Hi everyone

Its time for a newsletter. I've got some good news — Johannes and I are making strong progress on the “fermion problem” that I described to you in my last letter. We are now in the process of writing down, double-checking and polishing our analysis and thus I am not ready to write about this just yet — what I can say is that things are looking very promising.

In this newsletter I will instead write more generally about our work; what we seek, where we come from and what motivates us. This is a story that begins in 2002 on the Kangshung glacier in Tibet and involves an unhappy Yak ox. I thought it might be interesting to write a less technical newsletter — more fun, less tedious details.

I'll also end the newsletter with a call for sponsors. Whereas Johannes is financed by the Leibniz University in Hannover, where he is an assistant professor in mathematics, I rely on private funding and donors. And right now I need more funding to keep my ship afloat.

So this is the plan. Lets begin — buckle up and hang on!

**WHAT ARE THE QUESTIONS?**

The single most important task in contemporary high-energy physics is this:

— *To explain the mathematical structure of the standard model of particle physics.*

This piece of mathematical machinery, that tell us how the elementary particles — quarks, leptons, etc — interact with the strong, weak and electro-magnetic forces, represents the deepest level of reality that we have probed experimentally so far. It is our ultimate experimental data, which we must understand and explain.
The standard model, which was found in the 1970’s and which has so far withstood all attempts of falsification, has a very rich mathematical structure. And just as we can understand the periodic system as a product of atomic physics and our knowledge of electron orbits we also expect that the standard model owes its particular structure to a deeper and yet unknown theory. The task is to figure out what that theory is.

This can be divided into three sub-tasks:

1. **The algebraic structure of the standard model.** The standard model has a very particular algebraic structure: the choice of symmetries — rotations in 2, 3 and 4 internal dimensions; the choice of the fermionic sector (i.e. matter); the choice of the Higgs mechanism, which provides the masses to the various particles and the choice of three particle generations. All this structure screams for a deeper explanation.

2. **What is quantum field theory?** The standard model is a quantum theory and since its basic constituents are fields the relevant framework is quantum field theory. But here is a problem because it is not known how to construct a non-perturbative quantum field theory in 3 spatial dimensions (plus time). Thus, the question of finding a well-defined quantum field theory in 3 spatial and one temporal dimension must be addressed as well.

3. **What role does gravity play?** Einsteins theory of general relativity is not included in the standard model. The question is what role gravity plays in a fundamental theory. Is a quantum theory of gravity required — as is widely believed — or does gravity remain classical? We have no clear indications how gravity is to be included — this too must be addressed.

As I said, the standard model is the experimental data, which we must explain, and that explanation must answer these three questions.

**ON THE KANGSHUNG GLACIER, TIBET**

But before we throw ourselves into the fiery fire of contemporary theoretical physics I wold like us to travel back to 2002, when I had just received my Ph.D. title in theoretical physics at the Technical University of Vienna in Austria. A few days after I defended my thesis I travelled to Tibet to hike in some very remote mountain regions on the northeast side of mount Everest. On this trip I kept thinking about some problems, that I had encountered during my Ph.D. studies. Questions about gravity, quantum theory and the standard model of particle physics. Something was troubling me and while I hiked over high mountain passes and across beautiful glaciers I came to realised that my Ph.D. thesis was based on assumptions that could not be true. I became convinced that they were wrong.
The temperatures were well below freezing — winter was approaching — and the Yak ox that carried our supplies didn’t want to walk up the Kangshung Glacier, where we were heading. I pulled its rope while I leaned on the ski pole, that I held in my other hand. We were at 5500 meter above sea-level and the wind was howling. I looked up ahead where I could see the Kangshung face of Mount Everest; a huge white wall of ice, rock and snow.

I was thinking about how to combine my knowledge of non-commutative geometry — a fascinating new research field in mathematics, pioneered by the famous Field medalist Alain Connes, who had demonstrated an intriguing link to the standard model of particle physics — and quantum field theory. And gravity too; the question of quantum gravity was looming over all my considerations, like an eagle high in the sky: I could see its shadow on the ground but not the bird itself.

I now knew that my Ph.D. thesis was based on wrong assumptions. But if what I had been doing for the past three years, and what many other researchers were still doing, was wrong, then what was right?

During my trip to Tibet I got an idea. Or a hunch; I caught the scent of an idea. But it wasn’t until 2004 when I met a young mathematician named Johannes Aastrup, who had just finished his Ph.D. at the University of Copenhagen, that I knew what to do with it. Johannes and I met at the Mittag-Leffler Institute in Stockholm, Sweden, and when I told him about my thoughts he decided to join me — and we have been a team ever since.

**OUR APPROACH**

Since we met in 2004 Johannes and me have developed a new approach to a fundamental theory, that takes its point of departure in Alain Connes work. And during the past 3-4 years this work has materialised into something solid, something that looks like a theory, which offers novel — and surprising! — answers to the three questions that I listed above.

Our idea is to begin with something exceedingly simple. If a theory is to be fundamental then we believe it must be based on a principle, that borders the trivial. If the starting point has too much structure it will be exposed to further scientific reductions; if it is to be final it must be based on something almost empty, conceptually. And what can be more simple than the action of moving *stuff* around in a 3-dimensional space?

What we do is to start with a basic mathematical object, namely the *algebra* of moving stuff around in space. Just that: *moving stuff around*. We then combine this elementary mathematical object with *a metric principle* — basically we apply...
the machinery of non-commutative geometry — and what comes out is a highly interesting framework.

The idea is to derive everything from this algebra. And the exciting fact, that we are right now busy analysing, is that a very rich mathematical structure does indeed emerge from it.

This is exactly what we have been hoping for. After all, the standard model of particle physics is complex, it involves a lot of structure. If we are to explain this then we must come up with something that gives rise to a lot of mathematical structure. And as I said, this is precisely what we are finding.

The million dollar question is, of course, whether the structure, that we find, will eventually match the standard model of particle physics. We do not know the answer to that question but things are beginning to look very promising:

**First promising sign:** Our theory automatically includes both bosonic and fermionic quantum field theory — i.e. quantum field theory of *forces and matter* — and it does so in a way that automatically takes Einsteins theory of general relativity into account. Specifically: we are operating on a curved space. This means that an answer to the second question about quantum field theory seems to emerge from our framework.

**Second promising sign:** Key elements of Einsteins theory of relativity are automatically included in our framework (we think its the full package, but I’d better not write that until I’m certain). And most interestingly, it is *not* quantised. If we are right then gravity does not appear to a quantum theory. This goes against everything that theoretical physicists have been thinking for almost a century.

**Third promising sign:** Our framework includes a big fat arrow that points in the direction of Connes’ work on the standard model. Connes has shown that the standard model of particle physics can be understood as a purely geometrical theory if one employs the machinery of non-commutative geometry. This result is based on a particular type of mathematics and we begin to see signs that our framework produces precisely this type of math.

All together it looks like a candidate for a fundamental theory. In fact, that is what we believe we have found. Time will tell if we are right.

Note that our theory actually offers an answer to the question “*why quantum?*”. The quantum aspect itself — the canonical commutation relations (i.e. the Heisenberg uncertainty relations), both bosonic and fermionic, as well as time evolution (the Hamilton operator) — is in our framework an *output*. It emerges from the basic *ansatz* of ‘moving stuff around’ combined with a metric principle. I think this is incredibly cool.
There are of course a number of open questions. Lot of them, in fact. I'll tell you more about them in my next newsletter, where I should be able to tell you about our solution to the “fermion problem”.

THE GOAL — AND OUR MOTIVATION

So what are we aiming at? What is it that we are really trying to achieve? And why?

Consider the scientific project in its entirety. During the past centuries scientists have advanced deeper and deeper into the fabric of reality, through biology, chemistry, atomic and nuclear physics and down to particle physics, where we stand today. Imagine if that process came to an end. Imagine if we actually found the final theory.

We have reasons to believe that the tower of scientific theories cannot be infinite; that there must be a bottom. So imagine if we actually reached that place.

What would it do to us? What would it tell us? About the Universe; about ourselves?

The idea, that Johannes and myself are working on, is that the final theory will be almost empty. That the final theory will not tell us much of interest except that the Universe has based its physics on a principle that is almost trivial.

To find such a theory will of course be a huge success, an unprecedented intellectual achievement of humankind. It will be celebrated; it will be crazy. But such an achievement will also place us in a very strange situation. Because the scientific project is not merely about finding scientific truth, it is also about understanding our own role in cosmos. In its core the scientific project has an element of mythology; we seek to understand what we are doing here. The old mythologies used to answer that question; modern man has instead turned to science.

What will happen if we find a theory that tells us nothing about all that? Okay, we might have expected such an outcome, but there is a huge difference between thinking that things will go wrong and things actually going wrong. If we find a final theory that tells us nothing of existential significance then we will have nowhere else to go in the external world to look for answers. The scientific project will be a dead end. It will be a door closed; forever.

I find such considerations incredible interesting. Personally they frighten me; I know that science will not tell me about my role in cosmos but I can’t deny that I have an irrational hope that at the foot of the ladder we will nevertheless find answers.
And if we find no answers at all; if we find something so trivial that its hardly worth the work of digging out, well, what are we going to do then?

The search will be over, what then?

In a strange way I think that the world needs to find such a theory. I am convinced that the discovery of a final theory will have a major impact on our societies, just as all major scientific discoveries throughout history have, and I suspect that the discovery of a final theory, that tell us nothing of significance, just might force us to think differently about the world — something that I believe that we need right now.

So my motivation is twofold: on the one hand the quest for a final theory is intellectually stimulating — its fun. I enjoy the work immensely, its the most interesting treasure hunt I can imagine. And on the other hand I believe that this work is important. I believe that we, humans on this planet, need to complete the work that the Greek began several thousand years ago. They started the scientific project — I believe that it is possible to end it1.

A CALL FOR SPONSORS

As I said, I would like to end this newsletter with a call for help.

The kind of research that Johannes and I are doing is not the kind of research that is being supported in the present academic system. Theoretical physics today is dominated by a very small number of ideas with very little room for alternative, independent ideas to be developed. Research grants are, broadly speaking, handed out to those with highest citation- and publication-counts, but to get a high number of citations you need to work close to mainstream — I have written about this on my blog — and thus a consequence is that the research communities tend to cluster in large tribes, where people talk to and cite each other and ignore outsiders. To be specific: the string theory community has completely dominated theoretical high energy physics for the past several decades and left very little oxygen for other ideas to be developed.

A small research team like Johannes and myself, who has wandered off far away from mainstream research in a completely new direction simply has no future in this system. We get few citations and that means that our research is not attracting

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1 And yes, science will not end if we find a final theory. There will be endless questions, it will never stop. But the opening of new territories, the discovery of yet deeper layers in reality, THAT will end. And that is the core of the scientific project, to dig deeper and deeper into the fabric of reality.
financial support. The fact that we publish in world leading journals in mathematical physics\textsuperscript{2} does not matter.

This has left me penniless and thus I am calling for support. The crowdfunding campaign in 2016 gave me a necessary financial injection, which has kept me afloat since, but that money is now spent. I’ve got a sponsor — the engineering company Regnestuen Haukohl & Køppen — who pays me around 300$ per month. I’m eternally grateful for their support but it is not enough and therefore I am looking for more donors and sponsors. Everything counts — you can support me through Paypal (a Paypal button is also available on my homepage) or you can contact me. For amounts larger than 2000 US$ I will acknowledge you in the acknowledge section in our next papers; for amounts larger than 10,000 US$ I will acknowledge you on the front page of our next papers. All donors will be acknowledged on my homepage.

I am also looking for more permanent sponsors: a fixed monthly payment in exchange for official recognition wherever I am present (papers, homepage, interviews, etc.).

I would like to encourage you to pass this on to others, whom you think might be interested in sponsoring one of the boldest and most innovative research projects in contemporary theoretical physics.

The work I do with Johannes is a full-time job. For the past 18 years I have dedicated almost all my time to this project. As long as we believe in our ideas and as long as we believe that we are capable of realising them then we will keep going; nothing can make us stop.

**HAVE A NICE SPRING**

With this I end this newsletter. I expect to write the next one sometime in the late summer or early fall — or whenever I have something interesting to tell you about.

Take good care of yourselves!

Best wishes, Jesper

www.jespergrimstrup.org

\textsuperscript{2} For instance: *Communications in Mathematical Physics, Classical and Quantum Gravity, Journal of Geometry and Physics, Journal of Noncommutative Geometry*. Our publications can be found on the Inspire database.