Migration and the Generation of New Scientific Ideas

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THE GENERATION of new scientific knowledge often occurs through the coming together, re-working and re-formulation of previously distinct pieces of older knowledge and technique into a new scientific synthesis. This then becomes the basis for further work in what may subsequently develop into a new scientific specialty. Awareness of other pieces of advanced scientific knowledge typically arises either through the direct contact of an individual scientist with other scientists, or through the reading of the relevant literature. The temporary or permanent migration of scientists or technologists from one scientific institution within the same society to another, or from one society to another, is an important condition of such contacts.

International migration is one of the most fruitful forms of the movement of scientists between institutions. Another is the migration from one institution to another within the same society. Such mobility of scientists and technologists is important in the transfer of skills and technological knowledge between societies, between one branch of industry and another, and between universities and industry. The diffusion of scientific information through journals is certainly one of the most prominent ways in which pieces of older knowledge are brought into juxtaposition with each other. But it can accomplish this only for those bits of knowledge or technique which are translated into written form. Michael Polanyi has emphasised the central role of the tacit component of scientific knowledge, particularly at the frontier of research where codified formulations have not been achieved.[1] Polanyi stressed the effect of personal, face-to-face relationships in the transmission of scientific knowledge and the scientific ethos from one generation to the next. Such relationships are no less important in the intellectual interactions of scientists of the same generation. Institutional migration, and the new personal scientific interactions they make possible, foster the transmission of advanced scientific knowledge. Indeed such knowledge is sometimes transformed—not just transmitted—when its carriers move from one social and institutional situation to another. Major scientific syntheses, and the consequent formation of new specialties, have thus been fostered primarily—not simply by the "transfer of information" or "displacement of concepts" across intellectual or territorial boundaries—but by the

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[1] Polanyi, Michael, *Personal Knowledge: Towards a Post-Critical Philosophy* (London: Routledge, 1958), pp. 69ff; Polanyi, M., *The Tacit Dimension* (London: Routledge, 1967); Ziman, John M., *Ideas Move Around Inside People* (London: Birkbeck College and J. W. Ruddock, 1974).

movement between institutions of individuals with the requisite knowledge and skills, imagination and intellectual force to adapt and transform these in a new scientific, social and institutional situation. This has often led to the crystallisation of new intellectual specialties.

The Transformation of Physics

The period from 1930 to 1960 was marked by the re-ordering of physics around the poles of physics of solids and a physics of the atomic nucleus; with the concomitant development of yet a third major field which was indispensible to the other two, namely, theoretical physics. The older disciplines of biology and chemistry were in part eclipsed in this period by the rapid expansion of biochemistry and the birth of the new "molecular" biology. In the United States there was a rapid efflorescence of the previously uncommon interdisciplinary specialty of applied mathematics, and of a new astrophysics closely allied to theoretical physics. The older interdisciplinary specialty of physical chemistry also made progress. All these changes occurred concomitantly with migrations—especially the immigration to Great Britain and the United States of scientists who were dismissed from their posts in Nazi Germany beginning in 1933 and in other countries occupied by the Germans later in the decade.

The scientific transformations were also stimulated, to some extent, by the migrations of scientists between academic institutions within the United States, such as the temporary movement of academic scientists into large-scale industrial laboratories after the First World War, and radar and atomic weapons projects during the Second World War. Both kinds of institutional migration—intranational and international—produced opportunities for new scientific syntheses. These occasionally resulted in the far-reaching modification of disciplines and sub-fields, as well as the birth, or spread to new countries, of new interdisciplinary specialties.

New Institutions, New Colleagues, New Fields

Solid state physics grew up in this period at the juncture of physics with chemistry, crystallography and metallurgy. By 1960, by virtue of its applications to solid state electronics and materials engineering, it had become quantitatively and technologically the most important sub-field of physics. It provides the scientific knowledge underlying the transistor. Many of the founders of this sub-field had emigrated especially in the decade following 1933 from Central Europe to institutions in Great Britain and the United States. In the United Kingdom, the main international migrants who participated in its consolidation included Hans Bethe, Max Born, Peter Paul Ewald, Herbert Fröhlich, Dennis Gabor, Fritz and Heinz London, Kurt Mendelssohn, Egon Orowan, Rudolf Peierls, Franz Simon and E. P. Wohlfarth. In the United States: Bethe, Felix Bloch, Leon Brillouin, Arthur von Hippel, Karl Lark-Horowitz, Roman Smoluchowski, Lazslo Tisza and

Eugene Wigner. Some like Bloch and Brillouin had done their best work in this field while still in continental Europe. Brillouin's migration to the United States from France helped to transmit or diffuse European theory. Others created important new syntheses following their migration. For example, working at Princeton in 1933 with his research student Frederick Seitz, Wigner devised an important new technique—the Wigner-Seitz cellular method—that laid the basis for a new specialty oriented towards the theoretical calculation of the electronic properties of real materials. Wigner's migration—and his subsequent synthesis with Seitz—helped transform European theory. Wigner also did important work with another of his research students, John Bardeen, on the doubly charged layer at the surface of a conductor—which became later of considerable importance to Bardeen's work on the transistor after the Second World War. Indeed Wigner's research students—Seitz, Bardeen and Conyers Herring—became, with John C. Slater, the founders of the quantum theory of solids in the United States.[2]

The number and stature of continental emigrants among the founders of nuclear physics in the United States and Great Britain were at least equally impressive.[3] They made up a large fraction of the first generation of nuclear theorists in the United States; they included such persons as Bethe, Bloch, Enrico Fermi, George Gamow, Maria Mayer, Lothar Nordheim, Leo Szilard, Edward Teller, Victor Weisskopf and Wigner. Fermi and his collaborators had already done very important work in this field in Italy. Their migration to America resulted in a significant diffusion of their previous scientific achievements. Bethe, however, had been relatively inactive in the subject in Germany, where he was principally known for his work with Arnold Sommerfeld in Munich on the electron theory of metals, which, in its fully quantum mechanical formulation, provided the basis for a theory of the solid state. He began to do serious work in nuclear physics only after his emigration to Great Britain, when he worked at the University of Manchester in the academic year 1933–34. There, under the stimulus of another refugee, Rudolf Peierls, Bethe began work on a theory of the splitting of the deuterium nucleus by gamma rays.[4] They had first become acquainted with this problem in the course of a visit to Cambridge, where James Chadwick and a refugee physicist, Maurice Goldhaber, were

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[2] Hoch, Paul K., "The Development of the Band Theory of Solids, 1933–1960", in Braun, Ernst *et al.* (eds), *The History of Solid State Physics* (Oxford: Oxford University Press, in press).

[3] For details see Weiner, Charles, "A New Site for the Seminar: the Refugees and American Physics in the Thirties", in Fleming, Donald and Bailyn, Bernard (eds), *The Intellectual Migration: Europe and America, 1930–1960* (Cambridge, Mass.: Harvard/Belknap Press, 1969), pp. 190–234; Stuewer, Roger H., "Nuclear Physicists in a New World: The Émigrés of the 1930s in America", *Berichte zur Wissenschaftsgeschichte*, VII (January 1984), pp. 23–40.

[4] American Institute of Physics Center for History of Physics (hereafter AIP), interview with Hans A. Bethe by Jagdish Mehra and Charles Weiner (27–28 October, 1966), p. 7, available at the Niels Bohr Library, American Institute of Physics.

performing this experiment.[5] Peierls was to become much more active in nuclear physics after 1936 when he was appointed to a chair of applied mathematics at the University of Birmingham, and worked alongside the professor of physics, Marcus Oliphant, who was himself a recent migrant from Ernest Rutherford's Cavendish Laboratory in Cambridge. Peierls was further stimulated by the arrival two years later of another refugee, Otto Frisch, coming with the news of the first experiments in atomic fission. This led to the detailed calculations by Peierls and Frisch of the possibilities of a nuclear chain reaction, and their memorandum which catalysed the British atomic bomb project.

After a brief period at the University of Bristol in the autumn of 1934, Bethe moved to America where his activity in nuclear physics was further encouraged:

> At Cornell he worked closely with M. Stanley Livingston, who had taken his doctorate with Ernest O. Lawrence and who had played a major role in building the first cyclotron at Berkeley in the early 1930s . . . One consequence of this interaction for Bethe was the project he launched with the assistance of Livingston and Robert F. Bacher, his colleagues in the physics department at Cornell, to write a comprehensive three-part series of review articles on nuclear physics, including both the theoretical and experimental aspects in an integrated form . . . [which was to be] a tremendous stimulus to the development of nuclear physics . . . known among physicists as the "Bethe bible" and [which] more than any other single publication marked the coming of age of that field.[6]

This is clearly an example of how migration can occasionally result in an important new synthesis: in this case, it was built around the combination of Bethe's comprehensive knowledge and skill in theoretical physics, and Livingston's previous experience and skill as an experimentalist. Bacher, who also shared in the creation of the synthesis, was himself a recent migrant from the University of Michigan, where he had taken a doctorate with the expatriate Dutch theorical physicist Samuel Goudsmit.[7]

Eugene Wigner was another expatriate of this period whose migration was followed by important new syntheses in nuclear physics. In Europe he had been known mostly for his book on the physical applications of group theory to the quantum mechanics of the 1920s. As a visiting professor at Princeton in the early 1930s, he had some close contacts with some of the experimental nuclear physics work—then being led by yet another European expatriate, Rudolf Ladenberg—and he also collaborated with a visiting theorical physicist from the University of Wisconsin, Gregory Breit, in this field. Subsequently, having been refused permanent tenure at Princeton, Wigner himself spent the year of 1937–38 at the University of Wisconsin. It was at this time that he further consolidated his fundamental

[5] Bernstein, Jeremy, *Hans Bethe: Prophet of Energy* (New York: Basic Books, 1979), p. 42.

[6] Weiner, Charles, "A New Site . . .", *op. cit.*, p. 224; AIP interview with H. A. Bethe by Charles Weiner, Session II (17 November 1967), pp. 137–138; Bernstein, J., *op. cit.*, pp. 43–45.

[7] Goudsmit, Samuel, "It Might as Well be Spin", *Physics Today* XXIX (June 1976), p. 42.

contribution to nuclear theory in collaboration with Breit. A similar case of migrations followed by a new synthesis was the collaboration of the refugees George Gamow and Edward Teller at George Washington University in the same period; this resulted in the Gamow-Teller selection rules for nuclear transformations. Special mention should also be made of the syntheses of Anglo-American and émigré German expertise in nuclear physics and that of the Italian school of Enrico Fermi. This took place in the institutional setting of the Manhattan project.

The initial entry of the Fermi group into nuclear physics in Italy in the early 1930s was partly propelled by a series of temporary postdoctoral migrations of its members to the main centres, chiefly European, of experimental nuclear physics:

> [Franco] Rasetti had earlier [in the 1920s] gone to Millikan's laboratory in Pasadena to work on the Raman-effect. [Emilio] Segrè had gone to visit Zeeman in Amsterdam to work on the Zeeman-effect of quadrupole radiation . . . The second phase of "expeditions" started in 1931 with Rasetti going to Lise Meitner's laboratory at Berlin-Dahlem and learning how to make a cloud chamber, prepare polonium samples and neutron sources, and make counters. Segrè went to Hamburg to work with Otto Stern [on particle beams], and Amaldi to Debye's laboratory in Leipzig.[8]

The purpose of migrations was to "learn a new experimental technique, and bring them all back . . . with an eye to enlarging our fields."[9]

By 1934, the synthesis of the skills and knowledge brought back from these temporary migrations with Fermi's widely ranging abilities led to the discovery by the group in Rome of artificial radioactivity in fluorine and aluminium. With the enactment of the racial laws in Italy, after 1938 these skills and knowledge were re-transferred to America—with the most fertile centres of nuclear physics growing up after the war in Fermi's Institute of Nuclear Studies at the University of Chicago and in Segrè's work in the circle around E. O. Lawrence at the University of California.

From Mathematical to Theoretical Physics

Theoretical physics before the 1920s was a specialty for the most part only in Germany and some other places in continental Europe. In Great Britain it existed almost exclusively as either "mathematical physics" or occasionally as an "applied mathematics", confined, with rare exceptions, to faculties or departments of mathematics. Only occasionally did it have any active relationship to fundamental experimental practice. It is true that Cambridge in 1933 had certainly had a considerable mathematical element to its physics;

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[8] Holton, Gerald, "Striking Gold in Science: Fermi's Group and the Recapture of Italy's Place in Physics", *Minerva*, XII (April 1974), pp. 169–170.

[9] Archive for History of Quantum Physics interview with Emilio Segrè by Thomas Kuhn, 18 May 1964, p. 18, available at American Institute of Physics, New York, or the Science Museum library, London.

its tradition went back at least to Clerk-Maxwell, and embraced the work of Kelvin, Rayleigh and J. J. Thompson, among others. However, it was still assumed even within this tradition that physicists did their own experiments and that those not doing experiments were not physicists but belonged properly in the faculty of mathematics. This was the way in which the Stokes lecturer in mathematical physics, Ralph H. Fowler, and his most promising former students, Nevill Mott and Alan H. Wilson were viewed in the early 1930s. All of them were attached to the mathematical faculty. The same was true of another of Fowler's former students, P. A. M. Dirac. Even after the Second World War, when another former student of Fowler's, Douglas Hartree, was appointed to a chair, he too was affiliated to the mathematical faculty. Fowler himself, who happened also to be Rutherford's son-in-law, did however have a room in the Cavendish Laboratory, setting a precedent in this respect for Mott. In the years immediately after 1933 a great number of internationally migrant theoretical physicists obtained temporary accommodation at Cambridge; these included Bethe, Born, Ewald, Peierls and Weisskopf, among many others. None of these was able to obtain a post on a permanent appointment and all went elsewhere. Born and Peierls took up chairs of applied mathematics at the sub-centres at Edinburgh and Birmingham; Ewald had to go to Belfast for a lectureship; and Bethe, Weisskopf and others eventually moved on to the United States. By the 1940s the situation had changed sufficiently for Egon Orowan and Nicholas Kemmer to obtain longer term Cambridge appointments, and after the war for Otto Frisch—a nuclear experimentalist—to be appointed to a professorship.

At Oxford the situation was similar. Mathematical physics under E. A. Milne and A. E. H. Love was kept quite separate from the "experimental philosophy" of the Clarendon Laboratory right into the 1930s. Indeed physics was probably the least developed of the natural sciences—at least compared to chemistry, medicine and physiology—and it had a foothold only in a few Oxford colleges. Starting in 1933, the Dr Lees professor of experimental philosophy, Frederick Lindemann, was able—with financial support from Imperial Chemical Industries—to bring into the Clarendon Laboratory a number of refugee physicists including at least one theorist, Fritz London.[10] But even then the general pattern of Oxford and British physics reasserted itself quickly: many of the refugee experimentalists at Oxford—including Simon and Mendelssohn—eventually prospered, while the few theorists all went elsewhere within two years time. The latter group also included Leo Szilard, who was in fact brought into the laboratory as a nuclear experimentalist rather than a theorist[11]—as well as the London brothers. The most prominent of the Oxford refugee theorists, Erwin Schroedinger, never held any post at the Clarendon Laboratory and he left

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[10] File D. 96, Cherwell Papers, Nuffield College, Oxford.

[11] Szilard, Leo, "Reminiscences", in Fleming, D. and Bailyn, B. (eds), *op. cit.*, pp. 104–105.

Oxford after the first two years of his five year college fellowship.[12] Having come quite recently from Berlin, Schroedinger was astonished by the relative indifference to theoretical questions that he found at Oxford.[13] Moreover, both Oxford professors of physics, Lindemann and J. S. E. Townsend, were sceptical about the new quantum mechanics—especially wave mechanics—and they were hardly more favourable to the older mathematical physics done by Milne.[14] Thus, the growth of a theoretical physics in Great Britain—even after the arrival of the migrants from Central Europe—was by no means as smooth a process as the metaphor of the "branching" of old into new specialties would lead one to imagine.

In the United States the spread of "mathematical physics" from departments of mathematics to departments of physics beginning in the 1920s is associated with a considerable series of migrations to and from the continent of Europe. Under a special programme financed by the International Education Board of the Rockefeller Foundation, and a similar programme financed by the Guggenheim Foundation, aspiring specialists in theoretical physics and other fundamental fields of physics were encouraged to spend a postdoctoral year in the main institutional centres, predominantly in Berlin, Göttingen, Munich, Leipzig, Copenhagen and Zurich. Concomitantly, there were a significant number of appointments of European theoretical physicists to the teaching staffs of American universities on either a temporary or permanent basis, including Goudsmit in 1927 and Wigner in 1930.

The main influx of European theorists began in 1933 as a result of the dismissals by the Nazis. Among international migrant theorists emigrating to Great Britain and the United States in the next decade were Bethe, Bloch, Born, Brillouin, Gamow, Walter Heitler, Fritz London, Nordheim, Peierls, Schroedinger, Teller and Weisskopf.[15] The School of Theoretical Physics at the Dublin Institute for Advanced Study—which like its older counterpart in Princeton was a major centre for the new sub-field—was founded in 1939 by Schroedinger, who was eventually succeeded as director by Heitler. The best known theorist at the institute at Princeton was, from 1933, Einstein. He was joined there by Hermann Weyl, Leopold Infeld, Wolfgang Pauli and, during the Second World War, by Niels Bohr. The

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[12] Hoch, Paul K. and Yoxen, Edward, "Schroedinger in Transition between Britain and Ireland", presented to the International Congress of the History of Science, University of California at Berkeley, August 1985.

[13] Popper, Karl R., *Unended Quest* (London: Fontana/Collins, 1976), p. 108.

[14] Lindemann, Frederick A., *The Physical Significance of the Quantum Theory* (Oxford: Clarendon Press, 1932) pp. vi, 126, 143; Harrod, Roy, *The Prof: A Personal Memoir of Lord Cherwell* (London: Macmillan, 1959), p. 66; Dr. A. J. Croft to the author (4 February, 1985).

[15] See Rider, Robin E., "Alarm and Opportunity: Emigration of Mathematicians and Physicists to Britain and the United States", *Historical Studies in the Physical Sciences*, XV (1985), pp. 107–176; Holton, Gerald, "The Formation of the American Physics Community in the 1920s and the Coming of Albert Einstein", *Minerva*, XIX (Winter 1981), pp. 569–581; Hoch, Paul K., "The Reception of Central European Refugee Physicists of the 1930s: U.S.S.R., U.K., U.S.A.", *Annals of Science*, XXXX (Spring 1983), pp. 217–246.

German émigrés obviously played a leading role in the implantation of theoretical physics in Great Britain and the United States. The growth occurred most spectacularly after the Second World War, following the successful synthesis of Central European theoretical physics with advanced Anglo-American experimental practice—particularly of the schools of Rutherford, E. O. Lawrence and A. H. Compton—which occurred in the Manhattan project. The same synthesis led, in due course, to the new particle physics.

Along with theoretical physics, the other major interdisciplinary sub-field between mathematics and physics which grew up in the United States in this period was applied mathematics. This interdisciplinary specialty had originally been created by Felix Klein and his students at the turn of the century at Göttingen, and it was then carried on by his successors David Hilbert and Richard Courant. It quickly spread to the school of aerodynamics formed by Klein's student Ludwig Prandtl and the latter's collaborator Theodor von Karman at the Technische Hochschule in Aachen. In Great Britain, too, by the 1920s this specialty was closely associated with the aircraft industry. In 1930 von Karman was appointed to assemble a similar centre of aerodynamics at the California Institute of Technology. He also applied his mathematical techniques acquired at Göttingen to the understanding of structural stability—for example, on rail tracks and bridges; in Germany, this would have been part of the specialty of "applied mechanics". The latter was then being pioneered in the United States by yet another immigrant—this time from the Russian revolution—Stephen Prokofievitch Timoshenko. After the rise of National Socialism in Germany, Courant too emigrated to the United States, where he eventually founded what became the main American centre of applied mathematics and mathematical physics, at New York University. The other major American centre for applied mathematics—at Brown University—also had a staff after 1933 which included a majority of émigrés. At the borderland between mathematical and theoretical physics, the major figure in this period was John von Neumann. A former holder of a fellowship from the International Education Board with Hilbert, he—with another international migrant from Göttingen, Hermann Weyl—brought the techniques of applied mathematics and mathematical physics from Göttingen to the Princeton Institute for Advanced Study, where they both specialised in the mathematics of the new quantum mechanics. Von Neumann also applied his skills as a theorist to the development of a first generation computer; he might not have attempted this had he remained in Europe.[16]

[16] Reid, Constance, *Courant in Göttingen and New York* (New York: Springer-Verlag, 1976); G. Richardson to G. W. Gray (24 December) in File RF, 1.2/244D, "Brown: Applied Mathematics 1942–43", Rockefeller Foundation Archive Center, Pocantico Hills, North Tarrytown, New York; Ulam, S. *et al.*, "John von Neumann 1903–1957", in Fleming, D. and Bailyn, B. (eds), *The Intellectual Migration*, *op. cit.*, pp. 235–269; Greenberg, John L. and Goodstein, Judith R., "Theodore von Karman and Applied Mathematics in America", *Science*, CCXXII (December 1983), pp. 1300–1304; Hanle, Paul A., *Bringing Aerodynamics to America* (Cambridge, Mass.: MIT Press, 1982).

International migrants also helped to create an important new synthesis of the primarily German theoretical physics—especially general relativity and quantum mechanics—with the older American astrophysics, leading to the rapid growth and transformation of the latter field. Among émigrés and expatriates active in this process were Walter Baade, Rudolf Minkowski, Martin Schwarzchild, Rupert Wildt and Fritz Zwicky. It was the synthesis of the European and other émigrés' expertise in theoretical physics with the American skill in precise observation that contributed to this rapid progress. It may be added that the director of the Yerkes Observatory of the University of Chicago, Otto Struve, who came to America as a refugee from the Russian revolution, also played a part in the general development of American astronomy. In Great Britain the most influential émigrés in this field were the Scottish Astronomer Royal, E. F. Freundlich, at St Andrews; his eventual successor in this office, Hermann A. Brück at Edinburgh; and in the next generation, the émigré theorists of the steady-state universe at Cambridge, Herman Bondi and Thomas Gold.

At the intersection of quantum mechanics, nuclear physics and astrophysics, it was Bethe who first formulated the "carbon cycle" as the power-source of the stars. He was awarded a Nobel prize for this work. His activity in this area arose, following his immigration to Cornell and increasing participation in American nuclear physics, from his attendance at the annual conference in Washington on theoretical physics in 1938, organised by the refugees Gamow and Teller. In that year, the conference was devoted to the problem of stellar energy.[17]

Re-ordering Chemistry and Biology

Physical chemistry has existed in Great Britain and the United States ever since the second half of the nineteenth century. In the latter country, however, it was to be greatly invigorated in the 1930s by the work of such European immigrants as George Kistiakowsky, Peter Debye, Immanuel Estermann, Kasimir Fajans, James Franck, Lars Onsager and Otto Stern.[18]

[17] Brück, Hermann A., *The Story of Astronomy in Edinburgh from its Beginnings until 1975* (Edinburgh: Edinburgh University Press, 1983); Bethe, Hans A., "Energy Production in Stars", *Physical Review*, LV (January 1939), pp. 103ff, 434ff; AIP interview with Hans Bethe by J. Mehra and C. Weiner, *op. cit.*, pp. 48–49; AIP interview with George Gamow by Charles Weiner, 25 April 1968, p. 64; Bernstein, J., *op. cit.*, pp. 45ff; DeVorkin, David H. and Kenat, Ralph, "Quantum Physics and the Stars", *Journal of the History of Astronomy*, XIV (January 1983), pp. 102–132, 180–222; Osterbrock, D. E., "Rudolf Minkowski: Observational Astrophysicist", *Physics Today*, XXXVIII (April 1985), pp. 50–57; Hall, Robert D., "German Influence on American Astronomy: Migrations of Astronomers and Students", unpublished paper presented to the West Coast History of Science Society meeting at Oregon State University, Corvallis, Oregon (12 November, 1983); DeVorkin, David H., "The Maintenance of a Scientific Institution: Otto Struve, the Yerkes Observatory and Its Optical Bureau During World War II", *Minerva*, XVIII (Winter 1980), pp. 595–623.

[18] Carroll, P. Thomas, "Immigrants in American Chemistry", in Jackman, Jarrell and Borden, Carla (eds), *The Muses Flee Hitler: Cultural Transfer and Adaptation, 1930–1945* (Washington, DC: Smithsonian Institution Press, 1983), pp. 189–204.

The most outstanding American-born quantum theorist in this field was Linus Pauling. While still an undergraduate at the California Institute of Technology in 1924, he wrote a paper with Peter Debye who was then a visiting professor from the Eidgenössische Technische Hochschule in Zurich. In 1926–27, Pauling was a Guggenheim fellow at the universities of Munich and Zurich with Arnold Sommerfeld and Erwin Schroedinger, became "well acquainted with Fritz London and Walther Heitler", and—stimulated by their work on the covalent bond—intensified the research that led to his own book *The Nature of the Chemical Bond* in 1939.[19] In the United Kingdom a similar invigoration of physical chemistry took place after 1933 under the leadership of Michael Polanyi and Fritz Paneth, and a sizable group of physicists and chemists brought into the country under a special programme sponsored by Imperial Chemical Industries.[20]

Biochemistry, especially in America, also grew quite rapidly after 1933 into virtually a separate field, being no longer constrained by its previous predominantly service role to medicine and agriculture. Its most active centres were Columbia University[21] and the Rockefeller Institute for Medical Research,[22] at both of which émigrés were predominant. At Columbia, the oustanding figures were undoubtedly Rudolf Schoenheimer, Erwin Chargaff and—at least in an administrative role—the chairman of the department of biochemistry, Hans T. Clarke. Schoenheimer had come to the department from the University of Freiburg, where the professor of physics George K. von Hevesy had introduced the use of radioisotopes as biological tracers many years previously. Schoenheimer was therefore familiar with this general technique, when a particularly propitious isotope came his way through the work of the Columbia physical chemist Harold Urey, who had recently isolated deuterium. Another bridge to Urey's work was provided by one of his former students, David Rittenberg, who joined the biochemistry department in the same period and worked closely with Schoenheimer. Together they used deuterium as an isotopic marker in their important study of intermediary metabolism. Schoenheimer also used radioactive nitrogen prepared by Urey to label amino acids, and to trace their pathways through the body. Chargaff in 1935 entered the department of biochemistry of Columbia University by way of the College of Physicians and Surgeons, where he worked on a Carnegie grant with two surgeons, Frederick D. Bancroft and Margaret Stanley-Brown, who were

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[19] Pauling, Linus, "Early Work on Chemical Bonding in Relation to Solid State Physics", *Proceedings of the Royal Society of London*, Series A, CCCLXXVIII (February 1981) p. 208.

[20] File 21/2/2 1920–1939, Imperial Chemical Industries, Central Files, Millbank, London.

[21] Chargaff, Erwin, *Heraclitean Fire: Sketches from a Life Before Nature* (New York: Rockefeller University Press, 1978), pp. 65ff; Abir-Am, Pnina, "From Biochemistry to Molecular Biology: DNA and the Acculturated Journey of the Critic of Science Erwin Chargaff", *History and Philosophy of the Life Sciences*, II (January 1980), pp. 3–60; Hoch, Paul K., "Social and Intellectual Factors in the 1930s Migration of Refugee Biochemists", unpublished paper, presented to the George Sarton memorial meeting of the Society for Social Studies of Science at Ghent, Belgium, 17 November 1984.

investigating the mechanisms of blood coagulation. He was able to synthesise his experience in biochemical research, obtained in Otto Warburg's laboratory at the Kaiser-Wilhelm-Institut in Berlin-Dahlem, with their knowledge of the medical and physiological aspects of the circulation of the blood.

Hans Clarke had been born in Great Britain of American parents; he was educated in organic chemistry there and in Germany, and worked in Emil Fischer's laboratory at the University of Berlin. Upon returning to America in 1914, he worked as an organic chemist with the Eastman Kodak company for 14 years, until appointed to be head of the biochemistry department of Columbia University in 1928. Although his subsequent achievements in research were modest, he used his European connections and judgement of prospective staff to build up the most outstanding department of biochemistry in the United States. Moreover, from his years at Kodak he had managed to build up "a huge repository of often difficult accessible substances without which the great advance in organic chemistry would have been impossible".[23]

As well as Schoenheimer and Chargaff, other biochemists arriving in the United States in this period included Max Bergmann, Henrik Dam, Fritz Lipmann, David Nachmansohn, Hans Neurath, Albert Szent-Györgyi and—although mainly of symbolic importance because of his advanced age—Otto F. Meyerhof, who had obtained the Nobel prize in 1922.

In Great Britain, the centre of biochemical research was undoubtedly the laboratory of Frederick Gowland Hopkins at Cambridge, which after 1933 had a number of émigrés including Hans Krebs and Ernst Chain, the latter as a student. Krebs, on the basis of his earlier work on the biochemistry of enzymes in Warburg's laboratory in Berlin, worked in Hopkins' laboratory—together with another refugee, H. Weil-Malherbe—on "the conversion in the kidney of proline into glutamate and glutamine; and with N. L. Edson, [he] showed that hypoxanthine was involved in the biosynthesis of uric acid by birds."[24] Subsequently, at Sheffield University, where he took up a lectureship in 1935, with his student W. A. Johnson he laid the foundations for the later formulation of the "citric acid cycle" of oxidation in muscle tissue, for which in 1953 he was awarded a share of the Nobel prize. Chain in 1935 moved from Hopkins laboratory to Oxford to work with Howard

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[22] Dubos, René, *The Professor, the Institute, and DNA* (New York: Rockefeller University Press, 1976); Kohler, Robert E., *From Medical Chemistry to Biochemistry: the Making of a Biomedical Discipline* (New York: Cambridge University Press, 1982); Emergency Committee in Aid of Displaced Foreign Scholars Papers, Manuscripts Room, New York Public Library, esp. Boxes 136, 148 and 195.

[23] Chargaff, E., *Heraclitean Fire, op. cit.*, pp. 74–75, 70, and 66–67.

[24] Kornberg, Hans, "Hans Adolf Krebs", *Biographical Memoirs of the Fellows of the Royal Society*, XXX (1984), p. 365; Krebs, Hans A., *Otto Warburg (1883–1970)* (Oxford: Oxford University Press, 1981), and "The Physiological Role of Ketone Bodies", *Biochemical Journal*, LXXX (Spring 1961), pp. 225–233.

Florey, who was professor of pathology, and who "had long believed that experimental pathology would benefit from the collaboration of pathologists with chemists."[25] This indeed proved to be the case. It was the basis of their joint work on the bacteriolytic enzyme lysozyme, and of their successful analyses of the biochemical structure and medical usefulness of penicillin, for which they later shared a Nobel prize with Sir Alexander Fleming.

The chief contributor to the growth of biophysics as a field institutionally established in Great Britain was Bernard Katz, who from 1952 to 1978 was the head of its major institutional centre at University College, London. After he obtained the doctorate in medicine at Leipzig University in 1934, Katz emigrated to London where he did biophysical research between 1935 and 1939 on nerve excitation and muscle contraction with A. V. Hill. He then emigrated once more, this time to Australia, where he worked as a Carnegie research fellow in Sydney Hospital on problems of neuro-muscular transmission. With this background, he returned to London after the war to serve as assistant director of what was then the biophysical research unit at University College, and which in 1952 became a separate department. In the 1950s, important work on radiological applications to medicine was being done at St Bartholemew's Hospital medical school in London by the Polish émigré, Josef Rotblat, who had first come to Great Britain in 1939. His initial knowledge of the biological effects of radioactive materials had been acquired in Poland between 1933 and 1939, when he worked as a research fellow in the radiological laboratory of the Warszawskie Towarzystwo Naukowe (scientific society of Warsaw).

The pre-history of molecular biology was markedly influenced by the synthesis between physics and genetics originally conceived in the United States by the "phage group", which held summer schools at Cold Springs Harbor Laboratory on Long Island. The dominant figure in this group was the émigré physicist Max Delbrück, and it also included such refugees from Germany as Gunther Stent and Leo Szilard, from Italy as Ugo Fano and Salvador Luria, and the Spaniard Severo Ochoa.[26] Luria's American student James Watson was central in the development of this discipline through his formulation with Francis Crick of the double helix structure of DNA. The work of the Austrian expatriate Max Perutz, who at Cambridge became one of the pioneers of the specialty of protein crystallography,[27] and whose techniques were essential to Crick's effort, was also of crucial

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[25] Abraham, Sir Edward, "Ernst Boris Chain", *Biographical Memoirs of the Fellows of the Royal Society*, XXIX (1983), p. 45; Clark, Ronald W., *Ernst Chain* (London: Weidenfeld, 1981); Macfarlane, Gwyn, *Howard Florey* (Oxford: Oxford University Press, 1979), pp.254–259.

[26] Fleming, Donald, "Émigré Physicists and the Biological Revolution", in Fleming,D. and Bailyn, B. (eds), *The Intellectual Migration, op. cit.*, pp. 152–189.

[27] Law, John, "The Development of Specialities in Science: The Case of X-Ray Protein Crystallography", *Science Studies*, III (June 1973), pp. 275–304.

importance. At the borderland with biochemistry, Chargaff's work at Columbia on the nucleic acids in the late 1940s was influential in focusing attention on DNA as the carrier of genetic specificity, indirectly—and perhaps unwittingly—paving the way for later progress in molecular biology.[28] Parallel to this, important work was done in the mid-1930s by an émigré from the Soviet Union, Boris Ephrussi, and his American colleague, George Beadle, at the California Institute of Technology on the control of metabolic sequences by genes—which helped solidify the linkage between genetics and biochemistry. Delbrück's first work in the area between physics and genetics had been done in Berlin, jointly with another Russian expatriate (who was eventually to return) N. V. Timofeyev-Rossovskii. In 1937, Beadle moved to Stanford University, where he worked on the mould neurospora with Edward Tatum. Building on his earlier work with Ephrussi, Beadle and his colleague demonstrated in detail that the function of a gene is to control the production of a particular enzyme in order to catalyse a particular chemical reaction. For the demonstration of this "one gene-one enzyme" hypothesis, the geneticist Beadle and the biochemist Tatum were awarded a share of the Nobel prize for 1958.

There are various reasons for the success of international migrants in their countries of immigration, particularly Great Britain and the United States. In most disciplines—particularly those involving broad theoretical perspective—scientists of the German-speaking countries were still pre-eminent until the Nazi expulsions, beginning in 1933. Furthermore, in order to make their way in their host countries, the émigrés frequently had to try to bring together the approaches of their native and adopted milieux. This often required them to bring their previous knowledge to bear on the state of knowledge and skill in their host societies, leading in a few important cases to a synthesis between primarily "German" theoretical ideas and a primarily "Anglo-American" orientation towards exacting experiment in physics, chemistry and astronomy. Such a synthesis was not always immediately possible as a result of disjunctions between theoretical and experimental orientations as well as the resistance of scientists who were attached to the previously prevailing approaches. However, the movement towards such a synthesis was promoted by the international and other migrations of this period. This was undoubtedly a central factor in the re-ordering and expansion of the natural sciences in the middle third of the century.

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[28] Olby, Robert C., *The Path to the Double Helix* (London: Macmillan, 1974), pp. 208, 211ff; Watson, J. D., *The Double Helix: A Personal Account of the Discovery of the Structure of DNA* (New York: Athenaeum, 1968), p. 130; Judson, Horace F., "Reflections on the Historiography of Molecular Biology", *Minerva*, XIII (Summer 1982), pp. 387ff; Chargaff, E., *Heraditean Fire, op. cit.*, pp. 100–103.

Migrations between Universities and Other Scientific Institutions

Significant new combinations and re-crystallisations of scientific fields have arisen from the migrations of scientists between universities and other scientific institutions. One example of such an institutional migration was that which paved the way for the establishment of physical chemistry as an institutional specialty at the end of the nineteenth century. The focal centre for this process was the school of research formed by Wilhelm Ostwald at Leipzig in 1887[29]—at least 53 of whose students became professors[30] and one of whom, Willis Whitney, became the founding director of the research laboratory of the General Electric Company in Schenectady, New York. Ostwald was himself a migrant to Leipzig from the polytechnic at Riga in Latvia.

> In his six years as professor at Riga, Ostwald said he had only one good student dedicated to research. In Germany a new research school could grow far faster than in any other European country . . . [for only there] was there sufficient diversity in the university to permit minority interests to flourish . . . [and] the system of teaching students how to do experimental research established on a large scale.[31]

The synthesis needed to lay the foundations for a flourishing new specialty arose when a particularly creative individual came into a setting, which was particularly propitious for the re-crystallisation or expansion of his work. This was so in the case of Wilhelm Wundt, who was enabled to establish his experimental psychology laboratory only after his move in 1875 from Heidelberg and Zurich to the more intellectually flexible and fertile milieu provided by the University of Leipzig.[32]

A movement from the university to a more technologically-oriented milieu, followed by an important change of focus in research, was a feature of the careers of two of the most outstanding theoretical physicists of the past century—Arnold Sommerfeld and John Slater. Sommerfeld had spent much of the last decade of the nineteenth century in the atmosphere of Göttingen mathematical physics, publishing—beginning in 1897—with his mentor Felix Klein, *Die Theorie des Kreisels*, a monumental treatise of 1,000 pages on the theory of the gyroscope. However, when in 1900 he became professor of mechanics at the Technische Hochschule in Aachen: "He asked his engineering colleagues to point out problems on which he could exercise his mathematical skill. From this there arose a novel branch of theoretical

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[29] Dolby, R. G. A., "The Case of Physical Chemistry", in Lemaine, G. *et al.* (eds), *Perspectives on the Emergence of Scientific Disciplines* (Paris: Mouton, 1976), pp. 63, 69ff; Ihde, A. J., *The Development of Modern Chemistry* (New York: Harper and Row, 1964), p. 341; Hiebert, Erwin, "Developments in Physical Chemistry at the Turn of the Century", in Bernhard, C. G. *et al.* (eds), *Science, Technology and Society in the Time of Alfred Nobel* (Oxford: Pergamon, 1982), pp. 97ff.

[30] Szabadvary, F., *History of Analytical Chemistry* (Oxford: Pergamon, 1966), p. 354.

[31] Dolby, R. G. A., "The Case of Physical Chemistry", *op. cit.*, p. 69.

[32] *Ibid.*, p. 70.

physics: the mathematical treatment of the propagation of electromagnetic fields and waves."[33]

Upon moving to Aachen, he also "began work on hydrodynamics in connection with technical problems".[34] He subsequently wrote important papers on the dynamic aspects of the strength of materials, the oscillation of dynamos, the action of railway brakes, and the resistance of electric coils to alternating current, as well as two especially significant papers on the hydrodynamics of lubrication and on the stability of laminar flow. It was Sommerfeld's particular genius to apply the wave equations he had learned at Göttingen to practical problems in both electricity and hydrodynamics, under the stimulus of his move to a new institutional environment, where technological interests were more prominent than they had been in Göttingen. Even his subsequent instalments of *Die Theorie des Kreisels* were increasingly oriented to practical engineering problems. This was of course very similar to the subsequent re-orientation of a Prandtl and von Karman—who also moved from Göttingen to Aachen a few years later—to problems of applied mechanics and aerodynamics.

John Slater's move from Harvard to the nearby Massachusetts Institute of Technology in 1930 catalysed a similar transformation. Although Slater had done a dissertation in experimental physics at Harvard in the early 1920s, measuring the compressibility of various alkali halide crystals, his subsequent work throughout that decade was primarily focused on the purely theoretical problems of the new quantum mechanics. By 1930 he was best known for the "Slater determinants" as a tool for analysing multi-electron phenomena and complex atomic spectra. With his move to Technology, as it was then called—and particularly after 1933—his work and that of his students was devoted mainly to detailed calculations of the electronic structure of real materials, which were becoming increasingly interesting to the major industrial research laboratories of the electrical industry. He himself would probably not have explained the redirection of his interests as such a direct consequence of his migration to an institution primarily interested in technology. He has emphasised rather the continuity between his work on real materials after 1933, his early experimental work on the alkali halides, and his work during a temporary sojourn at Leipzig in 1929–30 where—under the stimulus of Werner Heisenberg—he first examined the theoretical problems of cohesion in metals and ferromagnetism. He explained his plunge into band structure calculations in 1933 as a result of the increased possibilities of progress opened by the newly developed Wigner-Seitz synthesis. He added:

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[33] Elasser, Walter, *Memoirs of a Physicist in the Atomic Age* (New York: Science History Publications, 1978), p. 36; Born, Max, "Arnold Johannes Wilhelm Sommerfeld", *Obituary Notices of the Fellows of the Royal Society*, VIII (1952–53), p. 279; Klein, Felix and Sommerfeld, Arnold, *Die Theorie des Kreisels*, 4 vols (Leipzig: Teubner, 1897–1909).

[34] Kuhn, Thomas S. *et al.*, *Sources for the History of Quantum Physics* (Philadelphia: American Philosophical Society, 1967), p. 140.

> But of course [in the department of physics of the Massachusetts Institute of Technology] we had a lot of interest in solid-state things through [Wayne B.] Nottingham in his electronic work. This was affecting the interests of the graduate students quite a good deal. Nottingham had very many good students . . . interested in electronics of crystals and things of that kind. In fact, people would shift back and forth. [William] Shockley did his thesis with me, but he also did some work with Nottingham while he was around. And I think this tended to give an interaction between the theory and practical things that was quite useful.[35]

Nottingham had extensive connections with the research laboratories of the electrical industry, high executives of which also played a central role in Slater's departmental visiting committee. Moreover, at one point the president of the General Electric Company, Gerard Swope, served as chairman of the board of trustees of the Massachusetts Institute of Technology itself. The president of the institute in this period, Karl Compton, had long served as a consultant to the same firm's research laboratory. While Slater's redirection of research interests cannot be reduced to a preoccupation with practical engineering problems, the intellectual climate of the institute, even under Compton, was at that time still substantially different than that at Harvard. Compton and Slater were instrumental in transforming their institution from a superior college of technology to a centre of fundamental scientific research—especially in the electron theory of real materials—a subject also of considerable importance to industry.

Also important to the development of the institute's and Slater's interests in physics in this period was the continuing influence of a number of other temporary or permanent migrants from Europe and from other American institutions. In 1924, long before Slater's arrival, Debye had visited from his laboratory in Zurich, and had been accompanied by his student Hans Mueller. The latter was persuaded to remain, and specialised in the properties of dielectrics and similar industrially significant topics. In 1927, W. L. Bragg, then of the University of Manchester, was a visiting professor. As Slater remembered:

> One of the young MIT students, Bertram E. Warren, so impressed him that he arranged for Warren to spend some time with him in England, and also to visit other European X-ray centres . . . and [thus] started a lifetime of most useful work at MIT. [Warren] became universally respected among X-ray crystallographers, and always had one of the most productive experimental groups in the department. My interest in directional properties of covalent bonds was an outgrowth of a great deal I learned from him about the geometrical arrangement of atoms in crystals.[36]

Two American theorists who joined the department of physics in the academic year 1930–31 after Slater's arrival were Julius Stratton, who was

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[35] AIP interview with John C. Slater by Charles Weiner, Session 1, 23 February, 1970.

[36] Slater, John C., *Solid State and Molecular Theory: A Scientific Biography* (New York: John Wiley, 1975), p. 166; Warren, Bertram E., "Personal Reminiscences", in Ewald, P. P. (ed.), *Fifty Years of Electron Diffraction* (Utrecht, Netherlands: N. V. A. Oosthoek's Uitgeversmaatschappij, 1962) pp. 667–671.

originally trained at the Eidgenössische Technische Hochschule in Zurich, and who moved from the department of electrical engineering, and Philip Morse, who had received a doctorate at Princeton in 1929. One permanent international migrant who provided an important bridge between experimental work on excitons in R. W. Pohl's laboratory at Göttingen and the theoretical physics at the Massachusetts Institute of Technology was the refugee Arthur von Hippel. Arriving at the institute in 1936 from the University of Istanbul, where he went in the first years of his exile from Germany, he quickly founded what he called the laboratory for insulation research, and maintained close relations with Slater.

> One of the first things he [von Hippel] did was to give me a preprint of a paper which he had just written, giving an explanation of the excitons observed in alkali halides [by Pohl] . . . Shockley had already become interested in excitons . . . [and] got busy asking how one would describe [von Hippel's] localized excited states in wave mechanics.[37]

This led Slater and Shockley to formulate a simple one-dimensional model for describing excitons in terms of quantum mechanics. This technique then formed the basis the following year for Slater's application to ferromagnetism of Felix Bloch's theory of "spin waves" which Slater had learned about during his earlier visit to Leipzig.[38] The new work on ferromagnetism was done during the spring of 1937, when Slater made another temporary migration—this time to the Institute for Advanced Study in Princeton. There he established contact with a visiting Swiss postdoctoral fellow named Gregory Wannier, who "had just figured out a very ingenious technique for handling the excitations" in terms of a linear combination of what are now called Wannier functions.[39] While at Princeton Slater also discussed his work with Wigner's research student, Conyers Herring, who then decided to take up a postdoctoral post for the years 1937–39 with Slater's group at the Massachusetts Institute of Technology. However, once there, Herring's closest intellectual relationship turned out to be not with Slater, but with his own Princeton contemporary, John Bardeen. The latter was then close at hand, working as a junior fellow in the physics department at Harvard. Herring recalled that he was "particularly impressed, in view of my developing interest in [electronic] band calculations by Bardeen's improvements in the Wigner-Seitz approach. . .". Herring then developed his own improved version of the Wigner–Seitz synthesis—the method of the orthogonalised plan wave—which was for many years thereafter the most successful technique for calculating the electronic structures of a large class of solid materials. At the institute, Herring also had fruitful discussions with a newly arrived instructor from the University of Rochester, Albert G. Hill, with whom he was sharing an apartment. Hill, while still a postgraduate student at Rochester had started calculations of the Wigner–Seitz type on

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[37] Slater, J. C., *Solid State and Molecular Theory*, *op. cit.*, pp. 200–201.
[38] *Ibid.*, pp. 202–203.
[39] *Ibid.*, pp. 204–205.

the band structure of the divalent atom metallic beryllium; he did these with another of Wigner's former students, Frederick Seitz. Unfortunately, this encountered obstacles, and it seemed unlikely that this technique could be applied to divalent atoms. As Herring recalled: "Although primarily an experimentalist, Hill was very interested in collaborating . . . So I proposed that we should combine [my] orthogonalized plane wave calculations for states at the [band] boundary, with Wigner-Seitz-Bardeen calculations for states near the bottom of the band."[40] This was the first extension of the quantum theory of solids to explain the electronic properties—other than ferromagnetism—of multi-valent metals.

The re-focusing of scientific interests engendered by the movement of university scientists to industrial laboratories is similar to the process under discussion here, though it has rarely led to any major new scientific synthesis. For example, Willis Whitney's move from the Massachusetts Institute of Technology to General Electric resulted only in his own shift from fundamental to applied research, and ultimately to his successful career in the management of industrial research. Irving Langmuir, who made a similar transition only a few years later, was however particularly adroit in choosing to investigate fundamental problems which also promised long-term economic advantages to General Electric. Percy Bridgman commented, "It is not easy to separate Langmuir's work into physical, chemical and engineering components, [for] all three are intertwined with an intimacy not often exhibited in the work of other scientists."[41] Langmuir was appointed by the General Electric Company at a time when the company was facing stiff competition from Walther Nernst's ceramic-filament "glower" lamp—in part because he had obtained his doctorate with Nernst at Göttingen, and could therefore be expected to elucidate some of the fundamental physical and chemical principles which would permit further commercial success.

Another example is provided by the migrations of William Shockley and John Bardeen from the academic world to the Bell Telephone Laboratories in 1936 and 1945 respectively. This resulted in the late 1940s in a synthesis of their earlier theoretical work under Slater and Wigner, with the laboratory's experimental expertise—for the most part gained in work on radar—on the properties of the relevant semiconductor materials needed to produce the first transistor. This was followed a few years later by the fairly rapid development of solid state electronics.

The development of the transistor, and its subsequent technological repercussions, had a significant effect on the redrawing of the boundaries

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[40] Herring, Conyers, "Recollections", *Proceedings of the Royal Society*, Series A, CCCLXXI (January 1980), pp. 68–69; interview by the author with Albert G. Hill, 4 August, 1982.

[41] Bridgman, Percy W., "Some of the Physical Aspects of the Work of Langmuir", in Suits, C. Guy (ed.), *The Collected Works of Irving Langmuir* (New York: Pergamon Press, 1962), p. xxxi.

between different sub-fields and specialties within physics. In the early 1930s the main interest—and the principal focus of Wigner's and Slater's work in this area—had been in what was then called the electron theory of metals. Subsequently, in the later 1930s, based in part on Pohl's experiments at Göttingen on the alkali halides—as well as the increased importance of ionic crystals to the photographic and luminescent lighting industries—there developed alongside this an electronic theory of ionic crystals. (Frederich Seitz moved from the University of Rochester to General Electric, where it was hoped he would be able to apply his previous theoretical technique to this new class of industrially important problems.[42]) Together with the increased interest in semiconductors brought about at first by their use in radar crystal receivers during the Second World War—and then in the transistor—there was a gradual modification of disciplinary boundaries, so that a variety of specialties in different classes of materials came together as the broader sub-field of solid state physics.

The outstanding Austrian experimentalist Karl Lark-Horowitz, who came to the United States in 1927 and whose war-time research group at Purdue made the fundamental experimental observations of semiconductor properties which were indispensible to their successful later use in the transistor, had a marked influence on this change in disciplinary boundaries. Lark-Horowitz brought with him to the United States outstanding experimental skills, learned initially with Fritz Paneth at the University of Vienna before and after the First World War. There he had used radioactive materials to study adsorption by metallic salts, as well as the structure and surface characteristics of platinum crystals. During the course of an International Education Board postdoctoral fellowship at the universities of Toronto, Chicago and Stanford and at the Rockefeller Institute, he acquired considerable facility as a crystallographer and further developed his ability to produce pure crystallographic specimens. This skill was of immediate relevance to the Purdue group in the Second World War, as their first task was to produce purified germanium crystals. Their next task, which Lark-Horowitz's previous experience also fostered, was the production of p- and n-type germanium doped by addition of other metals, and then to investigate the change of electrical properties in terms of known impurity contents.[43] In a sense, the migration of Lark-Horowitz and his colleagues from university research to the more technological requirements of the radar programme opened up a crucial new field of research on the electrical properties of doped semiconductors. After the war, Lark-Horowitz made use of his earlier work with Paneth and his later work at Purdue with semiconductors in order to complete a further study of the modifications of semiconductor properties induced by radiation.

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[42] Seitz, Frederick, "Biographical Notes", *Proceedings of the Royal Society*, Series A, CCCLXXI (January 1980), p. 90.

[43] Johnson, Vivian, *Karl Lark-Horowitz: Pioneer in Solid State Physics* (Oxford: Pergamon Press, 1969), pp. 6–7, 33–34.

Migrations between Universities Within the Same Country

Migration to another university within the same country or cultural area does not normally expose the migrant to the same contrast of national scientific traditions as is sometimes the case in international migration. It does, however, expose him to new colleagues, occasionally working on very different problems than those he has dealt with previously—and thereby it also opens up opportunities for extending his previous techniques to new areas, and for creative synthesis. This is what happened in the case of Niels Bohr during the course of his migration in 1912 between two English universities. In 1911, after completing his doctoral dissertation in Copenhagen on the electron theory of metals—the field which in later years formed the theoretical basis for a physics of the solid state—he moved to Cambridge to spend a postdoctoral year with the leading British theorist of electrons, J. J. Thomson. However, when the latter proved too busy to read the English translation of his thesis and the Cambridge Philosophical Society declined to publish it, Bohr decided to move to the physics laboratory of Ernest Rutherford at Manchester. In this new institutional setting, Rutherford and Bohr had a particularly intense collaboration, the result of which was the Rutherford–Bohr atomic theory, set out initially in the three papers by Bohr, submitted by Rutherford to *The Philosophical Magazine* in the following year.

In an earlier "memorandum"[44] on the basis for the new theory, submitted to Rutherford in 1912, Bohr explained that as Rutherford's experiments on the deflection of alpha particles had shown that the positive charges within an atom must be concentrated in an area which is small compared with its total dimensions, he had decided to approximate them by a central *kern*, i.e. nucleus. Then "by an analysis analogous to the one, used by Sir J. J. Thomson, in his theory of the constitution of an atom",[45] in which Bohr had recently been immersed in Cambridge, he had found that an atom consisting of such a central positive core surrounded by rings of electrons would possess no stability, according to classical electrodynamics. Rutherford advised him against "going into detailed calculations of any special system apart from the most simple."[46] Bohr was thus encouraged to investigate only the simplest case: that of a single electron circling around a positive point charge—and found that from this model he could determine the frequency of

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[44] Bohr, Niels, "On the Constitution of Atoms and Molecules", (memorandum to Rutherford of June or July 1912), reprinted in Hoyer, Ulrich (ed.), *Niels Bohr, Collected Works, Volume 2: Work on Atomic Physics (1912–1917)* (Amsterdam: North Holland, 1981), pp. 136–143; Heilbron, John L. and Kuhn, Thomas S., "The Genesis of the Bohr Atom", *Historical Studies in the Physical Sciences*, I (January 1969), pp. 234ff; Moore, Ruth, *Niels Bohr: The Man, his Science and the World they Changed* (Cambridge, Mass.: MIT Press, 1985) pp. 31–39; Hoch, Paul K., "Bohr's Decisive Years", *Contemporary Physics*, XXIV (March 1983), pp. 203–204.

[45] Bohr, N., "On the Constitution . . .", *op. cit.*, p. 136.

[46] Niels Bohr to Ernest Rutherford, 31 January 1913, reprinted in Hoyer, U. (ed.), *Niels Bohr, op. cit.*, p. 107.

revolution as a function of the electron's radius, which was otherwise unrestricted. Since atoms only occasionally radiate energy and are otherwise stable, he reasoned that the radius and frequency are normally fixed. This led him to make the further postulate that the kinetic energy of such an electron is proportional to its frequency, an assumption similar to that made by Planck in his earlier treatment of the "quantised" harmonic oscillator. This produced the needed second equation which uniquely fixed the values of frequency and radius for given units of the constant of proportionality.

After his return to Copenhagen, Bohr was further stimulated by a personal conversation with the Danish spectroscopist H. M. Hansen, who had himself recently returned from Göttingen where he had visited the spectroscopy group. Hansen reminded Bohr of the Balmer formula which phenomenologically expresses the line spectra of atomic hydrogen. Bohr was then encouraged to derive this formula from his previous equations, on the assumption that his constant of proportionality was a simple half-integer multiple of Planck's constant. Bohr was subsequently reappointed as a visiting reader in applied mathematics at Manchester for 1914–16 and—through his continuing interest in the experimental work of Rutherford's group—he later became interested in the still embryonic physics of the atomic nucleus. Thus it was that Bohr's temporary migration to Rutherford's laboratory brought the experimental work of the latter into closer proximity with the earlier formulations of Planck. It led directly to the Rutherford–Bohr synthesis of experiment and theory exemplified in their planetary model of the atom, and its phenomenological quantum-based explanation for transitions between excited atomic states. This was the outstanding accomplishment of the old quantum theory of the atom, and the basis of its analysis of atomic spectra. The intermediary between the initial "German" quantum theory of Planck and the "British" experimental atomic theory of Rutherford turned out to be a Dane whose sojourns in both countries had put him into immediate contact with individuals embodying the two traditions.

A similar example is provided by the short-term migrations between Great Britain and the main German theoretical centres in the middle 1920s of Ralph H. Fowler, the Stokes lecturer in mathematical physics at Cambridge University. This helped to make him the leading exponent in Great Britain of the quantum mechanics of Heisenberg, Born and Schroedinger. Fowler's students, including J. E. Lennard-Jones, P. A. M. Dirac, Nevill Mott and Alan H. Wilson, subsequently became the leading British figures in the application of "German" quantum mechanics to problems in chemistry and in both nuclear and solid state physics. Lennard-Jones himself must have been helped to propound the scientific formulation for which he is best known—the "Lennard-Jones potential"—by the combination of migration and synthetic capacity. He began his professional career as a lecturer in mathematics after the First World War at the University of Manchester, where "he became interested in the kinetic theory of gases through his contact with Sydney Chapman",[47] then professor of mathematics and natural philosophy. Chapman was then in the process of working out the transport coeficients in a gas for various inter-molecular force laws. In 1922 Lennard-Jones extended this work to rarified gases. He then moved to Cambridge University, where he worked with Fowler, and was encouraged by the latter to consider the internal electrical forces in a solid. Thus, it was in 1924 that he proposed his formulation of the force law $F(r) = a/r^n + b/r^m$. Later this was adapted to yield a representation for the internal crystalline potential in a solid for use in quantum mechanical calculations by Slater and many others using the Schroedinger equation. In the view of his biographer Lennard-Jones's work was "based on [a synthesis of] Chapman's gas theory and the Heisenberg-Schroedinger quantum mechanics . . ."[48]. In fact, the latter was not available until after Lennard-Jones made his original proposal. The synthesis, facilitated by his movements between Manchester and Cambridge, was initially one based on his knowledge of Chapman's work on inter-molecular force laws in gases and his work with Fowler on the inter-atomic force law in crystalline solids. It was then extended to a further synthesis with the new quantum mechanics which Fowler was bringing back from his many visits to the main centres in Germany.

The work of Nevill Mott offers another example of institutional migration leading to a movement of theoretical expertise from nuclear into what became solid state theory. In 1933 he left his fellowship at Cambridge, and his role as adviser on theory to the atomic and nuclear physicists of the Cavendish laboratory, to take up a chair in theoretical physics at the University of Bristol in succession to Lennard-Jones. Attention at Bristol was then beginning to turn to the physics of metals and other solids and important work had already been accomplished in formulating a quantum theory of the phases in alloys. The latter work particularly impressed Mott, who later recalled that he "was fascinated to learn that quantum mechanics could be applied to such practical problems as metallic alloys, and it was this as much as anything else that turned my interest to problems of electrons in solids".[49] Also crucial to Mott's movement into a new field of research was the experimental work performed the previous year at the Massachusetts Institute of Technology by another of Mott's new colleagues at Bristol, H. W. B. Skinner, on the soft X-ray emission spectra of various light metals. Both the theoretical work on alloys and the experimental work on emission spectra were consistent with the simplified model of a metal which ignored the interaction between conduction electrons, even though such interactions were expected by many physicists to be quite large. These surprising results

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[47] Brush, Stephen G., "Lennard-Jones, John Edward", *Dictionary of Scientific Biography* (New York: Scribner, 1973), Vol. VIII, p. 185.

[48] *Ibid.*

[49] Mott, Nevill F., "Electrons in Crystalline and Non-Crystalline Metals: from Hume-Rothery to the Present Day", *Metal Science*, XIV (December 1980), p. 557.

were explained in detail by Mott and his new colleagues in an important paper—his first in this new field—which demonstrated the suitability of the "free electron" model for an actual metal. In his memoir on the history of the Bristol physical laboratory, its then director, Arthur M. Tyndall, expressed his surprise and pleasure at how quickly Mott had been persuaded to drop his previous interest in nuclear physics and to apply instead his quantum mechanics to the problems of the solid state which were of major concern in his new institutional environment.[50] The result of Mott's migration, his orientation to a new context, and his subsequent syntheses, was that Bristol became the main British centre of the new sub-field of solid state physics.[51] Another stimulus was the concurrent migration to Bristol of Herbert Fröhlich and a number of other highly trained émigré theorists from the schools of Max Born and Arnold Sommerfeld at Göttingen and Munich. The other British centres of this sub-field were likewise founded by migrants from the continent. The Mond Laboratory at Cambridge was initiated by the Russian expatriate P. L. Kapitza; the low temperature groups at the Clarendon Laboratory at Oxford were led by Franz Simon and Kurt Mendelssohn; and the group in solid state theory at Edinburgh formed around Max Born.

The early career of Kapitza represents an interesting instance of intellectual syntheses consequent on migration. He was originally trained as an electrical engineer in the Imperatorskii Tekhnicheskii Institut (the imperial polytechnical institute in St Petersburg), subsequently working with Abram, F. Joffe on experimental determinations of the physical properties of solids. In 1921 he moved to the Cavendish Laboratory at Cambridge, and gradually became an expert on the electronics of nuclear experimentation. From this he turned to the electronic production of the high magnetic fields needed to deflect alpha particles. Out of this work eventually came what at first was called the "magnetic laboratory". In the second half of the 1920s he took up again his earlier work in solids, focusing his attention on the modifications in the electrical properties of metals that could be produced by his high magnetic fields. In order to avoid the complications of thermal vibrations, he conducted his research at low temperatures. This was the origin of what was to be the Mond Laboratory, which was for the next generation the main centre for examining the electronic properties of metals, particularly at low temperatures.

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[50] Mott, Nevill F., "Memories of Early Days in Solid State Physics", *Proceedings of the Royal Society*, Series A, CCCLXXI (January 1980), p. 58, and "Notes on my Scientific and Professional Career", p. 12, unpublished manuscript, made available by Professor Mott; Jones, H., Mott, N. F. and Skinner, H. W. B., "A Theory of the Form of the X-ray Emission Bands of Metals", *Physical Review*, VL (February 1934), pp. 379–384; Tyndall, Arthur M., "A History of the Department of Physics in Bristol 1876–1948, with Personal Reminiscences", p. 27, unpublished paper, in the Tyndall Papers, University of Bristol archives.

[51] Keith, S. T. and Hoch, Paul K., "Formation of a Research School: Theoretical Solid State Physics at Bristol 1930–54", *British Journal for the History of Science*, XIX (March 1986), pp. 19–44.

Kapitza's outstanding student at the Mond Laboratory was David Shoenberg, whose father had immigrated to Great Britain from Russia in 1914. Shoenberg's efforts to understand the changes of metallic magnetic susceptibility at low temperatures in the presence of high magnetic fields were somewhat disrupted in 1934, when his mentor Kapitza was prevented by the Soviet authorities from returning to Great Britain from the Soviet Union. The situation was partially retrieved for Shoenberg as a result of the presence of a short-term migrant to the Cavendish from India, the experimentalist K. S. Krishnan, who described the details of a new experimental technique for measuring magnetic susceptibility which he had developed at Dacca and Calcutta. This was a vital clue for Shoenberg about how to pursue his own experiments on bismuth, which he was then enabled to carry through during the course of his own visit in 1937–38 to the Institut Fizickil Problem Akademii Nauk USSR (the institute for physical problems of the Soviet Academy of Sciences), which had been created for Kapitza in Moscow. There he met another recent internal migrant—the oustanding Russian quantum theorist L. D. Landau, whose unorthodox political opinions had impelled him to leave the politically besieged Fizicko-Tekhnicheskii Institut (physico-technical institute) at Kharkov, where Landau and the majority of laboratory directors were at one time or another under arrest.[52] Landau had already worked out a theoretical explanation of what was called the de Haas–van Alphen effect in relation to the magnetic susceptibility of bismuth, which turned out to be consistent with Shoenberg's experimental findings. Landau was prevented by existing political restrictions from publishing his findings. Shoenberg, although lacking the mathematical ability to reconstruct Landau's calculations, was nevertheless, on his return to Cambridge, able to turn to a recent immigrant from Germany, Rudolf Peierls, who had until recently been the resident theorist at the Mond Laboratory. With Peierls's aid, Shoenberg was able to publish his crucial paper on the de Haas–van Alphen effect in bismuth. This was the first experimental determination of the "Fermi surface" of an actual metal, and an important basis for the later development of a specialty concerned with measuring the Fermi surfaces of other metals.

The person who was most active in the development of this new specialty was Shoenberg's outstanding research student, Brian Pippard.[53] He began his research at the Mond Laboratory just after the Second World War, during which he had worked on radar development. He then applied his experience with radio waves to a substantial physical problem—the various low temperature anomalies in the penetration of microwaves into the surface "skin" of metallic copper. (This effect had been discovered in the

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[52] Weisenberg, Alex, *Conspiracy of Silence* (London: Hamish Hamilton, 1952), pp. 359ff; Hoch, Paul K., "Held Back by Years of Tyranny", *The Times Higher Education Supplement* (20 January, 1984), p. 15.

[53] Hoch, Paul K., "A Key Concept from the Electron Theory of Metals: History of the Fermi Surface 1933–1960", *Contemporary Physics*, XXIV (January 1983), pp.3–23.

late 1930s by the refugee Heinz London at Bristol.) After Pippard had performed his experiments, he tried to formulate the problem theoretically, but encountered difficulties in the mathematics. He turned to a young refugee who was a fellow research student, Ernst H. Sondheimer. The latter was able to formulate the necessary integral equation but not solve it. Sondheimer passed the problem, through another refugee mathematician, F. G. Friedlander, to yet a third refugee mathematician, then at Manchester, G. E. H. Reuter, who with Sondheimer was able to provide the complete solution. With the help of two more theorists who were visitors at the Cavendish Laboratory from the United States, Paul Marcus and Lars Onsager, and with the further aid of Sondheimer, Pippard was eventually able to see how his experiment on the anomalous skin effect could be used to determine the shape of the Fermi surface of copper. However, to obtain the necessary precision, he needed very precisely cut and polished single crystals of copper, which at that time were not available at Cambridge. During the course of a visit of another American theorist, Morrel H. Cohen, Pippard learned that the Institute for the Study of Metals at the University of Chicago, as a result of the participation of some of its staff in the Manhattan project, had acquired the skill to produce highly polished single crystals. Cohen arranged for Pippard to visit the University of Chicago for the academic year 1955–56. It was in that year, in the course of his sojourn in Chicago, that he made his important measurement of the Fermi surface of copper. Pippard's success was made possible by the confluence of his own skill in experimentation, the mathematical knowledge of a number of migrants from Germany and the United States, and the technological skill at Chicago. It was a remarkable coming together of skill and theoretical capacity, brought about in considerable part by the movement of scientists from a variety of institutions and countries into face-to-face relationships.

Another important discovery facilitated by migrations between institutions was that of J. D. Watson, who came to the Cavendish Laboratory at Cambridge in 1951. There he met a slightly older British crystallographer, Francis Crick. Watson had "grown up" in the phage group of Delbrück and Luria, both refugees from Europe, taking his doctorate under the latter and attending the Cold Springs Harbor summer school organised by Delbrück.[54] In common with other members of that "school", he seems to have been seeking the physical mechanism by which "coded information" was passed on by the genetic material. Francis Crick—after taking a degree in physics in 1937, and working in experimental cytology at the Strangeways Laboratory—became a crystallographer at the Cavendish Laboratory in 1949. This was also the period when Max Perutz was working there on his X-ray analysis of the structure of haemoglobin.[55] The

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[54] Watson, J. D., "Growing Up in the Phage Group", in Cairns, John *et al.* (eds) *Phage and the Origins of Molecular Biology* (Cold Springs Harbor, NY: Cold Springs Harbor Laboratory of Quantitative Biology, 1966), pp. 239–245.

[55] Olby, R. C., *Path to the Double Helix*, *op. cit.*, pp. 310–311.

collaboration between Watson and Crick consisted essentially of a synthesis of the physical genetics programme of the phage group with the structural crystallography as it was applied to complicated organic molecules by Perutz and the "structural school". This resulted in their joint formulation of the structure of DNA—the double helix.

Institutional migrations of both an international and intranational kind were also very important in the generation of the scientific syntheses of Maria Groeppert Mayer.[56] A student of Max Born at Göttingen in the 1920s, she obtained from him a sound grounding in the new quantum mechanics and in the applications to it of group theory, then being developed there by Weyl, Wigner and von Neumann. After obtaining her doctorate, she moved with her husband, the chemist Joseph Mayer, to Johns Hopkins University in the United States, but continued to spend her summers between 1931 and 1933 with Born in Göttingen. There they completed their "Dynamische Gittertheorie der Kristalle" for the *Handbuch der Physik*; this article presents one of the bases for the theory of lattice dynamics in solids, a subject subsequently developed by Born and his students at the University of Edinburgh. At Hopkins, under the influence of her husband and the refugee physicist Karl F. Herzfeld, she acquired a considerable knowledge of quantum chemistry, which was further developed when in 1935 James Franck joined the staff at Johns Hopkins. Franck had been Joseph Mayer's teacher at Göttingen, and the relations between the two families were close. In 1939 the Mayers moved to Columbia University, where Maria Mayer came into contact with the Italian refugee Enrico Fermi. He suggested that she use her knowledge of quantum chemistry "to predict the valence-shell structure of the yet-to-be-discovered transuranium elements. By making use of the very simple Fermi-Thomas model of the electronic structure of an atom, she came to the conclusion that these elements would form a new chemical rare earth series."[57] After the Second World War the Mayers moved to the University of Chicago, where she held a joint appointment at the newly founded Argonne National Laboratory—successor to the Metallurgical Laboratory of the Manhattan project—and at Fermi's new Institute for Nuclear Studies at the university.

> The main interest at Argonne was nuclear physics, a field in which she had little experience, and so she gladly accepted the opportunity to learn . . . Among the many subjects being discussed at the Institute was the question of the origin of the chemical elements. [Edward] Teller was particularly interested in this subject and induced Maria Mayer to work with him . . . She became involved in analyzing the abundance of the elements and noticed there were certain regularities associating the highly abundant elements with specific numbers of neutrons or protons in their nuclei.[58]

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[56] Sachs, Robert G., "Maria Groeppert Mayer, June 28, 1906–February 20, 1972", *Biographical Memoirs of the National Academy of Sciences*, L (1979), pp. 311–328.

[57] *Ibid.*, p. 317.

[58] *Ibid.*, p. 319.

It was this that she sought to explain in terms of a possible shell model for nuclei analogous to the electron shells that determine chemical properties. While she was searching for a convincing theoretical handle on the problem: "It was Fermi who asked her the key question, 'Is there any indication of spin-orbit coupling?', whereupon she immediately realized that this was the answer she was looking for, and thus was born the spin-orbit coupling shell model of nuclei."[59] Maria Mayer's ability to recognise this as the solution to the problem was a direct consequence of her deep understanding of both quantum mechanics and of the representations of the rotation group, both of which had been obtained at Göttingen. Also important was her knowledge of atomic shell models, for the most part learned at Johns Hopkins and Columbia. But it was her move to the University of Chicago that brought her fully into nuclear physics, and made possible her direct connections with Edward Teller and Fermi. This led to the synthesis of group theory, atomic shell models and nuclear data summed up in her nuclear shell model, for which she was awarded a share of the Nobel prize in 1963.

The Problem of Institutionalisation

When an individual changes membership from one institution to another, he does not necessarily acquire the knowledge, imagination or techniques needed to produce important new ideas or discoveries. Migration between institutions, even when followed by a scientific innovation, does not necessarily lead to the "institutionalisation" of a new specialty or discipline as a relatively continuous intellectual activity. A minimum requirement for institutionalisation is the assembly of a certain amount of institutional resources—such as funds for new appointments and training of graduate students—without which a new specialty cannot become academically established. British solid state physics, for example, was at most a loose federation of research groups until at least the 1950s, when academic posts and courses of study were finally developed for this broad sub-field.

Under present conditions, although specialties may originate in the intellectual creations of at most a few persons, they must quickly be consolidated in a few centres; unless this occurs, institutionalisation is hampered. For example, in the second half of the 1920s the major applications of the new quantum mechanics to problems of the solid state occurred primarily in Sommerfeld's group at Munich, and, to a lesser extent, in Born's group at Göttingen. They then spread—to a great extent by migration—to the groups founded by Sommerfeld's students: Heisenberg in Leipzig and Pauli at the Eidgenössische Technische Hochschule in Zurich. They were then taken up, after 1933, by Wigner and his students at

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[59] *Ibid.*, p. 321.

Princeton, Mott's group at Bristol, Slater and his students at the Massachusetts Institute of Technology; and after 1936 by Born's group at Edinburgh, as well as by Landau and his collaborators at Kharkov and then Moscow.

In this roster of the members of the first generation of solid state theory, the only ones who were not at least temporary migrants at the original centres at Munich and Göttingen were Mott, Slater and Landau. All of them however had, early in their careers, been travelling fellows in Germany, and Slater had worked with Heisenberg's group in Leipzig. Landau learned the precise experimental details of the new de Haas–van Alphen effect in bismuth, the theoretical explanation of which he was eventually to provide for Shoenberg, from the Russian experimentalist Lev Schubnikov, who had spent four years in de Haas's laboratory in Leiden between 1926 and 1930. Indeed, it was Schubnikov's discovery of the change of electrical resistance of a bismuth sample in the presence of a high magnetic field at low temperatures that led de Haas and van Alphen to look closely at the magnetic susceptibility under the same conditions. Subsequently, Schubnikov became director of the low temperature laboratory at the Fizicko-Tekhnicheskii Institut in Kharkov, where Landau directed the theoretical physics group. Both were imprisoned in 1936 by the Soviet secret police. Schubnikov died in prison on the eve of the Second World War, and Landau spent almost a year in prison before being freed through the intervention of Kapitza. From the spring of 1937, Landau had direct contact with Kapitza at the Institut Fizickil Problem, and through him had direct access to some of the most important problems being turned up by experimentalists in the study of solids. The generation of a scientific innovation and the establishment of a specialty often resembles a tree, with the founding fathers as the trunk, the sustaining roots running in many directions, their students and postdoctoral collaborators further up, and the proliferation of new branches occurring at the top. But arboreal imagery is insufficient since further innovations became trunks with roots bringing sustenance from many directions and re-crystallising in new ways.

New knowledge, new syntheses and resulting new specialties often result from immediate social interactions in which diverse ideas confront each other and in which new ones are generated. The idea that knowledge proliferates mainly in "invisible colleges"—which stresses the informal unpublished communication of ideas in science—is obviously sensible, but it is too simple and undifferentiated. It pays no attention to the significance of face-to-face relationships among scientists, and of the personal coming together of scientists with different pieces of knowledge. It is often the stimulus of new face-to-face relationships that lead a scientist like Mott or Mayer to think of things, or to engage in new activities, that they had not thought of before.

In 1928, Robert Park described the potential contributions of the "marginal man" who shares in the *Weltanschauungen* of two separate societies, strata or groups.[60] If this idea is reformulated more precisely, and the cognitive elements in a culture are given greater prominence, the idea of the "marginal man" is suggestive for the analysis of scientific discovery. Contact with an alien way of seeing things discloses new possibilities inherent in his own knowledge but not perceived by the person who possesses that knowledge until he has come into close contact with another person whose knowledge can be made to complement his own, and to be formed into a new synthesis. It is not just a matter of quantitatively adding new knowledge to what he already possesses. It is a matter of new insights being engendered which were not explicitly contained in either the previously possessed or in the newly acquired knowledge. The scientist who moves from one society to another—or even from one institution to another within the same society—belongs partly to what is, in a limited sense, the universal scientific culture of a particular discipline, and partly to two distinct national or institutional scientific cultures. If he has the imagination of insight, he can perceive creative potentialities in each of them.

The scientist who moves only between institutions in the same country has similar opportunities. But they are not as great as those of the international migrant who, to make his previous approaches understandable in the scientific idiom and emphases of his new culture, has an impetus to see his own prior knowledge and approaches in a new way. It is true that many international migrants have not produced such creative syntheses. Nevertheless a few have. The result has been new theories and new specialties of very great importance.

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[60] Park, Robert E., "Human Migration and the Marginal Man", *American Journal of Sociology*, XXXIII (May 1928), pp. 881–892, reprinted in his *Race and Culture* (Glencoe, Ill.: Free Press, 1980), pp. 345–356; see also Simmel, Georg, "The Sociological Significance of the Stranger", in Park, Robert E. and Burgess, Ernest W., *Introduction to the Science of Sociology* (Chicago: University of Chicago Press, 1924), pp. 322–327; Stonequist, Everett V., *The Marginal Man: A Study in Personality and Culture Conflict*, 2nd edn (New York: Russell and Russell, 1961); Schutz, Alfred, "The Stranger", in his *Collected Papers* (The Hague: Nijhoff, 1964), Vol II, pp. 91–105.