Condensed matter theory and the end of quantum, temperature, and gravity

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Mijnheer de Rector Magnificus,
Mijnheer de decaan,
Geachte toehoorders,

———

Welcome to this inaugural lecture on "condensed matter theory and the end of quantum, temperature, and gravity".

I will first say a few words in Dutch and then return to English for the remainder of the lecture.

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Dus, eerst even in het Nederlands: welkom allemaal.
Bedankt dat jullie allemaal hier aanwezig kunnen zijn voor mijn oratie.

Ik ga zometeen, in het Engels, uitleggen wat ik denk dat de rol is van de wetenschap, en natuurkundig onderzoek, in de samenleving; of wat ik denk dat die zou moeten zijn. Daarnaast zal ik uitleggen dat wij als mensheid volgens mij op het punt staan volkomen nieuwe aspecten van de natuur te ontdekken. Hoe succesvol ook, de natuurkunde zoals we die nu kennen, rammelt aan alle kanten. Ik zal uitleggen hoe ik denk dat de lessen van de zogenaamde "gecondenseerde materie," mijn voornaamste vakgebied, de weg wijzen naar ontbrekende, nog onbekende delen van de natuur die fundamenteel anders zullen zijn dan alles wat we tot nog toe kennen.

Voordat ik jullie meeneem op die opwindende reis door de moderne wetenschap, wil ik eerst het college van bestuur van de Universiteit van Amsterdam en de decaan van de faculteit der Natuurwetenschappen, Wiskunde en Informatica bedanken voor het in mij gestelde vertrouwen bij deze benoeming. Ik wil ook graag deze gelegenheid gebruiken om een aantal mensen publiekelijk te bedanken wiens steun gedurende de afgelopen decennia het voor mij mogelijk heeft gemaakt op dit punt te komen.

[..Persoonlijk dankwoord..]

Tenslotte will ik op dit punt graag expliciet maken dat mijn aanwezigheid op dit podium, als Nederlandse, relatief oude, witte man, overduidelijk niet bijdraagt aan de diversiteit van academia in Nederland. Ik heb me plechtig laten beloven dat mijn promotie vandaag op geen enkele manier de carrière van anderen in de weg kan staan of vertragen, en ik accepteer het hoogleraarschap natuurlijk graag. Maar het begeleiden van een groot aantal ijzersterke studenten en postdocs tijdens de afgelopen jaren, en alle open gesprekken die ik in die tijd met hen heb mogen voeren, hebben voor mij wel duidelijk gemaakt hoe veel meer problemen dan ik velen van hen moeten overwinnen om op op deze spreekstoel aan te komen. Ik ben van jongs af aan getraind, vaak impliciet, om mijn senioriteit, witheid, en mannelijkheid te projecteren en te gebruiken. Ik heb dat uitgebreid gedaan, en er ontegenzeggelijk voordeel van gehad. Ik ben er dankbaar voor dat mij gedurende de afgelopen tien jaar steeds meer de ogen geopend zijn, en dat ik in verschillende verbanden heb mogen nadenken en uitproberen hoe het anders zou kunnen. Het zal binnenkort wel weer hip worden om "anti-woke" te zijn, maar ik in ieder geval, zal door blijven gaan nieuwe kansen te creëren waar ik kan, en de academische wereld hopelijk stapje voor stapje een beetje eerlijker en beter te maken.

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Back to English now, and on to physics.

In the coming half hour or so, I would like to argue that the lessons of condensed matter physics force us to consider the limits of several scientific theories and concepts, including those that give rise to the most pressing conceptual problems in modern physics:

quantum, temperature, and gravity.

Before going there, however, I feel that I should explain to you why I even care about such things. Isn't the point of physics, and of condensed matter physics in particular, to provide society with technical solutions to its (many) practical problems? As a professional physicist paid from taxpayer's money, shouldn't I be designing materials that enhance battery power, or figuring out how to make mobile phones without rare minerals, rather than thinking about abstract concepts like the meaning of temperature or the limits of quantum theory?

Well, no. Not if you ask me.
Scientifically trained university graduates make great practical-problem solvers, who can be hired by companies and national labs to earn a decent wage confronting the problems that affect us all. But I believe independent scientists, working at universities, have a different role to play in society.

To explain what I believe science is for, and what you may expect scientists to produce, I would like to introduce the idea that knowledge and understanding come in several levels. This idea was recently described in a beautiful way by one of my former PhD students, in his popular science book (which you should definitely read). He does it so well that I will only slightly paraphrase his words for you now[1]:

"Scientists, like everyone else, are on a journey of understanding. It is a three-stage journey, which all people in the world have begun. When we are young, we are fascinated by the world around us. This is the first stage, enjoying the discovery of previously unknown and unimagined feats of nature. As we get older, or progress in our study of physics, we begin to learn how things work. We approach the second stage, understanding how feats of nature are accomplished and being able to predict and employ the tricks of nature. It is easy to get stuck here, and succumb to cold rationality. But if you manage to keep a bit of curiosity smouldering, then with patience and perseverance, you can go on to become a scientist and proceed to the third level of understanding. Here you not only comprehend things you studied before, but you recognise new tricks being performed by nature, you see how they all fit in and complement one another, and what their limits are. Having reached this stage, rather than feeling disappointed for readily seeing through the wonder of the new tricks of nature, you love it all the more for seeing the skill of the performance."

So far the description of the three levels of understanding that scientists pursue.

Notice that there is no level four.
Every theory of physics and every description of nature is an imperfect approximation of reality. Once you understand a topic in science so well that you can apply it across a range of seemingly unrelated, vastly different settings, and once you start seeing also the limits of its applicability, you are a true master of the topic. At this point, you will start to ask new, fundamentally different questions, at a level where the previous theory simply doesn't make sense anymore. You then arrive at level one again, but for a new part of science and a new journey of discovery into previously unexplored parts of nature.

To give just one example, you might wonder why, if you hit your hand on a table, your hand doesn't just fly through. After all, the atoms in your hand are tiny and far apart, and the empty space in between actually makes up most of your hand as well as the table. The level one understanding here, is that there actually is something to wonder about when you combine the idea of sparse atoms with the observation of rigid tables. How do the atoms in my hand know not to pass through the table? And more importantly, why do they pass through a volume of water or oil, in which the atoms look more or less the same as those in the table?
After you learn a bit of condensed matter physics, you will find that the answer lies in concepts of rigidity, order, and something called spontaneous symmetry breaking[2]. Once you learn how to define these things mathematically and you understand philosophically how they relate to the real world, you can predict which objects your hand will move through, and which it will not. You can

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[1] Felix Flicker, *The Magick of Matter: Crystals, Chaos and the Wizardry of Physics*, Profile Books (London), 2022.

[2] Aron J. Beekman, Louk Rademaker, and Jasper van Wezel, *An Introduction to Spontaneous Symmetry Breaking,* SciPost Phys. Lect. Notes **11** (2019).

even become quite practical and calculate just how much your hand will hurt if you hit any given object. This is level two. You achieved a realistic understanding and the ability to make useful predictions. It is this part of science that society typically values, and promising to produce more level two results is the way in which academics have always convinced politicians and tax payers to fund scientific research. I personally believe, however, that it is not the real goal of science, nor the motivation for many scientists to conduct research in a high-stress, low-reward environment with many interfering responsibilities.

Coming back to our hand and table example, once you've learned how symmetry breaking underlies the interaction between them, you might go on and learn about other theories of modern physics. You could for example learn about exotic and fantastic things like superconductors[3], which allow electricity to be transported without any resistance; or about altermagnets[4], which produce opposite magnetisation when pressed in orthogonal directions; or even about the Anderson-Higgs effect[5], which gives mass to massless particles. At some point you might then realise that all of these descriptions of wildly different subjects work according to the same principles of symmetry breaking, and that in a very real way, a superconductor is just another incarnation of a table[2]. Even the Anderson-Higgs effect, which requires multi-billion-euro accelerator experiments to observe for elementary particles[6], is deep down the same thing that you can see by hitting your hand on an only slightly exotic type of table.

At that point, you have reached level three. You now understand the generic principles that separate order and rigidity from fluid phases of matter and you can apply them even to objects you never studied before, be they canted antiferromagnets, pair-density waves, or vector bosons. Most importantly, you can now identify the limits of your newfound understanding. Seeing how a table and a superfluid are two manifestations of the same physics allows you to ask what other types of objects may behave like a table, as well as which variations of the table physics can never be realised. For example, solid neutron stars are certainly feasible[7], but how to realise a time crystal is far from clear[8].

This now, is the actual product of science. Ultimately, science is about pursuing questions in order to find new questions, and not about the answers to any of them. The long-term, consolidated knowledge that scientists as a species strive for, always opens up more new questions than it answers old ones. This is also precisely the point.

Of course I am not saying that scientists should not be interested in level two goals, nor that they should avoid looking for practical answers and solutions to the current problems of society. Even if level three insights are the goal, it requires breathtaking amounts of level two work to get there. Moreover, no one can get to level three on their own. Science is inherently a cooperative endeavour, in which level two results obtained by generations of scientists across the world all eventually contribute to the collective emergence of level three understanding. Anyone having a truly new level three realisation is rare. Still, I believe this is what we do science for, and at the end of the day, it is what propels us, humans, in our understanding and appreciation of nature.

As a side note, I think it would be good if society, and in particular funding agencies, keep this ultimate aim and the naturally collaborative nature of scientific progress in mind. It is currently impossible to fund research that explicitly aims at anything other than level two results. Moreover, and even worse, all funding instruments are currently competitive. As it stands, scientific funding

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[3] Michael Tinkham, *Introduction to Superconductivity*, McGraw-Hill (New York) 1975.

[4] Libor Šmejkal, Jairo Sinova, and Tomas Jungwirth, *Emerging Research Landscape of Altermagnetism*, Phys. Rev. X **12**, 040501 (2022).

[5] P. W. Anderson, *Plasmons, Gauge Invariance, and Mass*, Phys. Rev. **130**, 439 (1963); F. Englert and R. Brout, *Broken Symmetry and the Mass of Gauge Vector Mesons*, Phys. Rev. Lett. **13**, 321 (1964); P. W. Higgs, *Broken Symmetries and the Masses of Gauge Bosons*, Phys. Rev. Lett. **13**, 508 (1964).

[6] ATLAS Collaboration, *Observation of a new particle in the search for the Standard Model Higgs boson with the ATLAS detector at the LHC,* Phys. Lett. B **716**, 1 (2012); CMS Collaboration, *Observation of a new boson at a mass of 125 GeV with the CMS experiment at the LHC*, Phys. Lett. B **716**, 30 (2012).

[7] J. M. Lattimer and M. Prakash, *The Physics of Neutron Stars*, Science **304**, 356 (2004).

[8] Alfred Shapere and Frank Wilczek, *Classical Time Crystals*, Phys. Rev. Lett. **109**, 160402 (2012); Alfred Shapere and Frank Wilczek, *Quantum Time Crystals*, Phys. Rev. Lett. **109**, 160401 (2012); Patrick Bruno, *Comment on "Quantum Time Crystals"*, Phys. Rev. Lett. **110**, 118901 (2013); Haruki Watanabe1 and Masaki Oshikawa, *Absence of Quantum Time Crystals*, Phys. Rev. Lett. **114**, 251603 (2015); Dominic V. Else, Christopher Monroe, Chetan Nayak, and Norman Y. Yao, *Discrete Time Crystals*, Ann. Rev. Cond. Mat. Phys. **11**, 467 (2020).

in all countries that I know of is organised like a game of monopoly, with very much the same indicators of success, and the same results in terms of distribution of resources, strategic politics, and incessant scheming.

Since I have some type of podium here today, perhaps it would be good to point out that we can collectively decide to embrace a different strategy. It is not impossible to organise science and science funding as a cooperative, rather than a competitive endeavour. If you're unsure how to do that, there is plenty of inspiration to be had from the actual games industry, in which cooperative games have been steadily gaining in popularity and sophistication for decades[9].

That being said, let's return to the pursuit of level three insight, and how in particular the techniques of condensed matter physics naturally guide us there. For me personally, this lesson was brought home already early in my scientific career, starting with one of my postdoc advisers asking me to look into a so-called "charge ordered" material.

Already at the time, charge order was generally considered to be a completely understood, somewhat old-fashioned, stuffy materials science topic[10]. Just around that time, however, new experiments were being performed on particular types of charge ordered materials[11], and I quickly realised that the old descriptions did not quite fit. Several theoretical models of charge order had in fact been proposed and widely used during the preceding decades, each focusing on the explanation of different observed features. Some of the theories explained how electrons in these materials group together, others discussed the movement of atoms, and yet different ones focused on the dynamics of domain walls. Each of the descriptions employed approximations. This is utterly natural. Physics is all about making mathematically precise predictions in situations that cannot possibly be captured in an exact way. The approximations used to understand different aspects of charge order were also completely reasonable, and the resulting theories yielded many experimentally confirmed predictions[11].

Superficially the newly studied charge ordered materials seemed to fit right in, as boring, dirty metals with typical signatures of the plain-vanilla, old fashioned charge order described by the classic theories. However, putting together the results of several high-precision measurements, obtained within different collaborations spanning several jobs in multiple continents and countries, it turned out that the different theories describing individual observations could not be made mutually consistent. What had been perfectly reasonable approximations in the modelling of isolated features of traditional charge order turned out to be untenable when describing the full set of new observations.

For example, it is perfectly fine to approximate the interaction between electrons and atoms to be the same for all particles in a material, as long as only a few electrons and atoms contribute to the conductivity you're making predictions about. But if at the same time a different experiment shows that all electrons in the material participate in shaping the current, and an independent calculation shows that in any stable material interactions have to vanish for at least some electrons and atoms, then you arrive at an internal inconsistency.

This is a good thing.

The confluence of experimental and theoretical results shows that approximations that are reasonable for describing individual measurements cannot always hold when pushing our understanding to the limit, describing all available experimental observations together, in one go. In this way, the approximate theory reveals its own limitations, and moreover, it indicates precisely what a higher-level theory needs to avoid, and what to include.

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[9] Alfie Kohn, Foreword to *Cooperative Games in Education* by Suzanne Lyons, Teachers College Press (New York) 2022.

[10] Rudolf Ernst Peierls, *Surprises in theoretical physics* Vol. 10, Princeton University Press (Princeton), 1979.

[11] K. Rossnagel et al., *Fermi surface of 2H-NbSe$_2$ and its implications on the charge-density-wave mechanism*, Phys. Rev. B **64**, 235119 (2001); M. D. Johannes, I. I. Mazin, and C. A. Howells, *Fermi-surface nesting and the origin of the charge-density wave in NbSe$_2$*, Phys. Rev. B **73**, 205102 (2006); G. Li et al., *Semimetal-to-Semimetal Charge Density Wave Transition in 1T-TiSe$_2$*, Phys. Rev. Lett. **99**, 027404 (2007); H. Cercellier et al., *Evidence for an Excitonic Insulator Phase in 1T-TiSe$_2$*, Phys. Rev. Lett. **99**, 146403 (2007); S. V. Borisenko et al., *Two energy gaps and Fermi-surface "arcs" in NbSe$_2$*. Phys Rev Lett **102**, 166402 (2009); J. Ishioka et al., *Chiral Charge-Density Waves*, Phys. Rev. Lett. **105**, 176401 (2010). F. Weber et al., *Extended Phonon Collapse and the Origin of the Charge-Density Wave in 2H-NbSe$_2$*, Phys. Rev. Lett. **107**, 107403 (2011); Yejun Feng et al., *Order parameter fluctuations at a buried quantum critical point*, Proc. Nat. Ac. Sci. **109**, 7224 (2012); D. J. Rahn et al., *Gaps and kinks in the electronic structure of the superconductor 2H-NbSe$_2$ from angle-resolved photoemission at 1K*, Phys. Rev. B **85**, 224532 (2012); Anjan Soumyanarayanan et al., *Quantum phase transition from triangular to stripe charge order in NbSe$_2$*, Proc. Nat. Ac. Sci. **110**, 1623 (2013); U. Chatterjee et al., *Emergence of coherence in the charge-density wave state of 2H-NbSe$_2$*, Nat. Commun. **6**, 6313 (2015).

In this particular case, after obtaining a full set of observations and asking the right questions, it still took an ambitious PhD student a few years to figure out how to realistically model charge order with electrons and atoms that don't all behave the same way. This was actually possible though, and it resulted in a theory that quantitatively captured all experimental results known at the time for one particular charge ordered material[12]. More importantly, it could subsequently be applied to similar materials and eventually led to what I believe is a level-three understanding of "structured coupling" in charge ordered materials. As expected of a level three result, it opened up entirely new directions of research in for example orbital order and pair density waves.

On its own, this example can be seen as a nice little success story in a very particular, somewhat niche area of research. But the theme is general: perfectly reasonable theories of physics signal their own breakdown by developing internal inconsistencies when pushed to the limit where new physics takes over. Of course I don't what to claim that this mechanism for identifying the reach of approximations is in any way a new discovery. In fact it occurs and is used all the time in physics.
For the experts, a similar very well-known example discussed in all textbooks on condensed matter theory is that of mean field theory. The mean field approximation typically suffices to establish what the order parameter of any particular system is, and what the relevant, emergent degrees of freedom in the ordered state will be. But it fails miserably in predicting the scaling relations between critical exponents that you find by comparing outcomes of multiple types of experiments. The theory gets these wrong because the approximation that fluctuations are absent cannot be maintained when describing the relation between experiments. Moreover, the very breakdown of this approximation shows the way to a better theory. By including fluctuations beyond the mean field, we cannot only quantitatively predict scaling relations, but we can also start asking new questions about the approach to criticality.

In my own work after the charge order story, similar journeys led to the realisation that phonons and excitons cannot be distinguished in so-called excitonic insulators and a universal theory of bound electron-hole pairs takes over[13]; that all topological invariants describe the interplay of symmetry and curvature and can be captured in a single classification[14]; and most recently that dualities go hand in hand with anomalies and cannot be described without taking their physical implementation into account[15]. These are small, but nice, insights, obtained by examining the limits of established theories, and I should stress that they're not mine alone. Besides building on heroic results of others and working with many fantastic collaborators, I have also been lucky to work with a succession of students who were smart enough to see beyond the immediate calculations they were doing, and brave enough to keep asking questions and exploring further. The point of mentioning all these specific results is that in all of them the combination of multiple experiments and theoretical analyses laid bare internal inconsistencies of known theories in a systematic way.
There is one more setting I would like to discuss, in which the same motif is currently playing out and becoming more and more visible. It is a much larger setting in which a limit of modern physics becomes visible: that of quantum theory itself.

Quantum theory has been rightfully hailed as one of the deepest and most accurate descriptions of nature that we've ever had. It correctly describes with incredible precision everything from the scattering of elementary particles to the colour of gold. It allows and explains fantastical but true things like teleportations and quantum computation. And yet, there is no reason to believe that it should be any less of an approximation to nature's dynamics than any other theory of physics. It

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[12] F. Flicker and J. van Wezel, *Charge order from orbital-dependent coupling evidenced by $NbSe_2$*, Nat. Commun. **6**, 7034 (2015); F. Flicker and J. van Wezel, *Charge ordering geometries in uniaxially strained $NbSe_2$*, Phys. Rev. B **92**, 201103(R) (2015); F. Flicker and J. van Wezel, *Charge order in $NbSe_2$*, Phys. Rev. B **94**, 235135 (2016).

[13] J. van Wezel, P. Nahai-Williamson, and S. S. Saxena, *An Alternative Interpretation of Recent ARPES Measurements on $TiSe_2$*, Europhys. Lett. **89**, 47004 (2010); Anshul Kogar et al., *Signatures of exciton condensation in a transition metal dichalcogenide*, Science, **358**, 1314 (2017).

[14] Jorrit Kruthoff et al., *Topological classification of crystalline insulators through band structure combinatorics*, Phys. Rev. X **7**, 041069 (2017); Jorrit Kruthoff, Jan de Boer, and Jasper van Wezel, *Topology in time-reversal symmetric crystals*, Phys. Rev. B **100**, 075116 (2019); Jans Henke et al., *Topological invariants of rotationally symmetric crystals*, Phys. Rev. B **104**, L201110 (2021).

[15] José Dupont and Jasper van Wezel, to be published.

obviously has a large realm of applicability, but there is no cause to think that it should be a universal theory of everything.

In fact, we known it is not.

Literally every single thing you see around you has physical properties that require quantum theory to explain. Nevertheless, none of them seem to behave particularly quantum as a whole. They don't tunnel, teleport, interfere, or do any other typical quantum tricks. This inconsistency has been noted by many since the advent of quantum theory in the 1930's.

The most usual response these days is to assume that large objects are in fact fully quantum, but that typical quantum behaviour is hidden, either in a parallel universe or guiding wave, or more commonly, in the quantum evolution of large numbers of unobserved microscopic particles that constantly interact with or bounce into any large object. Philosophically pleasing as these responses may be, they do not resolve all inconsistencies that arise when combining the results of all observations and calculations that physicists collected over the past century[16].

For example, observations of phase transitions, outcomes of quantum computations, chemical reactions, or particle-antiparticle annihilation events, in which information is fundamentally destroyed rather than lost in an environment, are all inconsistent with the unitary evolution that has been experimentally established many times over to govern the behaviour of individual quantum particles.

Despite endlessly repeated attempts, discussions, and arguments, this inconsistency cannot be resolved by reinterpreting quantum theory, nor by invoking unobserved interactions. Taking these results at face value, then, I hope that by now you recognise how they fit into the general pattern of a theory reaching its limits. In the present case, the inconsistency clearly points towards the breakdown of a central assumption in the way quantum theory is constructed: that of unitary time evolution.

The term unitarity might not mean much to non-experts, so let me point out that it is mostly a very convenient property to have in any model of nature, and especially in any field theory. In fact, the enormous success of quantum field theory has instilled an almost mythical reverence for unitarity in most, if not all, theoretical physicists growing up during the past fifty years or so. I should also point out, however, that it is not a necessary ingredient of any physical theory per se. There are some things that we consider unbreakable in physics, like causality for example, but there is no observational, mathematical, or philosophical reason that unitarity should be one of them.

Having come to this point, and recalling the discussion of theories indicating their own limits when confronted with experiments spanning large ranges of different scales, it seems clear to me that we should at least consider the possibility that the assumption of unitary evolution breaks down at some point. I am of course not the only one reaching this conclusion. In fact, possible non-unitary theories beyond quantum physics have been proposed for a while[17], and multiple new types of cutting edge, extreme-precision measurements are currently being developed to search for evidence of the breakdown of unitary quantum evolution in large scale quantum experiments. What I, with the help of a series of talented students, tried to add to this story, is that if unitarity evolution breaks down in macroscopic objects, it will inevitably do so in the same way that we've seen other approximations break down in the same limit[18].

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[16] J. van Wezel, *Broken Time Translation Symmetry as a model for Quantum State Reduction*, Symmetry **2**, 582 (2010).

[17] A. Bassi and G. Ghirardi, *Dynamical reduction models*, Phys. Rep. **379**, 257 (2003).

[18] Jasper van Wezel, *Quantum Dynamics in the Thermodynamic Limit*, Phys. Rev. B **78**, 054301 (2008); Jasper van Wezel and Tjerk H. Oosterkamp , *A nanoscale experiment measuring gravity's role in breaking the unitarity of quantum dynamics*, Proc. R. Soc. A **468**, 35 (2012); Jasper van Wezel, *An instability of unitary quantum dynamics*, J. Phys. Conf. Series **626**, 012012 (2015); Lotte Mertens et al., *Inconsistency of linear dynamics and Born's rule*, Phys. Rev. A **104**, 052224 (2021); Jasper van Wezel, *Phase transitions as a manifestation of spontaneous unitarity violation*, J. Phys. A: Math. Theor. **55**, 401001 (2022); Lotte Mertens, Matthijs Wesseling, and Jasper van Wezel, *An objective collapse model without state dependent stochasticity,* SciPost Phys. **14**, 114 (2023); Lotte Mertens and Jasper van Wezel, *Environment-assisted invariance does not necessitate Born's rule for quantum measurement*, Entropy **25**, 435 (2023); Aritro Mukherjee et al., *Quantum state reduction of general initial states through spontaneous unitarity violation,* Entropy **26**, 131 (2024); Aritro Mukherjee and Jasper van Wezel, *Colored noise driven unitarity violation causing dynamical quantum state reduction*, Phys. Rev. A, **109**, 032214 (2024); Lotte Mertens, Matthijs Wesseling, and Jasper van Wezel, *Stochastic field dynamics in models of spontaneous unitarity violation,* SciPost Phys. Core **7**, 012 (2024).

Remember the hand that will not pass through a table when it hits it? The rigidity of the table and the hand that cause them to interact so violently, is a typical property of macroscopic objects, which does not occur in the microscopic quantum world. In this case, there is no question how the quantum fragility breaks down and macroscopic rigidity emerges. It is the approximation of perfect symmetry that becomes untenable for large objects, and the theory extending beyond this approximation is called spontaneous symmetry breaking. One of its main lessons is that the larger a quantum object is, the more sensitive it will be to imperfections in any symmetry. This is why any approximation involving symmetry must necessarily break down for everyday objects, which are so large compared to quantum scales that literally anything will take them out of the regime where the perfect symmetry approximation applies[2].
Coming back to the unitary evolution of quantum theory, what we managed to show over the past years, is that it behaves in precisely the same way as a symmetry. The larger a quantum object, the more sensitive it becomes to imperfections in unitarity. And just like for spontaneous symmetry breaking, this implies that the approximation of unitary evolution necessarily breaks down in everyday objects. As it turns out, this insight is not yet widely shared by others working on the topic, probably because, as I tried to show you before, it follows a typical condensed matter chain of reasoning that is perhaps not pervasive in other parts of physics. As a consequence, the theory that we found by going beyond the broken approximation and overcoming the internal inconsistencies, is somewhat different than other proposed models for the breakdown of quantum theory. On the one hand it naturally solves a few problems that other models still struggle with, such as the preferred basis problem, energy conservation, and the prediction of definitive experimental signatures. But on the other hand it still has some unique problems of its own, such as the need for a particular fluctuation-dissipation relation, whose origin and role we are yet to uncover[18].
The upshot is that the theory of spontaneous unitarity violation, and the limits of quantum theory are not fully resolved yet, but that the problem of unitarity breaking falls into the general theme of approximate theories reaching their limits, and that we do know how to proceed from there using the well known tools and techniques of condensed matter physics. In fact, things are even a bit better than that. Although the final beyond-quantum theory has not fully crystallised yet, its origin in broken unitarity guarantees that it will have certain features.
Excitingly, these must involve some as-yet unknown physical entities, such as fields that do not host quantum excitations themselves, but that do subtly influence the dynamics and interplay of all quantum particles we know[18]. The effects of such new ingredients of physics will almost certainly cause deviations from quantum behaviour that will be experimentally accessible within some years from now. Moreover, given that they cause the spontaneous breakdown of unitary, these signatures will be non-quantum in a predictable way, and we already formulated methods for definitively distinguishing them from the decoherence that plagues existing macroscopic quantum measurements[18].

Thrilling times filled with fundamentally new physics of the sort that we haven't seen in science for about a century seem just around the corner. Even better, at least for a theoretical physicist like myself, is that everything we now predict about the breakdown of quantum physics based on the unitary approximation breaking down, points towards the emergence of two very specific ingredients for the beyond-quantum theory: deterministic evolution towards energy eigenstates, and entropic forces between macroscopic objects. These may sound like incomprehensible technicalities, but they are actually both key ingredients in various existing proposals for understanding the already observed limits of the two other corner stones of modern physics: temperature and gravity.

Thermal physics gave us the steam engine and thereby arguably set mankind on an irreversible course towards individual humans escaping the daily struggle for survival, and collective humanity destroying life on earth. Despite this practical success, the very meaning of temperature and the seemingly ubiquitous increase of entropy in natural processes have always been at odds with unitary quantum evolution. The so-called "eigenstate thermalisation hypothesis" brings some respite, but it still invokes a typicality argument and an averaging procedure that make it hard to apply to individual, isolated quantum processes in the lab[19].

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[19] Luca D'Alessio et al., *From quantum chaos and eigenstate thermalization to statistical mechanics and thermodynamics*, Advances in Physics **65**, 239 (2016).

It has already been suggested, however, that if the limits of quantum theory cause quantum states to evolve to energy eigenstates in uncorrelated and practically unpredictable ways, this may yield a way of reaching the ingredients required for eigenstate thermalisation without any typicality or averaging[19]. That would be a beautiful unification of quantum theory and thermal physics, which would simultaneously teach us about the limits of both theories, and allow us to begin exploring what lies beyond.

Similarly, the theory of gravity, as formulated by the Einsteins, is famously at odds with quantum theory, and the two are yet to be observed together within a single experiment. One way out of this conundrum, which has been popularised by several groups worldwide, is that gravity may emerge as an effective force between massive objects, caused by wild fluctuations of some intermediary field[20]. This field cannot be part of quantum theory, as that would inevitably reintroduce the gravity-quantum dichotomy. It seems, however, that the limits of quantum theory itself point towards the existence of practically unpredictable fluctuations of a type that does not obey the laws of quantum physics. These could in principle mediate entropic forces between sufficiently large objects.
It is tempting to speculate that such forces emerging at long length scales could constitute what we observe as gravitational interactions between massive bodies. Even I, however, would consider that a long shot. It will take at least twenty more years of level two research all over the world before we can come to any solid level three understanding of a possible relation between the emergence of gravity and the limits of quantum theory.

What I'm trying to convey, is that we are currently in an era of science where we start to see the limits of all central ingredients of modern physics. The combined input of many, often challenging and state of the art, experimental measurements lay bare internal inconsistencies when we try and obtain a simultaneous theoretical understanding for all of them. These in turn point to approximations that were perfectly reasonable for describing isolated experiments, but are no longer viable when applied across the range of conditions encountered in the collective interpretation of many experiments.
Right now, precisely this appears to be happening to what previously seemed to be fundamental building blocks of physics. That all of them may moreover turn out to be connected in unexpected ways, makes the current time a very exciting moment to be part of the collective, cooperative game of science.

I look forward, in my newly obtained role, to continue the pursuit of comparing observations and theories from all scales and regimes of physics, and using their combined lessons to lead the way to the end of quantum, temperature, and gravity, and beyond.

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Ik heb gezegd.

[20] T. Jacobson, *Thermodynamics of Spacetime: The Einstein Equation of State*, Phys. Rev. Lett. **75**, 1260 (1995); T. Padmanabhan, *Thermodynamical Aspects of Gravity: New insights*, Rep. Prog. Phys. **73**, 6901 (2010); E.P. Verlinde, *On the Origin of Gravity and the Laws of Newton*, J. High Energ. Phys. ***2011**, 29* (2011).