

Guiding question. *In our quantum world, how far can we push learning and computation?*

MY RESEARCH TO DATE

My PhD research [1–30] has focused on how efficiently we can *learn*, *simulate*, and *compute with* quantum systems. I have explored these questions across a broad range of systems central to physics and quantum computing, from qubits [1–3, 19, 21–27] to bosonic [8–13] and fermionic systems [4–8], with direct relevance to current experiments. My research has also addressed the fundamental power of noisy quantum circuits and their classical simulability [14, 15], the power of random unitary ensembles [4, 16, 17, 21–23], and the prospects for verifiable quantum advantage [14–16]. It has also uncovered structures and tools of independent interest, from Clifford symmetries [21], central to quantum error correction, to structural properties of bosonic and fermionic systems [4–13], with applications beyond their original motivation.

1. Quantum learning [1–13, 23]

Qubits [1–3]. One of my favorite results [1] (*Nature Physics*, accepted) has been addressing an open problem in quantum process tomography: *how many uses of an unknown quantum channel are needed to learn it in diamond distance*, the standard metric for distinguishing quantum processes? While the analogous problem of quantum state tomography had been settled nearly a decade ago [31, 32], it remained open for general channels. We designed an algorithm with optimal dependence on the input and output dimensions and Kraus rank, recovering optimal state tomography as a special case. In Ref. [2] we then established a *quantum memory advantage* for the same task, showing that retaining quantum information across channel uses reduces the number of experiments needed compared with protocols that retain only classical information, even with adaptivity and unrestricted classical computation. My work has also concerned learning *stabilizer states* and, more generally, states prepared by Clifford+ T circuits [3]. While it was long known that n -qubit stabilizer states can be learned optimally using $\Theta(n)$ copies with two-copy Bell measurements, we showed that the same scaling can be achieved without quantum memory at all, using only efficient, adaptive single-copy Clifford measurements [3].

Fermions [5–8]. Fermionic systems hold particular importance due to their crucial roles in physics, from quantum chemistry to condensed-matter physics. My research has investigated the fundamental limits of learning and testing fermionic quantum states, from free fermions to systems with interactions. For free-fermionic (Gaussian) states, we developed efficient and robust tomography algorithms [6], substantially improving on previous results, and, going beyond Gaussianity, we showed that states prepared with up to $t = O(\log n)$ local non-Gaussian gates can still be learned efficiently via an experimentally feasible algorithm, while proving that learning becomes computationally intractable for superlogarithmic t under cryptographic assumptions [5]. We also investigated *Gaussianity testing*, showing that while testing general mixed states requires exponentially many copies [6], pure-state Gaussianity can be tested with only a *constant* number of copies [8].

Bosons [8–13]. Bosonic, or continuous-variable (CV), systems play a central role in quantum optics and quantum technologies, from photonic quantum computing to communication and sensing. Yet, despite decades of experimental work, their fundamental learning limits were much less understood, in contrast to the extensive theory developed for finite-dimensional quantum systems [31, 32]. A major part of my research has been devoted to filling this gap. In our first work on the topic [9] (*Nature Physics*), we established fundamental limits on bosonic tomography, showing that learning general states can be extremely inefficient even under energy constraints, while Gaussian states and states with limited non-Gaussianity can be learned efficiently. We then developed an experimentally feasible protocol for Gaussian-state tomography whose sample complexity is almost independent of energy, improving the previous polynomial dependence to doubly logarithmic [11]. Subsequent works established its near-optimality among all Gaussian protocols [33, 34], and our algorithm is now being implemented in an ongoing experimental collaboration [35]. We also extended efficient learning to Gaussian quantum processes [13] and developed optimal and experimentally feasible algorithms for testing pure-state Gaussianity using only a *constant* number of copies, independent of system size and energy [8]. More recently, we explored a broader question connecting quantum learning to the emergence of classicality: *when do quantum states become as easy to learn as classical ones?* We showed that thermal fluctuations govern this transition for Gaussian states [12]: while cold Gaussian states can be fundamentally harder to learn, warm states can be learned just as efficiently as classical Gaussian distributions.

2. Quantum algorithms, noise, and classical simulation [14–20]

A major part of my research has focused on understanding how noise limits the computational power of quantum circuits in the pre-fault-tolerant era. In our *Nature Physics* work, we showed that noise makes typical deep quantum circuits effectively shallow: only the last $O(\log n)$ layers significantly influence local observable expectation values [14]. This led to efficient classical simulation algorithms for a broad range of circuit architectures and noise models, which we later improved using Pauli propagation methods [15]. Importantly, our results go beyond the depolarizing and unital noise models commonly studied in earlier work, covering non-unital noise such as amplitude damping and bringing these fundamental results closer to real quantum devices. Moreover, I have also studied the power of quantum randomness, from how efficiently it can be generated to its role in demonstrating quantum

advantage. We constructed *strong unitary designs* in optimal logarithmic depth without auxiliary qubits, using all-to-all interactions [17]. At Google Quantum AI, I investigated a question central to *verifiable quantum advantage*: *can we find observables in deep random circuits whose signal survives as the system grows?* We showed that out-of-time-order correlators (OTOCs), which probe how quantum information spreads, retain measurable fluctuations even at depths growing with system size [16], settling an open question motivated by recent experiments towards verifiable quantum advantage. We also developed a faster classical algorithm for estimating these observables in one-dimensional circuits. My earlier research has also explored a central challenge for variational quantum algorithms: *how can we make quantum models efficiently trainable?* Motivated by applications to many-body physics and machine learning, we showed how problem symmetries can improve quantum models [19], developed initialization strategies to help avoid barren plateaus [18], and showed that measurement noise can help optimization escape saddle points [20].

3. Mathematical structures, tools, and teaching [17, 21–27]

One aspect of my research I have found particularly rewarding is how solving specific problems has revealed mathematical structures and tools with applications far beyond their original motivation. For example, our study of the Clifford group, central to quantum error correction, led to a complete theory of its higher-order symmetries, with applications to stabilizer testing, quantum magic, and the study of unitary designs [21]. Our work on learning bosonic and fermionic systems also led to optimal trace-distance bounds relating experimentally measurable low-order moments to the distinguishability of Gaussian states [6, 10], and revealed how non-Gaussianity generated by a few gates can be compressed into a small subsystem using Gaussian operations, with applications to efficient quantum circuit compilation [5]. Making these tools accessible to a broader community has also been an important part of my work. My tutorial on the Haar measure, which develops fundamental tools for quantum information starting from basic linear algebra, has become my most-cited published work [28]. I have also taught a 12-hour QMATH masterclass on representation theory for quantum information, and given invited tutorials and lectures at EPFL, QISCA, and AIMS [29, 30, 39].

MY FUTURE DIRECTIONS AND VISION, IN SHORT

My research goal for the coming years is to help **discover new useful quantum algorithms** and bring useful fault-tolerant quantum computing closer to reality. I want to find compelling applications of quantum computing in physics, chemistry, machine learning, and beyond, and develop new quantum algorithms for sensing and learning that can advance scientific discovery. Yes, this may sound too ambitious, but I believe that discovering genuinely new quantum algorithms and compelling applications of quantum computers is the most important challenge for quantum computing theory right now.

And the reason I now feel so determined to pursue such an ambitious goal is, of course, **AI**. I did not initially intend to make this a research statement about AI, but I cannot hide how much it has changed my vision and the research directions I want to pursue in the coming years. Earlier this year, I was writing a version of this statement with a more conservative set of goals: well-defined open problems in quantum information that I found interesting and felt were within my reach. I had to change that statement because I was able to solve a good fraction of them by feeding their statements and a few prompts into frontier AI models, which by now have even been used to tackle hundreds of long-standing open problems in mathematics [38]. Seeing these newfound capabilities has made me rethink my research agenda and become much braver about pursuing difficult, high-risk ideas with potentially much greater impact!

Even with AI, we still have too few compelling examples of useful quantum algorithms and applications. As Dorit Aharonov [36] and Robbie King [37] have emphasized in recent years, we need more researchers willing to take on the difficult search for fundamentally new quantum algorithms. I think AI makes this call to action more exciting and less scary than ever! I would find it a shame if, even as AI is settling some of the hardest conjectures in mathematics, we did not manage to use these newfound tools to discover entirely new ways to make quantum computers useful. AI can also help us connect ideas across fields. Discovering new applications of quantum computing in the physical sciences may require deep expertise in very specific problems in high-energy physics, chemistry, materials science, or even quantum biology, together with the ability to recognize hidden mathematical structures that quantum algorithms can exploit. Traditionally, exploring these connections has required bringing together experts from different fields, often through slow and specialized collaborations; AI can accelerate this exploration dramatically. I want to make the most of this opportunity in the coming years and be more creative, not only in how I solve problems, but also in the questions I ask and the projects I pursue. During my PhD, I often focused on well-defined problems whose answers I already suspected and wanted to prove; now I want to aim higher, explore questions whose answers I do not yet know, and build castles of theorems, informed by numerical explorations and real-world experiments.

There is still so much research to do in the coming years, both to discover what quantum computers can be useful for and to make fault-tolerant quantum computing a reality. I want to help get us there!

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