu Sunada, Riemannian coverings and isospectral manifolds. *Ann. of Math. (2),* **121**(1):169–186, 1985. For the case of Riemann surfaces, see e.g., Peter Buser, *Geometry and spectra of compact Riemann surfaces,* Progr. in Math. 106, Birkhäuser (1992).
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[7] Connes-Donaldson-Sullivan-Teleman: see Connes's book [3], section IV.α; compare: Alain Connes, Dennis Sullivan and Nicolae Teleman, *Quasiconformal mappings, operators on Hilbert space, and local formulae for characteristic classes*, Topology **33** (1994), 663-681; John Lott, *Limit sets as examples in noncommutative geometry*, K-theory **34** (2005) 283-326; Igor Nikolaev, *On complex and noncommutative tori*, Proc. Am. Math. Soc. **134** (2006), 973-981; Christian Bär. Conformal structures in noncommutative geometry. *J. Noncommut. Geom.,* **1**(3):385–395, 2007.

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