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Physics in Perspective

What's in a Name Change?

Solid State Physics, Condensed Matter Physics, and Materials Science

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When solid state physics emerged in the 1940s, its name was controversial. By the 1970s, some physicists came to prefer "condensed matter" as a way to identify the discipline of physics examining complex matter. Physicists and historians often gloss this transition as a simple rebranding of a problematically named field, but attention to the motives behind these names reveals telling nuances. "Solid state physics" and "condensed matter physics"—along with "materials science," which also emerged during the Cold War—were named in accordance with ideological commitments about the identity of physics. Historians, therefore, can profitably understand solid state and condensed matter physics as distinct disciplines. Condensed matter, rather than being continuous with solid state physics, should be considered alongside materials science as an outlet for specific frustrations with the way solid state was organized.

Key words: Solid state physics; condensed matter physics; materials science; discipline formation.

Introduction

Oliver E. Buckley, president of Bell Laboratories, stood before the National Research Council's Conference of Physicists in May 1944 and mused: "When Solomon said that 'a good name is rather to be chosen than great riches,' he knew what he was talking about."[1] The name Buckley had in mind was "physics." He worried that this term evoked nothing concrete to the average American, rendering the field needlessly obscure. The assembly at the American Philosophical Society in Philadelphia was organized to identify and discuss major challenges American physics would face following World War II.[2] Prominent among them were education, the needs of industrial researchers, and the peacetime relationship between physics and government. Buckley spoke under the title "What's in a Name?"—an ironic invocation of Juliet's soliloquy for a talk that championed, rather than lamented, the power of names. He insisted that professional identity

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was the most primal challenge American physicists faced and encouraged his colleagues to consider how their discipline's appellation shaped activities from undergraduate teaching to government advising.

Pace Juliet, names matter, and the mid-1940s were an auspicious time for physicists to scrutinize them. The end of World War II heralded a wave of specialization. One notable new specialty, solid state physics, illustrates just how sound Buckley's instincts were. Solid state's name betrays the unusual manner of its constitution, by mid-1940s standards. The growth of alternatives to solid state later in the century—in particular, condensed matter physics and materials science—reveals the evolution of the professional pressures that drove solid state's consolidation. The labels scientists adopt expose complex professional politics, conceptual shifts, and the fingerprints of scientific ideologies, all of which were on display in the case of solid state physics.

Naming has long been considered an important component of professional identity and discipline formation. Historians have considered the significance of affixing new names to scientific disciplines in a range of eras, specialties, and national contexts. The decline of "natural history" in favor of "biology," the rise of "physics" at the expense of "natural philosophy," and the eclipse of "alchemy" by "chemistry" all coincided with new community structures, institutions, and methodological standards.[3] In light of this tradition, it is curious that a similar transition in the second half of the twentieth century has been commonly regarded as a simple rebranding, indicative of little substantive change. This paper reevaluates that transition and argues that "solid state physics," "condensed matter physics," and "materials science" can be constructively understood as distinct historical entities.

Between the 1960s and the 1980s, a subset of those working on the physics of complex materials abandoned "solid state physics" in favor of "condensed matter physics," while others aligned themselves with a new interdisciplinary specialty, "materials science." Historians and physicists alike commonly treat "solid state physics" and "condensed matter physics" as effective equivalents, distinguished only because they were preferred in different eras. Philip Anderson, a member of the first generation of American physicists trained in solid state theory and an early adopter of the "condensed matter" label, assumes continuity when referring to "solid state (now 'condensed matter') physics."[4] Similarly, Helge Kragh writes: "From a sociological and historical point of view, solid state physics did not exist [in the 1930s]. It was only after World War II that the new science of the solid bodies, later to be called condensed-matter physics, took off."[5] These claims are not without merit. The shift from solid state to condensed matter physics was marked by substantial continuity of physical problems and practices; however, topical and methodological continuity do not translate unproblematically into disciplinary continuity.

This straightforward equivalence between solid state and condensed matter is sometimes complicated by pointing out condensed matter's broader topical scope. Walter Kohn's historical treatment of his home discipline suggests that "'solid state physics' … was enlarged to include the study of the physical properties of liquids and given the name 'condensed matter physics.'"[6] Spencer Weart, in his contribution to the kaleidoscopic history of solid state physics, *Out of the Crystal Maze,* points out that condensed matter resolved difficulties intrinsic to solid state: "the newly popular name included liquids and, like 'materials science' in a different manner, reflected a persistent uncertainty as to whether 'solid-state physics' was the best way to group subfields."[7] Weart's observations point towards a richer story about the name change, which was more than either a simple rebranding or the rectification of a longstanding error. Condensed matter did respond to nagging skepticism about solid state, but the parallel growth of materials science indicates that addressing these concerns was neither simple nor straightforward. In fact, as this paper argues, these names identify fields defined by distinct professional ideologies.

The ideological differences at issue can be illustrated by returning to Buckley's proverb: "A good name is rather to be chosen than great riches," which continues: "and loving favor rather than silver and gold." The arguments solid state physicists could muster for each of these goals—intellectual esteem and research funding—did not often align. Frontier-oriented pure science rhetoric, successful for a time at justifying high energy accelerator research, made little headway on behalf of solid state.[8] Although technological relevance played well with funders, solid state physicists remained leery of veering too far into applied territory as they fended off dismissals of their work as "squalid state physics" or "*schmutzphysik*" ("physics of dirt"). These pejoratives, the stuff of water-cooler banter rather than published invective, are attributed to Murray Gell-Mann and Wolfgang Pauli respectively. As Christian Joas observes, they have been perpetuated by solid state physicists themselves as a way of developing professional identity rooted in defiance of such condescension.[9]

The tension between funding and prestige was resolved differently in condensed matter physics and materials science. Condensed matter physicists organized to advance their field's intellectual reputation, attempting to unshackle it from demands for direct economic or technological payoffs. In contrast, materials science emerged as an effective strategy for securing federal largess by addressing strategic bottlenecks in the development of new materials. As such, they each carried forward different aspects of solid state's legacy.

The relative prevalence of competing names for research on the physical properties of complex matter can be seen schematically in figure 1. Before World War II, none of these terms was much used. The rise of "solid state physics," beginning in the mid-1940s, mirrors the discipline's growth following the war, which began around the time the American Physical Society's Division of Solid State Physics (DSSP) formed in 1947. It wanes following the rise of "condensed matter physics," which first shows appreciable usage in the mid-1970s. "Materials science" rises in the mid-1950s, surpassing the use of "solid state" in the mid-1970s, just as solid state physicists began to worry that funding for the systematic study of

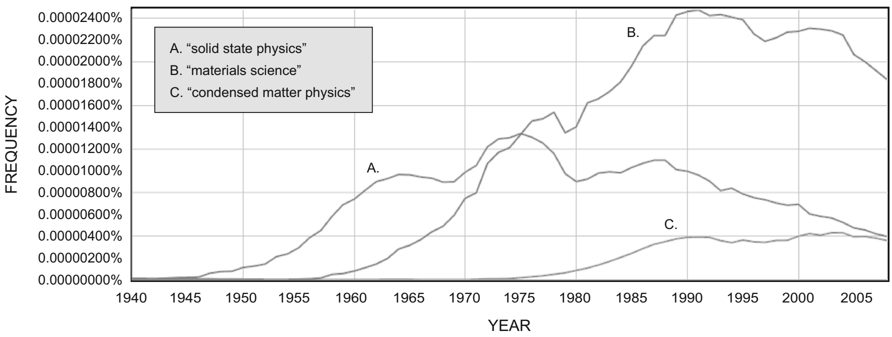

Fig. 1. Usage of terms for the study of complex matter, 1940–2008. Credit: Google Ngram Viewer, http://books.google.com/ngrams (accessed September 16, 2011). Smoothing level 3. Ngrams isolate the rate with which search terms occur in Google's digitized catalogue as a percentage of all word strings of that length (*y*-axis). These data are limited to texts Google has digitized, leaving the possibility of sampling error and other inconsistencies. The chart is used here to illustrate general trends, which will be otherwise substantiated in the text.

complex matter was being funneled principally into developing strategically useful materials.

The story of solid state's name exposes a basic feature of its constitution: it formed around an era-specific set of professional challenges and did not owe its purpose to a methodological program, conceptual framework, or pedagogical tradition.[10] It was therefore susceptible to contextual pressures exerted in later eras. The growth of materials science on the one hand and condensed matter physics on the other shows how the conditions favoring solid state's formation evolved. Understanding how and why physicists gravitated towards new names for the physical investigation of complex matter therefore highlights the role of professional ideologies in demarcating disciplines, even when those disciplines enjoy considerable conceptual and methodological continuity.

Why Solid State?

Solid state originated in industry. Late in 1943, General Electric research physicist Roman Smoluchowski composed a letter with five cosigners, which he circulated to fifty-three of his colleagues nationwide. The "group of six," as it came to be called, was composed mostly of industrial physicists. Smoluchowski's co-authors were Saul Dushman (General Electric), Thomas A. Read (Frankford Arsenal), Frederick Seitz (Carnegie Institute of Technology), Sidney L. Siegel (Westinghouse), and William Shockley (Bell Laboratories). The letter proposed a new American Physical Society (APS) division dedicated to metals physics, disclosing Smoluchowski's eagerness to build a community that would support young researchers and supply a mechanism for industrial physicists to exert greater influence in the APS, including a forum for dialogue with their counterparts in the academy.[11] The proportion of physicists in industry, slight before the war, was expanding and physicists were discussing how to address this institutional change. The APS Council had nonetheless rebuffed proposals for a division of industrial physics on the grounds that they violated Article IX of the Society's constitution, which required that divisions form to advance "the knowledge of a specified subject or subjects in physics."[12] Industry, the Council insisted, was not a subject.

Smoluchowski's proposed division aimed to circumvent this requirement. Metals physics, in his judgment, promised rapid postwar growth "not only from the point of view of fundamental science, but also from the point of view of practical problems in industry."[13] Attuned to the need for an appropriate name, he chose "metals" both because industrial researchers worked primarily with metallic substances and because it implied a willingness to collaborate with metallurgists, much as he did in his day-to-day work at General Electric.[14] Smoluchowski resisted other names on the grounds that "the cooperation from purely metallurgical quarters may be more active if we are 'all out for metals.'"[15] This was an unconventional approach. Before the war, natural categories defined the subfields of physics, to the extent that it was divided at all. In the early 1940s, the War Policy Committee of the American Institute of Physics, which was interested in defining the scope of physics in order to delineate its relevance to war work, defined a physicist as "one whose training and experience lie in the study and applications of the interactions between matter and energy in the field of mechanics, acoustics, optics, heat, electricity, magnetism, radiation, atomic structure, and nuclear phenomena."[16] A proposal for a field that combined almost all of these categories as they applied to metals or solids, while addressing an institutional challenge, was radical by contemporary standards.

Karl K. Darrow, the secretary of the American Physical Society and overseer of new divisions, was reluctant to support a group devoted narrowly to metals and insisted that the new division encompass the whole of the solid state.[17] Léon Brillouin, a pioneer in the quantum theory of solids, agreed, maintaining that "the distinction between metals and other solids has no scientific basis and is only a matter of engineering."[18] Solid state was an effective compromise, even if it set up a similarly slippery distinction between solids and other phases of matter. It achieved Smoluchowski's objective of accommodating industrial physicists—whose research might take them from thermodynamics to optics to quantum mechanics in the course of a few months—while allowing the APS Council to maintain its restriction to subject-based divisions and appease somewhat the Society's influential old guard, who found divisions of any kind offensive.

Foremost among the opponents of new divisions was Harvard quantum theorist John Van Vleck, who complained bitterly about balkanization within the APS.[19] His was not merely an aesthetic preference for less institutional structure but reflected his commitment to the purity of physics, which he felt would be threatened if narrow interest groups became too cozy with other disciplines. Van Vleck,

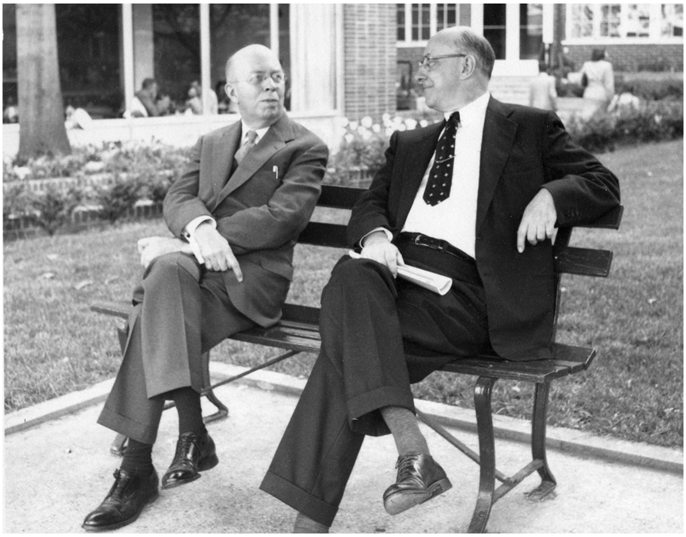

Fig. 2. Karl Darrow (*left*) chats with Henry Barton, American Institute of Physics Director from 1931–1957, during an APS meeting in Washington, DC, 1960. Credit: Emilio Segrè Visual Archives, American Institute of Physics, Physics Today Collection.

responding to Smoluchowski's letter advocating for a metals division, registered his concern "that our meetings will have too many hangers-on whose main interest is not that of pure physics or science" and fretted about the impact on annual APS meetings in Washington, DC, which Van Vleck revered for their "Quaker spirit" and informal atmosphere (figure 2): "When we sit on the lawn of the Bureau of Standards, we do not want to feel '*qu'un sang impur abreuve nos sillons* [that an impure blood should water our fields]'."[20] Their difference was ideological: Van Vleck considered overtures to metallurgy and engineering a compromise of the Physical Society's basic research mission, while Smoluchowski saw cross-disciplinary dialogue as essential to the continued health of physics in an era with abundant opportunities for growth.

Darrow found himself in an uncomfortable position. Van Vleck, having been among the first generation of domestically-trained theorists and an active player in the quantum revolution of the 1920s, wielded considerable influence. The two were also personally close.[21] Van Vleck would write to Darrow regularly to register his concerns about APS politics. In their frequent correspondence, the two practiced their Latin and traded mildly off-color banter.[22] Darrow recognized the growth of divisions as inevitable, but in deference to Van Vleck and other traditionalists held fast to the interpretation of the APS constitution that required divisions to form around subject areas. By recommending a division of solid state (rather than metals) physics, Darrow sought to mend the ideological gulf, mitigating Van Vleck's objections while still giving Smoluchowski's energetic group the institutional space they craved.

As a compromise category, solid state invited skepticism from its inception. The solid state of matter, rigid though it be, struck some as ill-suited for building the boundaries of a discipline. The physical concepts, theoretical methods, and experimental techniques used to investigate solids were frequently just as suited to fluids, gels, plasmas, or molecules. When the name was proposed, University of Iowa theorist Gregory Wannier crisply summarized the unease abroad when he observed, "solid state physics sounds kind of funny."[23] The name stuck, but it was not universally beloved. Aversion to "solid state" would catalyze the development of alternative approaches to categorizing the physics of complex matter later in the century when the impetus to link academia and industry was less pressing.

The vast array of questions and techniques physicists could apply to solids made for a diffuse field that lacked a cohesive set of motivating questions, techniques, or concepts. Solid state's breadth was evident as early as January 1945 at an APS symposium organized to discuss the proposal for a new division. The slate was strong with theoretically sophisticated talks. Wannier outlined new statistical treatments of cooperative phenomena.[24] John Van Vleck reviewed theoretical approaches to ferromagnetism, beginning with phenomenological treatments of the early twentieth century and continuing through descriptions of competing quantum approaches based on exchange interactions. Although part of the standard quantum mechanical repertoire since the late 1920s, exchange was sufficiently obscure within the general American physics community that Van Vleck needed to caution his audience that, "[exchange] forces cannot be described in simple intuitive language."[25]

Van Vleck's explanatory care reflects the meeting's broad representation, which emphasized the applications of solid state physics and included a contribution focusing on the use of magnetic materials in war research.[26] Unabashedly applied rhetoric shone through in a treatment of the fracture stress of steel, which beginning by noting that the "sinews of warfare, namely guns, projectiles, and armor, are made of steel."[27] An argument that the increasing importance of catalytic processes in chemical technology merited further attention to the topic from physicists reinforced the interdisciplinary streak Smoluchowski championed.[28]

The strong applied component and expansive scope of solid state physics as established in postwar America are evident in its pedagogy. The first textbook to describe physical approaches to solid matter comprehensively, Frederick Seitz's *Modern Theory of Solids*, appeared in 1940, predating solid state physics' arrival as a separate discipline.[29] Accordingly, it emphasized the transition from classical to quantum approaches, with particular emphasis on the approximation methods that made regular crystalline solids susceptible to quantum mechanical description.[30] Charles Kittel's *Introduction to Solid State Physics* became the standard text after its second edition in 1955.[31] The second edition expanded the textbook by about 200 pages over the original 1953 printing. Much of the additional material dealt with practicalities that would be relevant to engineers and industrial physicists. Compared with Seitz, Kittel's approach to theory was straightforward, in most cases relegating full quantum mechanical treatments to the appendices. Kittel's textbook also dedicated more space to applications, addressing in detail, for example, the properties of alloys and the behavior of transistors, illustrating concepts with descriptions of experimental techniques and appeals to easily observable laboratory phenomena. It represented a field with a strong applied inflection. As John J. Hopfield remarks in his recent recollections of his training in solid state at Cornell in the 1950s: "The weakness of the book was that it left you (as a theorist) with no idea of where to start to develop a deeper understanding of any of the topics covered."[32]

Seitz and Kittel wrote for different audiences. Seitz assumed a stronger background of his readers, targeting graduate students and practicing physicists. Kittel's text was designed to be accessible to undergraduates. The differences nonetheless ran deeper. By the 1950s, solid state had not only been established as a much broader enterprise than Seitz's treatment would suggest, but its first major coup, in the form of the transistor invented at Bell Laboratories, had come from industrial quarters.[33] To be marketable in the 1950s, a text on solid state physics had to take into account the range of the field's applications, not just its conceptual structure, and remain accessible to chemists and engineers. The broad cross section of work on display in the APS solid state symposium and in Kittel's textbook represents the breadth of approaches and questions that the Division of Solid State Physics unified. As Kittel noted in his preface: "Solid state physics is a very wide field."[34] Concern with how the exchange interaction might provide a robust causal account of ferromagnetism had little to do with the phenomenology of steel; that they both addressed some property of solid matter was a superficial commonality at best. Discontent with "solid state physics" persisting after the name was validated by the APS and emblazoned on textbook covers was a symptom of a deeper dissatisfaction with a category possessed of little inherent cohesion.

Squabbling over the title of a new division of a professional society might seem picayune, yet both the choice of "solid state" to label a new discipline and the discomfort many physicists felt with it reflected the ideologies shaping postwar physics. Smoluchowski, in the 1940s, was unorthodox in suggesting that the internal boundaries of physics could be arbitrarily drawn so as to bolster professional initiatives. Much of the physics community, certainly those in power at the APS, insisted that physics only be divided along lines they assumed to be present in nature, a belief manifest in their objection to an industrial division and their squeamishness about "metals physics." Solid state formed by mediating between the competing visions of these two constituencies and so was a fractious alliance, susceptible to being carved up differently as the professional context evolved.

The Amalgamation of Materials Science

"Materials science," as indicated in figure 1, gained traction in the mid-1950s. The term's prehistory lies in the federal advisory system of the United States. Materials language first surfaced in discussions of the factors limiting technical and military development. In 1951, the National Research Council (NRC) formed a Materials Advisory Board (MAB) to evaluate how advances in materials research might address this strategic bottleneck. MAB replaced the Minerals and Metals Advisory Board. The name change reflected "recognition of the interrelations of the metals and nonmetals, particularly in structural applications."[35] Despite the broadened scope of the new committee, MAB's early-1950s incarnation had little contact with physicists. A 1954 report described the change by noting: "The Board has been reconstituted to include materials engineers, chemists, and metallurgists … to provide advisory services to the Office of the Assistant Secretary of Defense for Research and Development and to the Administrator of the General Services Administration."[36] Other early uses of the term are similarly engineering-centric.

Military research organizations followed the NRC's lead with little hesitation, cementing "materials research" as a prominent strategic target for Cold War research and development. The restriction to engineering began to erode towards the end of the decade as MAB honed its mission, reached out to physicists, and embraced basic research in a limited fashion. A 1957–1958 NRC report noted that "increased attention to materials brought about by the needs of weapons system development has resulted in a considerable expansion of activity for the Materials Advisory Board."[37] The expansion referenced here did not just indicate new personnel, but also voices from new disciplinary camps.

MAB's expanded topical breadth is evident in its 1960 report, "Fundamental Aspects of Materials Research." The committee included Cornell physicist James Krumhansl as deputy chair. The presence of a physicist among the metallurgists, chemists, and industry mavens who previously composed the committee indicates MAB's emerging preference for close connections between basic research and its applications, a position that was overt and urgent by 1960. The committee criticized the Department of Defense's existing efforts to mobilize basic research to strategic ends, remarking that "in-house basic research capability is grossly inadequate." The committee urged the research arms of the Army, Navy, and Air Force to sponsor "strong centralized laboratories in which basic research, comprising the entire spectrum of potentially pertinent science including the materials sciences, can be promoted."[38]

These recommendations, designed to enhance "the ability to bring knowledge to bear on the defense needs of the nation in the shortest possible time," came to define the mission of materials science as it was imagined within the federal advisory infrastructure.[39] The concept of basic research was appropriated in service of technological defense needs, which were not being addressed with alacrity sufficient to appease the Department of Defense and its army of advisors. Responding to pressure to mobilize basic research resources in order to accelerate blackboard-to-battlefield turnaround, the NRC's conception of materials science broadened still further. In 1960, a committee to consider the "Scope and Conduct of Materials Research" was formed "to view the total materials research needs of the country with relation both to national defense and the public welfare more generally; to appraise the adequacy of present research programs to meet those needs; to consider the resources of personnel, facilities, and administration that are available; and to make recommendations for the correction of deficiencies that the Committee may identify."[40]

This committee included solid state physicist Frederick Seitz and metallurgist, historian of metallurgy, and advocate for the importance of basic research Cyril Stanley Smith alongside the regular complement of engineers, chemists, and industrialists. The report's recommendations reflected a closer integration between science and engineering. It advocated centralized funding, coordination, and oversight of materials research as well as "strengthening the universities in their dual role of training scientists and engineers and also doing basic research."[41] By 1960, scientists advising the federal government regularly advocated mechanisms to increase dialogue between those studying the properties of materials and those implementing that knowledge in strategically relevant ways.

The advisory emphasis on materials, in particular with respect to training and basic research, exerted its influence within American universities. The first major textbook for materials scientists and engineers appeared in 1959: Lawrence Van Vlack's *Elements of Materials Science* aimed to synthesize traditional engineering approaches with basic science. Van Vlack informed his readers: "The subject matter taught in Engineering Materials courses is changing rapidly. Formerly, this subject was taught on an empirical basis. Now, although the science of materials is far from complete, it can be approached from a more scientific viewpoint, because of the development of principles which relate the properties and behavior of many materials to their structures and environments."[42] Nonetheless, the volume remained focused towards the needs of engineers: "This introductory text … is designed for freshman and sophomore engineering students with a background in general physics and chemistry; it does not use the rigorous approach which is common in solid state physics books."[43] True to Van Vlack's description, the textbook is light on formalism, opting to deliver content through prose, pictures, and diagrams, appealing to visually and mechanically oriented engineering students. Although early appropriations of the term "materials science" to identify a research area acknowledge the desirability of increased contributions from basic science, this new field was ensconced in an engineering tradition through the end of the 1950s.

The Advanced Research Projects Agency (ARPA), founded in 1958, hastened the transition towards an interdisciplinary definition of materials science.* One of ARPA's first large-scale funding initiatives supported a series of university-hosted interdisciplinary laboratories (IDLs) dedicated to the study of materials. The IDLs prompted universities to collapse departmental divisions within the context of materials science, creating sites where students could be acculturated to think broadly about problems related to materials and their limiting effect on technological development. Within these IDLs, the technical, scientific, and administrative character of materials science began to take shape.

ARPA's call for proposals reflected the 1950s advisory emphasis on materials as a bottleneck for strategic development because "[t]he Government has a vital stake in the establishment of the best possible materials research and development program. This is true because materials are a limiting factor in the performance of the advanced systems and devices essential to the operations and missions of Government agencies and departments."[44] The next paragraph indicated ARPA's intent to expand upon the materials science concept: "In order to strengthen basic research in materials sciences … the Government decided to support the establishment of a number of interdisciplinary materials research laboratories in universities. The objective of this Interdisciplinary Laboratory Program is to expand the national program of basic research and training in the materials sciences."[45]

Given ARPA's emphasis on attacking technological limitations by training students, it is notable that the agency chose to promote the development of a new interdisciplinary field, rather than to support efforts in existing disciplines, such as solid state physics, which already maintained a similar balance between basic and applied aims. Materials science grew from the same type of synthesis between research on metals and non-metals as solid state, but solid state, which staunchly maintained its physics bona fides, was not serving defense needs, as ARPA saw them. Materials science, as it coalesced within ARPA's IDLs did, however, mimic the strategy solid state had pioneered of organizing a new discipline to address contingent contemporary needs. These needs were professional, in the case of solid state, and technological, in the case of materials science, but they similarly sacrificed close cohesion to other ends.

Twelve universities won IDL contracts between 1960 and 1962. The first three contracts were hosted at Cornell University, the University of Pennsylvania, and Northwestern University, with the remainder appearing in quick succession at the University of Chicago, Brown University, Harvard University, the University of

* The agency's name vacillated between ARPA and DARPA (Defense Advanced Research Projects Agency) as the agency underwent sinusoidal acknowledgement of its focus on military research. To avoid confusion, I refer to it as ARPA throughout. For reference, it was founded as ARPA in 1958, changed its name to DARPA in 1972, dropped the "D" in 1993, and restored it in 1996.

Maryland, the Massachusetts Institute of Technology, the University of North Carolina, Purdue University, Stanford University, and the University of Illinois–Urbana.[46] ARPA funds prompted these institutions to consolidate their materials research efforts, which were often scattered across several departments and distant campus locations, in centralized "Materials Science Centers," generating early examples of the "center model" described by Cyrus Mody and Hyungsub Choi.[47] The Massachusetts Institute of Technology provides an apt case study. John C. Slater spearheaded MIT's IDL application. Slater, like John Van Vleck, earned his PhD at Harvard under Edwin Kemble in the early 1920s and represented the first generation of domestically trained quantum theorists.[48] By the 1950s, he was an Institute Professor of Physics—the first at MIT to be granted this honorary title. He devoted the bulk of his time to a research program in solid state and molecular theory, which used the latest digital computers to attempt calculations of the properties of solids and molecules from first principles.

Slater maintained a strong commitment to a research program rooted in the physics department. Four of the five faculty members affiliated with the solid state and molecular theory group were physicists—Slater, Lászlό Tisza, George F. Koster, and Michael P. Barnett—and one, Walter R. Thorson, was a chemist. Slater's *ab initio* approach used the most recent digital computing technology to calculate wave functions for solids and molecules with as few simplifications as possible. Known as "Slater physics" around the Institute campus, this approach proved too abstract for the teaching needs of MIT's engineering department, which brought Mildred Dresselhaus from Lincoln Laboratories to develop a course in solid state for engineers in 1967.[49] Nevertheless, Slater was in tune with the collaborative, interdepartmental efforts that characterized MIT's research culture in the 1950s. In his correspondence with ARPA administrator John F. Kincaid, Slater identified "Aeronautics and Astronautics, Chemical Engineering, Chemistry, Civil Engineering, Electrical Engineering, Mechanical Engineering, Metallurgy, Naval Architecture, and Physics" as the departments in which materials research was conducted.[50] Slater, conscious of the emphasis on interdisciplinary collaboration within advisory circles, took pains to emphasize this aspect of MIT's existing research programs.

MIT's initial proposal to ARPA was titled "The Interdisciplinary Nature of M.I.T. Research" and asserted that "[o]ne of the fundamental features of our proposal relates to the way in which we expect the various disciplines to cooperate in the research."[51] The proposal leaned on the record MIT had established promoting connections between departments, emphasizing in particular the Laboratory for Insulation Research (LIR),[52] founded in 1940, and the Research Laboratory of Electronics (RLE), founded after World War II to preserve facilities, equipment, and programs associated with wartime radar research.[53] Slater was equally attuned to ARPA's interest in graduate training, reassuring Kincaid: "We feel that establishment of interdisciplinary laboratories would be the best way to encourage expansion in graduate training and research."[54]

The neat correspondence between Slater's rhetoric and ARPA's ideals might be read as cynical kowtowing to a funder's demands if not for the similar initiatives that had been underway at the Institute prior to ARPA's call for proposals. In October of 1958, Slater circulated a memo reporting "a good deal of discussion of the desirability of some mechanism for getting closer liaison between persons in various departments of the Institute interested in solid-state and molecular science" and indicating broad support from "members of the chemistry, electrical engineering, mechanical engineering, metallurgy, and physics departments."[55]

The commitment to cross-department collaboration at MIT is evident as far back as the early 1940s, as the Institute began mobilizing its resources for war work. Arthur von Hippel, the force behind the Laboratory of Insulation Research, commented in 1942:

> There is no real boundary between physics and electrical engineering. Our field is a branch of applied physics mainly concerned still with the applications of Maxwell's theory. While the physicist stood "clean" of such useful tasks and strove for insight, the electrical engineer built a new economy and talked in a new technical language appropriate for his tools. Thus the link between the two was wearing thin, until events forced both sides into closer co-operation. The physicist began to toss into the domain of the electrical engineer new instruments, such as thermionic tubes and photocells, rectifiers, thermistors, and fluorescent lamps, which could not be understood on the old classical basis. And the electrical engineer replied in kind with magic eyes, complex impedance bridges, high-frequency generators, high-voltage machines, and magnets for cyclotrons, which revolutionized the experimental technique of the physicist.[56]

In consequence, according to von Hippel, "the fence between the two fields [physics and electrical engineering] is falling into disrepair."[57] The LIR was von Hippel's prime example of MIT's recognition that departmental divides could impede progress.

Following in that tradition, MIT scientists had been itching for a more consolidated materials program since the pre-ARPA 1950s when a proposal for an expanded program of materials research was compiled at the behest of the Atomic Energy Commission.[58] The proposal recorded a total of 88,020 square feet distributed over nine departments, serving 87 academic staff and 419 support personnel.[59] It called for consolidating these efforts in a 100,000 square foot Materials Research Laboratory, which would promote a "more fundamental approach" to material limitations on development.[60] By the time of the ARPA proposal, the estimate for the size needed to accommodate campus-wide materials science efforts had more than tripled to 350,000 square feet.[61]

At MIT and elsewhere, ARPA provided the infrastructure to make consolidation feasible on a large scale. Slater wrote to Kincaid with a frank assessment of MIT's physical constraints: "At present all of our work in materials research is very crowded, and we could hardly expand at all in number of students in some parts of the field without providing additional building space. To accommodate all the work in the field, on the scale on which we should like to operate, would require a building of approximately 350,000 square feet gross floor space. This would cost something like $14,000,000 to build."[62] When APRA funded an IDL at MIT in 1961, it included a 200,000 square foot building.[63] Although this fell short of full consolidation, it provided the Institute with space to serve as the crucible in which a stable interdisciplinary field could form.

The physical spaces ARPA provided were disciplinary laboratories as well as materials research laboratories.[64] They hosted a nationwide experiment in interdisciplinary collaboration, out of which the field of materials science emerged. A memorandum sent from ARPA to its IDLs in 1962 described the terms of the experiment:

> As you know, we have undertaken the responsibility of initiating a program in the national interest with universities for "basic research and graduate education" in a somewhat loosely defined area called material sciences. You have to a great extent defined what is meant—at least in your university—by material sciences by listing in your proposals to us the names of individuals you believe to be the core of the program at your institution. The collective research interests of these individuals defines in more detail material sciences.[65]

For ARPA, defining materials science was an empirical question, the caveat being that the goals of the field were established in advance. "Materials sciences" referred to the collaborations that proved productive according to the standards determined by the IDL program's objectives.

The inclusion of solid state physics in this disciplinary experiment underwrote ARPA's assertion that it was concerned with basic research. By maintaining its identity within physics, solid state retained a claim, however contentious it would become, to being a fundamental scientific discipline. That claim made it useful to materials science, which tried in part to set down a smooth stretch of pavement traversing the arduous path between theoretical insight and practical mastery. APRA's success in promulgating this picture is evident in figure 3, taken from a 1987 survey text, which represents solid state physics as the scientific core of materials science, grounding a full range of strategic applications.

The forging of materials science within ARPA's IDLs represented the large-scale adoption of solid state's heterodox approach to defining professional categories. ARPA started with a set of concrete objectives: to consolidate research on materials from a range of disciplinary standpoints and to train students within this new synthesis. It then promoted the formation of a field to address those objectives, showing no regard for whether or not the field that emerged obeyed traditional boundaries. In doing so, it created a space in which a portion of the solid state community could make a home and cement the case for its utility. At the same time, it exacerbated the tensions underlying the discontent over solid state's name, lending momentum to the case for an alternative: condensed matter physics.

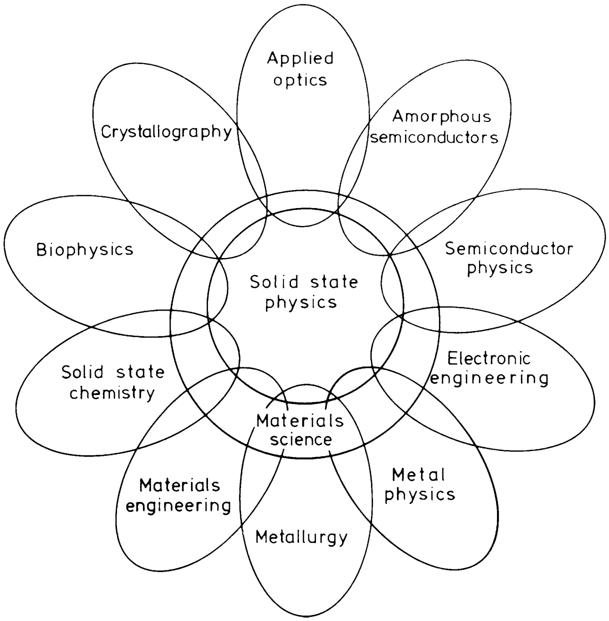

Fig. 3. Solid state physics as the basis for materials science. The original caption reads: "Some of the major overlapping research fields within Solid State Science. Solid State Physics is at the core of nearly all fields, while Materials Science embraces the wider application of the basic studies." Source: D. L. Weaire, ed., *Solid State Science: Past, Present and Predicted* (Bristol: Adam Hilger, 1987), x.

Whence Condensed Matter?

Discomfort with "solid state physics" lingered in some quarters well after the discipline established itself in the American Physical Society. In 1963, by which time the DSSP was the largest APS division, the second edition of the *American Institute of Physics Handbook* appeared. The comprehensive reference work added a section devoted to solid state.[66] Its editor, Dwight Gray, quipped: "Adding a chapter so named to the conventionally labeled group of mechanics, heat, acoustics, and so forth is, of course, a little like trying to divide people into women, men, girls, boys, and zither players" and recounted his co-editor's droll suggestion "that perhaps the book should contain only three major sections—Solid-State Physics, Liquid-State Physics, and Gaseous-State Physics."[67] The bemusement Gray and others shared would translate into action by the 1970s, when physicists who were bent on bolstering solid state's intellectual reputation and fought shy of materials science's technical thrust adopted the new appellation "condensed matter physics."[68] Although condensed matter physics would encompass many of the topic areas that comprised solid state physics, its aims were substantively different. Far from being a simple rebranding, the name change signaled the culmination of long-standing tensions within the solid state community between the pro-industry agenda that motivated Roman Smoluchowski and a desire for a conceptually coherent definition of the discipline's purpose and scope.

Condensed matter, like solid state, was a technical term before becoming a discipline. When physical questions did not depend on a specific state of matter, so long as it was dense enough, physicists talked about "condensed matter" as a medium through which, for example, a muon might travel and exhibit noteworthy behavior.[69] Early uses of this type were scattered, used almost exclusively by particle physicists. As a designator of a field, the term appeared first in Europe. In 1962 the journal *Physik der kondensierten Materie* was founded in West Germany, published by Springer-Verlag and edited by Swiss physicist Georg Busch.* The journal was published simultaneously as *Physique de la matière condensée* and *Physics of Condensed Matter* and accepted articles in German, French, or English. The boilerplate description added in the second issue described its scope as "relating mainly to thermal, electrical, magnetic and optical properties of solids and liquids in the broadest sense." It explained the decision to cast its net beyond solids: "Inclusion of work in the physics of both solid and the liquid phase is intended to increase closer contact between both areas and especially to further research in the area of liquids."[70]

Similar usages were slow to catch on in the United States. The *Physical Review* announced in October 1963 that in the following year the journal would appear in two topical sections—a measure presaging the full-scale split of the journal in 1970: "Section A will be primarily devoted to the physics of atoms, molecules, and condensed matter, and Section B will be primarily devoted to the physics of nuclei and elementary particles."[71] This instance is somewhat anomalous because it is the only clear occurrence of "condensed matter physics" in APS journals throughout the 1960s; discussions of reorganization in the APS Executive Committee uniformly call section A the solid state section. It is plausible that the term was in the air at Brookhaven National Laboratory, where managing editors Samuel Goudsmit and Simon Pasternack were based. Brookhaven's accelerator program provided the opportunity for them to be exposed both to the technical term and, through communication and collaboration with international labs, most notably CERN, to the newly common European usage.[72]

In 1964, a National Research Council report was commissioned to provide a broad survey of research in physics. The survey committee included established solid state physicists Harvey Brooks, Roman Smoluchowski, and Charles Townes, along with David Pines, who had recently earned his stripes exploring the implications of the Bardeen-Cooper-Schrieffer theory of superconductivity. The Pake Report (named in honor of its chairman, George Pake) was published in 1966 and

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* In 1975, this journal was absorbed into *Zeitschrift für Physik* when the latter split into two sections, the second of which was devoted to condensed matter and general physics.

identified "Solid-State (and Condensed-Matter) Physics" as one of the primary divisions whose progress it addressed.[73] The solid state and condensed matter subcommittee inserted a footnote into a draft of the report in April 1964 explaining the naming decision,[74] pointing out that around 90% of the field consisted of work on solids, thus using "solid state" as a general term was good enough for government work.[75]

"Condensed matter" in the Pake Report was both literally and figuratively parenthetical. Despite the passing acknowledgement that it might be a more appropriate term, the compilers referenced condensed matter only when the phenomenon under discussion deviated too uncomfortably from the realm of solids. They described early research in the field, for example, by slipping seamlessly from talking about solids to invoking condensed matter when discussing superfluidity: "Until the beginning of this century … the science of solids remained almost entirely empirical and descriptive. Between 1912 and the early 1930s, most of the salient properties of condensed matter, with the striking exception of superfluidity, were understood at least qualitatively." Such results ensured, the report continued, slipping back into the language of solids, that "[t]he stage was set for the beginning of solid-state physics in its present sense."[76]

"Condensed matter" papered over spots in the Pake Report where the restrictive nature of "solid state" became too obvious for comfort. As of the mid-1960s, the term did not presage sweeping changes to the structure and identity of solid state physics. Though the report insisted that solid state "is a fundamental branch of physics" and suggested that future progress in solid state "could well turn out to be of greater significance to our knowledge of the world than further progress in elementary-particle physics," it saved the greatest emphasis for solid state's technical contributions.[77]

The section entitled "Intellectual Challenge" began: "In solid-state physics there is at present no clearly visible need for radically new concepts" and made the case for conceptual importance by pointing to inchoate research areas, such as non-crystalline solids, as the potential but unproven source of "new concepts and principles."[78] The report reflected a broader pessimism at large in the 1960s regarding solid state's potential to make foundational intellectual contributions. Cambridge physicist Brian Pippard published a gloomy screed in *Physics Today* in 1961 prognosticating that "ten years is going to see the end of our [solid state physicists'] games as pure physicists, though not as technologists," and recommending that solid state physicists start training students for careers in industry.[79]

In a similar, if less defeatist spirit, the Pake report, while tepid about solid state's basic research potential, raved about its "indispensible [role] in numerous technological developments," boasting that "the whole [of] communications technology is being fundamentally affected by these [solid state] developments."[80] The authors, shifting from their cautious tone when discussing solid state's intellectual importance, emphasized the "indispensable," "vital," and "essential" contributions solid state research made to technological systems that were "totally dependent" on solid state devices and know-how.[81] Solid state was still building its intellectual portfolio, but its technological track record was strong. So long as the field justified itself primarily on technological grounds, its most prominent research programs would be focused around solids; nagging concerns about solid state's appropriateness as a category could be swept under the rug.

In the early 1970s—as dismay over the widening prestige gap between solid state and particle physics peaked—"condensed matter" unseated "solid state" as the preferred term in advisory circles. An analogous NRC report published in 1972 included a chapter on "Physics of Condensed Matter."[82] It eschewed the language of solid state when referencing the contemporary field, though it retained solid state terminology for historical observations. In all, the two terms appear with about equal frequency. The 1972 committee contained many of the same members as the 1966 group—notably Brooks, Smoluchowski, and Townes—but some new recruits wrote the chapter on condensed matter physics. Among them was Morrel Cohen, a University of Chicago physicist who was one of two Americans who had served on the editorial board of *Physik der kondensierten Materie* since its founding and who had begun describing his research specialty as "physics of condensed matter" no later than 1964.[83]

The shift in language was accompanied by newly potent concern over funding patterns. The condensed matter chapter explained that "basic research in physics of condensed matter, performed solely to understand in the deepest possible way the complex behavior of solids and liquids, has been the source of two decades of unprecedented achievement in critical new technologies. We see no way in which these achievements could have been planned in the past and no way in which further progress can be programmed except by continued support of basic research."[84] Programming—the planning and funding of research programs based on preconceived technological goals—was an ever-present bugbear for solid state and condensed matter physicists concerned with their intellectual prestige. The formulation of research programs with specific practical outcomes in mind, the modus operandi in materials science, threatened to undermine their intellectual autonomy and motivated the condensed matter panel to state the field's intellectual value much more vociferously than their counterparts had in 1966.

The 1972 panel was accordingly circumspect about solid state's technological contributions. They asserted that solid state research had been responsible for steady innovation, but were careful to emphasize "the richness and complexity of these events [inventions of solid state technologies], as well as the varied motivations of the scientists and engineers involved."[85] By emphasizing the complexity and capriciousness of the routes from basic research to technological applications, the panel sought to protect federal funding for basic solid state research by undermining the federal government's tendency to link basic research expenditure to clearly articulated outcomes: "The United States would not spend its research and development dollars nearly so well if it insisted either that basic research be strongly motivated by and directed to practical goals or that all basic research be isolated from practical considerations."[86] The shift away from solid state and towards condensed matter coincided with rising concern about an environment that married research support to practical outcomes.

The same forces to which the 1972 NRC report reacted prompted Bell Labs' Philip Anderson to defend the intellectual merit of complex physics in a *Science* article entitled "More Is Different" that same year. Anderson had earned his PhD with John Van Vleck, with whom he and Nevill Mott would share the 1977 Nobel Prize. "More Is Different" introduced another wrinkle to the name question, referring to "many-body physics." The term calls to mind the quantum many-body problem, the notoriously intractable difficulty of calculating a wave function for quantum systems containing three or more interacting bodies. The impossibility of deducing the properties of many-body systems from first principles meant, for Anderson, that the concepts employed to understand complex systems were just as intellectually valuable as those used to understand elementary particles.[87] "Many-body physics" did not stick, in part, no doubt, because of its narrow focus on theory. Anderson's usage of the term, however, mirrors the way condensed matter began to be used in the same era, sometimes by Anderson himself.* Both terms refocused attention on methodological commonalities and so fit more conveniently into the narrative around intellectual prestige and research funding that condensed matter physicists preferred.

That "condensed matter" was a professionally motivated category in the 1970s is evident when examining its published usage. Between 1970 and 1979, only 32 articles in APS journals contained "condensed matter" in their titles or abstracts. In comparison, 121 included "solid state," 1,019 contained "solids," and "high energy" appeared 1,399 times.[88] The move towards condensed matter language in the NRC, then, was not a reflection of prevailing research practices or self-identification patterns. "Solid state" and "condensed matter" were both capacious umbrella categories that did little to shape day-to-day research. They had greater meaning as funding categories, so that the terminological shift reflected a change of professional ideology brought about as solid state physicists confronted challenges to their intellectual prestige and funding for exploratory research.

The watershed moment for condensed matter was the renaming of the APS division. A proposal at the January 1978 Council meeting to rename the Division of Solid State Physics the "Division of Condensed Matter Physics" scandalized representatives from the Division of Fluid Dynamics (DFD), who perceived an invasion of their turf: "[François] Frenkiel expressed concern over the overlap

* Common lore holds that Anderson and Volker Heine coined "condensed matter" in 1967 while Anderson was a visitor in Cambridge and they changed the name of the solid state theory group at the Cavendish Laboratory to "theory of condensed matter." Earlier occurrences of the term, in particular in the Springer journal title, belie this simple origin story, but the adoption of the term by a major UK research unit no doubt raised its profile in the Anglophone world.

between the subject matter covered by a Division of Condensed Matter Physics and by the Division of Fluid Dynamics, and noted that approval of such a change would force the Division of Fluid Dynamics to change its name. [Tony] Maxworthy noted that the Division of Fluid Dynamics Executive Committee expressed strong feeling against the name change."[89] The matter was postponed until April, giving the two divisions some time to hash out their differences. The motion succeeded at the April meeting, but it remained controversial, passing by a vote of 15 in favor to 7 opposed at a time when unanimity was the norm.[90]

The DSSP had been identified with solids for so long that the new name stepped on the toes of other divisions. The Division of Fluid Dynamics did not need to change its name, but its members' resistance shows that the shift from solid state to condensed matter included significant reorientation of solid state's professional goals at a time when research on fluids was gaining ground. As liquid helium, for example, became more central to superconductivity—one of the more colorful feathers in the solid state cap—the language of solids became correspondingly more inconvenient and the observation from the Pake report that solids accounted for upwards of 90% of the activity in the field no longer applied.[91] The DFD understandably saw the DSSP's efforts to bring fluids under its aegis as imperialistic. The condensed matter partisans within the DSSP, for their part, pursued a new conceptual focus that inconvenienced the existing institutional structure that had grown in the days when solid state maintained a more thoroughly industrial reputation. The DFD had been founded in 1948, shortly after the DSSP. The renaming in the late 1970s signaled a newfound commitment to advocating for condensed matter as a basic research enterprise and, in so doing, threatened the organizations that had filled that void in the years when solid state physics was a more thoroughly industrial pursuit.

Solid state physics had always had a strong industrial patina. In the National Research Council's 1946 survey of US industrial laboratories, just before the DSSP's founding, only Bell Laboratories counted "solid state physics" among is research specialties.[92] By the 1960 edition of the same survey, the term was ubiquitous, not limited to the likes of Bell, which maintained a large basic research staff. The American Machine and Foundry Co. in Stamford, Connecticut, Hughes Aircraft Company's Newport Beach lab, and Control Data Corp. in Minneapolis were among the dozens of laboratories that counted solid state among their specialties, including many that listed no physicists among their researchers.[93]

Solid state's applied bent was well understood by its practitioners, even those who preferred to nudge it away from industry. Research for the Pake Report concluded that in 1963 60% of funding for solid state was spent in industry.[94] The success of the IDLs in the universities, though widely celebrated, generated concerns about the relationship between solid state and physics as a whole. Harvard's Harvey Brooks worried, in a letter to Walter Kohn in 1964, that the growth of IDLs and applied physics departments had exacerbated an existing tendency for solid state to isolate itself. He was most concerned for solid state theorists, who when co-located with other materials researchers lost connections to other theoretical physicists, hampering their ability to participate in the verbal exchanges that he identified as being critical to theory.[95]

These issues, which were blossoming in the mid-1960s, became the cornerstones of condensed matter rhetoric in the 1970s and early 1980s. "Condensed matter" won favor among those who sought to distance the field from industry and reconnect with the pure science tradition that remained a strong ideological force in American physics. A 1986 NRC report addressed the problem of intellectual prestige by highlighting "the fact that condensed matter physics is the physics of systems with an enormous number of degrees of freedom." As a consequence, the report maintained, echoing the substance of Anderson's 1972 arguments, "[a] high degree of creativity is required to find conceptually, mathematically, and experimentally tractable ways of extracting the essential features of such systems, where exact treatment is an impossible task."[96] The 1986 volume goes to great lengths to identify condensed matter physics as a fundamental and intellectually valuable field of science, and thereby to distinguish it from materials science. "Condensed-matter physics is intellectually stimulating," the report emphasizes, "because of the discoveries of fundamental new phenomena and states of matter, the development of new concepts, and the opening up of new subfields that have occurred continuously throughout its sixty-year history."[97]

The choice of a sixty-year timeline for the field is telling of the authors' agenda, especially since the Pake Report committee thought Max von Laue's discovery of X-ray diffraction in 1912 a more appropriate landmark.[98] Harking back to 1926, the year in which quantum mechanics displaced the old quantum theory, placed condensed matter firmly in the tradition of the quantum theory of solids, which emerged in the late 1920s as some of the architects of quantum mechanics explored the new theory's utility for describing electrons in metals and the importance of such applications for understanding the foundations of the new theory.[99]

The nod to the 1920s made it clear that condensed matter physics was defined, at core, by its intellectual contributions to physics and that it was united around the methods required to describe the complex interactions of atoms and molecules in close proximity. Precisely dating condensed matter to the advent of quantum mechanics distanced the field from its applications. Solid state physicists before the 1980s preferred earlier touchstones, mostly in the late nineteenth and early twentieth centuries. Slater, for instance, launched an overview of the field in 1952 writing: "The physics of the solid state is nothing new. In 1900 it was as well realized as now that mechanics, heat, electricity, magnetism, optics, all have their solid-state aspects."[100] Following such a strategy for condensed matter would have made it more difficult to distinguish it from materials science. Making that separation evident emphasized that condensed matter physicists would not carry water for technological interests. The report began by declaring: "we are *not* surveying materials science nor the considerable impact of condensed-matter physics on technology."[101] By the mid-1980s, the distinctions between condensed matter—the "fundamental" discipline—and materials science—its applied cousin—were solidifying, while the fragile professional alliance that had sustained solid state physics through the preceding decades was loosening.

Though distancing themselves from industrial associations, condensed matter physicists notably did not seek a clean break with solid state physics, choosing instead to emphasize the conceptual and methodological continuities between the fields, a move made easier by the widespread impression that solid state had been poorly named. Solid state's technological accomplishments—such as the transistor, superconducting magnets, and magnetic resonance imaging—remained integral to condensed matter's rhetorical arsenal. The 1986 report touted condensed matter as "the field of physics that has the greatest impact on our daily lives through the technological developments to which it gives rise."[102] It was not, however, within the job description of condensed matter physicists to pursue those developments, as it was for materials scientists and had been for many early solid state physicists.

The implied division of labor kept condensed matter physicists one degree removed from actual technological applications, which fell to materials scientists and engineers. This perspective presupposed something akin to the linear model of innovation, namely the philosophy, unpopular with materials scientists, that unfettered basic research was the primary wellspring of technological advance.[103] Rustum Roy, cofounder of the Materials Research Society, called the notion that basic science begets innovation, which in turn begets prosperity, "preposterous, certainly in league with perpetual motion."[104]

Condensed matter physicists' linear arguments, even in detailed surveys such as the 1986 NRC report, were limited to showing that basic scientific knowledge was relevant to existing areas of technological importance. They did not spell out how funding for basic research would translate into technological advances on the ground. The growth of materials science as the development arm of the old solid state constellation allowed condensed matter physicists the latitude to bracket such concerns as somebody else's problem. Drawing too close an association between basic research and applied goals tempted explicit links between basic research funding and applied targets, the very specter condensed matter physicists hoped to avoid. They therefore walked a fine line, pressing hard for intellectual prestige while still hoping to maintain a reasonable claim to technological relevance.

The demanding problems presented by the physical complexity of solids were by no means unique to that phase of matter. The methods and concepts developed to investigate and understand solids transitioned fluidly into the broader category of condensed matter. As they did, the professional objectives to which they were turned changed markedly. Solids were originally chosen as a disciplinary category because of their shared relevance to industrial and academic physicists. In the 1940s, that choice served the perceived need to bring industrial researchers into the professional fold and to establish better lines of communication beyond the academy. By the 1970s, condensed matter served the opposite impulse. Solid state's industrial past was a liability in the eyes of those who defended the merit of its intellectual content, even if its technological accomplishments were a rhetorical necessity. The new category of condensed matter physics, by returning to shared practices as a way of defining the field, aided physicists interested in the basic problems posed by complex material systems in their efforts to emphasize the intellectually challenging elements of their enterprise.

Conclusions

As American physics become larger, more intricately structured, and more politically engaged after World War II, top-down professional concerns became potent motivators of disciplinary consensus. Solid state physics was aptly named for a distinctive transitional era and was engineered to meet the institutional challenges of the early postwar years. Just as a field defined by investigative practices is susceptible to reformation as those practices evolve, solid state physics, defined by a professional ideology calibrated to the challenges of its time, was reimagined and recast as new conditions subjected its practitioners to a different set of professional pressures.

Considering solid state physics to be defined by institutional détente highlights ideological differences between solid state, condensed matter, and materials science that complicate the very real conceptual, methodological, and topical continuities between these fields. It also raises the question of whether the disciplinary consensus that defined solid state physics is represented more accurately in materials science. If condensed matter claimed as its own the suite of quantum mechanical methods developed within solid state physics, materials science captured the aspects that were more essential to solid state's original disciplinary identity.

"A good name is rather to be chosen than great riches" is a clean, if somewhat poetical articulation of the professional tensions that favored the growth of disciplinary alternatives to solid state. Condensed matter physicists sought a good name by rebelling against solid state's unorthodox constitution. Physicists who preferred "condensed matter" returned to intellectual common ground—the suite of methods that had evolved to cope with many-body systems—as a basis for claiming autonomy. Condensed matter physicists understood themselves to be just as entitled as their colleagues in high energy physics to pursue fundamental knowledge and, no less importantly, to the esteem that came with it.

Materials science translated the great riches provided by federal funding for research programs with unambiguous practical objectives into vigorous growth and a strong alliance between university facilities and national needs. The Cold War climate ensured steady funding for research with demonstrated relevance to national defense. Materials science, adapting itself to this funding context, followed the disciplinary model solid state had pioneered, a model that lives on in present-day categories such as nanotechnology.[105] Like solid state, materials science was a loose professional alliance conglomerating diverse methodological approaches into an enterprise defined by a set of contingent contextual goals, rather than by conceptual and methodological coherence. Reveling in interdisciplinary collaboration and close institutional ties between academia, industry, and government, materials science more faithfully reproduced the founding spirit of solid state physics than did condensed matter physics.

Considering the transition from solid state to condensed matter a mere renaming obscures these dynamics and sells short the disciplinary complexity of Cold War science. In a balkanized scientific community, individual researchers became subject to more layers of disciplinary authority, some of which did little to reflect their day-to-day practices. An interpretation of the shift from solid state to condensed matter that emphasizes continuity of practice—which has its attractions if we understand disciplines to be built around shared practices—fails to capture some clear differences in the professional ideologies that were critical to the disciplinary consensus at the heart of both solid state physics, condensed matter physics, and materials science.

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[28] Otto Breck, "Catalysis—A Challenge to the Physicist," *Rev. Mod. Phys.* **17** (1945), 61–71.

[29] Seitz became a major figure in American solid state physics before transitioning to administrative and advisory roles in the 1950s. His institutional acumen paved his way to the positions that would make him infamous as an advocate for tobacco and petroleum companies, the role for which he is better remembered today. Naomi Oreskes and Erik M. Conway, *Merchants of Doubt: How a Handful of Scientists Obscured the Truth on Issues from Tobacco Smoke to Global Warming* (New York: Bloomsbury Press, 2010).

[30] Frederick Seitz, *Modern Theory of Solids* (New York: McGraw Hill, 1940).

[31] Charles Kittel, *Introduction to Solid State Physics*, 2nd edition (New York: John Wiley & Sons, 1955).

[32] John J. Hopfield, "Whatever Happened to Solid State Physics?," *Annual Reviews of Condensed Matter Physics* **5** (2014), 1–13, on 3.

[33] Detailed accounts of the transistor's invention and refinement can be found in Lillian Hoddeson, "The Discovery of the Point-Contact Transistor," *Historical Studies in the Physical Sciences* **12** (1981), 41–76 and Michael Riordan, Lillian Hoddeson, and Conyers Herring, "The Invention of the Transistor," *Rev. Mod. Phys.* **71** (1999), S336–S345.

[34] Kittel, *Introduction to Solid State Physics* (ref. 31), vii.

[35] National Academy of Sciences, National Research Council, *Report of the National Academy of Sciences, 1953–54* (Washington, DC: National Academy of Science, 1954), 60.

[36] Ibid., 60.

[37] National Academy of Sciences, National Research Council, *Report of the National Academy of Sciences, 1957–58* (Washington, DC: National Academy of Science, 1958), 46.

[38] Materials Advisory Board, "Standing Review of Department of Defense Materials Research and Development Program," Frederick Seitz papers, 1935–1965, University of Illinois Archives, Urbana, IL, box 1, folder Air Research and Development Command, 1952–61 #1.

[39] Ibid.

[40] National Academy of Sciences, National Research Council, *More Effective Organization and Administration of Materials Research and Development for National Security* (Washington, DC: National Academy of Sciences, 1960), frontmatter.

[41] Ibid., vii.

[42] Lawrence H. Van Vlack, *Elements of Materials Science* (Reading, MA: Addison-Wesley, 1959), vii.

[43] Ibid., vii.

[44] "Interdisciplinary Laboratories for Basic Research in Materials Sciences," John Clarke Slater papers, 1908–1976, American Philosophical Society, Philadelphia, PA (hereafter Slater papers), folder M.I.T. Dept. of Physics #39.

[45] Ibid.

[46] National Academy of Sciences, *Advancing Materials Research* (Washington, DC: The National Academies Press, 1987), 36.

[47] Cyrus Mody and Hyungsub Choi, "From Materials Science to Nanotechnology: Interdisciplinary Center Programs at Cornell University, 1960–2000," *Hist. Stud. Nat. Sci.* **43** (2013), 121–161.

[48] See S. S. Schweber, "The Empiricist Temper Regnant: Theoretical Physics in the United States, 1920–1950," *Historical Studies in the Physical and Biological Sciences* **17** (1986), 55–98 on the American style of theory that grew largely from the school Kemble established.

[49] Mildred Dresselhaus, interview with Joseph D. Martin, June 24, 2014.

[50] John C. Slater to John Kincaid, May 6, 1959, Slater papers, folder Kincaid, John F. #1.

[51] "The Interdisciplinary Nature of M.I.T. Research," Slater papers, folder Proposal for a Materials Center at M.I.T., 1960.

[52] Arthur von Hippel, who established the LIR, recalled choosing an abstruse name as "a camouflage trick … to avoid stepping on sensitive toes by encroaching on the entrenched interests of physicists, chemists, and metallurgists in the materials field." Arthur von Hippel, interview by Z. Malek, September 1969, Arthur von Hippel papers, Massachusetts Institute of Technology Archives and Special Collections, Cambridge, MA, box 1, folder 16.

[53] "Interdisciplinary Nature of M.I.T. Research" (ref. 51).

[54] Slater to Kincaid (ref. 50).

[55] John C. Slater, untitled memorandum, Slater papers, folder M.I.T. Dept. of Physics #10.

[56] Arthur von Hippel, "New Fields for Electrical Engineering," Arthur von Hippel papers, Massachusetts Institute of Technology Archives and Special Collections, Cambridge, MA, box 1, folder 44. This was a piece von Hippel prepared for the April 1942 edition of *The Tech Engineering News*, a periodical published by MIT undergraduates.

[57] Ibid.

[58] "Proposal for An Expanded Program of Materials Research at the Massachusetts Institute of Technology, July 12, 1956," Slater papers, folder MIT Materials Research #1. The AEC made a similar push within the national laboratories, as discussed in Peter J. Westwick, *The National Labs:*

Science in an American System, 1947–1994 (Cambridge, MA: Harvard University Press, 2003), 257–258.

[59] "Materials Research Program, (ca. 1956)," Slater papers, folder M.I.T. Materials Research #1.

[60] Ibid.

[61] Slater to Kincaid (ref. 50).

[62] John C. Slater to John Kincaid, April 30, 1959, Slater papers, folder Kincaid, John F. #1.

[63] John C. Slater, "On the MIT Materials Center," ca. 1960, Slater papers, folder Slater, J. C. On the MIT Materials Center.

[64] Scott Knowles and Stuart Leslie have argued that the campuses at industrial laboratories such as Bell, General Motors, and IBM mimicked what architect Eero Saarinen supposed to be the university model of organizing research, namely a linear model, in which basic research fed directly into industrial applications. The rhetoric around MIT's IDL reveal similar goals by suggesting that placing basic research in physics and chemistry alongside materials engineering fields would help to advance ARPA's technical aims. Knowles and Leslie, "'Industrial Versailles': Eero Saarinen's Corporate Campuses for GM, IBM, and AT&T," *Isis* **92** (2001), 1–33.

[65] ARPA, "Administrative Memo #1," July 20, 1962, Slater papers, folder M.I.T. Dept. of Physics #138.

[66] Dwight E. Gray and Bruce H. Billings, eds., *American Institute of Physics Handbook*, 2nd edition (New York: McGraw-Hill, 1963).

[67] Dwight E. Gray, "The New AIP Handbook," *Physics Today* **16**(7) (1963), 40–42, on 41.

[68] The implication that it was problematic to see physics as classifiable by states of matter was apropos. For instance, John Van Vleck's 1932 monograph *The Theory of Electric and Magnetic Susceptibilities* (Oxford: Clarendon Press, 1932) was seen as a classic of solid state physics, even though it dealt mainly with magnetic susceptibilities in gasses. See: Midwinter and Janssen, "Kuhn Losses Regained" (ref. 21).

[69] As in R. A. Ferrell, Y. C. Lee, and M. K. Pal, "Magnetic Quenching of Hyperfine Depolarization of Positive Muons," *Physical Review* **118** (1960), 317–319.

[70] *Physik der kondensierten Materie* **1**(2) (1963), frontmatter.

[71] "Important Announcement," *Phys. Rev.* **132** (1963), 1.

[72] Brookhaven's collaborative efforts are outlined in Robert Crease, *Making Physics: A Biography of Brookhaven National Laboratory, 1946–1972* (Chicago: University of Chicago Press, 1999).

[73] National Academy of Sciences, National Research Council, *Physics: Survey and Outlook* (Washington, DC: National Academy of Sciences, 1966), 67.

[74] The footnote was cut from the Pake Report but would resurface in a supplement that furnished more detailed reports on the subfields of physics. National Academy of Sciences, National Research Council, *Physics: Survey and Outlook, Reports of the Subfields of Physics* (Washington, DC: National Academy of Sciences, 1966), 143.

[75] "NAS-NRC Physics Survey Committee, Solid State Physics and Condensed Matter," draft, April 1964, Harvey Brooks Papers, Correspondence and Other Papers relating to National Academy of Sciences, 1962–1986, Harvard University Archives, Cambridge, MA (hereafter Brooks papers**)**, box 1, folder NAS Survey Committee March-April 1964.

[76] NAS-NRC, *Physics Survey* (ref. 73), 67.

[77] Ibid., 68.

[78] Ibid., 67–69.

[79] Brian Pippard, "The Cat and the Cream," *Phys. Today* **14**(11) (1961), 40–41.

[80] NAS-NRC, *Physics Survey* (ref. 73), 69.

[81] Ibid.

[82] National Academy of Sciences, National Research Council *Physics in Perspective, Volume II, Part A: The Core Subfields of Physics* (Washington DC: National Academy of Sciences, 1972).

[83] *Phys. kond. Mat.* **1**(1) (1963), frontmatter; "Authors," *IBM Journal of Research and Development* **8** (1964), 361–364, on 361.

[84] NAS-NRC, *Physics in Perspective* (ref. 82), 460.

[85] Ibid., 458.

[86] Ibid., 459.

[87] Philip W. Anderson, "More Is Different" *Science,* New Series **177** (1972), 393–396, on 393. For a discussion of Anderson's arguments for the fundamental nature of condensed matter physics, see Joseph D. Martin, "Fundamental Disputations," *Hist. Stud. Nat. Sci.*, forthcoming.

[88] American Physical Society Journals, http://journals.aps.org/search, accessed August 13, 2014. The ratio is starker in AIP journals, with 33 instances of "condensed matter" and 4,695 of "solid state." The difference here is amplified by several factors, including the applied focus of AIP journals during an era that witnessed an explosion in topics such as solid state masers and lasers and the fact that the AIP search algorithm includes the titles of citing articles, which generates a high rate of false positives. American Institute of Physics Journals, http://scitation.aip.org/search, accessed August 13, 2014.

[89] Minutes of the American Physical Society Council Meeting, San Francisco, California, January 22, 1978, American Physical Society meeting minutes and membership list, 1902–2003, Niels Bohr Library and Archives, College Park, MD (hereafter APS minutes).

[90] Minutes of the American Physical Society Council Meeting, Washington, DC, April 23, 1978, APS minutes.

[91] Hopfield, "Whatever Happened" (ref. 32), points to the success of the BCS theory of superconductivity as the theoretical development that encouraged physicists to see solid state problems as general physical problems.

[92] National Academy of Sciences, National Research Council, *Industrial Research Laboratories of the United States*, Sixth Edition (Washington DC: National Academy of Sciences, 1946).

[93] National Academy of Sciences, National Research Council, *Industrial Research Laboratories of the United States*, Eleventh Edition (Washington DC: National Academy of Sciences, 1960).

[94] Walter Kohn to George Pake, November 13, 1964, Brooks papers, box 1, folder NAS Survey Committee May-December 1964.

[95] Harvey Brooks to Walter Kohn, March 30, 1964, Brooks papers, box 1, folder NAS Survey Committee March-April 1964.

[96] National Academy of Sciences, National Research Council, Panel on Condensed-Matter Physics, *Condensed-Matter Physics: Physics through the 1990s* (Washington, DC: National Academy Press, 1986), 3.

[97] Ibid.

[98] NAS-NRC, *Physics Survey* (ref. 73), 67; NAS-NRC, *Physics in Perspective* (ref. 82), 142.

[99] See Lillian Hoddeson, Gordon Baym, and Michael Eckert, "The Development of the Quantum Mechanical Electron Theory of Metals, 1926–1933," in Lillian Hoddeson et al., eds., *Out of the Crystal Maze: Chapters from the History of Solid State Physics* (Oxford: Oxford University Press, 1992), 88–181 and Christian Joas and Jeremiah James, "Subsequent and Subsidiary? Rethinking the Role of Applications in Establishing Quantum Mechanics," *Hist. Stud. Nat. Sci.,* forthcoming.

[100] John C. Slater, "The Solid State," *Phys. Today* **5**(1) (1952), 10–15, on 10.

[101] NAS-NRC, *Condensed-Matter Physics* (ref. 96).

[102] Ibid., viii.

[103] For an overview of the linear model, see: Benoît Gordon, "The Linear Model of Innovation: The Historical Construction of an Analytical Framework," *Science, Technology, & Human Values* **31** (2006), 639–667.

[104] Rustum Roy, "Funding Big Science," *Phys. Today* **48**(9) (1985), 9.

[105] Matthew N. Eisler, "'The Ennobling Unity of Science and Technology': Materials Science and Engineering, the Department of Energy, and the Nanotechnology Enigma," *Minerva* **51** (2013), 225–251.

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