

## From Physical Chemistry to Chemical Physics, 1913-1941

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There has never been one unique name for the intersection of chemistry and physics. Nor has it ever been defined by a single, stable set of methods. Nevertheless, it is possible and arguably rewarding to distinguish changes in the constellation of terms and techniques that have defined the intersection over the years. I will speak today about one such change, the advent and ascendancy of chemical physics in the interwar period.

When the young Friedrich Wilhelm Ostwald first began to formulate his campaign for “physical chemistry” in 1877, he used the term almost interchangeably with two others, “general chemistry” and “theoretical chemistry.” According to his vision of what would soon become a new chemical discipline, physical chemistry would investigate and formulate the general principles that underlie all chemical reactions and phenomena. The primary strategy that he and his allies used to generate these principles was to formulate mathematical “laws” or “rules” generalizing the results of numerous experiments, often performed using measuring apparatus borrowed from physics. Their main fields of inquiry were thermochemistry and solution theory, and they avoided and often openly maligned speculations regarding structures or mechanisms that might underlie the macroscopic regularities embodied in their laws.<sup>1</sup>

In the first decades of the 20<sup>th</sup>-century, the modern atomic theory was firmly established, and with only a slight delay, the methods of 19<sup>th</sup>-century physical chemistry lost a considerable proportion of their audience. Theories relying upon atomistic thinking began to reshape the disciplinary intersections of chemistry and physics, and by the end of the 1930s, cutting-edge research into the general principles of chemistry looked quite different than it had at the turn of the century. For one, quantum mechanics had become the basis not only for a range of new theories, but also for a wide array of promising, if still primitive, new instrumental methods. In addition, the “homeland” of physical chemistry, the bulk of its leading research centers, was moving increasingly further from the German-speaking universities, as research centers in Great Britain and the USA as well as Japan took the lead in developing these new methods. This marked a disciplinary transformation both intellectually and institutionally, leading to tensions between older physical chemists and those employing the new methods. It also spawned a new disciplinary label, “chemical physics,” meant to include not only quantum chemistry, but also modern reaction kinetics, diffraction techniques (both x-ray and electron), and molecular spectroscopy, among other fields.

In this talk, I will provide an overview of the rise of chemical physics, albeit a brief one, and I will argue that the intellectual and geographical peculiarities of its establishment, especially its deep instrumental entanglement and its early reliance upon a relatively small, widely distributed research network are important keys to understanding the postwar development of chemistry, particularly chemical modeling and theory. The talk will be divided into three parts. The first part will cover how chemical physics differed from traditional physical chemistry and what this might tell us about the intersections between chemistry and physics in the interwar period. The second part provides some examples of how the methods that

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<sup>1</sup> For an overview of this development see John W. Servos, *Physical Chemistry from Ostwald to Pauling: The Making of a Science in America* (Princeton: Princeton University Press, 1990).

collectively made up chemical physics moved simultaneously between countries and between scientific disciplines. Most of these methods originated in Germany, and I will focus primarily on movement between Germany, Great Britain, and the United States, as these are the cases I know best. However, it would be remiss to leave out the Japanese case, as several Japanese researchers contributed decisively to these methods. The final section, will argue for two possible benefits of closer attention to the advent of chemical physics. First, it provides a vital basis for understanding the epistemological changes attendant on the widely discussed “instrumental revolution” or “second chemical revolution.”<sup>2</sup> Second, it helps to delimit the field of plausible explanations for why several prominent new methods at the intersection of chemistry and physics, though they originated in Germany, developed more rapidly in other countries between the mid-1930s and the 1950s

One of the classic signs of a new scientific discipline is the founding of new journals. The first issue of *The Journal of Chemical Physics* appeared in 1933. The journal was, in part, a response to the reluctance of Wilder Bancroft, a colloid chemist and editor of the well-established *Journal of Physical Chemistry*, to publish articles on the new quantum chemistry or technical articles on x-ray crystallography, electron diffraction, or molecular spectroscopy.<sup>3</sup> In the first issue of the new journal, Harold Urey, the American physical chemist and radioactivity researcher, as well as the new journal’s first editor, justified its establishment based on the following observation:

Men who must be classified as physicists on the basis of training and of relations to departments or institutes of physics are working on the traditional problems of chemistry; and others who must be regarded as chemists on similar grounds are working in fields which must be regarded as physics.<sup>4</sup>

In light of the articles that the *Journal of Chemical Physics* would host, it’s hard to disagree with Urey’s assessment. Chemists, like Linus Pauling, published on quantum mechanics; while, physicists, like John Slater, explained idiosyncratic structural transitions of specific compounds.

However, disciplinary labels at the intersection of chemistry and physics at the time were even more fluid than Urey’s assessment might lead one to believe. It was not simply that chemists were employing physicists’ methods and vice versa. The very same methods often belonged to physics (specifically molecular physics) in one location and chemistry (specifically physical chemistry) in another. I will provide some examples shortly, but first let me point out some concrete consequences of this sharing of research methods (in the sense of methods developed by Gaston Bachelard)<sup>5</sup> across disciplines. It meant that, to stay up to date in their field, physicists and chemists working with these new methods frequently read articles from journals outside their discipline. Many also attended conferences across the disciplinary divide, and some, such as Robert Mulliken, even pursued a disciplinarily ambiguous career.

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<sup>2</sup> On the instrumental revolution see, among others, Peter J. Morris, ed., *From Classical to Modern Chemistry: The Instrumental Revolution* (Cambridge: The Royal Society of Chemistry, 2002); Davis Baird, *Thing Knowledge: A Philosophy of Scientific Instruments* (Berkeley: University of California Press, 2004); and Carsten Reinhardt, *Shifting and Rearranging: Physical Methods and the Transformation of Modern Chemistry* (Sagamore Beach: Science History Publications, 2006).

<sup>3</sup> Servos, “A Dissenter’s Decline,” *Physical Chemistry from Ostwald to Pauling*, ch. 7.

<sup>4</sup> Harold Urey, “Editorial,” *Journal of Chemical Physics* 1 (1933), p. 1.

<sup>5</sup> See Gaston Bachelard, “Le Problème Philosophique des Méthodes Scientifiques (1951),” in *L’Engagement Rationaliste* (Paris: Presse Universitaires de France, 1972).

(Mulliken completed both his B.Sc. and his Ph.D. in chemistry before becoming a professor of physics at the University of Chicago but then received the Nobel Prize in Chemistry in 1963.)

As to the methods with dubious disciplinary identities that would make up the new field of chemical physics, the best researched is quantum chemistry—in the now standard sense of determining the electronic structure and energy of molecules. Kostas Gavroglu and Ana Simões, among others, have published several excellent studies on the origins of this field; how the term “quantum chemistry” came to be associated with just this, relatively narrow, application of quantum mechanics to chemical problems; and how the field shifted disciplinary identities between physics (in Germany), chemistry (in the USA) and applied mathematics (in Great Britain) before establishing itself as a relatively stable and independent discipline.<sup>6</sup>

But quantum chemistry was not the only new method reliant upon quantum theory and with dual citizenship in chemistry and physics. X-ray crystallography and electron diffraction also clearly fit the bill. X-ray diffraction was famously discovered by the physicists Max von Laue, Walter Friedrich, and Paul Knipping but soon attracted the attention of physical chemists such as Hans Georg Grimm. Its applications to crystals and chemistry were developed further in Britain under the guidance of the physicists, William and Henry Bragg, who soon took to employing chemists as assistants. Then, in the United States, it became one of the main research foci for physical chemists at the California Institute of Technology; although, the techniques first employed there were extensions of the photographic technique developed by the Japanese physicist Shoji Nishikawa.

A strikingly similar story lies behind the development of molecular spectroscopy in this period. In Copenhagen, Niels Bjerrum made it one of the central pursuits of his Physical Chemistry Institute. In the United States, on the other hand, molecular spectroscopy became a hallmark of at least three leading physics departments, at Harvard, the University of Michigan, and the University of California, Berkeley.<sup>7</sup> While in Japan, at least one of the early centers for molecular spectroscopy research was the physical chemistry laboratory of San-Ichiro Mizushima.

The case of reaction kinetics, particularly transition state or activated complex theory, was somewhat different. The researchers who developed this theory did remain almost exclusively in institutes for physical chemistry. But especially early on they encountered considerable resistance from older physical chemists, who, in some cases, not only did not accept the new theory but also hindered the professional advancement of those who did. These tensions were clearly visible in the 1937 Annual Faraday Society Meeting, whose main topic was reaction kinetics,<sup>8</sup> and in a published protest by the physical chemist Henry Edward Armstrong against that appointment of Michael Polanyi, one of the founders of transition state theory, to a physical chemistry post in Manchester.<sup>9</sup> Again, it would be interesting to know more about

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<sup>6</sup> Kostas Gavroglu and Ana Simões, *Neither Physics nor Chemistry: A History of Quantum Chemistry* (Cambridge, MA: MIT Press, 2012).

<sup>7</sup> Assmus, Alexi, "The Molecular Tradition in Early Quantum Theory," *Historical Studies in the Physical and Biological Sciences* 22, no. 2 (1992): 209-31, and "The Americanization of Molecular Physics," *Historical Studies in the Physical and Biological Sciences* 23, no. 1 (1992): 1-34.

<sup>8</sup> See Mary Jo Nye, *Michael Polanyi and his Generation: Origins of the Social Construction of Science* (Chicago: University of Chicago Press, 2011) p. 128-130.

<sup>9</sup> Henry Edward Armstrong, "Physical Chemistry in the University of Manchester," *Nature* 132 (1933) p. 67, and Polanyi to Bragg, 14 July 1933, Folder 2.12, Michael Polanyi Papers, Regenstein Library, University of Chicago.

how chemists working with these theories were received in Japan, in particular Juro Horiuti, who worked early in his career with Polanyi both in Berlin and in Manchester.

One thing all of the methods just discussed had in common was their reliance upon quantum theory, if not quantum mechanics. This, if nothing else, distinguished them from the bulk of the physical chemistry that came before them.<sup>10</sup> In the case of quantum chemistry, this connection is clear. In the case of the various instrumental methods, the relationship to quantum theory might appear more distant, but as Carsten Reinhardt has pointed out “the use of [new] methods in scientific experiments was closely bound to *accepting* the theoretical underpinning of the apparatus,”<sup>11</sup> and beneath all of the instrumental methods classified as chemical physics lay quantum theory.

Moreover, for a new generation of chemists (and physicists) interested in developing theories covering the whole of chemistry, the quantum mechanics became not only a tool, but also a grail after which to quest. Dudley Herschbach, who received his Ph.D. in Chemical Physics and later shared the 1986 Nobel Prize in Chemistry with Yuan Lee and John Polanyi, sketched a rather entertaining cartoon depicting his own, historicized version of this quest.<sup>12</sup> Representing the progress of field as a mountain being ascended, Herschbach placed at its base “thermochemistry.” From this base the field developed (ascended) toward theories of “structure,” a term that, in light of the names given in the sketch (Lewis and Pauling), clearly includes quantum chemistry, and then onward to “dynamics,” a term that, for Herschbach, includes transition state theory (Eyring and Polanyi) as well as his own later work on chemical kinetics. The figure that gives the cartoon meaning, though, is an angel holding a psi at the peak of the mountain, representing the Schrödinger equation, and symbolizing the eventual goal of tying all of these theories directly to quantum mechanics. This in spite of the fact that it was (and is) unclear how (or if) this linkage can be achieved in many cases.

As already mentioned, most of the new methods discussed originated in Germany. This is certainly true of x-ray crystallography, gaseous electron diffraction, and quantum chemistry, and arguably true of the chemical application of molecular spectroscopy; though, one might also claim it originated in Denmark. It is also true for transition state theory, which, though initially developed by the American Henry Eyring and the Hungarian Michael Polanyi, took shape while the two were working at Fritz Haber’s institute in Berlin. By 1941, however, one would be hard pressed to argue that Germany dominated any of these fields. The United States, Britain, and Japan all had prominent research groups, that arguably individually, and certainly if taken together, outweighed the German influence. Hence, the redefinition of research on the borders of chemistry and physics around these methods also came to mean, rather early on, a distancing of the methods of chemical physics from their “Germanic” roots. This phenomenon was closely wedded to the dual disciplinary identity of many of these methods, as it was often in crossing national boundaries that research methods acquired new disciplinary identities. This section will close with a few examples of these two-fold border crossings. Thereafter, I will argue briefly for the significance of these methods to the development of postwar chemistry, especially chemical models and theories. Then I will touch upon *some* of the reasons why those developing these methods in Germany might have seen progress there as lagging behind advances in the United States and in other European nations beginning in the mid-1930s.

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<sup>10</sup> One exception to this generalization is clearly Walther Nernst’s work in heat theory, in particular his development of the Third Law of Thermodynamics.

<sup>11</sup> Reinhardt, *Shifting and Rearranging*, p. 11.

<sup>12</sup> Dudley Herschbach, “Chemical Reaction Dynamics and Electronic Structure” in Ahmed Zewail, ed., *The Chemical Bond: Structure and Dynamics* (San Diego: Academic Press Inc., 1992), p. 175.

Quantum chemistry has become the classic, if not the only, well-researched example of how these new methods crossed oceans and changed disciplines. Several physicists, primarily but not exclusively German, worked on solutions to the Schrödinger equation for simple molecules during late 1926 and 1927.<sup>13</sup> Historians generally credit the first complete and practicable solution to the problem to Walter Heitler and Fritz London, but many also argue that it would be remiss to overlook the earlier papers of Friedrich Hund. All three were physicists, working in physics departments and were trained in German universities. When quantum chemistry came to the United States in the 1930s, however, largely through the efforts of Linus Pauling and Robert Mulliken, it found a home in chemistry, or perhaps, more precisely, on the borders of chemistry. Pauling and Mulliken both had difficulties publishing in the *Journal of the American Chemical Society* due to the technical nature of their articles; neither one published in the *Journal of Physical Chemistry*, and both would become contributors to the *Journal of Chemical Physics* after its launch in 1933.

In other cases, like transition state theory, it is quite clear that the inclusion of quantum theory and the move toward chemical physics, though developed in the German context, relied from the outset upon the work of foreign researchers. Neither the Hungarian Michael Polanyi nor the American Henry Eyring stayed long in Berlin after they completed their collaboration on chemical kinetics. In 1933, Polanyi would move to Manchester, England; while, Eyring returned to the USA and a post at Princeton University in 1931. Hence, in a sense, the rapid dissemination of transition state theory was built into the very conditions of its first formulation.

A different, less corporeal form of international exchange can be seen in the case of gaseous electron diffraction. The important step in this case was not the discovery of the phenomenon of electron diffraction itself, which occurred independently and near simultaneously in the USA and Great Britain. Rather, the key to developing a technique relevant to chemistry was the use of this phenomenon to create gaseous diffraction patterns. Herman Mark and his assistant Raimond Wierl were the first to manage this technical feat, in a BASF laboratory in Ludwigshafen.

For this technique to make it from Germany to the United States, however, researchers had to transport not only theoretical knowledge, but also apparatus designs and operation procedures. The technique was first established in the USA at Caltech, under the guidance of Linus Pauling, whom Mark had personally given plans for a gaseous diffraction apparatus in 1931. But it was Pauling's student, Lawrence Brockway, who actually built a copy of the apparatus with the help of the Institute mechanic.<sup>14</sup> I know less about how these instruments and their associated research methods came to Japan, but I would feel very negligent if I did not mention the work begun by Yonezo Morino, who built a gaseous electron diffraction apparatus with the help of Shigeto Yamaguchi. However, Morino was not the first Japanese scientist to publish on gaseous electron diffraction. As far as I know, that distinction belongs to Hazime Oosaka, but there are very few sources in English on Oosaka and how he managed to replicate this technique.<sup>15</sup>

I have granted the example of gaseous electron diffraction a bit of extra attention because, along with x-ray diffraction, it was one of the key experimental techniques used to determine the parameters of space-filling models (specifically covalent radii). As I have discussed

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<sup>13</sup> See Jeremiah James and Christain Joas, "Subsequent and Subsidiary? Rethinking the Role of Applications in Establishing Quantum Mechanics," *Historical Studies in the Natural Sciences*, forthcoming (2015).

<sup>14</sup> Jeremiah James, "Naturalizing the Chemical Bond: Discipline and Creativity in the Pauling Program, 1927-1942" (Harvard University, 2007), p. 263-74.

<sup>15</sup> See the contribution by Mari Yamaguchi in these proceedings.

elsewhere, these models and the ideas about the interactions between atoms within molecules which they embodied, were among the most visible ways in which methods from chemical physics affected the broader chemistry community.<sup>16</sup> Chemists built into these models not only the “raw” instrumental results of new chemical physics techniques but also their “theoretical underpinnings,” to use Reinhardt’s terminology.

Furthermore, the adoption of these generally implicit theoretical underpinnings could and did contribute to the development of later, explicit chemical theories. In the case of electron diffraction, there are clear ties between the method and the spatial parameters it defined, viz. inter-nuclear separations within molecules, and the postwar development of conformational analysis. Of the two researchers who shared the 1969 Nobel Prize in Chemistry for the development of the theory, Odd Hassel, the experimentalist, if you will, was an electron diffraction specialist, and Derek Barton, the theorist, developed his own personal set of precision chemical models based on results from electron diffraction to aid in his calculations.<sup>17</sup> In the years since the work of Hassel and Barton, conformational analysis has developed into an almost indispensable aspect of organic chemistry. Hence, though electron diffraction did not become a commonplace method for chemists, changing the material and social conditions of their laboratories in the way that instrumental methods such as NMR or mass spectroscopy did, its decisive role in the development of new models and theories did have an epistemological effect similar to that of these standard-bearers of the “instrumental revolution.” An analogous argument could be made for x-ray diffraction. Whereas molecular spectroscopy, which *is* one of the standard methods discussed in histories of the instrumental revolution, clearly had roots in the interwar chemical physics community. This epistemological contribution is the first reason that we should take into account interwar chemical physics when discussing the postwar development of the chemical sciences.

The second reason relates to the ‘gap’ that some chemists and historians have purported in the development of physical and theoretical chemistry in Germany from the late 1930s through the 1950s. According to the computational chemist Sigrid Peyerimhoff,

in the first decades after the war, the primary users of computers were quantum chemists...this outcome was logical *considering all the work that was stopped in the early 1930s and which was reanimated after the war making use of the new computational tools.*<sup>18</sup>

The most common explanation given for the relatively slow development in Germany is that chemical physics could not find a stable disciplinary home in the existing academic system, in large part due to the shortcomings or resistance of mainstream chemists. As Kostas Gavroglu and Ana Simões summarized the situation:

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<sup>16</sup> Jeremiah James, "Modeling the Scale of Atoms and Bonds: The Origins of Space-filling Parameters," in *Objects of Chemical Inquiry*, ed. Carsten Reinhardt and Ursula Klein (Sagamore Beach: Science History Publications, 2014).

<sup>17</sup> James, "Modeling Scale," p. 310-11.

<sup>18</sup> Sigrid D. Peyerimhoff, "The Development of Computational Chemistry in Germany," *Reviews in Computational Chemistry* 18 (2002): 257-292, quote on 259, emphasis added.

In Germany, there was a sharp division between the chemical and the physical communities, which hardly if ever communicated. And German chemists were in general ill prepared to cope with the challenges of quantum mechanics.<sup>19</sup>

This argument was put in perhaps its most pointed form by Hermann Hartmann, who took part in the ostensible renaissance in chemical physics in Germany in the 1950s, and the British quantum chemist H. Christopher Longuet-Higgins. In their biographical memoir of Erich Hückel, they presented the limited professional success of the, admittedly star-crossed, quantum chemist as the epitome of chemists' resistance to the new field. (In spite of the mentorship of Peter Debye and extensive publications, Hückel first achieved the rank of extraordinary Professor at Marburg in 1937 at 40 years of age and it was another decade until he was promoted to a newly established Chair for Theoretical Physics.) According to Hartmann and Longuet-Higgins:<sup>20</sup>

physicists in [Germany] in any case were not ready to accept investigations about more complicated chemical bond phenomena as a typical contribution of a physicist. Still more difficult was his (Hückel's) relationship to the chemists. Before World War II, especially in the Anglo-Saxon countries, chemical physics and within that field quantum chemistry also was accepted by both physicists and chemists as an interesting new field of science. Chemists in Germany, on the other hand maintained that chemistry is what chemists do. They did not do quantum chemistry and therefore this kind of science did not belong to chemistry.<sup>21</sup>

Implicit in this explanation is that what chemists "did" in Germany was primarily organic chemistry, particularly organic synthesis, which had strong ties to the German chemical industry.

Organic chemistry did predominate in Germany, and the field did have strong ties to the German chemical industry. Moreover, the chemistry community in the United States was more strongly oriented toward physical chemistry. *However*, the problem with using these factors to explain the dearth of chemical physics in Germany is that many German *organic* chemists were willing to grant support to new research methods on the borders of chemistry and physics; while, not all American and British *physical* chemists were so charitable, as illustrated earlier by the examples of Wilder Bancroft, editor of *The Journal of Physical Chemistry* and Michael Polanyi's detractor, Henry Edward Armstrong.

In fact prominent German chemists, including not only the physical chemists Walther Nernst and Wilhelm Ostwald but also the organic chemist Emil Fischer were already concerned in 1905 that Germany was falling behind in general and physical chemistry. This concern was one of the central motivators for the founding of the Kaiser Wilhelm Society (now the Max Planck Society).<sup>22</sup> Furthermore, it was the key reason that the first two Institutes of the

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<sup>19</sup> Gavroglu and Simoes, *Neither Physics nor Chemistry*, p. 86.

<sup>20</sup> More nuanced forms of this argument can be found in Andreas Karachalios, "Die Entstehung und Entwicklung der Quantenchemie in Deutschland," *Mitteilungen, Gesellschaft Deutscher Chemiker, Fachgruppe Geschichte der Chemie* 13 (1997) and *Erich Hückel (1896-1980): From Physics to Quantum Chemistry*, Boston Studies in the Philosophy of Science (Dordrecht: Springer, 2010); and in Gavroglu and Simões, *Neither Physics Nor Chemistry*.

<sup>21</sup> Hermann Hartmann and H. Christopher Longuet-Higgins, "Erich Hückel," *Biographical Memoirs of the Friends of the Royal Society* 28 (1982), p. 153.

<sup>22</sup> See Jeffrey A. Johnson, *The Kaiser's Chemists: Science and Modernization in Imperial Germany* (Chapel Hill:

society were the Institute for Chemistry, led by the physical chemist Ernst Beckmann, and a separate Institute for Physical Chemistry and Electrochemistry, led by Fritz Haber.<sup>23</sup> The structure of the Society would later be a model for RIKEN in Japan, and Fritz Haber's Institute would help guide Setsuro Tamaru, who had worked with Haber in Berlin, in designing the plans for Building No. 1 of RIKEN. Meanwhile in Germany, Haber's institute became one of the central hubs for researchers interested in chemical physics between 1911 and 1933. As already mentioned, Michael Polanyi and Henry Eyring wrote their first paper on transition states there. In addition, Otto Sackur worked there on integrating quantum theory with the thermodynamics of gases, and Paul Harteck and Karl Friedrich Bonhoeffer managed to explain the troubling specific heat of the hydrogen molecule based on the quantum mechanical concept of nuclear spin.

Of course, even in the days before big science, one institute supporting so many new lines of research was unlikely to suffice. But there were clearly other institutes in Germany supporting research into quantum chemistry, into the integration of quantum theory and thermodynamics, and into novel diffraction, spectroscopy, and photochemistry techniques. Although, all together, they numbered maybe a dozen and most were relatively small by comparison to, for example, the main chemistry laboratory at Berlin University,<sup>24</sup> this was not peculiar to Germany. In the United States during the 1920s and early 1930s, research in chemical physics was concentrated in just a few centers such as the California Institute of Technology, the University of Chicago, and the University of California, Berkeley.

Hence, the landscape for chemical physics in Germany, up to the early 1930s, did not look that much friendlier and better populated than the landscape in Great Britain or the United States. The fledgling field found its home in a handful of select research institutes, often highly distinguished but still a small minority. Arguably, the field was only even able to sustain a critical mass of research and researchers thanks to the international exchanges that defined its geographical distribution, if not its deeper character.

However, this thin, internationally distributed network also made chemical physics particularly susceptible to changes in university and science funding policies. In the United States, in 1941, war mobilization stripped Linus Pauling of so much of his staff that he was forced to write his sponsors at the Rockefeller Foundation to explain that he could not continue research in quantum chemistry and molecular structure.<sup>25</sup> In recent years, historians of science have justifiably questioned the notion that totalitarian regimes in general and the German National Socialist regime in particular were inherently detrimental to science,<sup>26</sup> but chemical physics, due to its size and distribution, was a field particularly sensitive to even small interventions. Moreover, given the *timing* of the initial decline in publications in quantum chemistry and related fields in Germany, it is worth considering the role of the 1933 "Law for the Reform of the Career Civil Service," in the initial onset of the purported German

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University of North Carolina Press, 1990).

<sup>23</sup> For later discussion of the relationship between physical or general chemistry (often used interchangeably) and organic chemistry, particularly in educational reform and debates, see Jeffrey A. Johnson, "The Case of the Missing German Quantum Chemists: On Molecular Models, Mobilization, and the Paradoxes of Modernizing Chemistry in Nazi Germany," *Historical Studies in the Natural Sciences* 43, no. 4 (2013): 391-452.

<sup>24</sup> Carsten Reinhardt, "Zentrale einer Wissenschaft. Methoden, Hierarchie und die Organisation der chemischen Institute," in *Geschichte der Univesität Unter den Linden 1810-2010, Bd. 5*, ed. Rüdiger vom Bruch and Heinz-Elmar Tenorth (Berlin: Akademie Verlag): 575-603.

<sup>25</sup> James, "Naturalizing the Chemical Bond," p. 105-06.

<sup>26</sup> See, among others, Margit Szöllösi-Janze, "National Socialism and the Sciences: Reflections, Conclusions and Historical Perspectives," in *Science in the Third Reich*, ed. Margit Szöllösi-Janze (Oxford: Berg, 2001), 1-35.

lag.<sup>27</sup> The law barring Jews from posts at universities and government research centers, like the Kaiser Wilhelm Institutes, led to the resignation of Fritz Haber and to the removal or resignation of much of his staff, including Michael Polanyi.<sup>28</sup> Other prominent researchers in chemical physics who departed Germany in response included Fritz London, James Franck, Ladislau Farkas, Karl Weissenberg, and Max Born. In addition, Max Bodenstein, Chair for Physical Chemistry at Berlin University, who had hosted Massao Katayama when he visited Germany, was deemed too “friendly” to his Jewish colleagues and upon his retirement in 1936 found himself unwillingly divested of many of his academic obligations.<sup>29</sup> In such a small and widely distributed field, these losses were not easily replaced, and unlike losses of manpower due to military mobilization, they were not easily reversed after 1945. This was due in part, as Jeffrey Johnson has explained, to the structure of German chemical education during the 1930s and 1940s, which did not offer the same grounding in modern physics as advanced chemical education in the United States, for example. However, the difficult material and political conditions for researchers in the immediate postwar should also be taken into consideration.

Once again, my comparison with the Japanese case will be sadly limited, due to a dearth of English-language sources. I would assume that the situation in Japan was quite different from that in Germany *or* in the USA in that there does not appear to be the same reduction in publications relating to chemical physics during the late 1930s and early 1940s. Yonezo Morino published several papers on gaseous electron diffraction during this period, and Masao Kotani, working in quantum chemistry, published important tables of molecular integrals. However, how this was possible and what other duties these men might have had in relation to, for example, military research, are questions I cannot yet answer.

After that caveat, let me conclude with a brief summary of the key features of the rise of chemical physics, at least as it occurred across Germany, Great Britain, and the USA. First, chemical physics was a relatively small-scale, widely internationally distributed, and heterogeneous endeavor before World War II, but one that was clearly different from “classical” physical chemistry, if in no other aspect, than its relationship to atomistic and quantum theory. Second, the methods that constituted the field and their theoretical underpinnings maintained if not increased their importance in the postwar period, in part through their contributions to the so-called instrumental revolution. Finally, the purportedly slow development of the methods of chemical physics in Germany from the mid-1930s through mid-1950s, likely began with the loss of a small number of key research centers and research group leaders in the 1930s that were difficult to replace at the time given international relations and the thin, internationally-distributed research network that supported the field, a situation then prolonged by the path of chemical education reform in Germany. It would be easy, however, to overemphasize this last point, as Germany’s “catching up” in the postwar period, though portrayed by some of the actors involved as painfully slow and prolonged, does not appear so far out of step with the postwar development of other scientific fields.<sup>30</sup>

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<sup>27</sup> For a discussion of more enduring factors, with a quite different relation to the National Socialist Regime see Johnson, “The Case of the Missing Quantum Chemists.”

<sup>28</sup> Jeremiah James et al., *One Hundred Years at the Intersection of Chemistry and Physics: The Fritz Haber Institute of the Max Planck Society, 1911-2011* (Berlin: De Gruyter, 2011), ch. 2.

<sup>29</sup> Hans-Georg Bartel, “Aus der Geschichte der physikalischen Chemie and der Friedrich-Wilhelms-Universität zu Berlin. Zum 200. Gründungsjubiläum der Alma Mater Berolinensis, Friedrich-Wilhelms-bzw. Humboldt-Universität zu Berlin,” *Bunsenmagazin* 12, no. 3 (2010): 105-10.

<sup>30</sup> See, for example, Dieter Hoffman ed., *Physik im Nachkriegsdeutschland* (Frankfurt am Main: WV Harrie Deutsch GmbH, 2003).