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The path to room-temperature superconductivity: A programmatic approach

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Room-temperature superconductivity is arguably the greatest challenge in condensed matter physics, with significant practical and commercial implications if it can be solved. There are no physical laws preventing this from occurring; indeed, superconductivity has been observed in so many different materials under so many different conditions that it is almost a "generic" property of nonmagnetic metals. This guides our viewpoint that high-temperature superconductivity is possible, if difficult to realize. Here, we lay out two grand challenges facing the field, titled the Prediction Challenge and the Engineering Challenge, and put forward a programmatic approach for overcoming them. The Prediction Challenge addresses the fact that our ability to predict new conventional superconductors has dramatically advanced in recent years, but most predicted materials are not experimentally synthesizable. To address this challenge, we propose a shift from modeling the superconducting critical temperature and dynamic stability toward high-throughput ab initio and predictive thermodynamics/synthesis modeling. The Engineering Challenge describes how we can control superconductivity with various "knobs," including pressure, nanostructuring, and light. However, our ability to predict how a specific knob will modify a given superconductor is limited, making it difficult to fully exploit them. We describe the current status and identify areas where additional work is needed to fully exploit six of the most common knobs. Progress in both of these grand challenges, while closely integrating theory and experiment into a continuous feedback loop and incorporating insights from fields beyond physics and materials science, could unlock the underlying keys to room-temperature superconductivity.

superconductivity | condensed matter | quantum

For more than a century, the dream of room-temperature superconductivity has been the central driving force motivating the vast amount of effort in this field. Indeed, this is arguably the greatest challenge in condensed matter physics, with significant practical and commercial implications if it can be solved.

For example, the two largest superconducting objects are the magnets used in the CERN Large Hadron Collider (1) and the ITER fusion energy experiment (2). Superconductivity is also critical for the magnets used in ~40,000 MRI machines as well as ultrahigh-field research magnets used at the National High Magnetic Field Laboratory and other high-field laboratories worldwide. These and many other applications would greatly benefit from the demonstration of room-temperature superconductivity, especially in conjunction with high critical currents and magnetic fields. Furthermore, a deeper understanding of superconductivity would impact fields ranging from Bose–Einstein condensation to strongly correlated quantum systems.

Despite the enormous attention devoted to this problem, the record superconducting critical temperature (T_c) at ambient pressure has remained at 133 K for decades (Fig. 1) (3), causing government funding and research priorities to shift away from this field (4, 5). Nonetheless, there has been substantial progress during this time, including the discovery of new classes of superconductors like the iron pnictides (6–10), nickelates (11–15), and high-pressure hydrides (16–21) as well as the ability to modify or create new superconducting states via nanoscale engineering, as in twisted bilayer graphene and WSe_2 (22–29). We therefore believe that the dream of high-temperature, even room temperature, superconductivity at ambient pressure is closer than ever before (5, 30).

We start with one key fact: To make a material superconduct, it is necessary to create bosons (Cooper pairs) by

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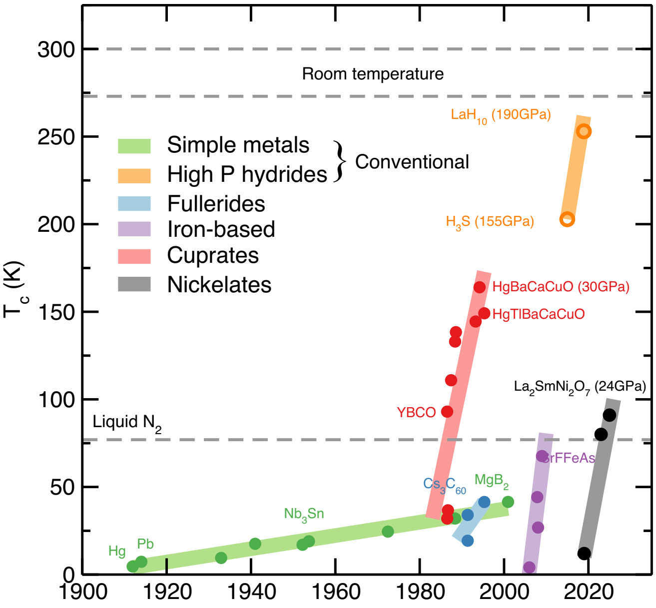

Fig. 1. Major advances in T_c vs. time.

pairing fermions (electrons), and these pairs need to be phase coherent to form the superconducting state. Whether pairing occurs via phonons ("conventional" superconductors) or potentially via another mechanism like spin fluctuations ("unconventional" superconductors) is secondary. Several mechanisms have been proposed, and in each case, the aforementioned criteria are satisfied—the electrons condense into a coherent quantum state. Moreover, there is no physical reason that superconductivity cannot exist at room temperature. In fact, as early as 1968, Ashcroft and Ginzburg had separately envisioned high-temperature superconductivity. Ashcroft's seminal paper predicted high-T_c superconductivity in hydrogen under pressure, based on Bardeen–Cooper–Schrieffer (BCS) theory (31–33). Ginzburg likewise noted the promise of hydrogen but also proposed an excitonic mechanism, suggesting interfacial superconductivity as a likely platform (34). Subsequent theoretical work suggested that there is no upper limit on T_c for conventional superconductors (16), barring dynamical instabilities or energetic barriers. More recent work has indicated that an upper bound on T_c is set by fundamental physical constants to ~1,000 K (35), permitting superconductivity at room temperature (36–40).

Experimental support comes from the observation of superconductivity near room temperature ($T_c \sim 250$ K) under colossal pressures (~200 GPa) in hydrides (33, 41–43). Importantly, high pressure is not a requirement for high-T_c superconductivity; it is only necessary to stabilize the right atomic arrangement, whose properties then allow pairing into a coherent quantum state, even at high temperatures. In fact, there are many predictions of materials that would have high T_c if their structures were stable at ambient conditions (44–48), underlining the point that predicting stable, synthesizable superconductors is one of the biggest challenges in this field. Finally, this is not limited to conventional superconductors; there is substantial evidence for preformed pairs and superconducting fluctuations at $T > T_c$ in unconventional superconductors like the copper oxides (cuprates) (49–51), further indicating that high-T_c superconductivity is possible.

In addition, superconductivity, initially thought to be a rare and fragile state of matter, has been observed in so many different materials and structures under so many different conditions—including bulk crystals at pressures comparable to those at Earth's core (33, 41, 42), interfaces between nonsuperconducting materials (52–61), and exotic aperiodic systems (62–64)—that it is better described as a common, if not "generic" property of nonmagnetic metals. Superconductivity can even be enhanced under seemingly nonideal conditions; for instance, low-dimensional superconductors, like granular aluminum (65, 66), quasi-one-dimensional (1D) Hg embedded in asbestos (67), and two-dimensional (2D) transition metal dichalcogenides (TMDs) (68), can have higher T_c's than their bulk counterparts. Other promising approaches for enhancing T_c include doping, interfaces, light, and strain, underlining the more general point that artificially structuring quantum materials to produce a "quantum metamaterial" can enhance existing functionalities or create new ones (69–72). Nevertheless, a comprehensive understanding of why these enhancements in T_c under ostensibly nonideal conditions occur is lacking, which prevents us from predictively engineering superconductivity with these "knobs."

Our goal in this Perspective is to leverage our broad experience and expertise—from ab initio materials science and machine learning (ML), to materials synthesis and characterization, to nanoscience, metamaterials and device-level physics and applications—to outline the grand challenges facing the field today as well as the scale at which we need to think about and strategically address them. We believe that this is best accomplished with a comprehensive, programmatic approach implemented by a diverse team of theoretical and experimental experts. In contrast with the Edisonian approach that was previously used to discover new superconductors (10, 73), our theoretical and computational capabilities can now guide experimental efforts that can in turn provide feedback that further refines our models; Fig. 2 depicts this programmatic approach. Importantly, we must bridge both conventional and unconventional superconductivity communities and appeal to the broader scientific community beyond physics and materials science for input and advice from their unique vantage points. Cross-pollinating knowledge between various fields will likely give us deeper insight into achieving room-temperature superconductivity. Below, we will describe this in more detail, acknowledging that given the sheer volume of research on superconductivity over the past century, we may inadvertently neglect some pioneering work, for which we apologize in advance.

The Prediction Challenge: Thermodynamics and Synthesizability. While there are superconductors used in applications today (e.g., NbTi in MRI machines), these are akin to germanium in the early days of the semiconductor industry: useful, but yearning to be supplanted by a better material that can operate at higher temperatures and scale to industrial production. While there may not be a "silicon" for superconductivity, this does not mean that we should not search for one, akin to searching for a small needle in a large haystack. Our predictions and searches will not be exhaustive, as we do not have a firm theory. Although the

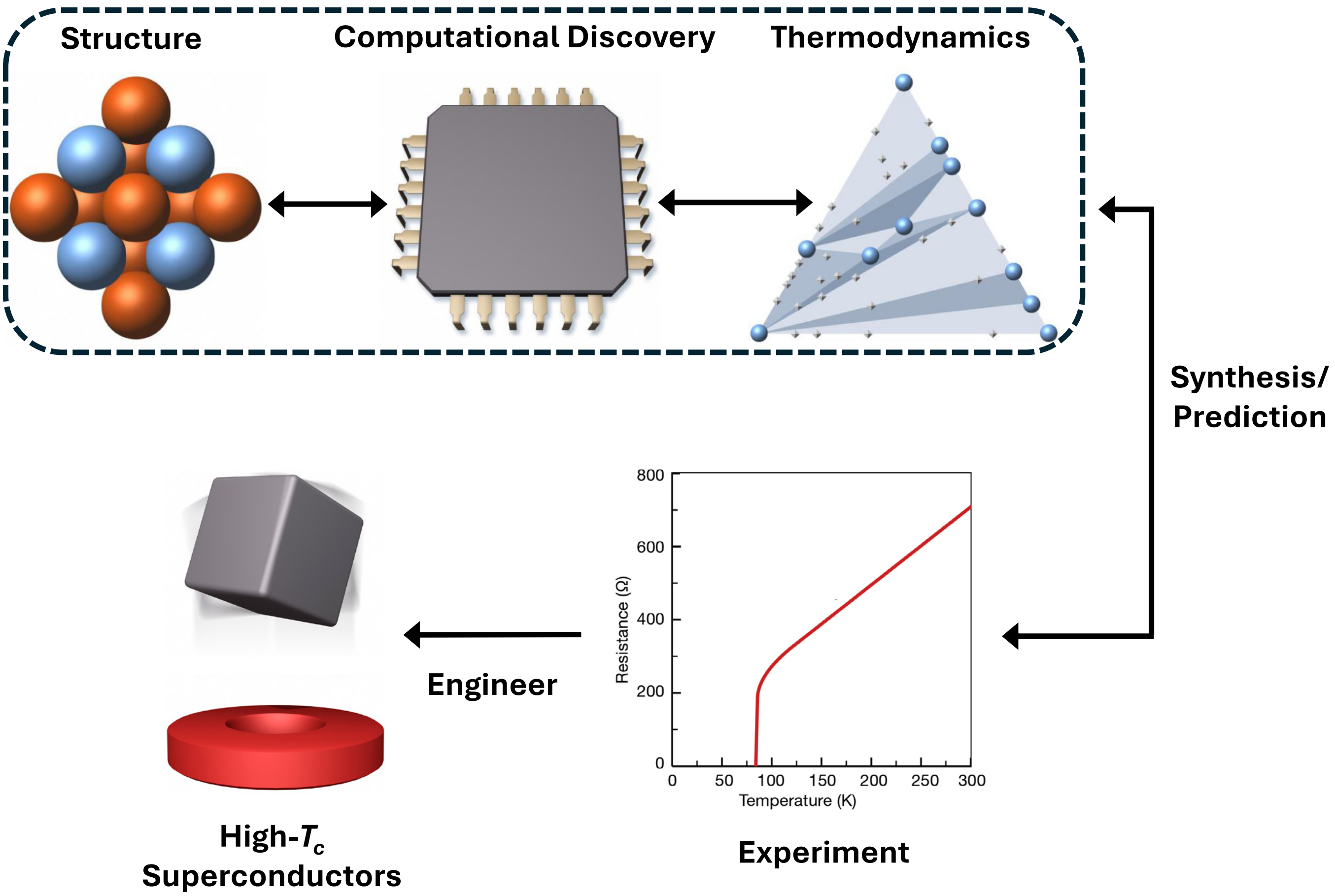

Fig. 2. A programmatic approach for enhancing superconductivity.

BCS and Migdal-Eliashberg theories for modeling the T_c of conventional superconductors are well established and agree reasonably well with experiments (74), the interplay between what is required for the highest T_c and what is required by thermodynamics for a material to be synthesizable is not well understood. Unconventional superconductors are another situation entirely, for which no widely accepted theory exists. However, that does not mean that something useful cannot be found—the full range of physically reasonable elemental combinations is an extremely large haystack. To effectively and efficiently parse this haystack, a programmatic approach is needed to explicitly identify various pathways to explore and determine what actually works. In this section, we attempt to identify these paths for the community.

Although the T_c of both unconventional and conventional materials has generally increased over time (Fig. 1), progress has largely stalled in recent years. As mentioned above, hydrides can have extremely high $T_{c,}$ approaching 250 K in LaH_{10} (43, 75), but only at pressures of several hundred gigapascals (19)—comparable to those found at the center of planets. Predictions have also been made of ambient-pressure, metastable high-T_c hydrides (e.g., Mg_2IrH_6) (76–78) but such compounds have yet to be experimentally demonstrated (79). This highlights the disconnect between what can be predicted and what can be made in the lab.

To bridge this gap, we need improvements in both high-throughput ab initio and predictive thermodynamics/synthesis modeling. Currently, we can reliably do two things. First, we can predict dynamically stable high-T_c materials—a necessary but not sufficient condition for finding a new superconductor that can be synthesized and persist at ambient conditions. Second, we can construct convex hulls of formation energy vs. composition, but more slowly and sparsely than is useful. To address the first point, we have seen a significant increase in the speed, efficiency, and capability of materials discovery models in the past decade, primarily driven by advances in high-throughput searches based on ab initio modeling techniques [e.g., AIRSS (80)] and ML. Numerous companies, including DeepMind (81), Meta (82), Microsoft (83), and ByteDance (84) have announced their interest in materials discovery and publicly released various forms of their databases and associated codes while revealing thousands of possibly novel compounds. Our ability to predict new materials is thus greater than ever, although there are key limitations. Most of these codes are efficient enough to run on a single workstation, but the availability of massive computing resources means that they often achieve “high throughput” by devoting more computing power to “brute force” density functional theory (DFT) calculations (85–88). More recently, focus has shifted toward accelerating this design process via ML (e.g., using ML potentials)—but at the cost of lower fidelity.

Beyond brute-force DFT and ML-potential acceleration, several non-ML crystal-structure prediction (CSP) routes are used for high-throughput exploration, including AIRSS, evolutionary algorithms, and particle-swarm methods (80, 89–91). These CSP methods can be combined with thermodynamic screening to prioritize stable candidates. For conventional superconductors, one can then evaluate T_c directly via density functional perturbation theory electron–phonon calculations followed by Allen-Dynes/McMillan-style estimates, without relying on a ML T_c predictor (32, 92–95). In parallel, property-first ML models trained on superconductivity databases are emerging, with some recent efforts incorporating structure-derived descriptors (96, 97).

Regardless of the approach, all materials discovery pipelines to date struggle when increasing the degrees of freedom. For example, complex unit cells are particularly difficult to model ab initio. This means that there are limited data on which to train ML tools to discover materials with complicated unit cells, so most discovered compounds tend to be relatively "simple." Similarly, we are also limited to simpler stoichiometries—(mostly) ternaries and below—in these high-throughput pipelines. As we increase the number of variables—i.e., the number of elements and unit cell size—the problems become exponentially more computationally intensive, limiting our search space and potentially causing us to miss out on promising candidates. Tackling this issue necessitates disruptive advances in our capabilities. Mirroring recent advances in protein folding (98), this speedup and efficiency will likely come from improvements in algorithms rather than new physical insights; the computing power is already there.

These recent advances in materials discovery have enabled a dramatic increase in the number of predicted high-T_c compounds, but these predictions tend to be marred by a common problem: While evidence of dynamical stability (i.e., "locally stable," or having strictly real phonon modes) of these compounds is often presented, seldom do they sufficiently account for *thermodynamic* stability (i.e., is this the lowest energy structure for a given composition at a specific temperature and pressure?). While both are important, the latter, in our opinion, is more often the limiting factor for guiding synthesis efforts and successfully realizing a new material. Now, the thermodynamics are not omitted without reason; modeling this can be involved and requires substantial computational resources. The energy landscapes of these compounds are typically very complex, with many competing phases and/or polymorphs very close in both formation energy and distance from the energy-composition convex hull (Fig. 3) (77, 99). Many of these high-T_c compounds also tend to be metastable at ambient pressure, meaning that a thermodynamic driving force could cause a transition to a more stable (but perhaps less interesting) state. Combining this metastability with the existence of many competing states at a given point in energy-composition space makes isolating the compound of interest extremely difficult in experiment. This underlines the fact that understanding *metastability* and how to stabilize metastable target phases must be a focus of our efforts moving forward.

In that vein, there are some examples of theoretically metastable materials being recovered at ambient pressure (e.g., superconducting SrB_3C_3) (100, 101). The known superconductor PdH_x is also predicted to be metastable at ambient pressure in several structure types (102, 103). We note that such materials are usually synthesized via bulk methods—moving to thin films may help stabilize metastable superconducting states (104). Advances in pressure quenching techniques, discussed in the next section, may also make it possible to capture metastable phases. Furthermore, as new synthesis recipes are developed, phases of interest may form only locally or as thin filaments in an otherwise inhomogeneous sample. In such cases, scanning probe techniques, such as magnetic force microscopy (105) or other approaches like magnetic field-modulated microwave spectroscopy (106, 107) will likely play an important role in helping identify superconducting phases within a sample.

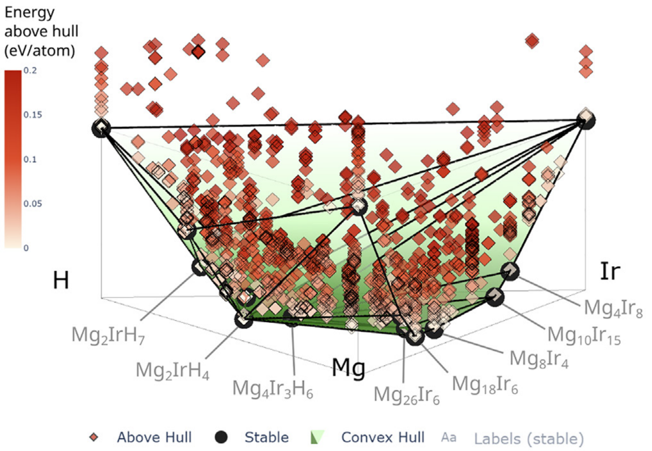

Fig. 3. Example of a ternary convex hull depicting a crowded thermodynamic and compositional space at ambient pressure. Black points represent thermodynamically stable structures that define the convex hull.

We also note that many phases, stable or metastable, may be stabilized by disorder, defects, and/or vacancies [NbN is a well-known example (108)]. Recently developed techniques like the Extended Generalized Quasi-Chemical Approximation (EGQCA) are becoming useful for capturing such effects (as well as compositional variation) from an ab initio thermodynamics perspective (109–111). Other approaches have incorporated the effect of disorder, e.g., on overdoped cuprates, as point-like impurities with a random potential (112). Incorporating the influence of disorder, defects, and vacancies in our models will further help close the loop between theory and experiment.

Although there is some frustration in the field with the cycle of "predict, grow, measure, repeat" to discover new materials, this has been a reasonable approach given the relatively few successes of predictive design thus far. Going forward, we contend that to break this cycle and take a material from the computer to the lab, close cooperation between theorists and experimentalists is required. Such a programmatic approach should consist of a constant bidirectional feedback loop to refine both models and experiments to maximize the chances of finding high-temperature superconductivity in a novel material. Progress in theory must be combined with experimentalists giving active feedback, both in terms of "chemical intuition"—i.e., an expert's knowledge of what compounds seem synthetically feasible—and results from failed/successful synthesis attempts to help refine models. An example feedback loop might be to identify a novel target stoichiometry/structure, predict a synthesis pathway, experimentally attempt to realize the predicted compound, and incorporate that feedback to refine the pathway (Fig. 4). In order to generate this experimental data, techniques like in situ characterization of material growth to understand which phases are occurring as a function of external thermodynamic quantities (e.g., pressure and temperature), and how that varies with changes in these quantities will be valuable.

Thus far we have largely focused on conventional superconductors. However, similar arguments apply to unconventional materials, with the caveat that the lack of an established mechanism pairing fermions into bosons has limited our ability to model these materials. Nevertheless, phonon-mediated pairing may still be relevant in unconventional superconductors like the cuprates (113). We need to continue improving our understanding of unconventional superconductors to construct similar prediction tools. There have been some attempts at ML-guided discovery of cuprates (97, 114–117), but this is quite different from the prediction of a new class of unconventional superconductors. Furthermore, thermodynamics has not yet been included in any of these approaches.

In all, the computational tools for such an integrated research program are nearly there, but some important developments are needed. These developments—e.g., synthesis prediction, including defects and compositional variation, understanding metastability, and extending these models to unconventional superconductors—are expected to be hugely impactful and will apply far beyond the field of superconductivity. We can thus imagine a future where materials databases are able to accurately predict properties like T_c and formation energies, along with viable recipes for making them, maximizing the capabilities in silico to minimize time spent toiling in a lab.

The Engineering Challenge: Quantum Metamaterials for High-T_c. It is well established that engineering the structures of materials across length scales can lead to enhanced properties. For example, we can build integrated circuits made of engineered structures like field-effect transistors, such that the circuit behavior no longer depends on the bulk properties of the constituent materials, but on the interplay between various engineered components. We believe that this can be extended to superconductors, in line with the "quantum metamaterial" concept mentioned earlier. Although most of the research aimed at increasing T_c has focused on finding and characterizing new superconductors as described above, enhancing superconductivity using external tuning "knobs" like pressure, strain, and doping to pair more electrons and/or enhance pair coherence has also been explored for decades. Despite this extensive body of work, our understanding of how these parameters affect superconductivity is at best limited. In short, we are rarely able to predict a priori how a specific knob will modify superconductivity in a particular material, which in turn makes it difficult to exploit such knobs for enhancing superconducting properties, let alone combine them for potential multiplicative enhancements. Addressing this grand challenge could unlock a host of new approaches for dramatically enhancing superconductivity across different classes of superconductors.

Below, we will concisely summarize our present understanding of different approaches that have been used to

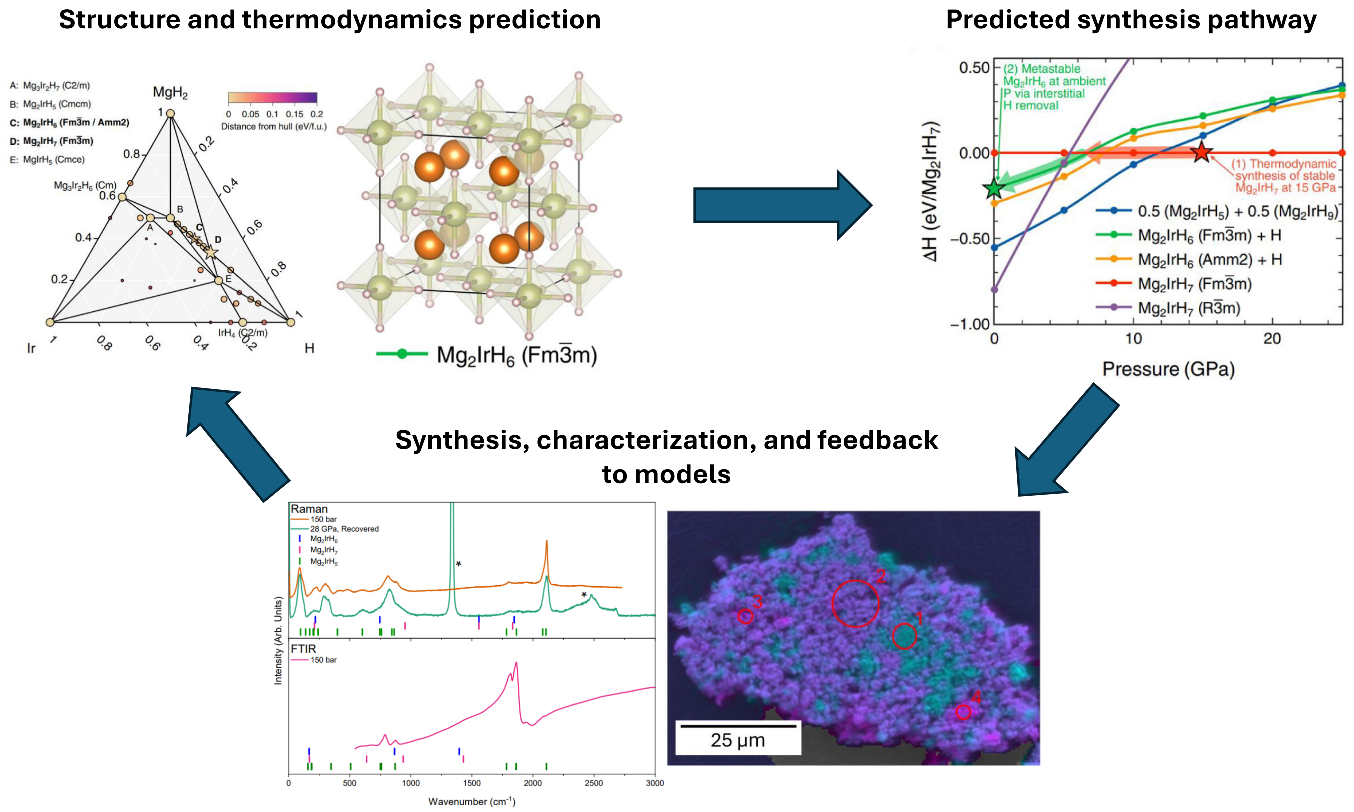

Fig. 4. Proposed bidirectional feedback loop to realizing novel intrinsic superconductors. Credit line: Adapted from ref. 77, which is licensed under CC BY 4.0, and adapted from ref. 79.

modify superconductivity (Fig. 5), emphasizing that this is not meant to be comprehensive. In each subsection, we will highlight gaps in our present knowledge that should be addressed to generally understand and predict how to use each knob to enhance superconductivity in a given system.

Doping. Carrier doping has been extensively used to optimize the properties of nearly every class of superconductors, from conventional superconductors like $Nb_xTi_{1-x}N$ to unconventional superconductors like the cuprates. In the latter, increasing doping levels turns the parent compound from an antiferromagnetic insulator to a lower-T_c superconductor with a "pseudogap" state, to an optimally doped high-T_c superconductor, back to a lower T_c "strange metal" superconductor, and eventually to a conventional metal (118). The pseudogap state is largely believed to consist of Cooper pairs that lack phase coherence (49, 50). The iron-based superconductors ("pnictides") and superconducting nickelates also display a similar phase diagram, suggesting that they could share a common origin for high-T_c superconductivity (118). Finally, intercalation can also be used to dope superconductors while also modifying the phonon spectrum and electron–phonon coupling (119–121).

Although the effect of doping on superconductivity is relatively well studied, there are still several fruitful avenues to explore. Doping can be modeled in conventional superconductors using approaches like the EGQCA (109). Incorporating this into the high-throughput searches discussed previously could dramatically advance both the quality and quantity of predicted new superconductors. This could also make it possible to synthesize superconductors that are predicted to be thermodynamically unstable by gradually alloying with a composition that is stable at ambient conditions.

Pressure/strain. Both pressure and strain have been used to modify the properties of superconductors by directly controlling their lattice spacing, which affects their phonon spectrum, electron–phonon coupling, and carrier density. The most spectacular example is the hydrides, but every superconductor is influenced by pressure and strain; for example, the T_c of the cuprate Hg-1223 increases from ~133 K at ambient pressure to 164 K at ~30 GPa (122). Furthermore, T_c ~80 K was demonstrated in the $La_3Ni_2O_7$ system under pressures of 14 GPa (123–125), but only stabilized at ambient pressure in thin films via epitaxial compressive strain with an onset T_c of ~20 to 45 K (126–128).

Pressure and strain are relatively easy to model in conventional superconductors by appropriately changing the lattice parameters along one or more directions before calculating T_c. However, despite extensive experimental studies, we do not generally understand how to use these knobs to optimize superconductivity, as there can be substantial variations in properties even within a given class of superconductors. Furthermore, the mechanism by which these parameters influence T_c varies across different classes of superconductors. For example, in conventional superconductors with the A15 crystal structure T_c is reduced with both tensile and compressive strain (129, 130), likely due to a reduced density of states (DOS), while it increases with strain in MgB_2 due to enhanced electron–phonon coupling (131). In the cuprates both pressure and strain give varying results for different materials; (132) e.g., T_c increases with pressure in Hg-1223 (122) and BSCCO (133) but decreases in optimally doped YBCO (134). This is strong motivation for studies aimed at understanding how pressure and strain can enhance superconductivity across different classes of materials.

Finally, metastable superconducting states can also be accessed by "pressure quenching", i.e., compressing a sample and then rapidly releasing the pressure at a given quench temperature (135). This has recently been used to stabilize pressure-induced/enhanced states at ambient pressure (135), including $T_c \approx 40$ K in bulk FeSe (136) and $T_c \approx 10$ K in $Bi_{0.5}Sb_{1.5}Te_3$ (which is not superconducting at ambient conditions) (Fig. 6) (137). However, our understanding of the mechanism underlying pressure quenching is lacking, preventing the properties of the metastable superconducting state from being optimized via parameters like the quench temperature, pressure, and rate. Nevertheless, the success of this approach across different classes of superconductors

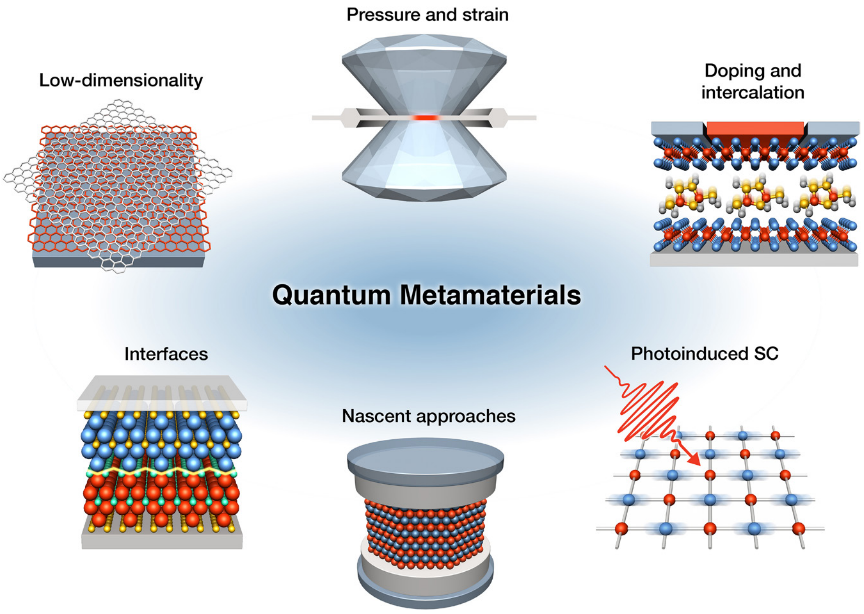

Fig. 5. Schematic depicting different knobs that can be used to tune the properties of superconductors.

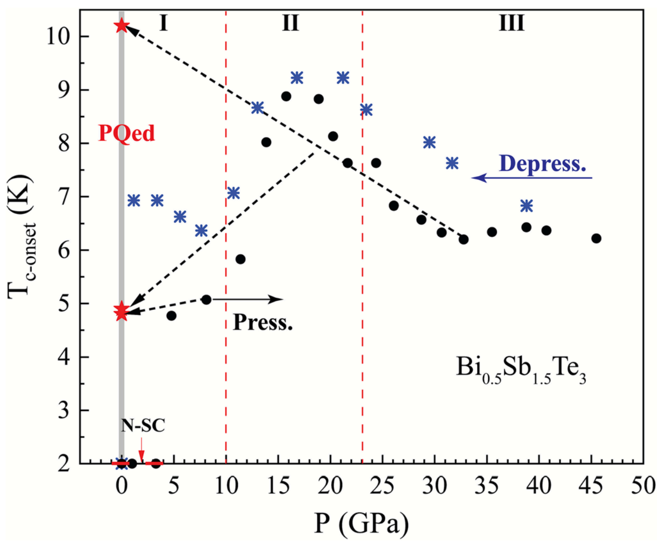

Fig. 6. Superconducting critical temperature onset as a function of pressure in $Bi_{0.5}Sb_{1.5}Te_3$ during pressurization ("Press."), depressurization ("Depress."), and after pressure quenching ("PQed"). Red dashed lines indicate critical pressures for structural phase transitions, red stars show the T_c's for PQed states at ambient pressure, and dashed arrows indicate the initial states from which pressure-quenching was performed at 77 K. credit line: Adapted from ref. 137, which is licensed under CC BY-NC-ND 4.0.

is promising and should drive a host of studies in this direction, which in turn could lead to new ambient pressure T_c records in the near future.

Interfacial superconductivity. Interfaces have been shown to both induce superconductivity between two nonsuperconductors [e.g., $LaAlO_3$ and $SrTiO_3$ (54, 55)] and enhance superconductivity in known superconductors [as in atomically thin FeSe on $SrTiO_3$ (52, 54, 56, 68)]. In the latter, reported $T_c$values vary widely, likely due to differences in samples (e.g., the quality of the interface) as well as measurement techniques (54). Understanding the mechanism for interfacial enhancement has therefore been difficult, with both interfacial phonons and charge transfer potentially contributing (55). Cuprates have also displayed both interface-induced and interface-enhanced superconductivity (54, 56, 138, 139), the latter being attributed primarily to oxygen content, with strain playing a secondary role. Finally, 2D van der Waals (vdW) heterostructures can also exhibit both interface-induced [e.g., twisted bilayer graphene (24, 25)] and interface-enhanced superconductivity (140, 141).

These examples highlight the potential for interfacial enhancement of superconductivity, but also underline its complexity, since effects including strain, charge transfer, Coulomb repulsion, proximity-induced spin–orbit coupling, and chemical content can play a role. For instance, we know that controlling charge transfer across the interface can enhance the DOS at the Fermi energy E_F; interfacial and/or substrate phonons can enhance electron–phonon coupling and increase the maximum phonon frequency; and a large dielectric constant can suppress Coulomb repulsion (54). However, design of interface-enhanced superconductors will require a general, quantitative understanding of the interplay between these effects for different combinations of materials. Theoretical work thus far has focused on modeling interfacial superconductivity in specific cases (142, 143), but to the best of our knowledge, systematic studies that would enable more general models have not been carried out. Such studies deserve further consideration, given the potential of this approach for dramatically enhancing T_c across a wide range of systems.

Superconductivity in lower dimensions. It has long been known that some superconductors display enhanced properties when confined to low dimensions. This confinement is typically defined by the scale of a given dimension relative to the superconducting coherence length ξ (a measure of the distance between electrons in a pair). Additionally, the Anderson criterion defines a dimension below which a material is no longer superconducting (when the confinement energy is larger than the superconducting gap, typically for length scales <10 nm) (144–148).

Enhancement of T_c with confinement has been demonstrated with several examples in 0D [granular Al (66) and NbN (149)], 1D (filaments of Hg, Ga, and Zn) (67, 150), and 2D (TMDs and graphene) (68), wherein all displayed T_c greater than their bulk counterparts. In some cases, such as twisted bilayer and trilayer graphene, T_c can even be tuned by varying the twist angle between the layers (which individually are not superconducting) (24, 25, 151). These examples underline the fact that superconductivity can persist under low-dimensional confinement with length scales well below ξ (145), though it is more susceptible to destruction via defects and fluctuations (144, 152).

Despite the extensive body of work showing enhanced T_c in low dimensions, few studies have quantitatively examined the influence of dimensionality on T_c. To the best of our knowledge, we still lack the ability to generally predict a priori how T_c will change for a given superconductor under different degrees of confinement. Given the tunability and flexibility offered by low-dimensional systems, understanding this could be transformative for our ability to optimize superconducting properties.

Finally, it is worth noting that electric-field gating is another promising approach for tuning superconductivity in low-dimensional systems (54, 68, 153–155). This has been used to dramatically enhance T_c in a wide range of superconductors, including pnictides, cuprates, graphene, and TMDs, often enabling the system to be tuned through the superconducting dome. Although this has most often been done with a static (DC) field, time-varying electric (and even magnetic) fields can offer advantages, as discussed in the next section.

Light-driven superconductivity. The groundbreaking discovery of a transient superconducting-like state induced by driving the nonsuperconducting cuprate $La_{1.675}Eu_{0.2}Sr_{0.125}CuO_4$ with a femtosecond mid-infrared (IR) pulse tuned to an in-plane Cu-O phonon mode sparked both excitement and controversy (156, 157). However, despite being extended to other superconductors (158–163) there were questions about whether the state was truly superconducting, as the observed response could originate from other phenomena (e.g., a transient high-mobility, nonsuperconducting state) (164–166). More recent work supported the interpretation of the photoinduced state as superconducting, as driving K_3C_{60} with longer mid-IR pulses created a transient metastable state that made electrical measurements possible (167). Furthermore, time-resolved Faraday rotation measurements on YBCO revealed a transient Meissner effect (165), supporting the idea that the driving mid-IR pulse enhances the phase coherence between Cooper pairs above T_c.

Nascent approaches to modifying superconductivity. Novel approaches to engineering superconducting properties are continually being developed. Artificial structures, like metamaterials, have been explored for enhancing T_c (168), but thus far the evidence that they can do so is limited. The idea of using cavities to enhance T_c is more promising, with several theoretical studies indicating that both resonant and nonresonant cavities could yield significant increases in T_c (169–174). Although there have not been any experimental demonstrations of cavity-enhanced superconductivity to date, cavities have been shown to enhance other phenomena (e.g., metal–insulator transitions) (175), and very recent work has shown that a cavity can modify superconductivity in the organic superconductor k-ET (176).

Quasicrystalline and other aperiodic structures have also been shown to host superconductivity (62–64), even though Cooper pairing between electrons with opposite momenta is not possible since momentum is not a good quantum number in these systems. Superconductivity was also observed in quasiperiodic twisted trilayer graphene (177), making this a unique system in which superconductivity can be tuned through both periodic (24) and quasiperiodic (177) regimes. Several theoretical approaches have been used to study superconductivity in aperiodic systems (178–182), though a clear understanding remains lacking. Developing this understanding could provide substantial insight into the nature of the superconducting state in both regimes and how it might be engineered.

Putting it all together. There are many knobs that have been shown to significantly affect superconductivity. However, in most cases, we do not have a firm understanding of whether these knobs are modifying pairing strength, phase coherence, or both in a given superconductor. As in The Prediction Challenge, our goal should be to employ a programmatic approach, based on close theoretical–experimental collaboration, that aims to understand the operation of each knob in more depth and as generally as possible so we can predictively use them to optimize superconducting properties via quantum metamaterial engineering. We can even think about turning multiple knobs at once to reap new benefits and learn new physics. Finally, in addition to the knobs that were discussed above, there may be others that emerge as our understanding continues to increase, providing new avenues to reach the desired high-T_c superconducting state.

Conclusion

We have presented our thoughts on the prospects of high-temperature superconductivity and outlined some key research directions to realize this in both intrinsic materials and engineered quantum metamaterials. Again, we emphasize that to the best of our knowledge, there is no fundamental physical reason preventing electrons from being paired to form a coherent bosonic state at high temperatures and ambient pressure; we “simply” need to find the right conditions for this to happen. We believe that overcoming this challenge requires a systematic, programmatic approach composed of well-planned paths of investigation spanning computational discovery and design as well as materials synthesis and engineering. It is critical to understand not only each path individually, but how they might interplay to create a multiplicative effect.

In exploring these interplays and developing new materials and devices, interventional experiments are key. The benefits lie not only in learning how to develop a good device but also in *how* we come to that understanding—the new physics that is uncovered along the way. Examples of such “learning along the way” have occurred throughout the history of the semiconductor industry. While making better transistors, smaller lithographic features, and novel computing architectures, we developed improved theories of solid-state carrier dynamics, uncovered new understanding in attosecond chemistry, and developed new quantum photon sources, laying the groundwork for quantum metrology, communications, and computing. As we learn more about the superconducting state, we will similarly open more doors.

To close, we leave the field with a “call-to-action” to our colleagues across physics, chemistry, and materials science. Our current understanding does not impose a limitation on T_c, though it is not trivial to achieve high temperatures. We as a community need to acknowledge that, in addition to all that we know, there is much that we *do not* know, which gives us optimism about the future of the field—there is still much to be learned, and much further to go.

Data, Materials, and Software Availability. Previously published data were used for this work (Fig. 4: Adapted from ref. (77), Figs. 1 and 2, and ref. (79), Fig. 3; Fig. 6: Adapted from ref. (137), Fig. 4).

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