6. Resetting at Birmingham:

Postwar Physics in the UK

“My God, what have we done?” Reportedly, these were the words of Robert Lewis, the co-pilot of Enola Grey, the B-29 that dropped the first atomic bomb on Hiroshima on 6th August 1945. The scientists who had spent the previous years developing the atomic bomb, knew about its fatal effects. Those who had witnessed the Trinity Tests, the explosion of the first atomic bomb in the New Mexican desert had been in awe. Robert Oppenheimer, the scientific head of the mission, not only remarked that scientists knew that the world would no longer be the same, he also famously commented the event with a passage from the Bhagavad Gita: “··· now I am become Death, the destroyer of worlds ···”.

Despite the destructiveness of the weapon which had been built, despite the horror caused by its use on Hiroshima and Nagasaki, and despite the deep concern over the destructive power among the scientists who had created the bomb, their scientific and technological achievements are without question. General Groves, who would later be at pains to minimise the contributions of the British contingent at Los Alamos was one of the first to congratulate James Chadwick, the head of the British Mission, on the success of the joint project.[1] Most members of this British contingent at Los Alamos, who had been instrumental in the Manhattan Project at the level of research and development, returned to the UK at the end of the hostilities in the Pacific. For them the subsequent years were dominated by the key issues of resettling into peacetime academia and of dealing with the consequences of their war-time research. Most chose to return to Higher Education and help

[1] Letter [380].

build or rebuild the academic landscape in the UK. While this was true to a greater or lesser extent for virtually all academics who had been involved in the war effort away from their home institutions, for those nuclear scientists who returned from the Manhattan Project, this task had qualities which differed significantly from that of their colleagues. The production effort of nuclear weapons had moved forward nuclear physics and chemistry in a way that would have been impossible without this wartime enterprise, and this in itself altered the scientists' outlook.[2] But the success of the Project also had momentous repercussions of political and psychological nature. The Manhattan Project, epitomised by the joint leadership of the military (General Leslie Groves) and the scientist (Robert Oppenheimer) had brought together politics/warfare and science in a way that had been unknown before. From now on, spheres of influence had to be determined, territory defended and scientists had to face the huge political, military and humanitarian consequences that their scientific discoveries had had and would continue to have at much closer range than ever before. It is not surprising, therefore, that after 1945 many of those leading scientists became involved in efforts to control nuclear weapons. They set up their own national scientists' organisations such as the American Federation of Scientists in the US[3] or the Committee of Atomic Scientists, later called the British Association of Atomic Scientists in the UK.[4] Beyond the non-governmental realm of these organisations, scientists also became increasingly involved in consulting governments on nuclear issues, both on questions of weapons development and nuclear energy and on issues of the control of these weapons.

In the UK, a further issue arose largely out of the changing nature of the Anglo-American relationship, namely how to develop one's own nuclear programme in response to American attempts to monopolise the production of nuclear power. At Los Alamos, (and at the other research centres of the Manhattan Project) science had been truly international — within the confines of a wartime enterprise under the

[2] See letters [381], [384], [386], [390–391], [394].

[3] Letter [390].

[4] Letters [395], [413–414], [418–19].

military command of General Leslie Groves. Hence, it is not possible to quantify precisely the achievements of what became known as the British mission to Los Alamos. Their number never exceeded more than about twenty-five (even counting the 'consultants') which was negligible in comparison with thousands of workers at Los Alamos at the peak of activities. During the war, General Groves commented that British research was 'substantial' and that the British scientists made an 'invaluable' contribution to the American project. Yet, after the war, he claimed that while the quality of the work of the British team was high, their number was far too small to have had a significant impact, a reasoning which was adopted by American policy makers who were soon to engage in a more restrictive policy of nuclear sharing. Others disagreed and argued that the British contribution to the project went far beyond what a mere head count would suggest and was out of all proportion to the team's size.

American accounts of the Los Alamos years dominated the early historiography of developments on the Hill, just as American economic and military power dominated international relations after the end of the war. Throughout the war, nuclear co-operation and/or lack of it had followed a very similar pattern to the general development of Anglo-American relations more generally, with strategic, psychological, economic and political considerations and suspicions playing a crucial part in the formulation of policies. The British government clearly expected wartime nuclear collaboration to be continued after the end of the war. Prime Minister Attlee, in a letter to President Truman in early August 1945, proposed a 'joint declaration of our intentions to utilise the existence of this great power not for our own ends, but as trustees for humanity in the interest of all peoples in order to promote peace and justice in the world.'[5] But the Americans had rather different ideas. Some, especially in the military, intended to build on what they perceived to be a five-year technological lead by accelerating nuclear research in national (and secret) projects, while at the same time controlling uranium supplies. This would allow the Americans to accumulate a stockpile of

[5]Attlee to Truman, 8.8.1945, *Foreign Relations of the United States, 1945*, pp. 36–7.

weapons and to extend their strategic advantage.[6] Others envisaged closer co-operation with the Soviet Union (at the expense of the United Kingdom) in order to avoid a nuclear arms race.[7] Others were contemplating some form of co-operation with the British (and Canadians) while expressing doubts about collaboration with the Soviet Union.

The terms of the Quebec Agreements of August 1943, the Anglo-American Declaration of Trust of June 1944 and the Hyde Park aide-memoir of September 1944, had led the British to expect a continuation of nuclear collaboration 'after the defeat of Japan unless terminated by joint agreement'.[8] This was not forthcoming. Within less than a year, the approach of substantial technology and scientific transfer had been replaced by a more restrictive American policy of the Atomic Energy Act, the so-called McMahon Act, of August 1946 which prevented the transfer of information about technical processes and 'restricted data', such as the production of fissionable material to other nations. The correspondence between Rudolf Peierls and James Chadwick, who continued to act as technical adviser and head of the British Mission until his return to the UK in late 1946, give some indication of the underlying tension.[9] Moreover, they touched on the implications of the McMahon Act for the future of British nuclear ambitions. On an immediate practical level the McMahon Act meant that the British scientists, some of whom had remained at Los Alamos beyond August 1945, were no longer allowed access to documentation and reports which they had been able to utilise without restrictions before. More significantly in the medium and long term it meant that the British were forced to (or felt forced to) develop nuclear weapons independently, because they were no longer allowed to benefit from the scientific collaboration within

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[6]Timothy J. Botti, *The Long Wait. The Forging of the Anglo-American Nuclear Alliance, 1945–1958*, New York: Greenwood Press, 1987, p. 9

[7]Henry Stimson to Harry S. Truman, accompanied by a memorandum, September 11, 1945, Washington, 11.9.1945, *Foreign Relations of the United States, 1945*, pp. 41ff.

[8]Anglo-American Declaration of Trust, 13.6.1944, www.nuclearfiles.org/redocuments/1944/440613-anglo-amer-decl.html; Hyde Park Agreement, *Foreign Relations of the United States*, 1944, Vol. 2, Washington D.C., 1967, pp. 1026–28.

[9]Letters [395], [402], [414], [416].

the gigantic American enterprise. And indeed, a decision as taken by a handful of people without full Cabinet consultation in January 1947 to develop nuclear weapons. It was a sensitive area of public policy, and the programme was not officially communicated to the public until five years later, when the then Prime Minister Winston Churchill, in February 1952, announced plans for testing the first British-built nuclear weapons.

The exchange of letters between Rudolf Peierls and G.P.Thomson indicates that fundamental and applied research in nuclear physics was continuing at British universities, and that Rudolf Peierls' expertise was sought on these issues.[10] But Peierls was also concerned about progress of the new Atomic Energy Research Establishment(AERE) at Harwell, where the Ministry of Supply had taken over an RAF airfield for the purpose of providing the infrastructure for the research and development of civil nuclear power. To direct the British effort, Air Marshal Viscount Portal of Hungerford was made Controller of Production, Atomic Energy; John Cockroft became director of the Atomic Energy Research Establishment, and Christopher Hinton, a senior ICI engineer, became the leader of the fissile material production programme.[11] At that stage, no decision on nuclear weapons development had been made, but in January 1946, already, William Penney had been put in charge of Armament Research, an appointment which suggested that the British Government was seriously considering the option of an independent development of a nuclear arsenal. In June 1947 Penney was chosen to lead Britain's nuclear weapons program, although this decision was not made public until much later. Secrecy surrounded the early stages of the project, with the press being discouraged to visit Harwell, and with public and political debates about atomic energy suppressed. As one MP put it during a parliamentary debate: 'When an Hon. Member asks the Prime Minister about the atomic bomb, he looks at him as if he had been asked something indecent.'[12] And even when one of the senior members of the AERE, the head of the Theoretical Physics Division

[10] Letters [393–394], [397], [400].

[11] See letters [402], [427].

[12] *Hansard*, House of Commons Debates 1948, Col. 574 (4 March 1948).

at Harwell, Klaus Fuchs, was arrested in early February 1950, charged with violation of the Official Secrets Act, little official comments were passed on the work carried out at Harwell.

The arrest of Klaus Fuchs was a severe blow to the British scientific community as a whole, and it was a particular blow to Rudolf Peierls and his family personally.[13] Klaus Fuchs, who had worked on Tube Alloys, the British atomic bomb research project, had been transferred alongside Peierls to New York and later to Los Alamos to work on the Manhattan Project. During his work on the British and Anglo-American atomic weapons projects he supplied the Soviet Union with valuable information about those, and he continued to do so after his return to England in 1946 when he joined the AERE at Harwell. In the late 1940s, as a result of the successful deciphering of the Venona transcripts, incepted messages that had been sent between several Soviet intelligence agencies, a leak of atomic secrets to the Soviets was identified as leading to the British Mission at Los Alamos, and eventually Klaus Fuchs was isolated as the key suspect. Under interrogation from MI5, he confessed, in January 1950, to having broken the Official Secrets Act by passing on classified information to the Soviets. On the basis of this confession, he was convicted to 14 years in prison in March 1950.[14] Klaus Fuchs had been a close friend of Rudolf and Genia Peierls', he had lodged with them when he first came to Birmingham, and he had collaborated closely with Rudolf Peierls who had not only hired him into his department at Birmingham but had also been instrumental in securing his appointment at Los Alamos. Much of the correspondence in early 1950 reflects this. The letters exchanged with friends an colleagues are part of the attempt to come to terms with the personal disappointment as well as an endeavour to limit the damage done to the scientific communities in the UK and the US.[15]

[13] Letter [493].

[14] Fuchs was released early after serving a little more than nine years of his sentence. After his release he returned to his native East Germany where he continued his scientific career, being elected to the Academy of Science and the Communist Party central committee. Eventually he became deputy director of the Institute for Nuclear Research in Rosendorf, from where he retired in 1979.

[15] Letters [495–497], item [500].

The Fuchs trial was interesting in what it revealed as much as in what it concealed. Many aspects of the case were kept from the public in order to cover important political secrets. A number of people in the highest echelons of British secret intelligence had a distinct interest in ensuring that the Fuchs case would not lead to further disclosures. They and the British Government did not want details of Fuchs' work at Harwell publicised, as he was involved in the then still secret British atomic weapons programme. In Britain, the case vanished from the public view within days of the verdict without Fuchs' spy contacts in Britain ever being apprehended fully. British anger over Fuchs dissipated very quickly. The initial public outrage and the discussion in the press about the wisdom of accepting political and religious refugees and employing them in sensitive fields soon subsided. It never escalated into the Mccarthyism of the US, which was shaken by espionage cases in the aftermath of Fuchs' exposure, when the Rosenbergs, Harry Gold, and the Greenglasses were exposed. One of the victims of the widespread anti-communism, often culminating in political hysteria, was Robert Oppenheimer.[16] Rudolf Peierls never shied away from expressing his views in public. He did so regardless of the effect this would have on is own position. He would later support Oppenheimer openly as well as privately,[17] just as he defended civil liberties in the aftermath of the Fuchs affair in his memorandum 'Lesson of the Fuchs Case'.[18] He was never secretive about his friendships with people from communist countries and of communist persuasion, he argued for the re-establishment of scientific exchange with the Soviet Union and its satellites. He rejected the idea of oppressing the voices of dissenters by arguing that this totalitarian measure would bring security at the expense of values that any democracy had to fight to retain. In the aftermath of the Fuchs arrest, Peierls' overt expression of these views led some to question his reliability, especially in view of the fact that he had access to sensitive and secret information in connection with the UK nuclear programme.[19]

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[16] See below, pp. 442–43.
[17] See e.g. letters [597], [601].
[18] Item [500].
[19] Letter [525].

However, at that time, as on many other occasions during the subsequent decades, it was recognised by people in authority that the views may have been uncomfortable at times, but at no point did they undermine the security and values of democracy in the UK, and at all times did Peierls prove loyal to the UK national interest.[20]

Another issue on which Rudolf Peierls expressed his views clearly and encouraged others to do likewise was the question of how to deal with defeated Germany and the German scientists in particular. The questions engaged the thoughts of many British scientists, and even more so many German-born British scientist many of whom, due to their Jewish origin, had been forced to leave Germany under National Socialism. In early 1948 Peierls circulated a memorandum,[21] which was widely discussed as a string of conferences in the UK required the scientists to clarify their position with regard to the invitation of German colleagues,[22] the organisation of social, non-scientific contacts, and decisions on foreign membership of learned societies, such as the Royal Society which conferred membership to distinguished foreign scientists.[23]

Beyond the attempts of dealing with the legacy of the weapons development, many scientists, especially in the early post-war years, were also concerned with rebuilding the international science community. The totalitarian regimes in Europe had led to a massive displacement of scientists, as had the war itself. Many who had been forced to leave their home countries in the 1930s and early 1940s, had no desire to return and were looking positions in their adopted homes, mostly in the UK and the US.[24] Given the shortage of highly trained scientific staff in UK academia, many such positions could be found. Conversely, filling Chairs even at prestigious British universities often proved difficult.[25] Rudolf Peierls, throughout the post-war era, received numerous offers to take up such positions, including Chairs at Oxford, Manchester and

[20] See below chapter 10, pp. 756–57.
[21] Item [440].
[22] Letters [404–406].
[23] Letters [442–443], [445], [447], [458–461].
[24] See e.g. communications with G. Wick, letters [382], [385], [387].
[25] See letters [388], [390], [415].

London. He considered few of these seriously, but the offer to take up the Plummer Chair at Cambridge was an offer which he thought about very thoroughly, and about which he consulted several close colleagues.[26] The correspondence in this context indicates some important aspects which help to explain his loyalty to Birmingham, but also his clear ideas of what he regarded as important for a prosperous theoretical physics community in the UK:[27] a balanced flexible system that provides good training and high standards without prejudicing against students outside Oxbridge. Beyond the question of securing academic leadership within the UK, there was also the issue of retaining Britain's standing as a recognised centre of research excellence. Rudolf Peierls had come to Birmingham in 1937 as the first professor of Mathematical Physics and had set himself the task of establishing a school devoted to both first-class research and first-class teaching. The war had put the effort on hold, but as soon as Peierls returned to Birmingham, he re-engaged in the process and, virtually from scratch, he developed a school of mathematical physics, or theoretical physics as it would be called later, which was arguably the best in the country and which could compete with any in Europe and with most others globally. His correspondence during the first post-war decade gives some clues as to why he succeeded where many others failed.

- Peierls regarded teaching as his main responsibility.[28] While he enjoyed his research and recognised its importance as a contribution to a discipline which was undergoing exciting developments, increasingly this research was being done in collaboration with research students and younger research staff and thereby became virtually indistinguishable from teaching.

- His enthusiasm for teaching and building up a viable team found expression in time and energy devoted to securing funding for young scholars and finding the best possible people to carry out the

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[26] Letters [383–384].

[27] See in particular letter [384].

[28] R.E. Peierls, *Bird of Passage. Recollections of a Physicist*, Princeton: Princeton University Press, 1985 (cited hereinafter as Peierls, *Bird of Passage*), pp. 249 ff.

increasingly complex research.[29] Within a few years, he had built a reputation for his institute, of being an ideal training ground — a reputation which helped achieving both the above aims; but it also enabled Peierls to work within a group of a critical mass which would always be certain of being supplied with the best of talent from within Britain and from abroad.

- Peierls' commitment to his students and research fellows did not end with the completion of their period at Birmingham. Much thought and letter writing went into the task of securing future positions and exchange opportunities. In this, the prospects of the individual scientist was as important as the future of his own institute at Birmingham.

- Collaboration with the US throughout the war and close contact with many friends and colleagues across the Atlantic, had sharpened Rudolf Peierls' awareness of the role reversal which had occured with regard to academic physics. As early as September 1945, he expressed, in a letter to Raymond Priestley, the Vice Chancellor of Birmingham University, that 'American universities [had] matured a great deal and contact with this country [was] now less important to them, and more important to us'.[30] The consequence of this, in Peierls' view, had to be regular academic exchanges which would allow the UK to benefit from scientific achievements of colleagues in the US. And his attempts to put Birmingham firmly on the academic map in theoretical physics meant that he was keen to secure a sizeable fraction of the exchange for this institution.

- A supplementary ingredient which could not be found in any other institute was what some would later term the 'Genia-factor'. Genia Peierls was an enthusiastic supporter of her husband's endeavours to attract the best young scientists to Birmingham, a place which — with post-war rationing, shortage of housing and

[29]Letters [389], [403].
[30]Letter [381].

generally meagre facilities was not the most appealing location. Her hands-on efforts which ranged from provision of short-and long-term accommodation to general advice, from organising social gatherings to job-advice for spouse and general counselling, had a significant impact on the cohesion of the growing 'Peierls school'.[31]

- Peierls himself had studied in an environment which had encouraged travel and intense exchange of research ideas through students moving from research centre to research centre and of course, through scientific meetings and conferences. Convinced about the stimulation brought about by this exchange of knowledge, he organised two international conferences at Cambridge, in 1948 and 1953. These and the regular seminar programmes organinsed in the department attracted some of the leading figures of the national and international physics communities, including Bohr, Heisenberg, Pauli, Mott, Born, Lise Meitner, Frisch, Oppenheimer, Bethe, and many others.[32]

That a professor would take his teaching and administrative duties seriously was not in itself something that set Rudolf Peierls apart from many of his contemporaries. But he was quite prepared to make personal sacrifices and put departmental interest before his own. One such example was his reaction to Robert Oppenheimer's invitation to spend the academic year 1951/2 at Princeton. The offer was very appealing, not only because of Peierls' friendship with Oppenheimer and the prospect of a year's research largely uninterrupted by administrative chores and teaching commitments. Princeton, at the time, was buzzing with activity in the wake of recent developments in field theory in which Peierls himself was very interested. Nevertheless, he informed Oppenheimer that he could only spend one term at the Institute, because he felt responsible for a large number of new research students and considered it necessary to take his share of administrative responsibility

[31]See *Genia Peierls. Reminiscences collected on the occasion of Genia Peierls' 70th birthday, July 1978.*, copy in *Peierls Papers*, Supp. A.119.

[32]See e.g. letters [445], [447], [451], [456], [474–476].

at Birmingham for the first half of the academic year in question.[33] The war had interrupted some of Peierls' research, most notably the famous Bohr-Placzek Peierls paper.[34] The three scientists picked up the threads in 1945 and communicated extensively about the manuscript.[35] The paper remained unpublished at the time, although the results were written up in a manuscript which several decades later was published in Bohr's Collected Works.[36]

Peierls' closest friend from his student days in Munich, Hans Bethe, had moved to Cornell in 1935. At Los Alamos, the two scientists again had the opportunity to work together closely with Hans Bethe as the Head of the Theoretical Physics Division and Rudolf Peierls in charge of the hydrodynamics group. After the end of the war, the two friends, again, parted to take up their roles as leading theoretical physicists in their respective adopted home countries, the US and the UK. Over the subsequent decades, they remained in close contact through correspondence, academic and social exchanges and, equally significantly, through establishing a network of research collaboration of their students and junior colleagues.[37] Most influential among these was the recommendation of Hans Bethe to Freeman Dyson, in early 1949, to spend some time at Peierls' institute.[38] The fellowship arranged between Rudolf Peierls and Robert Oppenheimer, at the time director of the Institute of Advanced Studies at Princeton, where Dyson, the rising star of theoretical physics was based, demonstrates two points very clearly. Firstly, Peierls was excellent at spotting talent, and secondly he was flexible enough to make Birmingham an attractive option for your scholars to choose his institute despite stiff competition from

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[33] Letter [524].

[34] S. Lee (ed.), *Sir Rudolf Peierls. Selected Private and Scientific Correspondence*, Vol. 1, Singapore: World Scientific, 2007 (cited hereinafter as Lee, *Selected Correspondence*, Vol. 1), Chapter 4, pp. 522–524.

[35] Letters [392], [417], [421], [435], [444], [485–486], [490].

[36] R.E. Peierls (ed.), *Niels Bohr. Collected Works*, Vol. 9 Nuclear Physics (1929–1952), Amsterdam: North Holland, 1986, pp. 505–519.

[37] Letters [396], [401], [412], [424], [426], [428–430], [432], [434], [436], [438], [448], [453–455], [457], [465].

[38] Letters [468–470], [473].

Cambridge, Oxford, Liverpool, Manchester, Bristol and other universities.[39] Dyson was based at Birmingham, but it was agreed that he was at liberty to spend time at Princeton regularly, as long as it fitted in with departmental requirements at Birmingham.[40] This resulted in Birmingham being in direct contact with the development of quantum field theory, which at the time was worked on by Schwinger, Tomonaga, Feynman and Dyson.[41] Others similarly made the journey across the Atlantic, and the exchange went both ways with, among others, Byers, Lieb, Langer, Brown, Dalitz, Salpeter, Claude Bloch and Stanley Mandelstam moving between the US and Birmingham.

Another example of Peierls spotting talent and being slightly unconventional in securing it for Birmingham was Gerry Brown. A native of South Dakota, Brown had studied at Wisconsin and Yale, where he obtained an M.S. and a Ph.D. His short-lived membership of the Communist Party, from which he was eventually expelled, put his academic career in the US at risk despite his outstanding doctoral work with Gregory Breit. Various enquiries to universities in England led to the by now famous three-penny folded airmail return from Rudi Peierls saying: 'Come ahead.'[42] In February 1950 Gerry Brown arrived as a political refugee from pre-McCarthy anti-communist America; in 1960 he left to take up his appointment as full professor of Theoretical Physics at Niels Bohr's Nordic Institute for Theoretical Physics (NORDITA).

While not many of Peierls' students arrived as refugees in the same way as Gerry Brown did, many left into distinguished positions, and a significant number eventually ended up filling the Chairs of the most prestigious European, American, Asian and Australian Universities. Peierls' correspondence throughout the 1950s demonstrates the paths of some of these talented young men and women.

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[39] See also letter [484].

[40] Letters [477], [479].

[41] Letters [507–508], [510], [513–514], [518].

[42] G.E. Brown, 'Flying with Eagles', *Annu. Rev. Part. Sci.* **51**, 1–22 (2002), here, p. 6.

[380] L.R. Groves to James Chadwick

Washington, 10.8.1945

Dear Sir James,
I have received your nice letter of August 9 and I wish to convey through you to all members of the T.A. Directorate my sincere and heartfelt appreciation of their congratulatory message and for their own great contributions to the success of our project.
Sincerely yours,

L.R. Groves

[381] Rudolf Peierls to Raymond Priestley

[location unspecified], 13.9.1945
(carbon copy)

Dear Vice-Chancellor,
As I mentioned to you the other day, I believe that the universities in this country can gain a great deal by an arrangement that will make it possible for visitors from abroad, in particular from the United States, to spend short periods ranging from one month to one year according to circumstances, at British universities.

Such men could have a very welcome stimulating influence particularly in subjects in which at the moment, owing to better equipment and greater manpower, the United States are leading. In addition to the actual benefit derived from such visits, there would be the possibility that some of these men might like the life here, and might make themselves popular with their colleagues here so that they could be induced to accept permanent jobs, and thus help to alleviate the great manpower shortage that now exists among scientists of high standing.

After the last war this type of exchange was largely helped by Fellowships awarded by the Rockefeller Foundation, the Guggenheim Foundation, and similar bodies, most of whom I think have ceased to award such Fellowships, or have restricted them to special subjects (Rockefeller

Fellowships are now only available to men working in medical science and allied subjects).

I do not believe we can expect the Americans to provide new funds for this purpose, largely because American universities have matured a great deal and contact with this country is now less important to them, and more important to us, than in the past. I believe, therefore, that this country could try to provide funds for this purpose. This might either be done by individual universities, or by a national organisation for all universities, and the latter would probably make administration somewhat easier. In many cases such visitors could be given existing jobs, in particular Research Fellowships would appear very suitable; at a time when our own training of young scientists has been held up to a degree which will make it impossible to fill all these Fellowships immediately. In that case the Fund I am suggesting would merely have to provide travel expenses; which I believe should be on a generous scale, and shall allow senior people to come for extended periods and to bring their families; and it should also provide some subsistence allowance to take care of the fact that these people cannot bring their own furniture, and are not familiar with conditions in this country, so that they will not be able to live as economically as people who are at home here.

In the long run it may be undesirable to take up existing Research Fellowships in this way, and it would then be preferable to have the central fund responsible for both travelling expenses and living costs.

I believe most of my colleagues will agree that such a scheme would be of greatest importance to British universities, and I am sure that, in my own subject in particular, it would make an enormous difference. I know that there are many Americans in my subjects. However, this scheme should not start immediately but, say, in a year's time, when we have overcome the immediate administrative difficulties which will result from converting the universities to a peace-time basis.

[R.E. Peierls]

[382] Gian Carlo Wick[43] to Rudolf Peierls

Rome, 14.9.1945

Dear Peierls,
It is a long time now that I gave a friend who was coming to London your address, begging him to enquire about you. But without result. Now I venture to write to you directly, hoping to find you still in your old place. There is not much use now writing about the past years; perhaps we shall talk about our experiences one day. I sincerely hope that you and your family have gone through these hard times unhurt. I, for my part, have been in Rome most of the time. Fortunately I was not called up — except for twenty days in the summer 1943 — and I was not asked to do any war work. If I had, I hope I would have had the courage to refuse, but it would not have been pleasant!

Now I am rather anxious to get back to work, after a month of rest in a small village on the hills. We have received, only now, the Physical Review.* Although scientific production has been slowed down by the war, still four years of Phys. Rev. make a lot of reading! It will take much work to get up to date again. You would help me a lot with a hint or two. 1) I have received from Schrödinger several reprints on a new unitary theory which seems to explain a lot of things.[44] I am

*But not the Proceedings R.S., not the P. Cambr., nor Nature. These will be very hard to get, we are told!

[43] Gian Carlo Wick (1909–1992) a native Italian studied at Turin and later worked at Leipzig and Göttingen before returning to Italy, eventually working under Fermi, before becoming professor at Palermo and then Padova.

[44] Throughout the war, Erwin Schrödinger published a large number of papers, primarily in the Proceedings of the Royal Irish Academy on a variety of subjects. Most notably, in 1943 he published E. Schrödinger, 'The General Unitary Theory of the Physical Fields', *Proc. Roy. Ir. Ac.* **49A**, 43–58 (1943) and in the following year E. Schrödinger, 'The Point Charge in the Unitary Field Theory', *Proc. Roy. Ir. Ac.* **49A**, 225–35 (1944); E. Schrödinger, 'Unitary Field Theory: Conservation Identities and Relation to Weyl and Eddington', *Proc. Roy. Ir. Ac.* **49A**, 237–44 (1944) and E. Schrödinger, 'The Union of the Three Fundamental Fields (Gravitation, Meson, Electromagnetism)', *Proc. Roy. Ir. Ac.* **49A**, 275–87 (1943).

very intrigued and I would study this theory carefully if I were not engaged in a number of complicated papers on nuclear forces, strong interactions and so on. I have never bothered very much about unitary theories. What is the general opinion about this particular one? 2) Has non-linear electrodynamics made any important advance? 3) Has the theory of nuclear reactions made any important advance? I am specially interested in this, as I am writing a book with Amaldi;[45] I have left aside until now the chapter on the theory of resonance phenomena and so on; I expected that the paper by you, Bohr and Placzek would oblige me to change much of what I might have written. Has the paper ever appeared? Will it ever appear? Which subjects would be especially affected by it? I should be very indebted to you, if you were so kind to answer, even very briefly, to these questions. Have you heard of Amaldi's experiments on neutron scattering? They seem quite interesting.
With kindest regards, also to Mrs. Peierls,
Yours truly

Gian Carlo Wick

[383] Rudolf Peierls to John Cockroft

Washington, 13.12.1945
(carbon copy)

Dear Cockroft,
I have just received an offer of the vacant chair in Cambridge. I find it extremely difficult to make up my mind. I am tempted by the better supply of students in Cambridge, but doubtful about the chances of having contact with a strong experimental nuclear physics team in Cambridge. This depends on a number of factors, in particular on who is likely to be your successor in the Jacksonian Chair.[46]

[45] The book does not appear to have materialised.

[46] John Cockroft had held the Jacksonian Chair since 1939 but was leaving Cambridge to become the first director of the Atomic Energy Research Establishment at Harwell in 1946. In 1947 Otto Frisch accepted the Jacksonian Chair. See letters [415] and [428].

If you have any information about what is likely to happen there, or know anything more generally about the future of fundamental physics in Cambridge, I would be very grateful for your advice.

I am planning to go to Washington to see Chadwick and shall probably be there on December 19 and 20. If you plan to be in Washington at the same time, do not bother to reply. Otherwise, would you be good enough to send a copy of your reply to Washington for me.
Yours sincerely,

[Rudolf Peierls]

[384] Rudolf Peierls to Raymond Priestley

[location unspecified], 13.12.1945
(carbon copy)

Dear Priestley:
After having successfully disposed of an invitation from Oxford and one from Manchester, I have now received an offer of the Plummer Chair in Cambridge. As you can imagine, I find it somewhat harder to make up my mind on this. My personal preference is strongly on remaining in Birmingham. I have enjoyed the work in the University. I am looking forward to the collaboration with Oliphant on his new plans, and I like the Administrative machinery of a modern university which in Birmingham seems to run particularly efficiently and smoothly.

I feel I should not decide on personal preference alone, but the overriding consideration ought to be the chances of success in starting a school of theoretical physics and training up a new generation which is very badly needed, and I feel my decision should be based entirely on where there are the best prospects of success in this.

On this question, too, there are arguments on both sides. I believe research in theoretical physics can be successful only in close contact with experimental work and I believe I shall find much more fruitful collaboration with Oliphant's department on the kind of fundamental problems that I would be particularly interested in than I could expect in Cambridge. I also know that in any changes of policy, both as regards the teaching and organisation of research, I should find more support in Birmingham than the somewhat cumbersome machinery in Cambridge would make possible.

The important factor on the other side of the balance is, of course, the availability of good students and the present examination and scholarship system which, particularly in mathematical subjects, canalizes the supply of first-class students to Cambridge. If this situation remains unchanged my choice, therefore lies between first-rate students in Cambridge and between a first-rate set-up with a restricted supply of students or a plentiful supply of students with a somewhat difficult set-up.

This is true unless it is possible at this moment to break the vicious circle which attracts all good students to Cambridge and Oxford because of their high reputation and maintains a high reputation because they get the best students. For several reasons it might be the best psychological moment to change this now in a limited number of subjects, and it might be possible to attract to Birmingham a research team of graduate students, including some who had their undergraduate training in Cambridge.

I have put down my view on this question in so much detail because I think it is only fair that you should know the position fully, and also because you may have more definite views on the chances of putting the provincial universities, and in particular Birmingham, in a better place compared to Cambridge than was possible in the past. In that case, I would greatly appreciate your advice.

I would also like to make it clear that, in writing this letter, my object is not to obtain any further promises or concessions from the university. You probably know that in the case of the offer from Manchester, I wrote to Haworth in order to clear up beyond doubt a point on which I was almost satisfied, but on which a brief statement in Faculty minutes had raised some slight doubt. The reply from Haworth was very enthusiastic and gratifying and I am now satisfied that I can expect from the Faculty and from the other University authorities all the support and assistance I can reasonably expect. The one attraction of Cambridge which makes me hesitate now, the supply of first-class students, is clearly not a point for bargaining.

In replying to Cambridge, I shall try and postpone a decision until my return to England, which I expect to be approximately February 1, and I shall say that, whatever the decision, I shall not consider moving before the summer since I think the least I owe to the University of Birmingham is to return and get things running again after they have patiently put up with my absence for so long.
With kind regards,
Yours sincerely,

[R.E. Peierls]

[385] Rudolf Peierls to G.C. Wick

[location unspecified], 14.12.1945
(carbon copy)

Dear Wick:
I have just sent you a cable asking you whether you would be prepared to consider a research job in Birmingham. By the time this letter reaches you, you may already have replied one way or another but, in any case, I am writing to give you a little more explanation than is possible in a cable.

I intend to be back in Birmingham around February 1 and I shall then have a separate department of mathematical physics with a rather small staff. There is, at the moment, only one man besides me, who is interested in research,[47] but I have now managed to get funds from which to create new research fellowships, and we will be able to get more good people, provided we can find them. If you would find it possible to come, I think the least we could offer you is a research fellowship at £800 p.a. which would carry no teaching duties except that you would

[47]Hugh McManus had already been a research student in 1938/9. After the war, his results could not be reconstructed and he had to restart his career as a research student with work on infinite self energy. Peierls, *Bird of Passage*, pp. 137, 227. McManus eventually moved to Chalk River, the Canadian Atomic Energy Laboratory and later became professor of physics at Michigan State University.

probably like to give a small number of lectures, say three or four a week, to senior students. The post is not a permanent one, but it is not limited to any definite period, and can be held for a number of years. The shortage of competent theoretical physicists in England is now so great that I do not have any doubt that you would find an equivalent or better job before very long.

My department is in close contact with Oliphant's department, which is expanding greatly and which I hope will be one of the centres of fundamental research. Oliphant has plans of constructing a machine for accelerating particles to energies of the order of 10^9 volts. He has quite a good staff including Moon.

There is no specific date at which we would want you to take up your duties except, of course, that the sooner you can come the better. I have no knowledge of what difficulties we shall meet in greeting the necessary papers and permits, but I shall try to do my utmost to get this through in as short a time as possible.

I should express that technically, the appointment to such a fellowship has to be made by the university authorities, but I do not anticipate any objections from them. I have heard rumours to the effect that you have been offered a job in this country and it is not for me to give you advice one way or other, but personally I would very much hope that you still can and will choose England, since I would be looking forward to collaborating with you with great pleasure. It might be useful to bear in mind that there is an extreme shortage of good people in England, and for this reason advancement to senior and individual positions is comparatively easy (there are at least three professorships vacant now). I personally find life in England particularly attractive, even with the postwar difficulties in housing, food and clothing that are likely to continue for some time, but this clearly is a personal matter.
With best wishes,

[R.E. Peierls]

[386] Rudolf Peierls to James Chadwick

Washington, 14.12.1945
(carbon copy)

Dear Chadwick,
I was trying to arrange to come to Washington to see you after your return to this country, but I find transportation conditions at the moment so difficult that it would probably mean a great loss of time. I therefore decided to abandon the idea for the present, but I could always arrange to come if you think there are important matters to be discussed. I shall, in any case, try to speak to you on the phone early next week. In any case, I am planning to leave here finally on January 10 and expect to be in the East from January 14 until my ship sails, which will probably be around January 25. I intend to spend some days in Washington during that period.

Placzek will be able to tell you most of the news. I believe the most conspicuous fact since your departure is the amazing deterioration of morale in this place. There is very little work going on beyond writing up the "encyclopedia"[48] and many people whose original intention had been to stay on for the summer have decided to get out. Bradbury[49] is trying to rally his remaining forces and things are a little better now because, in most places, the new men have been appointed who will act as division and group leaders during the interim period. They have taken over the work in most places, and naturally pursue it more intensely than the people who are about to leave anyway. As regards the British team, Bretscher and with him French[50] have still

[48]After the development of the first operational nuclear weapons, the scientists of the Manhattan project wrote up the scientific developments in what became commonly known as the 'Encyclopedia'.

[49]Noris Bradbury (1909–97) replaced Robert Oppenheimer as director of the Los Alamos Scientific Laboratory in 1945 and led it for 25 years until 1970.

[50]Egon Bretscher (1901–1973) and Anthony P. French (1920–) both returned to England in 1946, the former to take up a post at Harwell, the latter to work at the Cavendish in Cambridge. French emigrated to the US in 1955 to join the Physics Department at the University of Carolina. In 1962 he moved to MIT.

plenty of work to do and I believe are quite satisfied with their facilities. Bretscher still is quite happy to stay on until summer, except that he is somewhat worried about his relation to Cambridge since all letters from there seem to imply that he is expected there immediately. I hope you have brought some information with you that might clarify his position.

Tuck has been given a free hand in doing experiments of fundamental interest and seems quite satisfied that these would keep him supplied with interesting work until June.[51]

Similarly, Titterton[52] has plenty of work to do; the proposal to make him group leader did not materialise after all, because it turned out that Hans was prepared to stay on. He is somewhat senior to Titterton and, in addition, is likely to stay not only during the interim period but also after June and it is therefore, better for continuity for him to be in charge of the group. In this connection, I found that the letter that Hans wrote to you on the subject of Titterton's salary was a spontaneous action of Hans on taking over the group and should, I think, be taken as a confirmation of the high value he places on Titterton's work.

Fuchs feels that when he has completed his writing he would be of more value in the development of the new Establishment[53] than he could be here and is, accordingly, planning to leave some time in February.

Skyrme[54] has been asked to remain until June and will do so unless pressure is brought to make him return to England.

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[51] James L. Tuck stayed on at Los Alamos and worked with Teller on the development of thermonuclear weapons.

[52] Ernest W. Titterton (1916–1990) continued work at Los Alamos until 1947, when he returned to England to become group leader in charge of nuclear emulsion and cloud chamber research at Harwell.

[53] Klaus Fuchs returned to England in 1946 to become head of the Theory Division at Harwell.

[54] Tony H.R. Skyrme (1922–1987) returned to a research fellowship at Birmingham after the war before moving on to M.I.T., Princeton and eventually also to Harwell as head of the nuclear physics group in 1950. In 1963 he succeeded Rudolf Peierls as Professor of Theoretical Physics at Birmingham.

Frisch[55] is completing his work and plans to leave approximately in February. Hughes[56] seems quite happy to work in Frisch's group and might even stay on after Frisch has left. Marshall, however, is still anxious to get back home at the earliest opportunity; I hope you have brought some information on the possibility of his returning to university work.

Placzek[57] will no doubt tell you himself about his plans.

You will have heard about the interviews with the various prospective staff members for the Establishment. In discussing who was or was not interested in the interview, it turned out that Bretscher was quite interested in the possibility of a job in the Establishment. I encouraged him to go to the interview with the others, but he evidently decided not to go. I have not seen him since then, since I was on vacation and he went on vacation just before I returned, but I believe what was in his mind was that he was anxious to talk the situation over with you before discussing any new job more seriously. He is still away, but expected back here on December 22, and I imagine will be anxious to see you as soon as possible after that.

While you were away I had offers of jobs from Oxford and Manchester, which I turned down after some reflection. I now had an offer from Cambridge and on this I find it very hard to make up my mind. I have one or two questions in that connection that I would like to ask you and I shall try to do that on the phone.

You may be interested to know that the censorship here has now been lifted and, in general, security has been relaxed a great deal. For

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[55] Otto Frisch (1904–1979) returned to England after the war to become head of the nuclear physics group at Harwell before taking up the Jacksonian Chair at Cambridge in 1947.

[56] Donald J. Hughes (1914–1960) had worked at the Metallurgical Laboratory. He was a member of the committee charged with studying the social and political implications of nuclear weapons which produced the so-called Franck Report. Hughes later became Head of the Neutron Physics Measurement Group at Brookhaven.

[57] George Placzek (1905–1955) had moved from the Canadian Nuclear Research Laboratory at Chalk River, where he had been head of the theoretical physics division, to Los Alamos in 1945. A year later he transferred to General Electric Co., before joining the Institute of Advanced Studies at Princeton in 1948.

example, in many cases friends or relatives have been allowed to come to the site as visitors.

Are you planning to come out here again in the near future? There are plenty of people who would be very happy to see you here, although I am afraid I cannot promise much in the way of startling new developments.

Yours sincerely,

[R. Peierls]

[387] Rudolf Peierls to G.C. Wick

[location unspecified], 18.12.1945
(carbon copy)

Dear Wick:

Thank you very much for your prompt reply to my cable. I was rather afraid that it might be too late, and if the reason against your being able to consider an offer is that you have already accepted the job at Notre Dame, I feel very foolish not to have thought of asking you in time.

Since I wrote to you there has been a further development since I have been offered Fowler's professorship in Cambridge. At the moment, I am very undecided as to what I should do. The offer is quite attractive, yet there are many reasons for staying in Birmingham. My point that affects my decision is what will happen to Birmingham if I leave. They will find it difficult to replace me because of the well-known shortage of theoretical physicists in England, and I feel under a strong obligation to them.

If I decide to go to Cambridge, I do not think they could do better than offer the job to you. Naturally, this is a matter for the Faculty at Birmingham to decide and I can only advice them, but I believe that my advice would carry some weight. I imagine that your answer to my previous enquiry would equally apply to such an offer if made at this time. However, if you think that the offer of a full professorship might put you in a better position to try and get released from commitments already entered into, please let me know. In any case, I would probably not leave Birmingham until next autumn and you might still be prepared to consider coming then or a little later.

Please tell me frankly what your feelings in this matter are. It is, of course, quite possible that having once gone to this country, you would want to remain here, particularly since you are liable to be offered better jobs fairly soon.

All this must, of course, at the moment be regarded as unofficial and confidential, but having an informal reply from you will help me greatly, both in making my own decision and in advising the Faculty at Birmingham.

Quite apart from all this, there is the question whether, on your way to America, you might find it possible to visit England for a short period. Clearly, if you catch a boat from England this would be very easy. If you were proposing to get a boat directly from Italy or elsewhere, it might raise some complications with transportation and papers. However, if it can be done at all, I would be very happy to see you again and have some discussions. I am sure that we could arrange to have your expenses met.

My movements are as follows: I shall leave here on January 10; can be reached after than until about January 24 through: c/o J.F.Jackson, P.O. Box 680, Benjamin Franklin Station, Washington 4, D.C. I hope to be back in England around February 1 and my address is then The University, Birmingham 15.
Yours sincerely,

[R.E. Peierls]

[388] Rudolf Peierls to James Chadwick

Washington, 4.1.1946

Dear Chadwick,
I have disobeyed your instructions to some extent and I have had a brief and very informal conversation with Bob Wilson about Cambridge. In doing so I have, of course, made it perfectly clear that this was at this

stage merely a vague idea — that as far as I knew the idea had never even yet been discussed by people concerned in Cambridge, and that I was talking to him purely on my own initiative to find out what the chances were of getting him into such a scheme.

I did not, of course, expect on this very vague basis to get any definite reply, but this reaction was sufficiently favourable to make me think that there is a fair chance of his accepting and it would certainly be worthwhile, in my opinion, to pursue this proposition with all possible energy. What I discussed with him was the idea of his going to Cambridge for a limited period of a few years, and as was to be expected, this idea would appeal to him more than a permanent job, provided, of course, he would not have to spend the greater fraction of his time in just preparing future developments and designing equipment that would not materialize during his period there.

At present his plans are, as you know, to go to Harvard, but he proposes first to go to Berkeley in order to design there a new cyclotron for Harvard. Clearly, if he was not going to go to Harvard for the time being, he would not be particularly interested in spending his summer on developing equipment and, for that reason, I believe that the chances of getting him would rather depend on how soon a definite approach could be made. This, of course, does not mean I am certain he would accept — one of the questions being whether he can arrange for leave of absence from Harvard or otherwise ensure that he will not spoil his position here and find himself stranded on his return.

Yours sincerely,

R. Peierls

[389] Rudolf Peierls to Niels Bohr

Birmingham, 21.2.1946
(carbon copy)

Dear Uncle Nick,[58]

(If you will permit me to retain this form of address which is now obsolete, but which we all got to like very much).

As you will see from the heading I have now returned to Birmingham, and am trying to get back to a somewhat more normal mode of life.

Before leaving America, I had received a message asking whether I would go to Cambridge to take Fowler's chair. As you can imagine, I found it hard to make up my mind on this, since Cambridge is so very attractive in many ways, and in particular because of the number and quality of students available there. After long deliberations, however, I finally decided to stay in Birmingham. My chief reason is that I like the spirit of that place, and that I think, it is most important, for any theoretical team to be in close contact with a life experimental group, and I have much more assurance of finding that here, with Oliphant and Moon close at hand. In addition, I think it would be healthier for the whole country, of the modern universities were to be strengthened and if there were subjects which are well represented in some of them, Birmingham seems a good place for this, and the present a very suitable time.

This means I am now trying to build up a good research team here. I have already one or two people for a start, and there are visitors whose presence will certainly help to create the right atmosphere. One of them is Jensen, a pupil of Møller's, who is here primarily to learn experimental physics, but who does show considerable interest also in theoretical problems.

I am naturally most anxious to hear about any other promising men whom we could induce to come here, and if you hear of any such people whose plans are unsettled and who are, or might be, considering to come

[58] At Los Alamos, Niels Bohr's code name was Uncle Nick, an address which Peierls continued to use after the war.

to England, I would be very grateful, if you could bear Birmingham in mind. I think it will be reasonably easy to get financial support for them, if necessary.

I hope you and your family are well, and are at last being given a chance to be all together.

With best wishes,
Yours sincerely,

[Rudolf Peierls]

[390] Rudolf Peierls to James Chadwick

[Birmingham], 26.2.1946

Dear Chadwick,
I hope you will forgive me for not writing to you until now, but you probably understood that, in addition to the first rush of business, I had the problem of deciding where to go and also where to put my family.

As rumours travel, you have probably already heard that we have decided to remain in Birmingham, and I think the chief argument that finally tipped the balance was the rather attractive prospects of experimental physics in Birmingham as compared with the uncertain situation in Cambridge and the need, in general, to build up the modern universities to get a fairer share of the good students, not to the detriment of Cambridge, but to reestablish fair proportions. I also felt that the spirit of Birmingham University as a whole and the greater flexibility of the modern university will make a lot of difference.

I realise, of course, that this makes things rather difficult for Cambridge, but I do not think the problem is insoluble. I have suggested to Bragg that they should consider Casimir,[59] and Mott is backing me on that. An alternative would be L.H.Thomas,[60] whom one might have a chance of persuading to return.

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[59]In 1942 Hendrik Casimir (1909–1945) had joined the Philips Research Laboratories in Eindhoven, where he remained until his retirement in 1972.

[60]Llewellyn Hilleth Thomas (1903–1992) had studied at Cambridge before taking up a professorship at Ohio State University in 1929.

You may have seen that I wrote a short and rather superficial article for the Sunday Dispatch,[61] at their request.* It is not a paper of which I approve, but the practice seems to be for everybody to write for papers indiscriminately as long as they are left free to say what they want. I was somewhat hesitant over the procedure that should be followed, since clearly there would not be time to send such an article to you for approval.

I did, however, attempt to write it in such a way that I was certain it contained no unpublished information other than obvious deductions from such information and that it was not likely to upset anyone on the other side. I also had it cleared by Akers,[62] who submitted it to the appropriate authorities in the Ministry of Supply.

Frightened by other people's experiences with papers, I made it a condition that they should not make any alterations without my approval. This condition was accepted and, surprisingly enough, observed.

There exists now a somewhat complicated situation with the Committee of Atomic Scientists. I believe you had a letter from the organisers of the committee, which at present is sponsored by the Association of Scientific Workers.[63] It consists of about twenty people whose names happened to be known to the sponsors, and at a meeting we had a few days ago, we all pressed strongly for a change in the set-up, in which the committee, in order to be able to speak for anybody, should contain at least representatives elected by the groups still existing in places where project work was being carried out. In addition it was felt widely that it would be better for such a committee to be independent of the Association of Scientific Workers, since such a connection would only antagonise

*Copy enclosed.

[61] *Sunday Dispatch*, 17.2.1946.

[62] Wallace Alan Akers (1888–1954) had been co-director of Tube Alloys during the Second World War and returned to ICI after the war as research director.

[63] The National Union of Scientific Workers was founded in 1918. In 1927 it changed its name to the Association of Scientific Workers; in 1968 it amalgamated with the Association of Supervisory Staffs, Executives and Technicians to form the Association of Scientific, Technical and Managerial Staffs.

certain people. It is, however, not yet clear, how an independent committee could be financed, unless, like the new American Federations of Scientists, it collects members from all branches of science outside the project. On the advisability of this step on other grounds there is much controversy. I personally feel that the two activities at present attempted by the Association of Scientific Workers, namely to act as a trade union for scientists, and to express the general view of scientists as unbiased experts, do not mix and should be carried out by separate bodies.

Everybody is asking now, how soon you will be back and I hope it will not be so very long now.
Yours sincerely,

R. Peierls

[391] James Chadwick to Rudolf Peierls

[location unspecified] 6.3.1946
(carbon copy)

Dear Peierls,
I have just received your letter of February 26.[64] I am very glad to hear that you have settled down already but cannot help but feel sorry that you have decided not to go to Cambridge. I appreciate your reasons and sympathise with them, and even share some of them, but I still regret the loss to Cambridge. I hope Bragg will consider the suggestion of Casimir seriously for this would be quite a reasonable solution of the difficulty.

I had not heard about your article in the Sunday Dispatch, and I am glad to have a copy of it. I think it is very good and you have avoided most adroitly a number of pitfalls while still appearing to give some information. There has been quite a lot of publicity on this side and a good deal of sheer nonsense. The chief topic we have to avoid in

[64] Letter [390].

discussion of the Navy Test is any mention of British participation.[65] The point is that they do not want to have to refuse requests from other countries, which might ask to send representatives, if it were known that we were sending observers as well as taking part in the work. A second topic is the animal experiments. Here they fear pressure from the anti-vivisectionists, who are very active just now. A little has leaked out but the arguments have been played down by saying that the animals are nearly all rats.

The preparations for the tests are most hectic. My own opinion is that there has been too much display and that it would have been better to carry out the operation quietly; also that they are attempting far too much in the way of measurements and observations. I think that much of the work is not necessary for the real purpose of the test.

Some of the scientists are more active than ever in public affairs. The young Federation is doing quite well, although I do not agree with a good deal of what they say publicly. I think they have gone too far and that they have lost support by being too emphatic, but they have not yet fallen into discredit. Urey[66] continues to be very vocal and I am afraid he is now losing ground. If Groves seizes his chances, he will be able to re-establish himself, after having almost lost the game. It is a very odd situation at the moment.

Yours sincerely,

J. Chadwick

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[65]The immediate post-war years were characterised by tensions in Anglo-American nuclear, defence and other relations. British hopes for 'full and effective co-operation', as postulated in the Quebec Agreement between the US and Great Britain in 1943, were not fulfilled in essential nuclear development matters. Technology transfer was limited to basic scientific research only. It did not apply to development, design, construction, and operation of plants *per se* and was in fact limited to mutually advantageous ad hoc arrangements. At that time great care was taken by the US not to raise awareness of continued British presence at Los Alamos or indeed at other venues of nuclear work. This was most evident during 'Operation Crossroads', a series of nuclear tests carried out on the Pacific island of Bikini in July 1946. See S.Lee, 'Birmingham — London — Los Alamos — Hiroshima: Britain and the Atomic Bomb', *Midland History* **27**, 146–64 (2002).

[66]Harold Clayton Urey (1883–1981), had been Director of War Research, Atomic Bomb Project, Columbia University, between 1940 and 1945; after the war he moved to the Institute of Nuclear Studies at Chicago.

[392] Niels Bohr to Rudolf Peierls

Copenhagen, 7.3.1946

Dear Peierls,
Thank you very much for your kind letter[67] which recalled so many pleasant remembrances of the time we all spent together at Los Alamos where you and your wife looked after Aage and me with so great kindness. It was a great pleasure to learn from your letter that you are now back in Birmingham and, after your great contribution to the war work, are able again to concentrate on general scientific problems and to cooperate with Oliphant and Moon. I shall certainly bear in mind your offer for a time to take up in your group a promising young physicist for whom a stay with you will be such a profitable experience. We are also here trying to reorganize our experimental as well as our theoretical work and I am myself endeavouring to complete the various investigations which I had to leave unfinished on my escape from Denmark. Among these I have an especially bad conscience about our common work with Placzek and I wonder whether there might soon be an opportunity where we could meet again to look properly into it.[68] I need not say how great a pleasure it would be to us all here if you would soon be able to visit us. My own plans are somewhat unsettled, but there is the possibility that I might come to England some time in May, and as soon as I can survey my obligations I shall, of course, write to you again.

With kindest regards from us all to your wife and the children, and also to common friends in Birmingham,
Yours,

Niels Bohr

[67]Letter [389].

[68]Georg Placzek, Niels Bohr and Rudolf Peierls had been working on a joint paper on resonance processes since the late 1930s. They published a short note in *Nature* with the intention of publishing a fuller exposition of the argument in a separate paper. Lee, *Selected Correspondence*, Vol. 1, Chapter 4. Their work had been interrupted by the war. At the conference in Copenhagen in September 1947, it was agreed that Peierls would complete and update the earlier draft of 1939–40.

[393] G.P. Thompson to Rudolf Peierls

London, 8.3.1946

Dear Peierls,
I send you herewith a M.S. in the hope that you will look through it. It contains an idea on the generation of nuclear energy from deuterons. I have discussed it with Blackman[69] who has helped in some of the calculations, and consulted Akers. He agrees with me that it is of sufficient importance to ask you to give an opinion, and I sincerely hope you will be able to do so.

Some of the quantities concerned are uncertain, notably the cross-sections. Perhaps you may know better figures than I have been able to find. The working conditions chosen are rather arbitrary, and I think it would be necessary to put up the quantities I have called X and H in order to retain a sufficient share of the products of the reaction to make the reaction self sustaining as regards temperature.

What I am most concerned about, however, is not whether the reaction would or would not be self sustaining, but whether the general scheme is sound. If it is, and the balance is not too far on the wrong side, somebody will probably be able to make it work even if some of the heat produced has to be fed back as electrical energy to keep the thing going. Besides that it would be a formidable source of plutonium using very little uranium and making comparatively little demands on cooling. It would probably be cheaper than a pile and would have at least political importance. For these reasons, if the scheme is sound, Akers wants to take out some kind of patent in my name.

Please let me know if you are likely to be in town any time so we can arrange a lunch, or if necessary I could come to Birmingham.
Yours sincerely,

G.P. Thompson

[69]Moses Blackman (1908–1983), lecturer at Imperial College, London, was working with G.P. Thompson on a gas discharge apparatus for a nuclear reactor. For details see M.G. Haines, 'Fifty Years of controlled fusion research', *Plasma Phys. Control. Fusion* **38**, 643–56 (1996).

[394] Rudolf Peierls to G.P. Thomson

[Birmingham], 12.3.1946
(carbon copy)

Dear Thomson,
Thank you for your interesting letter and manuscript.[70] I find myself in a very peculiar situation when I try to comment on this problem. The reason is that I am in possession of a great deal of information obtained on our topics by work at Los Alamos. This work concerns a part of the project which is regarded as particularly secret, and I am sure it would be felt very strongly that I ought not to pass on any information except to a very limited number of people.

I had previously obtained from Sir John Anderson permission to mention similar matters to members of the Technical Committee,[71] provided that they were warned to keep this information to themselves. This clearly means that I should also be able to talk freely to you about this, but if I do so it might handicap you in working along such lines with the help of further collaborators, and, for that reason, it seems to me possible that you might prefer not to receive this information from me.

If you wish to carry on work utilising the information I have brought on this subject from Los Alamos and to disclose it to other members of your staff, my feeling is that it would be advisable to report this to Chadwick and get his comments as to likely reactions in America.

I would like, however, now to make at least one remark, which I think I can make without drawing on this special knowledge, and that is that the beginning of your manuscript refers to heating the substance to temperatures of the order of 100,000 electron volts. It seems to me that this is the crux of the matter and that this will be extremely difficult, because, while it is true that a magnetic field will diminish the heat conductivity, it will not completely prevent the transmission of

[70] Letter [393].

[71] In the Tube Alloys project not all the information was distributed to all members of the project but rather kept within subcommittees.

heat unless the magnetic field were infinitely large and, for that reason, the amount of energy that will have to be fed into the system to reach or maintain this high temperature may well prove prohibitive. I find in your manuscript no estimate of the required energy input. I may have overlooked this and you may have considered this point, but it seems to me the chief difficulty.

For the rest, any comments I could make are of a more detailed kind and could not be made without using this special kind of information I possess, and I do not want to do so before knowing whether this is your wish. I shall not spend much time in London during the next two weeks or so, since I am tied up with lectures very heavily, owing to the illness of a member of staff here. I shall be glad to meet you in London early in April if you think that further discussion of the matter can wait until then. If you were able to come to Birmingham I should be very glad to see you here almost any time.

Yours sincerely,

R.E. Peierls

P.S. Please also regard as confidential information the fact that I know aspects of the problem that are specially secret, since it is not too hard to guess what these aspects might be.

[395] Rudolf Peierls to James Chadwick

Birmingham, 12.3.1946

Dear Chadwick,

Thank you for your letter of March 6th.[72] I have since seen Bragg again and he has approached Bohr on the question of Casimir and got a very favourable answer. My impression was that Bragg was in favour of this plan and felt he would be able to carry the other electors. It is not certain, however, whether Casimir would accept, although I think the chances are fairly good.[73]

[72] Letter [391].

[73] Casimir remained at Philips Research Laboratories. See letter [390].

The fact that the British observers are participating in the naval tests seems fairly widely known in this country, and in an introduction to my article the Sunday Dispatch, in fact, stated this. They said that British observers were on their way to the Pacific now. They did not get this information from me, and I did not think I could or should prevent them from printing this. In other newspapers I have seen lengthy accounts of the numbers and types of animals that are being used in the tests; I do not know, of course, whether this information is correct.

My own feeling on the tests is that the first two are not likely to give much information that could not be derived from blast measurements at Trinity and observations at Hiroshima, together with some commonsense and some model experiments on the formation of tidal waves. Their main effect would seem to me to be to delay the third experiment, which, of course, is the really interesting one.

You may be interested to know that an attempt is now being made to set up an Association of Atomic Scientists,[74] in some way similar to the first federation formed in America.[75] This started as a committee called together by the Association of Scientific Workers, whose papers, I believe, you have seen, but it has now been agreed, and I think very wisely, that it would be best to have such an organisation independent of the Association of Scientific Workers. A provisional committee has been formed of which I am a member, and it is expected that within a week or so things will have reached the stage where one can enroll members. In the meantime, the committee is taking over all the work started by the committee of the Association of Scientific Workers.

We are trying to avoid some of the mistakes made by the Americans, by insisting from the outset that a proper division should be made between statements of the scientific facts and opinions held by scientists, and that on the latter type of question the association should not express

[74]The Atomic Scientists' Association was set up in 1946 primarily to educate British public opinion about nuclear matters and to make a case for international control of atomic energy

[75]The Federation of American Scientists had been formed in 1945 by atomic scientists from the Manhattan Project to address a broad spectrum of national security issues of the nuclear age and to promote the humanitarian uses of science and technology.

any definite views or advocate any definite policies. Their job should primarily be to promote discussions which will help to make clear the implications of such views or policies and to make available statements of such views drawn up by individuals either inside the association or outside on their own behalf. They should, as an association, advocate views on such questions only if, after submitting proposals to all the members, they are found to be shared by all, or essentially all, the scientists in the association.

We have considered the question of widening this, as the Americans have done, into an organisation including all scientists in this country, but it was felt that this would be invading territory which at present other associations such as the A.Sc.W. and the British Association regard as their own, and that it would be wiser to wait until means of working through existing organisations had been explored, without, in doing so, getting definitely labelled in any political direction, as would be the case under A.Sc.W.

As soon as the necessary steps have been taken, we will, of course, send you all the literature about the new association, and even if in your present position[76] you would consider it unwise to become a member of it, we will, of course, always be grateful for any advice or opinion that you care to express.

I am very disappointed at the rate at which things are moving in the project in this country and at morale throughout the project. I have been trying to think out what are the main factors responsible for all this, and, while this letter is getting too long to get into details, I hope to write to you soon to explain my views on this.

With best wishes,

Yours sincerely,

R. Peierls

[76]Chadwick was still Head of the British Mission to the Manhattan Project.

[396] Rudolf Peierls to Hans Bethe

[Birmingham], 14.3.1946
(carbon copy)

Dear Hans,
Thank you very much for your article, which I think is admirable.[77] I believe it will fit into the series without altering a single word. This completes the series, except for the article from Phil Morrison from whom I have not heard yet.

As what you sent me was a carbon copy, I assume that you have sent the original to Groves' office. They have in the past been quite good about clearing things quickly, in particular when reminded from time to time, and Mr. MacMillan in Chadwick's office is looking after that. If, by any chance, you should not have sent your article to Groves' office, McMillan will get in touch with you directly.

I am in your debt in many ways, owing you (a) a letter, and (b) a nickel. On the latter, will you please begin to charge interest.[78] In other words, I have decided to stay here, largely because the spirit of the place is more attractive than it appears to be now in Cambridge, and it is hard to predict how Cambridge will change in the near future. It is, of course, possible that a good man will succeed Cockroft, but even then the whole administrative machinery is very cumbersome and conservative. Birmingham is rather fun and I have the ambition to put Birmingham on the map and get a good team of research workers started here, which would, I think, be a healthier thing from a wider point of view than to begin concentrating everything at Cambridge. People in Cambridge were naturally disappointed and they are now trying to find an alternative solution. There are, I think, one of two possibilities which would work quite well; if they are reasonable about it, but I know that amongst others, your name is on their list and I am trying to tell them that to attempt to offer this job to you would be a waste of time and of a postage stamp.

[77] Peierls helped put together a series of articles on atomic weapons as part of the Penguin News Series. *Science News* **6**, London: Penguin, 1948.

[78] Bethe and Peierls had had a bet over Peierls' future career.

Even in Birmingham, with all the help from the University, it takes, of course, time to build up. So far I have 1 (one) research student,[79] with two more expected in the near future and one apparently good undergraduate finishing in the summer who might stay on. There is practically any amount of money for fellowships, so that I could get more people if I could find them. In particular I am trying to induce Skyrme to leave Los Alamos, but have not heard from him as yet.

The process of settling down and getting rid of excess papers etc. is a very painful one, as no doubt you have found yourself, and it is not made easier by the fact that a week or so after my return one of our lecturers fell ill and I had to take over most of his work, which, for a month or so, involves me in about thirteen lectures a week, mostly to large classes of engineers. I do not regret this, because I always meant to try these engineer courses in order to see whether they cannot be given in a more reasonable way and it is rather fun. Anyway, there are only two more weeks of this and, on the principle of the old story with the pig and the goat, etc., I shall then find that never before in my life did I have so much free time.

My one research student is now tidying up the question of the integral theory of the electro-magnetic field. As you know I was going to try this out. So far it appears that the classic side of this is perfectly straightforward and contains no snags. We are trying to apply this to some specific problem, such as the emission of radiation by an electron, to see how it works, but the real question is, of course, that of quantization and I have no confidence that this can be done.[80]

You will by now have received an invitation to the conference in Cambridge in July.[81] It is, I think, unfortunate that the organisers are not able to pay people's fares but only their stay in Cambridge. Even so, I very much hope that you will be able to come. It goes without saying that once you are here, and if you are not too hard pressed for time, we would count on seeing you in Birmingham.

[79]Hugh McManus. See letters [385], [412].

[80]R.E. Peierls and H. McManus, 'Classical electrodynamics without singularities' *Proc. Roy. Soc. A* **195**, 323–36 (1948).

[81]Between 22 and 27 July 1946, a Physical Society Conference on fundamental particles and low temperatures took place in Cambridge.

We are still living in a boarding house, but we have bought the tail end of the lease of quite a nice house and hope to move in on April 15th. With very best wishes to all of you,
Yours sincerely,

[Rudi]

[397] Rudolf Peierls to G.P. Thomson

[Birmingham], 26.3.1946
(carbon copy)

Dear Thomson,
Thank you for your letter[82] which has made the position much clearer. I see that your scheme is based essentially on the assumption that the nuclei will not contribute to heat conduction at all. Granting this assumption, I believe your conclusions are correct, although I am not quite satisfied that it is altogether in order to count only collisions which give deflections of 90°. In this kind of problem, collisions with small deflections are very much more frequent, and I believe it would be more correct to take an average cross-section, weighing each collision with the factor $1 - \cos$. This is likely to result in a somewhat similar free mean path which, in your conditions, means a somewhat greater conductivity. However, since you have a considerable factor in hand, I do not believe this will essentially affect the argument.

The essential point is, then, that of the contribution to the nuclei. If I understand your argument rightly, it is that you are essentially in the Knudsen case and that only a negligible fraction of the nuclei will make a collision with other nuclei before hitting the wall after once they have acquired their high temperature, and you have further assumed that any nucleus that has hit the wall will get stuck there.

This will continue until the potential differential between the gas and the wall will have risen to a point where the potential energy becomes

[82] Letter could not be located.

greater than kT and, from then on, no further nuclei can reach the walls. The number of nuclei needed to produce this charge is small and the energy carried away by them is, therefore, negligible.

I am a little worried about this argument, for three reasons. In the first place, once you are dealing with deuterons striking a wall at high speed, it seems to me unavoidable that one should get some phenomena of secondary emission. Secondary emission of electrons, of course, is of no importance, since these will not leave the wall, but with the energies concerned I rather think it will be possible to knock off positive ions, which, of course, would break down the charge contribution, and this seems likely if the wall contains near its surface a lot of deuterons which had got stuck there previously. It might, however, be possible to make the wall of such a material that it would be very hard to dislodge any atom from its surface and that, at the same time, the deuterons would penetrate too deeply to find their way out again. Possibly some experiments could be devised to throw light on this question.

The second point is the following. Supposing that the surface itself acts as a perfect trap for deuterons, then there will be a strong electric field normal to the surface and this will give to any electrons circling there in the magnetic field an average velocity component at right angles to both fields. I find, for the kind of figures that you have quoted, an average electron velocity component in the direction of $2 \cdot 5.10^8$ cm./sec. which means a current density of 3000 amps./cm.^{2} This will cause a gradient of the field of the magnitude of 4000 gauss/cm. As a result, the magnetic field gets neutralised in the centre of the tube and, therefore, also the electric field will disappear from there. This will increase the electric intensity in the outer part and hence the current density, and in this way progressively the field will be forced into a narrower shell near the wall. In the end it will mean that your temperature difference, as well as your potential difference, would have to be maintained over a very small gap comparable to the radius of the electron orbits, and this is clearly impossible.

Thirdly, there is a fact that, owing to electron collisions, this gas will have a finite electric conductivity, and this is another reason why, together with the electric field, there will be a transport of electrons to the wall, since the migration of every electron to the wall requires a deuteron to go as well to restore the potential, it seems to me that this process would continue until all the gas has been cleared up.

I find it hard to say how one can escape all three of these difficulties together. But I would be very glad to have your comments on this.
Yours sincerely,

[R.E. Peierls]

[398] Rudolf Peierls to William G. Penney

[Birmingham], 29.3.1946
(carbon copy)

Dear Bill,
I have only just heard that you are back in this country. I hope there will be an opportunity of meeting you before you disappear again, as there are a number of questions, both about the project and otherwise, that it would be nice to talk over.

I have not seen you since you got your new job and, while I am sure you will make a success of this work and will enjoy it, I cannot help regretting that this means one more defection from the ranks of theoretical physicists in this country, of which, in any case, there are too few.[83]

I was glad to see your name on the new list of the Royal Society. I am sure that is well deserved.

I do not know whether you know that I helped put together a series of articles on the atomic bomb to make up a number of the new Penguin Science News. This number will deal entirely with atomic energy.[84] I have the articles all completed except for an article by Philip Morrison on "the atomic bomb as a weapon" This was promised for mid-January,

[83]William Penney (1909–1991), who had been part of the British delegation at Los Alamos, had taken up the position of Chief Superintendent of Armament Research (CSAR) which put him in charge of the Armament Research Department in January 1946.

[84]See letter [396], note 77.

but it is not complete yet and cannot be sent for another month. I have reported this to the publishers to find out whether they are prepared to wait so long for the article, but while I am waiting for a reply I would be glad to know whether, by any chance, you might be prepared to write such an article in a hurry.

This is intended to be on a rather popular level. The other articles are about seven thousand words each on the average, but this is not a very precise limitation either way. There are other articles dealing with the chain reaction in general, with the separation of isotopes and the production of plutonium, with the properties of the atoms, electrons and neutrons, so that for this article is remains to explain in some detail the processes involved in the actual explosion, in particular the competition between the rate of reaction and the increase in volume due to the expansion and the effects. All this, of course, within the limits laid down by present security rules. I would, of course, let you have the existing articles as a basis, Penguin pay, I believe three guineas for a thousand words, but they intend to publish the series also in other languages like Spanish and Portuguese, and they may well decide to publish it in America as well and there would be additional royalties from these.

I do not know whether you have heard that we are trying to get an atomic scientists' organisation along somewhat similar lines to the American organisations, though on a smaller scale, and suited to the conditions in this country. I hope you will agree to support this.

Does your business ever bring you to the Midlands? If so, I would certainly be glad to see you here, and otherwise, maybe we could meet in London. How easy is it for you to get to the centre of town?

With kindest regards,

Yours sincerely,

[R.E. Peierls]

[399] William G. Penney to Rudolf Peierls

Fort Halstead, Sevenoaks, 1.4.1946

Dear Rudy,
I was glad to hear from you.[85] If there had not been a delay I should have returned to the States on the 23 March; my present plans are to leave about the 15th April. I should also, very much like to talk with you on several matters but unless you are coming to London in the next fortnight and have a few hours to spare when you come, it is not likely that we shall meet before my return. It would not be difficult for me to get up to London at any time, if you came.

Your suggestion about an article for the Penguin Science is one which I am afraid I must refuse. To me, it is not at all clear what should be said in such an article. I am not so much worried about Security but rather about the effect that an ill-considered article might have. You may be interested to hear that it is likely that a Government White Paper will be issued on the atomic bomb damage in Hiroshima and Nagasaki. This paper will be written by the team from the Ministry of Home Security which went to Japan with the Strategic Bomb Survey Group. I have seen the report which these people have prepared and it seems to me that they can usefully prepare a White Paper; they report only things which were obvious to everybody who went into the cities; in fact, they missed a great deal.

I hope that you and your wife and children are all well and not finding austerity too severe.
Yours sincerely,

Bill

[85] Letter [398].

[400] G.P. Thomson to Rudolf Peierls

London, 2.4.1946

Dear Peierls,
Many thanks for your letter and criticisms.[86] The second point you make is a very serious one and invalidates the scheme as originally proposed except for very low densities. I think, however, that there is a way out as follows. In order to get the energy in for heating purposes we had already proposed to use the toroid as a sort of wave guide and drive the electrons forward by what is virtually the pressure of radiation. They would then transfer energy to the nuclei by collisions. Now this process will create a current round the toroid which will cause a circumferential magnetic field (circumferential round the circumference of the circular section of the toroid). This magnetic field will serve to anchor the electrons instead of the solenoid field originally proposed. The velocity of the electrons corresponding to the combination of this field and the radical electric field is now in the direction of the current and in the same sense. Another way of looking at it is to say that the different current elements attract and so balance the outward radial force on the electrons. The electrons once anchored, all is as before. This case is harder to work out than the former but as far as Blackman and I can see it should work out.

As regards your other objection:

There is no evidence to my knowledge that fields of the order considered $\sim 10,000$ volts/cm will produce any secondary emission of positrons from metals. There might be some spluttering of neutrals due to bombardment of the walls, but this can be made very small by lining with aluminium and perhaps in other ways. I envisage holes in the walls through which deuterium would be admitted and the products of the decomposition withdrawn. The <u>amount</u> of gas in use will be controlled by shooting electrons into the centre to make up for those driven to the walls by collision. Some nuclei of $\mathrm{H}^3.\mathrm{H}'.\mathrm{H}^3_l$ will reach the walls, some but

[86]Letter [397].

fewer H^2 since these will not have the energy of disintegration to help them overcome the field. At first they may penetrate a short distance into the walls but this thin skin will soon become saturated and the nuclei will collect on the surface picking up electrons from the secondaries formed. (If there is any difficulty about this the solid walls could be shielded from the electron field by a lining of gauze say 1cm away, but I don't believe this will be necessary.) This thin layer of gas will mix with the deuterium supply and be drawn off at some of the holes in the wall while deuterium is wasted in this way, but it could be recovered with the valuable tritium. Some of the deuterium gets ionised by collision and is drawn in to the body of the gas to an extent determined by the loss of disintegration nuclei and the balance of electrons shot in over those lost. The controls are two; — rate of shooting of electrons, supply of deuterium.

I should very much like to discuss all this as it is so difficult to explain by letter. I am going to Ireland on Friday for a holiday, returning on the 24th. If you are likely to be in London before the end of April, could you let me know? If not, I think I should come to Birmingham. What day would suit you? It is most kind of you to help as you are doing.
Yours very sincerely,

G.P. Thomson

[401] Hans Bethe to Rudolf Peierls

Ithaca, 8.4.1946

Dear Rudi:
Thanks a lot for your letter of 3rd April.[87] It is unfortunately true that I had not sent my manuscript to Washington, but I am doing this today. It had weighed on my conscience for about a week past, but I was away all the week.

I am very happy to have won the nickel. I think in the long run you are going to be happier at Birmingham than at Cambridge. Still, it is a

[87] Bethe almost certainly refers to letter [396] dated 14 March 1946.

pity that Cambridge has gone so much to the dogs as you indicated by your refusal. Bretscher is apparently so disgusted that he will not come back to Cambridge.

I am just having a visit with Wentzel for two days. He mentioned that there were several young theorists in Zurich who took their degrees in the last two years. He wanted to send one over to Cornell with a Fellowship, which we like very much. But I thought also of your problems and thought you might be interested in getting one or two to England. Wentzel is going back to Zurich at the end of this week. You might like to write to him there as to what kind of people you would be interested in.

I got the official invitation from Cambridge. It is very tempting to go, but I had promised myself to take a really long vacation this summer. So it is probably "no", but this is certainly the most difficult invitation to refuse.

I got settled here quite nicely and find some time to study mesons and nuclear forces and to do a little bit of work myself. Physics is, after all, a fascinating subject.

You may have seen the report of the State Department on international control of atomic energy of which Oppie is one of the authors.[88] I think it is very good. Greetings from Rose who is enjoying the comforts of civilisation and from myself to you and the family.
Yours sincerely,

Hans

[402] Rudolf Peierls to James Chadwick

[location unspecified], 3.5.1946
(carbon copy)

Dear Chadwick,
For some time I have intended to write to you my impression of the situation here as regards the project and what is wrong with it. In the

[88]United States Department of State Committee on Atomic Energy, 'Report on the International Control of Atomic Energy', (Washington, 28.3.1946).

meantime things have changed considerably through Portal[89] taking office and through his establishing a new Technical Committee under his Chairmanship. This has removed at least some of the difficulties, in that there is now at least a body in which the scientists meet from time to time and through which they can be kept informed of what is going on. But I still feel far from satisfied that things are going as well as they ought to. Probably the great snag is the incredible inefficiency and red tape in the Ministry of Supply. You will have heard stories about this from other sources. Here are some examples, by way of illustration. The general experience is that it takes three months to get any payment of outstanding balance to people returning from North America. I have not been able, in spite of intense pressure, to get these matters straightened out, but in my case this is less serious than in the case of younger people like Marshall, who has not had his payment yet, or Morris, for whom it took an incredible time, although it is finally settled now.

As you know, most of us had to be appointed honorary consultants in order to legalize our position in retaining reports. The writing of even the formal letters necessary for this purpose took ages, and probably would never have been done if Akers had not written such letters off his own bat, against instruction. I had, in fact, a letter from Summer telling me that it was a violation of the Official Secrets Act, if I retained any reports beyond 11th March. The letter reached me about 13th March. It was, of course, of no consequence, but similar things happen to people who are less familiar with the project and it does not actually make them keen to continue to help. The worst of all these things is, as no doubt you know, the situation at Harwell. Oliphant and I visited Harwell this week and there is practically nothing going on there. No construction work has progressed, or even started. Skinner has an uphill job fighting petty officials over petty regulations. The greatest difficulty was the arrangements for making appointments, but I understand that this is now being handled in a somewhat different way, giving Skinner more direct responsibility, and maybe things will work a little more effectively.

[89]Air Marshal Viscount Portal of Hungerford (1893–1971), had been Commander-in-Chief of RAF Bomber Command and then Chief of the Air Staff and Marshal of the RAF. After the war he became Controller of Production, Atomic Energy (CPAE).

Another example is that one hangar is filled with equipment from Valley. While we were there we had a visit from the former Foreman at Valley to explain the nature of some of the equipment. It turned out that a catalogue of this equipment had been sent to the Ministry of Supply but had never reached Skinner. It would evidently be desirable to dismantle some of the equipment and to take out parts that would be of value to Harwell, but they have at the moment not even a crew of people who could get hold of some screwdrivers to get to work on this. They have of course not the screwdrivers and spanners either. In addition to all this, every little job connected with living conditions there, such as the distribution of mail, leaking taps, transportation and so on, becomes a problem that needs to be fought out and takes time. I am not suggesting that any of these items is, in itself, serious. Probably by the time this letter reaches you, most of the difficulties will have been overcome somehow, and they will be fighting about something else. But all this wears out the personnel, in particular Skinner, completely and keeps their attention away from more important questions, and it illustrates the complete lack of comprehension in the official mind of what the project is about and how it ought to be tackled.

Asking for reasons for this, it seems to me that the chief reason is the position of the project in the Ministry of Supply, in which it is just one of many activities and in which at every level, not only at the top, it requires the action of people who are concerned with it for only a very small fraction of their time. For every little wall that has to be built at Harwell, the Ministry of Works has to be brought in and contact with the Ministry of Works is made by the Ministry of Supply officials who have no idea about atomic energy. I understood some time ago from Longmir that in anything he does he reports to civil servants in the Ministry who are busy men and not particularly interested in atomic energy, and his recommendations reach people at a higher level merely in the form in which these civil servants choose to summarise or forward them.

I believe there is hope that all these things will improve as the activities of the project grow, since it will then be possible to allocate more full-time staff to the project specifically, but it will involve a fight at each step.

What I am particularly concerned about is that before the decision was taken to place this project under the Ministry of Supply it was our general impression that things would not be allowed to get to this state, but that, if the project were to come under an existing ministry it would have a special organisation set up for it which would make use of the priority and financing arrangements of the ministry and would link up also with it at a high level but within that framework would have a completely independent organisation. All the scientists concerned with the project felt strongly about this and were prepared to fight. They are in an unassailable position because without them the project cannot be carried on, and most public opinion and Parliament would react very strongly if this project had to be abandoned because the scientists felt that they were not given the working conditions that would enable them to carry on efficiently. What is needed now for this is concerted action by all scientists. The new organisation under the Ministry of Supply was, I believe, accepted by Cockroft and yourself before anyone else knew what was proposed. Since Cockroft and you are the people who primarily have to fight out all these difficulties, very clearly no-one can raise a major protest against such arrangements as long as you two feel that you can make a satisfactory job of the proposed arrangements. It was certainly no-one's desire to make life more difficult for you than it is. What has gone wrong, I think, is that there is insufficient contact and cooperation between all the people who essentially are in agreement on the objects, and, in broad outline, what is needed to implement them, and I think that minor differences of opinion in technical or administrative matters have now been allowed to prevent concerted action in major matters. I feel sure that for the past years all the members of the Technical Committee, as well as Akers and Perrin, were in essential agreement on what had to be done, yet in talking to any of them (and I do not except myself) one would hear more criticism of the others, of their technical views, of how they handled a particular question of organisation or policy, than of essential agreement and concerted action.

The result is that the enormous prestige and power which the scientists possess is, in fact, lost. The only way in which we can bring pressure to bear is by making clear that there are certain limits beyond which we are not prepared to go, and that if the government departments want our collaboration they have to adhere to these limits or else they will not have us. It should not be necessary to carry things far enough to threaten resignation, or actually resign, but if things are handled in such a way that it is perfectly clear that certain steps will not be tolerated by the scientists it will have the same effect. This, of course, is a weapon that must be used very cautiously and to use it lightheartedly over minor matters would weaken our position more than not to use it at all. But I cannot help feeling, on looking at the present state of the project in this country, that things have been allowed to go too far and that if we had been in a position to get together and review the situation from time to time we would have found the point at which to dig our feet in.

Things were, of course, made difficult by the dispersion of the team across the Atlantic and the unavoidable break-up of the old Technical Committee. When, in due course, you and Cockroft get back and when we can again get together around a table, these things will be easier, but it does not look as if this can be achieved very soon, meanwhile the organisation may have drifted a long way in a direction from which it is hard to retrieve it and in any case time will have been lost which, from every point of view, is most vital.

I cannot offer any very constructive suggestion as to what should be done about it, except generally to stress the importance of trying to spread amongst this group, as far as possible, information on what action will be or has been taken and the reasons for which it may or may not be wise to accept such action and on people's attitude in general. I believe it is our job now, being on the spot and in close touch with the set-up here, to keep you and Cockroft informed of what is going wrong, and it is for you and Cockroft, as the people with the best knowledge of policy and the contacts at a higher level, to give us a lead in saying how far one must compromise and at what point one should stop.

A relatively minor point compared with all this, is the following. I believe there is some kind of vicious circle as regards Harwell, in that many people who will eventually go to Harwell are still in Canada because there is little for them to do here and there is a tendency for construction work at Harwell to be delayed because the urgency is not appreciated as long as the people who want to get to work are not on the spot. I believe that if all the people who are not essentially needed in Canada, but who are merely remaining there to learn what they can while they cannot be used in Harwell, were to come back so that the place would be filled with people who were pressing for accommodation, which is about to be completed, for equipment, which theoretically was promised but has not arrived, then if there was someone waiting for each of the instruments which are on order, the job would in fact be accelerated and the result would be a saving of time, although it might appear to be an uneconomical use of the men. At the same time, as people collect there, the structure of the various teams, their plans and responsibilities, would begin to crystallize out and one would realise more clearly what is needed in the way of additional staff. It is essential that this should be done while men can still be found.

I would therefore suggest that many of the people who can be spared from Canada should be brought back as soon as possible. This does not apply to all of them, since for the temperament of some people this situation I have described would have very unsatisfactory results and they might get dissatisfied with the project forever. One particular case I have in mind is Fuchs. In understand he is committed to stay in Los Alamos until June, and he is probably very useful there. I would feel strongly, however, that his commitments there should not be allowed to drag on and that, in the circumstances, he should not go to Canada after leaving Los Alamos, but should come back immediately to help settle the staff problems for his team, to discuss in broad outline the research programme and to choose the arrangements as regards machines, computers, etc. that will be required for this programme. If, when this is done, there is still nothing for him to do (this seems to me to be quite unlikely), he could still visit Canada afterwards and get some experience there. I believe, however, that from Montreal reports and from discussions with Buneman, Guggenheim, Pryce and others, he could, in fact, learn as much from a visit in Chalk River.

Another case which occurred to me was Arms. I do not know what he is doing and it may be of vital importance, but if it is not, I believe that he would help accelerate matters a good deal by chasing around after equipment and helping in the layout of laboratories, workshops, etc. and that he would do this quite readily if the situation were explained to him, even though it would mean that for a few months he would not do any scientific work.

It is a relief to get all of this off my chest, and I would be very glad to hear whether you think I am misunderstanding the situation or whether you agree with my attitude.
Yours sincerely,

R.E. Peierls

[403] Ed Salpeter to Rudolf Peierls

Santa Fe, 19.5.1946

Dear Rudy,
I thank you very much for your several letters concerning the Research Fellowships at Birmingham. I have waited to obtain the opinion and advice of others before reaching my decision, which is that I should like my name to be considered as that of a candidate for a junior fellowship.

I have come to this conclusion after weighing the disadvantages of (or rather my antipathy towards) living in your city against the following points — first the opportunity to participate in a more general field of fundamental research, second the probability of making contact with a wider circle of people, and third, which is perhaps the most decisive, my feeling that this would leave most open the ultimate direction of my career.

These last points are chiefly in comparison with going to Harwell, as regards other possibilities I have thought it desirable not to leave things until I should return to England.

I am sending a formal application to the Secretary of the Faculty of Science. Please give my best wishes to Genia and the children, and I shall look forward to seeing you all in the summer.
Yours sincerely,

Ed Salpeter

[404] Rudolf Peierls to Max Born

[Birmingham], 12.6.1946
(carbon copy)

Dear Born,
Thank you for your information about Miss Walker.[90] I had also encouraging reports from other people and we shall ask her for interview as soon as that can be arranged.

I saw Dirac the other day and he mentioned to me the remark in your letter about Heisenberg. I do not think the matter will arise because I do not believe that Planck is being invited and I understand, in fact, that the committee organising the Conference have ruled against inviting any Germans.[91]

I feel one must be careful about pressing for admission of people like Heisenberg, since I believe that for the resumption of international contacts it is essential that people should be made to understand the distinction between the behaviour of people like Laue and Hahn, of whom one hears from all sides that they have kept aloof from all government activities and have resisted any temptation to ingratiate themselves with the Nazis either by collaborating or making public statements, and the others of whom this seems much less clear. For example, the chief reason for excluding Germans from the coming conference seems to have been the argument that it would cause resentment among the people from the occupied countries. I believe, in fact, that amongst the latter, in particular the Dutch, Belgian and French people, the part played by Laue and Hahn is well known and that they would not have resented their coming, whereas they would have resented it if Heisenberg or Becker or others had been asked. They would, in fact, have appreciated it if their opinion had been obtained about people who could be trusted.

The whole problem is very difficult and one has, of course, to remember that Heisenberg is by far the best scientist of all the ones mentioned and in any question depending solely on scientific merit all other arguments should not count; but, in fact, in a conference of the kind

[90] Letter could not be located.
[91] See also item [440].

planned in Cambridge, scientific discussions will be inseparable from social gatherings and I, for one, while I would welcome an opportunity to hear Heisenberg's ideas on scientific matters and discuss them, would feel very reluctant to have any social contact with him until we are able to take a more detached view of the past than we can do just yet.

These problems are most complicated and I would be glad to know what you think.

Fuchs is still in Los Alamos but is due to leave there in few days and I expect he will be back in this country early in July. I imagine he will get in touch with you as soon as he gets back. No doubt you have heard that he has accepted a job at the new Atomic Energy Research Establishment at Harwell. If you want to get hold of him urgently, a letter addressed to him c/o J.F.Jackson, P.O.Box 680, Benjamin Franklin Station, Washington, 4, D.C. will reach him until he leaves North America.

I hope to see you at the Cambridge meeting in July. Will you be staying in the south between 11th and 22nd, and if so is there any chance that you could spend a day or so in Birmingham? I expect to have Rosenfeld here at that time and he would, of course, love a chance of seeing you.

Yours sincerely,

[R.E. Peierls]

[405] Max Born to Rudolf Peierls

[Edinburgh] 13.6.1946

Dear Peierls,

Thank you for your letter and the interesting remarks about the German physicists.[92] About 2 years ago, when I was a member of the Sectional Committee for Math[ematics] of the R[oyal] S[ociety], Dirac asked me to join him in proposing Heisenberg for election as a Foreign Fellow of the R.S. After some consideration I refused and kept to this decision in

[92] See letter [404].

spite of Dirac's persuasion who rightly said that Heisenberg's discovery will be remembered when Hitler is completely forgotten. I said I would reconsider the matter after the war when we had the opportunity of knowing exactly how far H[eisenberg] has collaborated with the Nazis. Meanwhile I have heard contradictory reports. He was certainly not behaving like Laue and Hahn. But it is said he tried to obstruct the development of nuclear explosives. I do not know whether this is true. My feeling is now that these fellows have got their punishment. They are not only having a hard life and little to eat, but their conceit is badly shaken. In my letter to Dirac I wanted to show that I was prepared to reconsider the question. But if you object, and if the people of the occupied countries object I shall not move any further.

Heisenberg's new papers on relativistic quantum mechanics (Z. f. Phys. <u>120</u>, pp. 53, 675, 1943)[93] have impressed me rather much. They contain no solution of the difficulties, but they clear away some rubbish. I hoped that Peng's work had removed some of the infinities,[94] but I got a letter from him telling me that there is a mistake in it and just these results are wrong. My own ideas look to me quite promising but I am too old and tired to work them out alone, and I have no collaborator suited for this kind of work. I hope Fuchs may spend some holiday weeks in helping me, when he returns. I shall write to him. I shall be in the south between July 11th and 22nd, and I intend to visit my daughters, Irene in Cambridge and Gritly in Oxford. I should like [to] spend a day or two in Birmingham, if it would fit in. I shall let you know.

With kind regards,

Yours,

M. Born

[93]W. Heisenberg, 'Die beobachtbaren Grössen in der Theorie der Elementarteilchen. I', *Z. Phys.* **120**, 513–38 (1943); and W. Heisenberg, 'Die beobachtbaren Grössen in der Theorie der Elementarteilchen. II', *Z. Phys.* **120**, 673–702 (1943).

[94]Huan-Wu Peng, educated at Tshingua University, was working with Heitler at the Institute of Advanced Study in Dublin. In 1944 he had published some papers jointly with Max Born. *Proc. Roy. Soc. Edinburgh* **62**, 40, 92, 127 (1944).

[406] Rudolf Peierls to Max Born

[Birmingham], 14.6.1946
(carbon copy)

Dear Born,
Thank you for your letter.[95] I am glad to know your views on this question and if you will be in the south next month I hope we shall have to opportunity to talk about this. If you were in Oxford, you might like also to discuss the question with Simon, who, I believe, has strong feelings in the matter.

I have just seen Peng's paper in the P.R.S.[96] and, while it is very interesting, it seems to me that he has not given enough information to say really how the calculation should be carried out. It does justify, however, the hope that something can be done along those lines and I hope his new calculations, which you mention, will not dispose of this completely.

I am not at all impressed with Heisenberg's new scheme or Møller's work on the same subject.[97] It seems to me quite an empty scheme which is completely indefinite until one formulates the laws to which his matrix S is subjected. One might, of course, hope ultimately that the matrix S itself will be found to obey some very simple laws, but since these must contain ordinary quantum theory as a limiting case and since in the ordinary case of a collision with non-Coulomb forces the expressions for S become extremely complicated, I do not see much hope in that direction. I did hear from Dirac about your idea of linking this up with "reciprocity" and that is very interesting, but I always had grave doubts as to what would happen to the reciprocity in a many bodied problem.

[95] Letter [405].
[96] H.-W.Peng, 'On the divergence difficulty of quantized field theories and the rigorous treatment of radiation reaction', *Proc. Roy. Soc.* A**186**, 119–147.
[97] See letter [405], note 93.

I would be extremely happy to see you here in July. So far I have no definite engagements during the time you mention, but no doubt there will be all sorts of meetings. As Rosenfeld will probably be staying with us, we may not be able to offer you a room, but I am sure something can be fixed up, if it would be convenient for you to stay overnight.
With kindest regards,
Yours sincerely,

[R.E. Peierls]

[407] Rudolf Peierls to Y.I. Frenkel

[Birmingham], 20.6.1946
(carbon copy)

Dear Frenkel,
I have now for some months been back at my normal job at Birmingham and I am trying hard to settle down again to some more normal work. I imagine you will be in a very similar position. I do not know whether you are back in Leningrad yet, but I hope this letter will reach you anyway. Has your institute suffered very much, and have you been able to resume work there?

I have seen the many papers you have published during the last years and been very interested in many of them. I expect that I myself shall try to work chiefly on field theory, meson theory and the theory of nuclei. On the latter we still have to finish some work started in Copenhagen before the war. I am at present trying to revive the idea of introducing a finite length into the theory of the electron, in order to remove the infinities, and I have found a way of doing this in classical theory without destroying Lorentz invariants, but at the expense of obtaining the field equations and the equations of motion in the form of integral equations in time. Classically, it all looks straightforward, but we have not yet succeeded in quantising the theory.

I am not at all convinced that this is the right approach, but I am also not very impressed with the view of Dirac, since I do not like his reinterpretation of the negative possibilities, nor do I believe the recent theories of strong coupling. The recent attempts by Peng to prove that all the infinities are merely due to the perturbation and that the field equations have finite solutions is very intriguing, but I do not see how this particular method of solution can be right.[98]

Many of these problems will, of course, be discussed at the International Conference in Cambridge in July and I very much hope that you will be able to accept the invitation to the Conference. It would be very nice to have a really good discussion of physical problems again after so many years and I believe that just now the resumption of normal contacts between scientists is of enormous importance.

I am trying to build up a bigger team here, of whom some, of course, will be rather junior. With this, and through the collaboration with Oliphant's department, who are completing the installation of a cyclotron now and are working on a machine for very much higher energies, I am looking forward to a very interesting time.

My family are back in Birmingham, too. With the great housing shortage it was quite a problem to find a place to live, but in the end we were lucky and got settled quite comfortably. The children are at school again and life is now more or less back to normal.

I very much hope to see you at Cambridge in July, but if you will not be there I should greatly enjoy hearing from you again about your family and friends.

With very best wishes,
Yours sincerely,

[R.E. Peierls]

[408] Y.I. Frenkel to Rudolf Peierls

Leningrad, 21.6.[1946]

Dear Peierls,
Thank you very much for your kind invitation to the conference. I shall be very glad to attend it, if that proves possible. Among other things I would like to discuss the question of relativistic quantum theory of

[98] See letters [405–406], notes 94 and 96.

complex particles, which I have considered rather loosely a couple of years ago.

I have transferred your letter to the Academy of Sciences and shall telegraph, if I get a positive answer to my application. I hope you are all well. Please give my kindest regards to Jenney and to our common friends.
Yours sincerely,

J. Frenkel

[409] Y.I. Frenkel to Rudolf Peierls

Leningrad, 21.7.1946

My dear Peierls,
Although I wrote you a few lines through a young physicist [??] who was expected to go to England, I am writing you again in reply to your letter of June 20th[99] which I received on the next day after sending you a telegram informing that I had just seen Jenney's parents in good health at Moscow (you probably interpreted this telegram as a preliminary reply to your letter.)

After 3 1/2 dreary years spent with my family in Kazan (where I lost my mother in 1944) we came back to Leningrad, rather amazed at the possibility of returning to our pre-war relatively comfortable existence. I changed my old flat for a better one, further away from the street and was lucky to find most of our furniture and other belongings, including the books, more or less intact. At the beginning of the war it seemed unthinkable that we should ever come back to the pre-war conditions. In some respects this maybe true, (particularly in America); but in regard to our living conditions — especially those of the scientific workers in large cities — we have practically reached to pre-war status owing to a number of facilities put at our disposal by the government. This may be illustrated by the fact that a great many scientists — of all professions

[99] Letter [407].

myself including are buying cars for our own private use. As a result of adverse living conditions and various troubles during the war period, I have developed a hypertonia, (i.e. abnormally high blood pressure or arterial tension), which I have to take care at present. Last year, when it was discovered, I succeeded to get rid of it after one month's rest at a sanatorium near Moscow. I am afraid that this will take longer this time. Outside my childhood I have never [been] sick in my life, and it worried me terribly to have to follow the prescription of the physicians. With respect to régime etc. in fact I have not begun to follow them yet, but shall try to be a "good boy" in a sanatorium near Riga where I am going in a few days with all my family for 4 weeks. My wife is always "so-so", since 1943 owing to an abdominal haemorage; but as a whole her health has improved since 1936 when she suffered from acute haemorages due to her trombopenia. The children are prosperous; The elder (23 years) studies physics at the polytechnical institute but is more interested in poetry (he has written and translated a number of excellent verses) and in chess; the younger will graduate his high school next year, but has not chosen any profession yet.[100]

I have been pretty active scientifically both at Kazan, and after my return therefrom to Leningrad. Unfortunately my activity has recently been arrested by my hypertonia and the associated headaches. As soon as I return to good health I shall set to work again. As usual I am interested in a large variety of different problems, which prevents me from digging deeply enough in a single place. I am of course very much interested in the problem of fundamental particles and field theory. I think, however, that the present dualistic treatment, in which the particles and the field are dealt with as two independent components of the whole is fundamentally wrong and that the particles must be considered as secondary entities, produced by the quantization of the field

[100]Viktor Zakovlevich Frenkel (1930–1997), became a physicist and historian of science in his own right. From 1948 he studied physics and mechanics at the Leningrad Polytechnic Institute. After work for the Svetlana Works on the design and construction of electrovacuum devices he joined the theoretical department of the Ioffe Institute of Engineering Physics in 1959, to which he remained attached until his death in 1997.

which is the bearer of all mechanical properties (energy, momentum, etc., there being no such thing as the kinetic energy of the particles or their mutual energy with the field). The actual realization of this monistic programme which has been initiated by Lorentz' theory of electron is of course very difficult; a modification of Lorentz' theory by introducing a certain length into the field equation may help to solve it.

What do you think of my approach to the problem of the relativistic quantum theory of complex particles (I think I have sent you reprint thereof.),[101] which I propose to treat as elementary particles with internal degrees of freedom. I have recently written another paper which may interest you on the asymmetrical splitting of heavy nuclei.[102] I shall send you a reprint as soon as it will be available. One of my luckiest achievements during the recent two years is probably a theory of atmospheric electricity which I have succeeded to greatly improve quite recently.

I also shall have a team of about 10 young people working with me during the next term. Most of them have been recently demobilized and require a certain induction period for beginning research work.

All our common friends in Moscow (Tamm, Landau, etc.) are in good health and probably will write to you themselves. I am visiting Moscow regularly once a month, owing to my association with two research institutions there (one in geophysics, the other in molecular physics).

It is a great pity that the present state of international affairs does not enable us to establish a direct contact with our colleagues and friends abroad. Let us hope that the situation will change for the better soon and that we shall be able to discuss scientific and other matters soon in a less cumbersome way than letter-writing.

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[101] J. Frenkel, 'On the theory of relaxation losses, connected with magnetic resonance in solid bodies', *J. Phys. (U.S.S.R.)* **9**, 299–304 (1945).

[102] J. Frenkel, 'Some features of the process of fission of heavy nuclei', *J. Phys. (U.S.S.R.)* **10**, 533–539 (1946) Frenkel had been working on the splitting of heavy nuclei for several years. See also Frenkel, J., 'On the Splitting of Heavy Nuclei by Slow Neutrons', *Phys. Rev.* **55**, 987 (1939).

Give my best regards to Jenny — also from my wife — and from us both to yourself.
Cordially yours,

J. Frenkel

Will you kindly subscribe me to the "Vogue". The expense will be covered by the Oxford Press which are just going to produce a new book of mine.[103] If you get across some really interesting book (fiction) please also send us a copy (stating the price). My wife will be very grateful to you for such things.

[410] Tony Skyrme to Rudolf Peierls

Santa Fe, 29.7.1946

Dear Rudy,
I had better acquaint you with the arrangements that I have made for the transmission to England of the document that I have decided to submit as a dissertation at Trinity College, Cambridge. At the moment this is in the General's Office in Washington for clearance; in the rather unlikely event that it should be completely declassified I have arranged that it shall be sent directly to Trinity.

I anticipate that it will remain more or less classified; and in this case I have asked the Washington Office to transmit it to you, as a responsible person who can keep custody of it. I have informed Trinity College of this arrangement and have asked them to tell you to which authorised person(s) they would wish the document to be transmitted for examination.

With regard to my leaving this country, I have applied for a passage home early in September; however, no sailing dates are yet known and I shall fit my plans to this date when I learn it. I shall leave Los Alamos at the end of this week and go to the West Coast and then up to Vancouver;

[103] J. Frenkel, *Kinetic Theory of Liquids*, Oxford: Clarendon Press, 1946.

I shall arrange that any mail reaching here during August is forwarded to me.

Mrs Daily has asked me to be remembered to you; with best wishes to your family,
Yours sincerely,

Tony Skyrme

[411] Rudolf Peierls to Robert Oppenheimer

Birmingham, 26.8.1946

Dear Oppy,
I am writing to let you know that one of our Members of Parliament, Captain Raymond Blackburn, is expecting to be in the United States from about September 3rd to September 27th and is most anxious to see you. His contacts are arranged largely through the Atomic Scientists of Chicago. He hopes to spend the first week of his stay on the west coast and if you are in California he would, if possible, like to see you during that first week. I think you will find it worth your while talking to him. He is a young Labour M.P. who has made atomic energy his special interest and he has repeatedly pressed in Parliament for a constructive discussion and for a statement supporting the Lilienthal Report.[104]

He is, of course, not a scientist and his knowledge and judgement have not always been perfect, but I think it is most important that we should keep reasonable people like him as well informed as we are permitted to do.

I heard from Oliphant that you were critical of the statement by the British Atomic Scientists on international control on the grounds that it recommended U.N.O. continued making bombs. This was I believe, based on press reports and in the meantime you will have seen the full text of our statement which was reprinted in the Chicago Bulletin and

[104] 'A Report on the International Control of Atomic Energy', prepared for the Secretary of State's Committee on Atomic Energy, Washington D.C., Dept. of State Publication 2498, 16.3.1946.

you will have seen that we have tried to keep an open mind on this question. I would be very interested to know if you still disapprove.

Here quite a stir has been caused by press extracts from an article by Urey in "Air Affairs" in which he is supposed to have said the United States might be forced to declare war itself "with the frank purpose of conquering the world and ruling it as desired and preventing any other sovereign nation from developing mass weapons of war."[105] This has produced a rather unfortunate effect, although I feel sure that the words must have appeared in the context that gave them a different meaning from that which they convey by themselves.*

We have regretted very much that you could not come to Cambridge. It was quite a successful and stimulating meeting, but it would have been better if we had had your views as well.

With best wishes,
Yours sincerely,

R. Peierls

*We have now seen the full text and confirmed this. Though I still think his wording was unfortunate.

[412] Rudolf Peierls to Hans Bethe

Birmingham, 24.1.1947

Dear Hans,
There are many things to write about after such a long time but for the moment I shall leave the domestic matters and write only about shop.

My department has got going and we have started on quite a number of problems, although we have not as yet produced much in the way of results. I would like, in this letter, however, to tell you what we are doing, for two reasons: firstly I would be glad of your comments on any

[105]See Harold C. Urey's papers, University of San Diego, Series 1, Box 135, folder 5.

of these points and, secondly, many of the problems are of such a type that I rather expect someone in America is also thinking about them. In that case it is quite likely that you may know and, while it may be quite worth while to go ahead tackling such problems in several independent ways, it would in any case be a comfort to us to know.

I think I can best describe our work by listing the people who are here and what they are doing.

> Jahn[106] is a man whose name I am sure you know. He has been here since August and in connection with his knowledge of group theory he was particularly interested in Wigner's theory of symmetries in nuclei. His ultimate aim is to see which of Wigner's results, in particular about the most reasonable coupling to assume, have to be modified in the light of the existence of non-central forces. No doubt this problem is also being thought about by other people, but we want to know the answers and want to learn the techniques. For the moment he is trying to alter the presentation of the Wigner theory by bringing in the explicit representation of the permutation group to see why the results appear only to depend on whether the nucleus is of the type $4, 4r + 1$ etc., although for the derivation one has to use explicitly the permutation groups of a higher order. What he is trying to do is analogous to Slater's method in the case of the atom. This, I think, would be a very useful step.[107]
>
> Preston,[108] a pupil of Infeld's from Toronto, came here with an almost completed paper on the theory of alpha-decay.[109] This carried the one-body-problem to a logical conclusion, fitting the

[106]Hermann Arthur Jahn (1907–1979) studied under Heisenberg and van der Waerden in Leipzig, joined Peierls' department in 1946 and later became professor of applied mathematics in Southampton.

[107]See among others H.A. Jahn, and J. Hope, 'Symmetry Properties of the Wigner 9j Symbol', *Phys. Rev.* **93**, 318–21 (1954).

[108]Melvin A. Preston, studied at Toronto before joining Peierls' research team in Birmingham where he completed his Ph.D.. In 1953 he returned to McMaster, where, apart from a few years at Saskatchewan, he spent the rest of his career.

[109]M.A. Preston, 'The Theory of Alpha-Radioactivity', *Phys. Rev.* **71**, 865–77 (1947).

phases of the wave function inside the nucleus and using an exact analytical solution, even for the case $l \neq 0$. It is doubtful whether it makes sense to carry the one-body model that far but in the course of his calculations two interesting points have come out: (1) the formula given by Gamov from the transparency barrier in the case $l \neq 0$ is completely wrong[110] and based on an inaccurate evaluation of the integral, which, of course, with some trouble can be done in closed form. Apparently this wrong formula has been used ever since, e.g. by you. (2) In the one-body model, the transparency of the barrier as a function of l first increases and then decreases. This surprising result is due to the fact that if you always assume a rectangular potential barrier, but fit its eigenvalue correctly, higher l forces you, for the same total energy, to assume a deeper well (because of the effect of the centrifugal term) and therefore both the density and velocity near the edge of the nucleus increases. It is, of course, very questionable whether this feature of the one-body problem has a real meaning, but it might have and there seems some evidence that the assumption of the decay constant behaving in that way allows a somewhat better fit for the experimental data.

Preston is also working on an explanation of Chang's peculiar results on alpha fine structure assuming that the nucleus is excited by long-range Coulomb forces after passing the potential barrier.[111] A simple calculation regarding the alpha-particle as a classical point charge gives a perfectly reasonable estimate for the probability. We felt, however, that this should be done more decently and we struck some snags in that calculation which have dissolved themselves now, leaving him only with somewhat nasty integrals to evaluate. We have seen now a reference to work by

[110] G.A. Gamow, *Constitution of Atomic Nuclei and Radioactivity*, Oxford: Clarendon Press, 1931, pp. 91 and 103; G.A. Gamow, 'Zur Quantentheorie des Atomkernes', *Z. Phys.* **51**, 204–212 (1928).

[111] W.Y. Chang, 'A Study of Alpha-Particles from Po with a Cyclotron-Magnet Alpha-Ray Spectrograph', *Phys. Rev.* **69**, 60–77 (1946); W.Y. Chang, 'Low-Energy Alpha-Particles from Radium', *Phys. Rev.* **70**, 632–39 (1946).

Dancoff[112] on the same idea but, as from this it is quite impossible to understand how Dancoff has done the calculation or what the answer is, we propose to continue. In any case, it is a fascinating problem.[113]

When this is finished he will tackle the question of the effect of non-central forces on proton-proton scattering. This, of course, appears only at higher l and therefore at higher energies than the $p-n$ scattering, but it is likely that such energies will be, or in fact have been, reached. In this connection we computed for the experimentalists some curves for the angular distribution of the $p-p$ scattering at about 13 MeV and the curves are very entertaining. They mean, in particular, that in order to sort out the contributions of s, p, etc. waves, one has to measure the scattering with quite a fine angular definition. Experiments along these lines are planned here now, but as the cyclotron is not working yet they will have to wait.

Skyrme is studying the problem arising from the dispersion formula (Bethe-Placzek,[114] Kapur-Peierls etc.)[115] with a view to seeing what one can get out about potential scattering and the like. We have just received Wigner's latest papers on this subject[116] but do not appreciate yet to what extent they dispose of the problem. Skyrme has also given a tidier proof of the completeness of the complex eigenfunctions used in my paper with Kapur.

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[112]S.M. Dancoff, Metallurgical Project Report, *Short Range Alphas in Natural Radioactivity.*

[113]Preston published his results two years later in M.A.Preston,'The Electrostatic Interaction and Low Energy Particles in Alpha-Radioactivity', *Phys. Rev.* **75**, 90–99 (1949).

[114]H.A. Bethe and G. Placzek, 'Resonance Effects in Nuclear Processes', *Phys. Rev.* **51**, 450–89 (1937).

[115]P.L. Kapur and R. Peierls, 'The dispersion formula for nuclear reactions', *Proc. Roy. Soc.* **A166**, (1938), 277–95.

[116]Eugene P. Wigner, 'Resonance Reactions', *Phys. Rev.* **70**, 606–18 (1946).

Salpeter,[117] an Austro-American, is tackling the problem of self-energy and has succeeded in proving that (contrary to the idea of Peng's paper[118]) the infinities are not due to using perturbation theory, at least in the following sense: if the integrals are cut off at a certain maximum wave factor k_0 and the wave equation are then solved rigorously for any k_0: then in the limit $k_0 = \infty$ the energy tends to infinity. This proof is now complete for all one particle problems. In the relativistic one-electron problem it leads to a divergence as $\int dk$ rather than $\int k$, as in Waller's case. This, however, we have convinced ourselves, justifies no hope of a similar improvement in the Weisskopf case.

The next step would be, of course, to try and generalise such a proof to Weisskopf's case of pair theory.[119] In attempting this, however, we discovered that Weisskopf's method is wrong, since the subtraction of the infinite charge density due to the electrons in negative states is done by reference to the states in zero field comparing the two infinite sums, term by term, for the same momentum. Since, however, momentum is not gauge invariant, the whole procedure is not gauge invariant, as I have shown years ago in the Royal Society Proceedings. This makes the whole theory inconsistent, since in the ordinary quantum electro-dynamics used by Weisskopf the relation $div E = 4\pi\rho$ can hold at all times only if all interactions are gauge invariant. We are trying to put this right, but cannot say yet whether this will affect Weisskopf's result.

[117] Edwin E. Salpeter (1924–), a graduate from the University of Sydney, completed his Ph.D. under R. Peierls in 1948. He then moved on to Cornell, where he became professor of physics in 1957.

[118] H.W. Peng, 'On the representation of the wave function of a quantized field by means of a generating function', *Proc. Roy. Ir. Acad.* **51**, 113-22 (1947).

[119] Weisskopf had published several papers on self-energy. See e.g. V. Weisskopf, 'On Self Energy and the Electromagnetic Field of the Electron', *Phys. Rev.* **56**, 72–85 (1939). See also R.E. Marshak and V.F. Weisskopf, 'On the Scattering of Mesons of Spin 1/2 $\hbar$ by Atomic Nuclei', *Phys. Rev.* **59**, 130–35 (1941). For a survey of developments in electron theory see V.Weisskopf, 'Recent Developments in the Theory of the Electron', *Rev. Mod. Phys.* **21**, 305–15 (1949).

McManus[120] is still working on the field theory for finite size electron, of which you know the general idea. There were many snags in the mathematics but as far as the classical theory is concerned everything straightened itself out, with one exception: we have obtained the equation of motion of an electron without external field which, in this case is an integral equation. We can easily prove that this has a trivial solution corresponding to motion at constant velocity and also that if you add external fields you get the usual radiation terms up to the normal radiation damping, with corrections only of higher order in the frequency than the Lorentz damping. It seems very plausible that these should be the only solutions of the type of Dirac's runaway solutions. We are still struggling, however, with a reasonable proof for this. As to the quantisation of such theory we need a brainwave but we have not yet given it up. An attempt to do this is also being made by Bleuler,[121] one of Wentzel's pupils whom I managed to get largely through your good offices and who also is tackling one or two more formal problems in the theory of quantised fields.

A young research student is just starting work on an attempt to calculate internal pair creation in the case of a gamma-ray of arbitrary multipole character.[122] I do not believe this has ever been done but in the case of gamma-rays of energies well above 1MeV internal pair creation should be quite well observable and experiments on the sharing of energy between electron and positron or on their relative angular distribution ought to give a convenient way of determining the order of the multipole. This, of course, can also be done with internal conservation, but as for high gamma-ray energy and low atomic number in the distinction between K,

[120]Hugh McManus who had been a research fellow at Birmingham before the war, returned to Birmingham from his war-time assignment at Chalk River, where he had been working at the Canadian atomic energy laboratory. He continued his work as a research fellow at Birmingham until 1951 and eventually became professor of physics at Michigan University.

[121]Konrad Bleuler, (1912–1992), completed his Ph.D. under Wentzel's supervision in Zurich.

[122]The student was R.A. Fatehally.

L, etc. electrons may not be easy, this alternative method seems of interest. This, in particular is a problem which one would hardly like to duplicate, so if you know of anyone else doing this I would be glad to know.

Krook,[123] a South African who formerly worked on astrophysics, is trying to calculate the continuous gamma-ray spectrum emitted in a proton-neutron collision when the neutron is not captured. For low energies, this will, of course, be quite a small effect although it should be observable. For high energies, it will be more likely than capture and may there help to get information about forces. What is planned at present is to calculate it for some simple type of force so as to have this available for comparison when we get experimental date.

Another research student is looking into Landau's theory of liquid helium.[124] We are rather dissatisfied with that theory because, while it makes out an excellent case for the existence of what Landau calls "rotons", he assumes that there is a lower limit to the roton spectrum from dimensional arguments, leaving out of account that there is a dimensionless number, namely the number of helium atoms, which may enter into this result. We think it should, in fact, be possible to prove that this is the case and that for a reasonable amount of helium the lowest roton level is practically zero. This, however, is not quite an easy problem. It is mathematically connected with the old problem of the rotation of nuclei, on which I am dissatisfied with the paper by Teller and Wheeler.[125] Probably if one can decently solve one of the two problems one can also get the solution to the other.

[123] Max Krook, had studied astrophysics at Cambridge before becoming research fellow at Birmingham. Eventually, he returned to astrophysics, becoming professor at Harvard.

[124] After the discovery of superfluidity and liquid helium in 1938, Lev Landau constructed a theory of 'quantum liquids' at very low temperatures. His papers between 1941 and 1947 are concerned with the quantum liquids of the 'Bose type' such as the superfluid liquid helium (^{4}He).

[125] E. Teller and J.A. Wheeler, 'On the Rotation of Atomic Nucleus', *Phys. Rev.* **53**, 778–89 (1938).

Another research student is learning all about luminescence in solids in order to assist Garlick[126] (whom I believe you met at a recent meeting at Cornell) in the analysis of the rather interesting experiments he is doing here.

Kynch,[127] who is rather busy with teaching, is working on the non-central forces in the nucleus. There seems, in particular, a peculiar discrepancy between your result and a similar result of Schwinger and Rarita on the one hand[128] and the result of Rosenfeld and Møller on the other.[129] You find that in order to account for the observed quadrupole moment of deuteron one has to assume a very strong tensor force, comparable in order of magnitude to the central force. Møller and Rosenfeld start with a tensor force which, coming only from the non-static terms, is about ten times smaller, treat it with perturbation theory and get the right quadrupole moment. One would say, at first sight that this simply means the perturbation theory is not justified, but it is the same kind of perturbation theory that one uses for calculating the fine structure in the atom and there we are reasonably confident that it works. It might, in fact, be that because of the peculiar nature of the problem the perturbation theory is more trustworthy than the rigorous solution. In this connection Kynch is also exploring

[126] G.F.J. Garlick was experimenting with phosphors and phosphorescence. See G.F.J. Garlick, 'Phosphors and Phosphorescence', *Rep. Prog. Phys* **12**, 34–55 (1948). He later published a joint paper with Fatehally: G.F.J. Garlick and R.A. Fatehally, 'Measurement of Particle Energies with Scintillation Counters', *Phys. Rev.* **75**, 1446 (1949).

[127] J.G. Kynch (1915–2003) had studied theoretical physics at Imperial College, London, he joined the Birmingham department initially to take over Rudolf Peierls' teaching, while the latter was engaged in work for the M.A.U.D. committee. His temporary appointment was extended several times and he stayed until 1952 when he accepted the Chair of Applied Mathematics at University College of Aberystwyth. In 1957 he became professor of mathematics at UMIST.

[128] W. Rarita and J. Schwinger, 'On Neutron-Proton Interaction', *Phys. Rev.* **59**, 436–52 (1941); W. Rarita and J. Schwinger, 'On the Exchange Properties of Neutron-Proton Interaction', *Phys. Rev.* **59**, 556–64 (1941).

[129] C. Møller and L. Rosenfeld, *Kgl. Danske Vid. Math.-Fys. Med.* **17**, No. 8 (1940); C. Møller and L. Rosenfeld, *Kgl. Danske Vid. Math.-Fys. Med.* **23**, No 13 (1945).

> the use of the Svartholm method, which treats the Schrödinger equation as an integral equation in momentum space and uses iteration. It appears that this works also for tensor forces and is, in fact, quite convenient.

This, I think, completed the list, except that I have not said what I am doing, but that is easy — I sit on committees and write letters.
I shall be writing about other things soon.
Yours sincerely,

Rudi

[413] G. Placzek to Rudolf Peierls

Schenectady, 7.3.1947

Dear Peierls,
Many thanks for your detailed letter[130] and the statement of the Council of the British Atomic Scientists, which I have read with great interest. It tends to show that, if even scientists of one nation cannot agree in the fundamentals of the matter we need not be surprised to see the politicians landed in the present hopeless mess.

You are probably aware that the statement has been extensively misquoted by Mr. Gromyko in yesterday's session of the Security Council.[131] This is of course not your fault since even if it had been possible to write it from a more unified point of view, this would not have afforded protection against distortions. I hope you can get a verbatim copy of the speech, it was rather long and the papers left out the most interesting passages. I happened to listen to it on the radio and it was rather characteristic to hear Gromyko state in terms which were clearly sincere conviction that the idea of an inspector who could freely travel

[130] Letter could not be located.

[131] On 5 March 1947 Soviet foreign minister Gromyko gave a speech in the U.N. Security Council denouncing American nuclear policy. See *New York Times*, 5.3.1947, p. 16.

in a "foreign" country and even fly over it, could not have been meant seriously.

The Russians, of course, have by no means a monopoly on such a mentality; the recent statement by Senator Taft, warning against the danger that communists might "infiltrate" into an international control agency, is on a very similar level.

Enclosed a pictorial record of yesterday's meeting, taken from today's New York Times. You will probably enjoy the subscript.

I am afraid I cannot entirely share the Olympian detachment of your American military friend who hopes that chances of an agreement might be better once stories of successful Russian bomb manufacture begin to circulate. I am rather inclined to believe the opposite. But, unfortunately, we shall see.

Clayton[132] (from Chalk River) asked me for a copy of the table of the exponential integral for complex argument, so that he could forward it to you. Unfortunately I have no copy here. I believe I left one at Montreal, but whether or not this is so, they cannot find it. The rest of the copies is in Carlson's hands[133] at Los Alamos and to extricate one from there, under present circumstances, might take longer than to have the whole tables recalculated. I therefore recommended to Clayton to get in touch with Lowan, (Math. Tables Project) who might have a spare copy or could perhaps get one reproduced.

I hope you will keep me informed on the result of your and Skyrme's investigations re Wigner Dispersion formula, and gauge invariance of Weisskopf's logarithmic divergence.

Newspaper reports here inspired, in some credulous souls, the hope that Schroedinger might have discovered the laws of the universe. Mr. W.L. Lawrence was speeded into action. Jumping at the opportunity to act as a great patron saint of science, he got hold of Schroedinger's

[132]Henry H. Clayton (1906–89), English-born physicists who had studied at the University of British Columbia. In 1945 he joined the Montreal Laboratory and moved to Chalk River in 1946. In 1950 he became head of the theoretical physics branch, a position held until his retirement in 1969.

[133]Bengt Carlson (1915–), Swedish-born physicist who had studied at Stockholm and Yale. He worked in Placzek's group at Montreal from April 1943.

manuscript, had it photostated at the Times expense, and distributed far and wide throughout the country. Of course, it turned out to be bunk. A lesson for the above mentioned souls showing that the more traditional channels of scientific information still seem to be fully adequate.
With best regards to you and Genia,
yours sincerely,

G. Placzek

[414] Rudolf Peierls to James Chadwick

[location unspecified], 10.3.1947

Dear Chadwick,
A few days ago the “Times” had an item about the last statement by Gromyko (I believe the date of the statement was 5th March) and in reporting this the American correspondent of the “Times” added that most of Gromyko’s ammunition seems to have been supplied by the memorandum from the British Atomic Scientists’ Association.[134]

In view of this we are naturally anxious, first of all, to get the verbatim text of Gromyko’s speech in order to see how he quoted us and whether the quotation was a fair one. I wonder whether you could be good enough to arrange for a transcript of this speech to be sent over by air mail?

The “Times” statement rather implies that the quotation has made an unfavourable impression in America, although I conclude this merely from the use “ammunition.” Any comment you could make about this would, of course, be very welcome. We had, some time ago, a letter from the Executive Officer of the American Delegation, in reply to our memorandum, in which his main criticisms were that some of the points we were pressing had already been taken care of in some of Baruch’s

[134] See letter [413].

statements.[135] This is, therefore, a useful confirmation that there seems to have been nothing in our memorandum that came as a violent shock to the American Delegation.
Yours sincerely,

R. Peierls

I enclose copies of both our memoranda, in case they are of use in this connection.

[415] Otto Frisch to Rudolf Peierls

[location unspecified], 10.3.1947

Dear Peierls,
Cockroft asked me today whether I was interested in becoming his successor in Cambridge (that is the Jacksonian Chair, isn't it?). I was not quite unprepared for this question since Dee had been here a few weeks ago and had told me that he felt he could not accept the offer and that he thought I would get asked next.

Now this is a difficult decision to take. I feel a certain amount of loyalty towards Harwell, and I also think that in a year or two it might be quite a well-equipped place. On the other hand, the emphasis will always — and perhaps increasingly — be on the engineering side and though we shall be able to [d]o some pure research we shall probably do it with a bad conscience, feeling that we are not going all out to develop atomic energy. In Cambridge this should not be so, and I should be able to put my whole heart in the job. And of course Cambridge is a much nicer place. On the other hand, again, my administrative responsibilities will be even greater than here, I suppose, and my inexperience in University teaching will be a handicap.

[135] Bernard Mannes Baruch (1870–1965) was the American delegate to the United Nations Atomic Energy Commission. On 14 June 1946 he had proposed the international control of atomic energy, the so-called Baruch-Plan.

I should very much like to hear what you think about it. You know the setup in Cambridge well (so well that you declined a job there!) and you might help me to understand why that chair is so hard to fill. Also, you know me fairly well.

I was very glad to hear from Fuchs that Genia is home again and that everything went well. I am going to Denmark and Sweden on Tuesday, returning immediately after Easter. (Unless I decide to cancel my berth in view of the Cambridge matter). I should be very thankful for a reply before I leave, please address it c/o Meitner, Flat 2, 85 West Hill, London, S.W.15. I shall be there on Friday night and again on Monday night, and I may go to Cambridge over the weekend or part of it.
Yours,

O.R.F.

[416] James Chadwick to Rudolf Peierls

New York, 13.3.1947
(carbon copy)

Dear Peierls,
I sent yesterday to Philip Moon the verbatim record of the meeting of the Security Council on March 5th. This is the only complete record which I have and it is my own file copy, so that I shall want it back.

Gromyko's use of your memorandum was just a typical example of Russian methods. You will be able to judge for yourself whether it was fair or not. The Security Council and the audience were certainly rather surprised to find Gromyko supporting the Russian views by means of quotations from a memorandum by the British Atomic Scientists' Association. Very few of them would, at any time, realise how dishonestly Gromyko was quoting, for very few read the memorandum. It was, as you know, published in the February issue of the Chicago Bulletin, but not many people outside the American, the Russian and our Delegation read this. Some of the journalists were naturally very interested to hear Gromyko claiming support for his thesis from English sources. They pursued that matter, and two days later both the New York Times and the Herald Tribune carried articles which I enclose. I should like to have these back.

Yours sincerely,

[J. Chadwick]

[417] Rudolf Peierls to George Placzek

[Birmingham], 24.4.1947
(carbon copy)

Dear Placzek,

Thank you for your letter of 7th March and the highly amusing picture of Uncle James.[136] Meanwhile we have had the full text of Gromyko's speech and, as you say, it is a very depressing document even if it has its amusing points. If, however, one looks at the earlier statements made at one time or another by Gromyko and Molotov and the many contradictions in them. It is clear that the fact of their saying decidedly "no" today rules out as little the possibility of their saying "yes" tomorrow as vice versa. As far as the British Atomic Scientists' Association is concerned, we feel we ought to appoint Gromyko honorary publicity manager or something for the way he has put us on the map. With one exception his quotations are quite correct, although, of course, torn from the context they give the wrong impression.

I saw the articles in the New York papers that followed Gromyko's speech by a few days in which other passages from our memorandum were quoted, in this case, of course, carefully picking those parts in which we agree with the American point of view.

I do not quite share your view that the document was quite futile; it is, of course, a question whether the present situation can be remedied at all by a rational discussion in the Atomic Energy Commission of the Security Council and it is likely that if any progress is made it may depend on discussions behind the scenes and on factors that have

[136] Letter [413].

nothing to do with the problem in hand, but to the extent that it was worthwhile to keep rational discussion going I do believe that we have raised points where obstacles to the acceptance of the details could be removed.

Even more amusing, perhaps, are the Senate hearings about Lilienthal.[137] I enjoyed particularly the picture of Groves releasing the Smyth report against his better judgement because of the bullying of scientists. On the question of tables of the complex potential integral, it will be interesting to see what ultimately comes out of the revolutions in the wheels of the machine. In practice, as you may remember, there was a blueprint of the original typescript of those tables here and that is still accessible to us. We can therefore await further developments with patience, unless in the meantime, Lowan has carried out further subtabulation. On the general dispersion formula we have not yet made much progress beyond one small point:

In the Kapur-Peierls method one expands the whole wave function inside the nucleus in a series of complex eigenfunctions. As a result, one gets a potential scattering term representing the scattering from a hard sphere and alongside with it a dispersion sum which seems to converge rather slowly. Skyrme has pointed out that one could equally use an alternative procedure, namely to expand not the whole of the function but its difference from the incident plane wave. In that case the potential scattering term represents the Born approximation and, in particular, the artificially chosen nuclear radius no longer appears explicitly. For high energies of the incident neutron it seems fairly clear that in this formula the contribution from the distant terms of the dispersion sum are smaller than in the alternative case. This will certainly be proved when the energies are so high that the Born approximation becomes a useful approximation and is likely to be true even for somewhat lower energies.

Once one has recognised that there is this amount of freedom in the method, a third alternative immediately suggests itself and that is to

[137]On 4 February 1947 David E. Lilienthal had issued a controversial statement ‘This I Deeply Believe’, also known as the ‘Credo’. See David E. Lilienthal Papers, Volume 1, 1900–1949, Box 118–119. Princeton University Library.

start by solving a one-body problem with a complex potential representing, in other words, the nucleus as a black rather than a reflecting sphere, and after subtracting the wave functions ascertained from the whole wave function expand the residue in a series as before. This means using the theory of a black nucleus as developed by Bethe as a starting point. Evidently this will lead to a formula in which the potential scattering appears explicitly as that due to a black sphere and added to it there will be again a dispersion sum.

On physical grounds I suspect that this description would be the most convenient one, i.e. it would make the dispersion sum converge more rapidly than the alternatives. Of the three possibilities this third one certainly is the most complicated one mathematically and there seems no obvious way either to prove that it is a good approximation or to decide the value of the absorption coefficient which ought to be used. I would much appreciate your comments on this situation. We have not been able to get much thrill out of the recent papers by Wigner or by Weisskopf on this subject.[138]

I agree with your comments on the wisdom or otherwise of letting newspapers distribute scientific manuscripts, but not that the traditional channels are very adequate at the moment. Printing is deplorably slow, particularly in this country, and one has to rely largely on casual gossip and, while this is apt to make more sense than the efforts of Mr. W.L.Lawrence, it is hardly more efficient.

With best wishes,

[R.E. Peierls]

P.S. I am working on a plan to have a small theoretical conference at Birmingham this summer, to deal with the fundamental difficulties and with elementary particles. The likely dates are July 23rd to 26th, and I hope to get some of the theoreticians in this country and a fair number

[138] E.P. Wigner, 'Resonance Reactions and Anomalous Scattering', *Phys. Rev.* **70**, 15–33 (1946); E.P. Wigner, 'Resonance Reactions', *Phys. Rev.* **70**, 606–18 (1946); E.P. Wigner and L. Eisenbud, 'High Angular Momenta and Long Range Interaction in Resonance Reactions', *Phys. Rev.* **72**, 29–41 (1947); H. Feshbach, D.C. Peaslee and V.F. Weisskopf, 'On the Scattering and Absorption of Particles by Atomic Nuclei', *Phys. Rev.* **71**, 145–58 (1947).

from the Continent to take part. Our funds do not make it possible to pay transatlantic fares but if, by any chance, you were intending to be on this side this summer, we would look after your board and lodgings, probably in the University hostel here, during the conference. You will get a more official notice in due course but I thought I would let you know at once that this is likely to happen. I need not say that we should be delighted to see you here.

[418] Rudolf Peierls to Robert Oppenheimer

Birmingham, May 1947

Dear Oppie,
I enclose particulars of an informal conference that we are hoping to hold at Birmingham this summer. If you or any of your colleagues are planning to be on this side of the Atlantic at the time (or are just waiting for a reason to make such a trip) we would be delighted to see you here.

I would also be grateful to have the names of others to whom you suggest such an invitation should be sent.
Yours sincerely,

R. Peierls

Congratulations on the new job! Hardly more restful?[139]

[419] Niels Bohr to Rudolf Peierls

Copenhagen, 2.6.1947

Dear Peierls,
I want to tell you that my plans have been somewhat changed since I wrote last. Circumstances are that under the pressure of various obligations I have recently been somewhat overstrained, and I have therefore

[139] Robert Oppenheimer had resigned from his post at Berkeley to take up the directorship of the Institute of Advanced Studies in Princeton.

felt it necessary to give up my journey to England and Ireland this summer and try to get some real recreation in July, such as I have not had for several years. I am sorry that I shall thus not be able to see you as I hoped, but on the other hand I am sure that the change in the plans will give us better possibilities in the near future to complete our work which had been so long postponed. I was very glad to learn that it will be possible to you to come here for a time in August or September. In the Institute we plan a little conference in the later part of September, and it would be very nice if before that time we could have some thorough discussions about the general problems. Perhaps it would be the best of you could come here in the middle of September and stay over the conference or as long as you can.
With kindest regards,
Yours,

Uncle Nick

[420] Rudolf Peierls to Niels Bohr

[Birmingham], 5.6.1947
(carbon copy)

Dear Uncle Nick,
Thank you very much for your letter. I am very sorry to hear that you have not been well, although it is hardly surprising that you should feel tired in view of the very many different things that must be making demands on your time now.

I had just heard from Møller about the proposed conference and I had written to him that things are a little difficult because of the two fairly official lectures that I had promised to give on the 20th and 25th September. I probably could get out of either or both commitments, but I would have to make the necessary arrangements very soon. Alternatively, if the conference were to start before the 20th I might be able to stay for at least part of the conference and get back in time to deliver the lecture on the 20th; but then I should start fairly soon booking my passage, as I believe pressure on space in the ships and planes is going to be very heavy. I realise of course that the precise arrangements for the conference must depend on many factors and they cannot be settled at once, but I would be very grateful to know the dates as soon as they are fixed. From what you say, it would at the moment sound best if I tried to arrive, say, about 10th September and to leave either in time to be back in London on the 19th or a little later, if the conference is later and if I can still cancel my lecture on the 20th.

I need hardly say that I am looking forward intensely to this visit.
Yours sincerely,

[R.E. Peierls]

[421] Niels Bohr to Rudolf Peierls

Copenhagen, 26.6.1947

Dear Peierls,
I am sorry not before to have answered your kind letter of June 5th,[140] but I have been somewhat doubtful what to propose as regards the best time for your visit here. Circumstances are that due to wishes of some of our American friends who could not come before, the conference here has so far been planned for the last week in September. Furthermore, Sir John Anderson is coming on an official visit to Denmark from September 11th to 18th and will be staying with us for some of the time. I hope very much that the schedule of the conference can be made to fit in with your own plans but, on account of the other commitments in September, it might perhaps be best if, instead of coming before the conference, you could manage to stay on a little longer to give us the opportunity to talk about how it stands with a possible completion of our work with Placzek.[141]

I take the occasion to tell you that Jacobsen is going to England for the last part of July and intends to come to Birmingham to learn

[140]Letter Rudolf Peierls to Niels Bohr, 5.6.1947, *Peierls Papers*, Ms.Eng.misc.b203, C.32.
[141]See Lee, *Selected Correspondence*, Vol. 1, Chapter 4.

about the developments in Oliphant's laboratory. It will also be a great pleasure for him to attend your conference, at any rate such a part of it which is not too specific mathematical. Further, one of the younger of the experimental collaborators in the Institute, Børge Madsen, who has done some beautiful work on recoil particles is going to England in July to visit various laboratories. Also Madsen intends to come to Birmingham towards the end of the months and hopes to be allowed to attend such parts of the conference as are not too far from his line. Please inform Oliphant about these visits. I need not say that it would be a great pleasure for us if he could attend your conference himself.
With kindest regards from us all,
Yours,

Uncle Nick,

P.S. Mr Madsen will arrive in London at July 2nd, where his address will be at the Society for Visiting Scientists, 5, Old Burlington Street, W.1.

[422] Rudolf Peierls to Niels Bohr

[Birmingham], 1.7.1947
(carbon copy)

Dear Uncle Nick,
Thank you for your letter of 26th June.[142] I would, of course, not have wanted you to alter the date of your conference to fit my particular plans and I think it will be possible for me to be present at least for part of the time.

It would be somewhat difficult to cancel my lecture to the Institute of Physics on September 25th since it is a rather formal affair and since I have already given them notes on what I propose to say, which have been used by the other speakers to plan their papers, and I think I

[142]Letter [421].

could not find anyone else who could take my place and say just what I would have said. I would try, however, to come across by 'plane on the afternoon of the 25th or, if that is impossible on the morning of the 26th and so could at least take part in some of the discussions. It would then be possible also for me to stay for part of the following week since our term does not start until 7th October. I would, however, want to be back here a few days before the beginning of term.

I am very glad to hear that Jacobsen and Madsen are likely to visit Birmingham and if they should find it convenient to be here at the time of our conference they will, of course, be welcome to take part in any discussions that they do not find too mathematical. Oliphant and Moon similarly are very glad to hear about these visits. It would be a help to know fairly soon when Jacobsen expects to be in Birmingham so that we can ensure that accommodation will be ready for him. I can get in touch with Madsen directly when he gets to London and find out more about his plans.

With kindest regards,
Yours sincerely,

[Rudolf Peierls]

[423] Rudolf Peierls to Niels Bohr

[Birmingham], 18.7.1947
(carbon copy)

Dear Uncle Nick,
In connection with your conference in September, I wondered whether you would allow Ferretti, who as you may know, is spending a year with me, to take part in it?[143] You probably know some of Ferretti's work;

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[143] Bruno Ferretti who been the remaining theoretical physicist in Rome after Giancarlo Wick had emigrated to America, spent a year at Birmingham. He was working on methods for solving scattering and eigenvalue problems. See B. Ferretti and M. Krook, 'On the Solution of Scattering and Related Problems', *Proc. Phys.* **60**, 481–90 (1948).

I have formed an extremely high opinion of his ability and judgement from what I have seen of him and in particular he is now working on a calculation of the energy loss of mesons in their orbits inside atoms and it is conceivable that he might produce some definite result one way or another in time for the conference.[144] I think we could probably arrange to get a grant for the expenses of his journey but before exploring this I wanted first to ask whether it would be all right for him to come.

Several others of my collaborators would, of course, also be very interested in the discussions of the conference but I imagine that, as on previous occasions, you want to keep the conference limited to a fairly small group, and then of all the people here Ferretti would seem the most likely one to make an important contribution to the discussion.

As for my own plans, I find there is unfortunately no 'plane in the evening so that I could only travel in the morning of Friday 26th September. I shall make sure of a reservation on that 'plane which would get me to Copenhagen by lunch time and I try to get a reservation to return towards the end of the following week. If you think these dates would not be suitable, would you let me know?

With best wishes,

Yours sincerely,

R.E. Peierls

[144]Peierls and Ferretti had collaborated on radiation damping theory and had produced a joint paper. R.E. Peierls and B. Ferretti, 'Radiation damping theory and the propagation of light', *Nature* **160**, 531–34 (1947).

[424] Rudolf Peierls to Hans Bethe

[Birmingham], 28.7.1947
(carbon copy)

Dear Hans,
Thank you for your letter. Our conference has just finished[145] and it was a lot of fun, though no doubt it would have been more fun if you had managed to get here. We discussed a number of problems, including the one related to your note[146]. My personal impression is that physically you are almost certain to be right, i.e. that the observed shift is due to self energy, but that this will not come out of the present theory and that, in particular if one applies present theory to this problem the reduction in the order of magnitude due to pair theory and that due to taking the difference between two states will not be cumulative, so that one will get at least still a logarithmic infinity in this result. One may not even get a definite result at all because, as far as I know, no formulation of pair theory exists which is consistent and Lorentz invariant beyond the first approximation. I do believe, however, that in a future theory in which one has eliminated the infinities the result for the level shift will look very much like yours.

We have discussed further the paper by you and Oppenheimer[147] on the Heitler theory and Ferretti has traced the trouble to the result that in the damping theory light signals do not propagate with light velocity.[148] This, of course, is the effect of the reinterpretation of the theory in which one gives up the ordinary space-time description. We have all come to the conclusion that this is a fundamental feature of all

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[145] Peierls had organised a small theoretical conference at Birmingham which dealt with the fundamental difficulties and with elementary particles. This took place between July 23rd and 26th. See letter [417].

[146] Bethe had just submitted a note on the electromagnetic shift of energy levels which was to be published later that year. H.A. Bethe, 'The Electromagnetic Shift of Energy Levels', *Phys, Rev.* **72**, 339–41 (1947).

[147] Hans A. Bethe and J.R. Oppenheimer, 'Reaction of Radiation on Electron Scattering and Heitler's Theory of Radiation Damping', *Phys. Rev.* **70**, 451–58 (1946).

[148] Ferretti's results were the basis of a paper published in Nature. See letter [423], note 144.

those theories in which one tries to throw out all self-energy terms and therefore one should now consider only theories involving a fundamental constant of the dimensions of a length in which only the contributions from very short waves are neglected but the others, which, as you point out, are needed, are retained. Your letter, of course, tends to strengthen this view.

How about plans for next year? I think it should not be difficult, if you can get leave of absence, to find a suitable position for you for a few months. Needless to say that it would appeal to me most if that position could be found in Birmingham, but it depends somewhat on what you want. There are a number of places which, at the moment, have no decent theoretician — for instance, Dee in Glasgow has only some rather junior men and a new Chair has just been created at Liverpool which I imagine it will be hard to fill. In one or the other of these places, you would therefore be carrying out a valuable job of putting the experimentalists on the right lines.

On the other hand, I take it your point in taking sabbatical leave is to get away for a time from administration and teaching duties and to sit in a place with the right atmosphere in which you could do your own research and where there would be enough younger people to pick up any spare problems you happen to scatter. From my point of view Birmingham might be the best place, though this would, of course, not exclude your meeting the people from other universities.

Another alternative would be a kind of general job, sponsored, perhaps, by the Royal Society, in which you would spend periods at all the places that want to see you, but if you want to sit down and get some work done this is hardly to be recommended. Please let me know what your ideas are on this subject so that we can set the wheels turning.

Another question is about the financial side of the arrangements. The new scheme of sabbatical leave that we are going to institute here provides that the full university salary continues and may be supplemented by a grant to cover travel expenses and higher cost of living. In trying to organise something here, one would have to know whether your regulations are similar, because if one of us went on study leave and got a grant by the university he was visiting, this would merely serve to reduce the cost to this home university, and naturally nobody here would be anxious to make a grant merely to save expenses to Cornell. On the other hand, we would, of course, be anxious to make sure that you are not out of pocket as a result of the transaction. Would you want to bring your family and, if so, would they be with you or perhaps in Belfast.[149]

There is no doubt whatever that we can arrange for you to get the necessary status and facilities and very little doubt that we can arrange an adequate grant.
Yours sincerely,

[Rudi]

[425] Rudolf Peierls to Robert Oppenheimer

Birmingham, 6.8.1947

Dear Oppie,
I enclose for your information a copy of letter we have sent to Nature[150] which I think strengthens the argument of your paper with Hans.[151] It seems to us that this objection is not confined to the specific formalism of Heitler's, but applies equally to any theory not involving a constant of the dimension of a length which would allow a quantitative distinction between the terms which are wanted and those which are not.
Yours sincerely,

R. Peierls

[149] Paul Ewald, Hans Bethe's father-in-law, had settled in Belfast.
[150] See letter [423], note 144.
[151] See letter [424], note 147.

[426] Rudolf Peierls to Hans Bethe

Birmingham, 14.8.1947
(carbon copy)

Dear Hans,
Thank you for sending me a draft of your paper on the two-meson hypothesis.[152] This is all very interesting as Uncle Nick would say, but I do not believe that the second kind of meson which you postulate can have anything to do at all with the Bristol photographs.[153] The chief reason for this is that in the Bristol technique for any heavy meson stopping in the plate, the probability of the track of the light meson, if any, also lying within the plate is very small. In other words, for any track which they have found in which a light meson is visible there must be many others which also exist, but do not appear. This means that the heavy mesons do not represent, as you say, something like 6% of all mesons stopping in the plate, but from the latest Bristol figures something like 50%. In the light of this, the discussion on your page two is somewhat misleading. Incidentally, the photographs were not taken at 30,000 feet as you say on page two, but on top of a mountain and your guesses on page five about the dimension of the plane do not make much sense, instead there was a lot of snow around.

Similarly, your footnote seventeen is hardly justified since it is very likely that there was in that case a secondary meson present but that it went out of the plane of the emulsion. Equally, of course, this may have been a light meson.

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[152] Hans A. Bethe and R.E. Marshak, 'On the two-meson hypothesis', *Phys. Rev.* **72**, 506–509 (1947).

[153] At Bristol, photographs had shown the development of light mesons from heavy heavy ones. C.M.G. Lattes, H. Muirhead, G.P.S. Occhialini and C.F. Powell, 'Processes involving charged mesons', *Nature* **159**, 694–97 (1947). The results were reported, among others, at a conference at Harwell, where Powell explained the experiments in detail. See also C.M.G. Lattes, G.P.S. Occhialini and C.F. Powell, 'Observations on the tracks of slow mesons in photographic emulsions', *Nature* **160**, 453–56 and 486–92 (1948). Further developments are described in S. Schweber, 'Shelter Island, Pocono, Oldstone. The emergence of American quantum electrodynamics after the war' *Osiris* **2**, 265–302 (1986).

I cannot help feeling that all the facts we have at present, if they are right, cannot even be understood with the help of two kinds of mesons.

I enclose a re-draft of our note to Nature which I hope is more intelligible than the first.[154]

Yours sincerely,

[Rudi]

[427] Rudolf Peierls to Klaus Fuchs

[Birmingham], 27.8.1947
(carbon copy)

Dear Klaus,

Thank you for your note about the plant design. My chief comment is that its wording, perhaps, is too aggressive for instance in the first sentence it is a bit condescending, it might be better to say "I have only minor comments on the L.S.D. plant. While I have not checked the detailed figures, I would like to mention the following points." This is almost what you have said, but I am trying to make it sound a little more polite.

In the addition to your item three, I think most of the members of the committee will understand what is meant here by differential flow, why not say "consequently the two sections will temporarily be isolated from each other."

Incidentally, I am no longer satisfied that it is really such a vital point, we tend to think too much in terms of stationary conditions, but if the differential flow ceases for a time between the two sections, will it not simply mean that the amount of light product increases at the top of the lower section so that when exchange is resumed not much has been lost?

This hardly affects your note because it is likely that the alternative place for the drum is better anyway and one ought not to accept it in the present position without checking its [e]ffect on performance.

[154] See letter [423], note 144.

H.S.D. item two. I do not know whether an increase of compression ratio would really be feasible, one might get too near to the un-stable region of the compressor.

Would it be worthwhile to end on a more optimistic note, pointing our that it is very likely that all these problems can be solved in the time available before the H.D.S. design is to be frozen. As your note stands it sounds as if there was serious doubt whether it is possible to build such a plant at all.

Yours sincerely,

[R.E. Peierls]

[428] Hans Bethe to Rudolf Peierls

Ithaca, 3.9.1947

Dear Rudi:

Thanks very much for your many letters, scientific and otherwise. I liked your paper about Heitler's Theory very much.[155] It is certainly the most striking argument against the theory that it does not give the correct velocity of light. It ought to be possible to show this not only by indirect argument but also by direct calculation. You are probably doing this right now.

I did a little more on the electro-magnetic line shift. I have done it relativistically and it is indeed finite. I do not have the numerical result because there are too many terms, each of which has to be evaluated. But the result is essentially the same as in the unrelativistic ca[l]culation. There are some interesting points in it, concerning the subtraction of the electron-magnetic mass, and also concerning the cut-off procedure for the self energy. But the main point is that it is finite and depends essentially on $\psi^2(0)$.

Thanks for your criticism of our note on the heavy meson. I am afraid the note was written in a great hurry, and what is worse, it will

[155] See letters [423], note 144.

be printed in the "Phys. Rev." without our getting any proof.[156] So the large density of mistakes will stay in. I am particularly unhappy about the mess I made concerning the altitude in which the measurements were made; as they were made on a mountain, our statement is not correct that most of the mesons are produced in the neighborhood of the measuring apparatus. You are also at least qualitatively right that there must be many heavy mesons which do not appear as such because they escape from the plate. However, I believe that quantitatively this is not quite as important as you say, because the emulsion is rather thick and amounts to about 40 * percent of the range. I think some change in ionization density should be noticed in that distance. The famous Footnote 17 is of course unintelligible, but the meson in question originates in a nuclear disintegration in the plate itself, and should therefore be a heavy meson according to our theory. Since this statement was omitted in the footnote, your objection that it might be a light meson will occur to most readers, but really it cannot be.

Now about the plans for the future. It was awfully nice of you to write immediately about the possibility for my possible visit. I have not yet got my leave approved but I am working on it and I hope it will be definite in about a month or so. The financial arrangements are that the University pays my regular salary but no expenses. I am losing a considerable amount in consultation fees from the General Electric Company which of course I cannot visit while I am in England. None of the money which I might get from an English University will go to Cornell. So it would be nice if I could get some sort of position; in this case I will probably come out just about even, that is I will be able to pay the travelling expenses and replace the loss of GE fees.

I should like to go to a place where I am really useful and should like to give some regular lectures, as long as there is not too much of this (let us say no more than six a week). I think at the present time, with physicists so scarce, I should not completely retire to the luxury of pure research. Concerning places I want to try — first of all to make a

*I did not have time to look up the numbers, so this may be wrong.

[156] See letter [424], note 147.

deal with Mott.[157] Our department here has invited Mott previously to come for a semester and would still be very anxious to have him. There is a lot of work in the solid state going on here, and I am too much out of this to give any advise. So the present plan is to ask Mott to change places with me for one semester. Please keep this to yourself for the time being, because I have not yet approached Mott, since my leave has not definitely been approved.

If this does not work out, I think Liverpool would be a very attractive place. Also, now with Frisch there,[158] Cambridge would be interesting. But I think I should let fate take its course and not put too many boundary conditions on this problem. Any University where there is an attractive program in experimental physics and where a theoretical physicist is needed, will be fine. I should like to stay at one place and not travel around too much. Of course I would want to see you frequently, but distances generally are not very great.

The plan is to take the whole family and this might make a difficulty from the standpoint of housing. We would visit Rose's parents during the summer and would expect to leave the children there while Rose and I go to the Continent for a few weeks. But when term begins, it would be nice to have the family happily reunited. The time would be approximately from July to the end of January. I do not know how this fits in with your terms; as I remember the fall term ends at Christmas. If the job I am going to get is just for the fall term, it might be very nice if I could spend a month with you in Birmingham.

This is just to give you a vague idea, I hope it won't make too much trouble for you to look for a suitable place. But both Rose and I are dreaming very much of this visit.

We had a very nice time together in southwestern Colorado. At present, Rose's mother is visiting us and it is really very nice. She has hardly changed at all in the nineteen years since I know her.
Best regards to the family.
Yours sincerely,

Hans

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[157] Nevill Mott (1905–1996) held the Chair of Theoretical Physics at Bristol at the time.

[158] Otto Frisch had accepted the Jacksonian Chair in Cambridge in 1947.

[429] Hans Bethe to Rudolf Peierls

Ithaca, 23.9.1947

Dear Rudy:
One of my students, Edwin Lennox,[159] is very much interested in spending a year or two in Europe. He will probably get his Ph.D. next June and would like to come over after that. He would like best to come to you if you would like to have him, and if you could give him some research position.

You may remember Lennox from Los Alamos while he was in Vicky's group. In the meantime, he has developed very well and had learned a lot of physics. When he was at Los Alamos, he had had only a very small amount of physics courses. I think he is very good; at least he shows great interest in all problems and tries to get a real understanding of everything in physics.

I remember that you had some research position available when you went back to Birmingham. Would any of these be available to Lennox for next year? What would be the salary that he could get? He just encumbered himself by marrying a widow with three children; however, they get some pension from the U.S. Government and possibly some financial support from the grandfather of the children. So the situation is not as desperate as it sounds and you may consider him simply as married without any children, as far as financial needs are concerned.

If it is possible for you to take Lennox, he would of course also be interested in the problems of life, especially whether there is any chance to find a place for him to live. If this looks very black, he may be better advised not to come. He would also be interested in knowing for how long your research appointments would run.

Physics continues to be very exciting especially the electromagnetic shift of the energy levels and the mesons. At a conference the other day,

[159] Edwin Lennox completed his Ph.D. with Hans Bethe and later took up a post at Ann Arbor before moving to the Department of Chemistry at the University of Illinois, Urbana.

we admired the latest pictures from Bristol, especially the one in which a meson is produced in a star and then produces another star.[160]

I hope to hear from Vicky and Placzek very soon on what happened in Copenhagen. I assume you were there.[161]

Best regards to the family.

Yours sincerely,

Hans

[430] Rudolf Peierls to Hans Bethe

[Birmingham], 27.9.1947
(carbon copy)

Dear Hans,

Thank you for your letter. I still do not agree with your interpretation of Powell's experiment.[162] The point is not that if the track goes at an angle to the plate it cannot be recognized as that of a meson, but that it is not detected at all. The plate is always horizontal under the microscope and if the track has a strong "dip" the focal plane will only contain one or a few grains at a time and, therefore, no recognizable track will appear. No doubt a more elaborate inspection technique would disclose such tracks, but there would be no point in trying this since all the information can be obtained by observing only the tracks which are nearly in the plane of the emulsion.

In any case, the others have not been looked for, and all the dozen or so cases found at Bristol are cases in which both primary and secondary

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[160] Prior to the advent of large-scale accelerators in the mid 1950s, Cecil Powell and his collaborators at Bristol (Occhialini, Lattes, Camerini, Muirhead, and many others) made a number of important discoveries in the study of cosmic radiation, so much so that Louis LePrince-Ringuet referred to Bristol as 'the big sun surrounded by little satellites'.

[161] Rudolf Peierls attended the latter part of the conference organised by Niels Bohr in late September 1947.

[162] Cecil Frank Powell (1903–1969), studied natural sciences at Cambridge where he gained his Ph.D. working under Wilson and Rutherford. He moved to Bristol where he eventually (1948) became Melville Wills Professor of Physics.

mesons lie in the plane of observation. Powell is going to publish his conclusions about the statistics.[163]

Now about your plans, I am not taking any action at present since the question of Bristol is best explored by your approaching Mott directly.

Here are, however, a few points for your information; the winter term lasts in all universities from early October until Christmas. The term is, however, not an important unit for teaching; all courses are planned on the basis of a whole session and as far as I can see it would make no difference if your visit did not cover an integer number of terms. (A whole session, October to June would be different but I take it that is too long for you.)

In Bristol, the teaching of Applied Mathematics is in the hands of the Mathematics Department. The theoretical physicists do not do the teaching. I do not think that Mott himself gives as many as six lectures a week.

At Birmingham (and in some other places) people with our background do lecture on mechanics, hydrodynamics, electricity etc. to undergraduates including engineers, and we have now a staff of three besides myself, for this purpose. It would be hard to justify a temporary appointment for this kind of work, particularly if it is only for part of the session, so that someone else would have to finish the courses which you start. Here, at any rate, it would be much easier to build a case on the stimulating effect your presence would have on the research in both experimental and theoretical physics. I cannot speak for Bristol, but I imagine things would be rather similar. If you just swap places with Mott, your main function would be to replace him in the administration of the department and in the supervision of research.

Housing is, as you know, not easy. One does, however, see from time to time furnished houses or flats at not unreasonable rents, and there are also boarding houses that might take you with the family. (Best of all close to somebody's house where Rose could go for washing or similar activities not tolerated in a boarding house.)

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[163] C.M.G. Lattes, G.P.S. Occhialini and C.F. Powell, 'Observations on the tracks of slow mesons in photographic emulsions', *Nature* **160**, 453–56 and 486–92 (1948).

Liverpool are trying to fill a new chair in theoretical physics.[164] This is likely to be filled before your visit. If they are unsuccessful and the chair is still vacant in 48–49 they would, of course, jump at the chance of having you for a short period and it would there be particularly easy to organize funds since the job exists.

Failing this, I think Cambridge should also not be difficult.

Next come some plans of mine. I find I shall have to attend a meeting in Washington on 14th and 15th November, though there is some talk of it being a week earlier. It comes at an awkward time from the point of view of my duties here and I do not want to stay away longer than necessary. But I might get in an extra day or two and in that case would, of course, very much like to come and see you. As we are very limited on dollars now, this would only work, if my fare from Washington to New York could be covered somehow. What are the chances of this? I could, of course, give a lecture on any subject.

I shall let you know more about dates as soon as possible.
Greetings to everybody.
Yours sincerely,

[Rudi]

[431] Y.I. Frenkel to Rudolf Peierls

Leningrad, 7.10.1947

My dear Peierls,
I am very sorry to have missed your attractive conference. I hope there will be another opportunity of meeting you either here or in England in the near future.

The negative theory of the results of an inspection of the present quantum theory and the mathematical intrications to which the attempts to remove its difficulties lead, seem to indicate that these

[164] The Chair was filled in 1948 by Herbert Fröhlich who remained at Liverpool until 1973.

attempts are not radical enough and that one must discard certain notions which seem inherent in the present theories and are considered as fundamental.

One of these notions is the association of the particle concept to the field one. In the case of radiation this association leads to the idea of "longitudinal photons" which are assumed to the particle correlate of the electrostatic field. In the case of meson theory a similar rôle is played by neutral mesons. I believe that both of these particles are non-existent while the field with which they are associated is a reality.

Further, matter is unusually described in a dualistic way, as a system consisting of field and particles interacting with each other, the interaction between the particles being transmitted by the field. I think that this dualistic conception of matter must be replaced by a monistic field conception, the particles appearing as quantum effects due to the field.

This programme has been outlined by Lorentz classical theory, when its development was, however, chivied by the intricacies of the problem of electron structure. Now, from the point of view of quantum theory, this problem must be considered as ficticious — just as the problem (which seems never to have been discussed) of the structure of a photon.

I do not think it necessary to introduce in the fundamental equation a quantity corresponding to the classical radius of the electron. I rather believe that one must introduce a quantity corresponding to the energy of formation of an electron pair. As a matter of fact this quantity appears in Dirac's equation as a mass of an electron. But Dirac's equation is an equation of motion, while the fundamental equation must be an equation of the electromagnetic field, the behaviour ("motion") of electrons and positrons following (as in Lorentz' theory) from the laws of the conservation of electromagnetic energy-tensor.

This is the programme which I think has to be realized.

In the case of nucleus we have a similar situation with the additional complications, corresponding to the interaction between the two fields (nuclear and electromagnetic). The main thing is to remove the particles from the field equations, introducing them eventually as quantum effects.

The publication of Soviet work in foreign languages has recently been wholly discontinued. I am afraid that this may lead to a complete loss of contact between ourselves and our foreign colleagues, unless the latter take some pains to study the elements of Russian language, which will enable them to read our papers in Russian.

It is hoped that we shall be allowed to publish a yearly report on our scientific work in English.

I shall be glad to help you, meanwhile, in getting our Russian journals for the libraries of the British universities, in particular the "Journal of Physical Chemistry, the Journal of Colloid Sciences, the Drolady (C.K. of the A of Sc.)

All is well with us both here and in Moscow. Give my love to *Genia*. *Natasha* is sending her greetings to both of you.
With kindest regards.
Yours sincerely,

J. Frenkel

[432] Rudolf Peierls to Hans Bethe

[Birmingham], 8.10.1947
(carbon copy)

Dear Hans,
Thank you for your letter about Lennox.[165] I am naturally very interested in the possibility of having him here. He must, however, realise that things are not going to be easy at all from his own point of view.

Our research fellow-ships vary in seniority and salaries range from £450 a year to £700. This is subject to the usual 5% deduction for superannuation with the University adding 10%. The superannuation is optional and would not apply to anyone who was not intending remaining in academic life in this country.

In addition there is a family allowance of £50 p.a. for each child of school age or below. I would, of course, have to know more about his

[165] Letter [429].

progress and ability before expressing an opinion where his salary would lie within the range I have quoted, but unless he has developed in quite a spectacular way, it is hardly likely that he would find himself at the top of the grade. My present guess would, therefore, be that he might well expect £500 to £550 and more if we can make out a case. I cannot also at this moment give an assurance that there will be a vacancy for him, we are likely to have a vacancy next session for which there may also be competition, but more vacancies may arise.

The terms of these fellow-ships are yearly appointments up to a total tenure of five years. This last limit is not likely to be serious since, if he wants to settle down in this country, it is quite likely that he will find a teaching job that would attract him either in Birmingham or elsewhere.

He would, however, have to pay his own passage as well as that of his family, since we have no grant that could be used for this purpose. I do not know whether it would be possible to find some outside grant to help him, but I am sure that at most his own passage could be covered in this way.

As regards living conditions here, the salaries I have quoted are adequate though not generous for young married men. To support three children on them would be very tough. Housing is still very short here and it is virtually impossible to rent a house or flat unless he is exceedingly lucky or unless he is allocated one of the very few houses under the control of the University for which, however, there is very strong competition. One can occasionally rent furnished accommodation, but that would be rather expensive and it may not be easy to find anything satisfactory for the size of his family.

It is possible to buy houses, a small modern house is likely to cost about £1,500, of which two thirds, or with luck a little more, could be covered by a mortgage. This would have to be furnished unless he can bring furniture from America, this again is difficult and expensive.

It should also be born[e] in mind that the style of dress, especially for the children, is rather different here from what it is in America, and he would, therefore, probably have to get almost completely new outfits before he came over.

It looks, therefore, as if the whole project was extremely difficult unless he was able to supplement his income at least for the initial expenses and during the first year quite substantially from private means. If he feels, provided things turn out the right way, he might settle in Europe for good, it may be worth going through all this trouble; if what he has in mind is a year or two on this side, it would seem too big a sacrifice.

Perhaps you would discuss these facts with him and if you would let us know a little more both about his progress in research and about his other plans, I may be able to give more definite advice.

The meeting to which I have to go in Washington has now definitely been fixed for 14th–16th November. I intend to leave here about the 10th and to start back on the 19th. If during that time there could be a chance of seeing you, that would, of course, be very nice.
Yours sincerely,

[Rudi]

[433] Rudolf Peierls to John Cockroft

[Birmingham], 26.10.1947
(carbon copy)

Dear Cockroft,
I have thought further about the recent difficulties over the release of photographs and other information. I feel strongly that this places us really in an impossible position for discussing declassification with the Americans. This may not merely involve technicalities, but we may have to support our advice by discussing reasons for our attitude. How can we do this if there is a likelihood of a government which instructs us acting on completely different ideas?

I feel so strongly on this point that I have seriously considered to withdraw from the delegation unless this point can be cleared up before our departure.[166] I have decided against this, since such a step by one

[166]Rudolf Peierls was planning on attending a declassification conference in Washington in November 1947. See letter [430].

member of the delegation would tend to give the impression that the scientists are divided in their attitude; this might do more harm than good. Any pressure that is brought could only come from you either personally or speaking on behalf of all of us.

There will, I hope, be a possibility of talking these things over at Malvern, but I thought it might help if you knew my views beforehand. Yours sincerely,

[Rudolf Peierls]

[434] Rudolf Peierls to Robert Oppenheimer

Birmingham, 1.11.1947

Dear Oppie,
I have to attend meetings in Washington on 14th – 16th November, and my only available transportation will get me to New York with any luck on the 6th of November. I would like to use the extra time for a few visits and I am trying to arrange to call at Princeton about November 12th. I am hoping to call at Chicago between 7th and 9th, at M.I.T. on the 19th and perhaps 11th, although I do not know whether these dates will be suitable in those two places. I shall telephone your office on arrival in New York, but if this letter reaches you in time, it would help if you would leave a message for me at La Guardia Airport where I am due to arrive by B.A.O.C. Flight 15–16a on the 6th, saying what your movements are likely to be during that week.

I do not know whether you will also happen to be in Washington at the time of our meetings,[167] and if so you may prefer to have a chat

[167]Since the passing of the Atomic Energy Act of 1946 (McMahon Act), the declassification of atomic energy information, both basic scientific and related technical information, was regulated by this legislation. Under the terms of the McMahon Act, the so-called 'restricted data' (RD) which concerned the manufacture and utilization of atomic weapons, as well as the production of fissionable material, was no longer to be shared with other countries. UK scientists, however, were consulted in declassification meetings, such as the one in Washington in November 1947.

there rather than in Princeton where you may be busy, but in that case I would still try to visit Princeton to see Wigner and others.
Yours sincerely,

R.E. Peierls

[435] Rudolf Peierls to Niels Bohr

Birmingham, 2.11.1947
(carbon copy)

Dear Uncle Nick,
Here is, at last, the promised redraft of the paper.[168] I was very disappointed that it took me so long to get it ready, but I struck a number of minor formal difficulties in the presentation. It also has increased somewhat in length, but I think it would be hard to say the same things in a substantially shorter paper.

I am particularly sorry about the delay since, owing to difficulties with transportation, I have to leave for the United States already on 5th November, so that there is no hope of getting your comments before I leave. Your comments would reach me, if they were sent c/o British Supply Office, P.O. Box 680, Benjamin Franklin Station, Washington D.C. to get there by 15th November, or c/o Bethe at Cornell, by the 18th. This applies in particular if there are any questions you would like me to discuss with Placzek.[169] Otherwise, it would, of course, be quite all right for you to make any changes you wish and send the paper off. I hope to be back here on the 21st November.

[168] Here Peierls refers to the second draft of the manuscript entitled 'On the Mechanism of Transmutations of Atomic Nuclei. II. Processes in the Continuous Energy Region of the Compound State', reproduced in R.E. Peierls (ed.), *Niels Bohr. Collected Works*, Vol. 9, Amsterdam: North Holland, 1986 (cited hereinafter as *Bohr. Collected Works*), pp. 487–502. It was produced after agreement had been reached by Bohr, Placzek and Peierls that the latter should update the pre-war drafts of their joint paper in order to publish it.

[169] Georg Placzek had moved from Los Alamos to a job with General Electric, until he secured a position at Princeton, working at the Institute for Advanced Studies with Oppenheimer.

It may help if I add a few notes on my reasons for changes which I have made. ("Old draft" here refers to the typescript dated 9.10.47, which you sent me.[170])

Section 1: This is meant to be a rough sketch, and you may like to change the wording of this.

Section 2: This is substantially the old section 1, but with the discussion of the Breit-Wigner formula omitted,[171] as we agreed. I have left the discussion of the phenomena at very high energies, which, I believe, helps to complete the picture. You were doubtful whether this should not be omitted. If that still is your view, it can easily be taken out without breaking the continuity of the rest. I have added on p. 3, the point that the "potential scattering" need not be exclusively elastic.

Section 3: This is the new section which gives an elementary derivation of the Breit-Wigner formula. I gave a misleading picture of this in Copenhagen by stating that four principles are involved. Actually, the conservation theorem is not necessary for this purpose.

Section 4: It seems more logical to discuss detailed balancing *before* the conservation theorem, and this required some slight changes in this section, which otherwise is just the old section 3.

I have shortened somewhat the discussion of the precautions necessary in applying detailed balancing to quantum problems. It seems a satisfactory point of view that, in all cases in which the states of the

[170] This refers to a typescript produced from pre-war manuscripts which were typed up to form the basis of Peierls' attempt to redraft the paper. See R. Peierls, 'Introduction', *Bohr. Collected Works*, Vol. 9, p. 50.

[171] Breit and Wigner had derived the general theory of resonance processes for a single resonance level (G. Breit and E. Wigner, 'Capture of Slow Neutrons', *Phys. Rev.* **49**, 519–31 (1936).) When applied to the case where the width of the resonance level is greater than their spacing, the Breit-Wigner method gives a result that does not conform to the derivation of the cross section for the formation of the compound nucleus by the capture of a particle from the general theorem of detailed balancing. Contrary to Kalckar, Oppenheimer and Serber, who believed the Breit-Wigner formula to give the correct answer (F. Kalckar, J.R. Oppenheimer and R. Serber, 'Note on Resonances in Transmutation of Light Nuclei', *Phys. Rev.* **52**, 279–82 (1937)), Bohr, Peierls and Placzek argued that, in fact, the answer from the detailed balancing argument was the right one.

compound nucleus can be described as definite states in the sense of the quantum mechanics formalism, the formalism automatically implies the law of detailed balancing, and it is therefore not necessary to construct an actual circular process which would violate the Second Law of thermodynamics, if detailed balancing did not hold. (It must of course always be possible to construct such a process.)

Section 5: This is based on the old section 2. I have altered the mathematics slightly by retaining the complex scattering amplitude S_i rather than splitting it at once into modules and phases. This seems to make it a little easier to see how the potential scattering term enters in (32). On the potential scattering term itself, it seems to me unnecessary to regard the application of (32) to the potential scattering alone as approximate (old p. 14 bottom) since the potential scattering by itself should be defined as the solution of a definite wave equation. The interference between potential and "true" scattering by itself would present no difficulty at all if the potential scattering were purely elastic. This was true in the very first draft of this part in which the potential scattering was defined in a formal way based on the Peierls-Kapur equations. In the view now taken, which I regard more satisfactory from a physical point of view, this is no longer true, but this leaves a certain amount of conjecture as regards the interference between the inelastic potential and "true" scattering. A more precise answer could, however, come only from a much more quantitative study, including a definite model for the potential scattering, and I feel we ought not to attempt this at the present stage. I have also put back the generalization to particles with spin to a later point so as to avoid introducing the quantity that is called δ_{AJ}. The reason is that in the case of spin one must consider, instead of one incident wave, a number of different waves with different spin directions, which are incoherent, and which, in general will have different phases. The cross section is then obtained by averaging. I believe therefore that an equation like (26) of the old draft could not be justified in the case of particles with spin, though, of course the inequality (35) with J in place of l will still hold.

I have omitted the analogy with the scattering of light by a system of oscillators in a box. It seems to me that most readers would not be sufficiently familiar with the theory of this model to accept the statements about its properties as obvious, and one would hardly want to present an extensive mathematical study of the model.

I have also omitted the statement at the bottom of p. 12 of the old draft that in the continuous level region the phase is always that corresponding to full resonance. It seems hard to justify this in a convincing manner. In the pre-war draft of the statement, this was justified from the Peierls-Kapur formalism,[172] but it depended again on a very formal, and probably inconvenient, definition of the potential scattering. Since the statement is not needed for the conclusion, it seemed wiser to omit it.

Section 6: is very short, and you may prefer to treat it as part of section 5. This can be done by just omitting the heading.
With best wishes.
Yours sincerely,

[R. Peierls]

Apologies for the typing, which is my own.

[436] Rudolf Peierls to Hans Bethe

Birmingham, 24.11.1947
(carbon copy)

Dear Hans,
On my return here, I found a list of people who have been invited to write reports for the Solvay Conference,[173] and I am glad to see your name. I had already promised to write a report on self-energy problems, not knowing, of course, the rapid progress that is now taking place in the United States in this field. Definitely we should try and avoid too

[172]This formalism was developed in P.L. Kapur and R. Peierls, 'The dispersion formula for nuclear reactions', *Proc. Roy. Soc.* **A166**, 277–95 (1938).
[173]The 8th Solvay Conference, the first post-war conference, took place in September 1948 in Brussels.

much overlap and we ought to agree what is the best division of labour, if any. Could you let me know as soon as possible whether you are proposing to come to the conference, whether you are agreeing to write a report and what your views are about the best division.

For example, I might try to summarise the account of the difficulties of the old theories including a criticism of the attempts by Dirac, Gustafson, Heitler etc., and deal with the position of theories not including perturbation. This would include your own proof that the divergence does not depend on perturbation theory. It would also include the classical theory of McManus and the general method of Feynman to the extent to which I have understood it, or shall understand it. It would leave you all those theories in which finite results can be obtained by an intelligent application of perturbation theory without modification, including your own work, the recent Princeton results, Schwinger etc. This, however, is only a tentative suggestions and any division would be all right with me provided it does not involve my writing about the Princeton results I have only heard in conversation, and Schwinger's calculations[174] which I do not know at all.

With best wishes,
Yours sincerely,

[Rudi]

[174]After Bethe had completed non-relativistic calculations about the Lamb shift and magnetic anomalies of the electron momentum, Weisskopf and Schwinger had done some relativistic calculations. See S. Schwinger, 'On quantum electrodynamics and the magnetic moment of the electron', *Phys. Rev.* **73**, 416–17 (1948).

[437] Rudolf Peierls to Abram Pais

[Birmingham], 27.11.1947
(carbon copy)

Dear Pais,
Thank you for your letter and typescript[175] which arrived here yesterday. I am writing at once to let you have my immediate reactions. If within the next few days I have any second thoughts or suggestions from my collaborators here, I shall send them off on the off chance that you may still be able to use them.

I have not so far found any further reference to be added to your survey or any further papers to reproduce. I am also not aware of any errors in my paper.

Your survey seems to me an admirable and most helpful piece of work. Of a number of comments, perhaps the most serious one refers to the discussion of the Lorentz and Abraham electrons starting on page six, and the corresponding quantum theories. To my mind the discussion involving self-stress has always appeared as a most unnecessary piece of learned complication, although I realise the part it has played in the historical development. Surely all that one must realise is (a) that a formula based on an electron with rigid charge distribution not subject to Lorentz contraction violated relativity in the most elementary way; (b) that a charge distribution which is subject to Lorentz contraction can be formulated in an invariant way as long as only motion with uniform velocity is considered, but that such a motion becomes impossible if acceleration is allowed because of the well-known fact that relativity does not permit the existence of a rigid structure capable of being accelerated. This, of course, can be traced to the fact that a charge distribution which depends uniquely on the location of its centre, involves

[175]Letter could not be located. Abraham Pais had attended the Shelter Island Conference 'on the foundations of quantum mechanics' between 1st and 3rd June 1947. On the suggestion of Oppenheimer and Wheeler, he edited a collection of earlier papers to be published in 1948. The book itself was never published, but the preface written by Pais was published as A. Pais, *Developments in the Theory of the Electron*, Princeton: Princeton University Press, 1948.

of necessity a transmission of impulses with infinite velocity (since a change of velocity at the centre must cause instantaneously corresponding change in velocity of the distant parts of the distribution). From this statement it is clear that neither the Abraham nor the Lorentz picture can be compatible with relativity dynamics.

Historically this discussion was important at a time when not only the electron problem, but also the problem of relativity dynamics were in question. Now that the principles of relativity are no longer in doubt, I see no advantage in discussing in detail the feature of schemes evidently not compatible with these principles. My own feeling would, therefore, be that the fewer equations written for these schemes, the clearer the story will become to the reader and one can, of course, refer for the mathematical details to the references you are quoting. Similarly, I do not see much advantage in classifying, as you do on page nine, the non-invariant theories according to just where they go wrong. Similarly, (unless I have misunderstood an important point) you place some weight on the fact that there is a procedure in quantum theory leading to your equations (7a') and (7b') which are equivalent to the classical theory which is of historical importance but which is inadmissible owing to its contradiction to relativity. Other smaller points, page ten, line fifteen, it is misleading to say that the Dirac theory leads to that of Lorentz. The Lorentz equation was meant only as an approximate one, neglecting higher terms which would be important if the frequencies of the motion became compatible to c/...

In this connection it would, I think, be useful to mention the remark, due, as far as I know, to McManus here, that the Dirac theory here is equivalent to putting into the Lorentz theory a negative mechanical mass so as to make the total mass equal to the observed value and then letting the electron radius tend to zero. I found this helpful in understanding the significance of the runaway solutions. Page fourteen, footnote — "The Kramers theory mentioned previously", I did not see the reference to which this applies. I suspect it might apply to your reference (27) which is only mentioned on the following page and which in any case is merely a private communication. If this is correct, would it be possible to give a little more detail? Otherwise, could you make it clearer which application by Kramers you had in mind? Page fourteen, line nine, “It should be recalled” this is one of Bohr’s favourite expressions, but I think one should be a little more explicit. Page 24a, your account of the difficulties conveys the impression that the source of the trouble is the fact that the new terms contain higher powers of the field intensities. If this were the only difficulty, one might, of course, question, as Bohr has done, the validity of the derivation of such equations from correspondents. Nevertheless, it might still be possible to derive the non-linear field equations resulting from such a scheme by means of a new Hamiltonian and then apply the standard procedure of quantization. I have always thought that the real trouble was the occurrence in the equations of motion of quantities containing time derivatives of higher order or even the values of the field equations at different times. This means that the equations are no longer of Hamiltonian form and that, therefore, the standard procedure of quantization can no longer be applied. This is important because if this were the main point, it would mean that there might be a chance of getting over the difficulty by means of the Feynman method which starts from the action function directly without the use of a Hamiltonian. Evidently the equations in question could be derived from an action principle. I am not yet satisfied, however, that the Feynman method wold be applicable to such a situation. Also, of course, it does not follow that the results would be physically reasonable. Page 28, footnote, you mention a quantity λ_{crit} of which I could not find a definition. This may be due to my hurried reading, but it would also help, if a clear statement were given somewhere what you mean by λ_{crit}. Page 30, line 5 from bottom you refer to page 20a which is not included in my copy. As page 20 refers to my old paper, II am naturally interested to know whether I have missed there an important addition. Also, in the list of references, items 104 and 105 are omitted in my copy and I would be glad to know what these are. Reference 87 strikes me as odd, only a minority of your readers will have been present at the conference and the purpose of the reference to such a discussion is usually to show the channel through which you have learned of somebody else’s views. In quoting yourself in this way the impression is given that you merely want to ensure the priority of having made an unspecified statement at a certain time, and I am sure that is not you intention.

I do not know whether it is worthwhile to include amongst the list of classical theories, say on page twelve, a reference to the integral field equations by McManus which I reported to you the other day. I still believe that from a classical point of view these are free from all objections one can raise against the proposals which you list. I have just sent to Feynman a typescript containing mathematical details to which, no doubt, you could refer if you wish. On the other hand, I appreciate that it is hopeless to attempt to be completely up-to-date if you ever want to go to press, and it may well be too late to include a reference to this theory.

One last remark, if it is possible in the available time, it would, I think, be worthwhile to check the language of your survey. Some of the sentences do not read very well and I had to read some of the passages several times, before I could make out their sense.

Yours sincerely

R.E. Peierls

[438] Hans Bethe to Rudolf Peierls

Ithaca, 4.12.1947

Dear Rudi,

Thanks for your letter of 24th November.[176] It tells me, among other things, that you arrived safely back in England which is fine. In the meantime, we also saw Fuchs whose visit was very nice.

For the Solvay Conference, I would have proposed exactly the same division of labor as you. I wrote Bragg that I would talk about level shift and related problems. By this I mean the Princeton results, Schwinger's results and any other results we might obtain in subtraction physics. I would leave everything about the free electron and the relation to classical theory to you. So in short, I agree.

I[t] was very fine to have you here and we all got a lot of ideas from your visit. Unfortunately, Feynman has not yet quantized your theory

[176]Letter [436].

but got sidetracked into some invariance problems. He and Lennox have shown tentatively that Pais' method for getting a finite self-energy does not give an invariant result.[177]

The last two days Vicki was here, so we got still more ideas and still less work done.

I will let you know if anything new develops.

Greetings to everybody, especially to Genia.

Yours sincerely,

Hans

[439] Rudolf Peierls to Nevill Mott

[Birmingham], 17.12.1947
(carbon copy)

Dear Mott,

In connection with the conference in Bristol that you mentioned to me, I have the impression that this would not overlap seriously with conference we might hold here on somewhat similar lines to the one last year. Ours would concentrate on fundamental theory, steering clear, as far as possible, of cosmic rays, but concentrating on the problem of self energy, pair theory and the like.

We might also include nuclear reactions and in that case widen the conference to one that would include experiments as well as theory and make it a joint affair with Oliphant's department. This will, I think, not overlap much with your plans unless the latter included some sessions devoted specifically to Powell's work on n-p scattering, or on the D-O reactions etc. As you said your conference would start on the 20th of September, I am planning to have ours start on the 13th, but perhaps finish it well before the end of the week so as to give some time for foreign visitors who want to visit other places in between. I would

[177]Hans Bethe reported the results at the Solvay Conference. H.A. Bethe, *Report to the Solvay Conference*, September 1948. See also R.P. Feynman, 'Relativistic Cut-Off for Quantum Electrodynamics', *Phys. Rev.* **74**, 1430—1438 (1948).

be glad to hear your comments on these suggestions and also whether I can assume that the date of your conference is fixed. I would also like to write to Bohr in order to make sure our plans would not interfere with anything he has in mind and in that connection would like to mention your conference. Is that in order or are your plans still confidential?

R.E. Peierls

[440] Our Relations with German Scientists

[date unspecified, probably early 1948]

Since the war there have been few occasions for contact between British and German scientists. Gradually scientific life in Germany seems to get organised again, and with the resumption of scientific publications there are bound to be more exchanges of scientific views and, sooner or later, more opportunities for personal contacts. How will we receive our German colleagues?

I have put off writing anything on the subject in the hope that what I am going to say might be said by others. I regard myself as poorly qualified to write, for two reasons: Firstly, I have not visited Germany since the war, and my knowledge of scientific life there is based on reading and on other people's reports. Secondly, as a former German, I may be suspected of prejudice. However, the matter is important and if others will not speak I cannot remain silent.

The problem of the scientists is, of course, not an isolated one, but it is linked up with the position of all the German people after the war. I shall concentrate on the scientist because that is the aspect of the problem of which I have detailed knowledge, and also because it is a particularly acute one in view of the particularly close personal relations that usually exist between scientists of all countries.

Amongst scientists, as amongst other people, one finds in this country a widespread readiness to forget the past, to blame the war and the pre-war events on a few political leaders who have been (or in some cases will be) tried before the courts and to reestablish with all our German colleagues the relations we had when we broke off. This is an attitude which deserves the greatest admiration. It is amazing that after a war which caused so much suffering in this country and which brought Britain so near to ruin, there should be so little ill feeling against the former enemies. It is almost incredible that, with food here severely rationed, pressure of public opinion should have forced the authorities to grant permission for food parcels to be sent to Germany, and that even now, the correspondence columns of the papers reflect more concern with the plight of the Germans than the opposite.

All this reflects a mature and enlightened understanding of the fact that there is no sense in collective retribution, that personal hardship on individual Germans will bring no comfort to people here who suffered as a result of the war. It is good to be generous, but there is danger in indiscriminate generosity. It must be remembered that in the interest of the Germans themselves, and in the interest of the future peace of the world, the rebuilding of a sane public life in Germany is of the utmost importance. We must, therefore, consider what effect our attitude is having on German public life, and scientific life in particular.

During (and before) the rise of the Nazi party to power, there were some German scholars who were active supporters of the party and who were taken in by the catch phrases and the 'theories' of Nazism. They were few in number. The ideas of the party were so crude that extreme passion, ignorance or stupidity were necessary to be taken in by them, and the few who fall in that category were not as a rule scholars of rank. They can safely be ignored.

But there were vastly greater numbers of people who had perfectly sufficient intelligence not to be taken in. When in 1933 the Nazi "reforms" hit the German universities there were certainly a large majority of their staff who would not have themselves advanced such changes, and who, if you had asked them, would have declared themselves in favour of academic freedom. They did not like the idea of colleagues being dismissed for no other reason than that they were Liberals, Socialists or Jews. Yet, when the changes came, nearly all of them acquiesced and if they were not personally affected, continued to serve under the new system. This involved in many cases adopting the new language of the Nazi system, taking part in ceremonies glorifying this regime and professing admiration for it.

A few people would not play. Some left quickly and accepted jobs abroad, some went underground and tried to fight the regime. They soon ended up in a concentration camp or realised the hopelessness of their struggle and left the country. The number included many young people with no established reputation, or with professions in which the work cannot easily be transplanted and these often had to struggle hard to find work abroad in spite of the very generous reception many refugees found in other countries. Some stayed in their jobs with barely concealed disgust, but kept away from administrative responsibility so as to avoid becoming tools of the vicious system. They did not usually go as far as to oppose the regime outright and thus to court certain disaster, but they tried to keep their hands clean.

But the majority of university teachers made their peace with the system. Certainly they regretted the excesses, though reading only German newspapers it was easy to forget such uncomfortable facts as concentration camps. They did not like to see useful members of their staff discharged for non-professional reasons and if by pleading it was possible to retain such a person they did their best. But on the whole they accepted the instructions of the authorities loyally just as, being good citizens, they had followed the laws of the previous regime. In fact, as it was considerably more dangerous to be found disloyal to the new regime, the new laws were observed more scrupulously than the old ones.

Many of these men must at some time have considered the question of resigning, but as a rule they persuaded themselves that it was their duty to stay in their jobs (or to advance into the jobs vacated by the dismissals) so as to exercise a restraining influence and perhaps to protect some of their junior staff (in practice such protection was never effective for long) or at the most to save their families from hardship. There is, of course, a vast difference here, between junior people for whom resignation might have meant starvation for themselves and their dependents. Much as one admires the few who would not bend no matter what the consequences, one cannot expect everyone to be a hero. But no heroism was involved for the front-rank men, many of whom could have found jobs abroad at once, or who (during earlier years) could have left the country with part of their savings and would never have been destitute, though resignation would have entailed serious sacrifice for them too. Few would admit that they stayed for the sake of their own comfort. This did not always mean that the money mattered; many were too keen to keep their positions because of the prestige (and sometimes because of the research facilities) attached to them.

These front-rank men could have given a lead that might have been followed by many of juniors (though the risk was greater for the juniors). We have heard of the refusal of the personnel of a Dutch University to sign a loyalty declaration during the occupation. This resulted in wholesale arrests and deportations. But even the Nazi regime would have hesitated to carry on without the majority of German University teachers. Resignation of a majority might have won the universities respect and a measure of freedom.

All of us who were lucky enough not to be faced with a decision of this kind will wonder how sure we are of our own courage in such a situation, and none of us can be quite sure before we have faced it in reality. Yet I refuse to believe that any authority whose justification rests on the shoddy formulations as the Nazi ideology could manage to dismiss a number of University teachers in this country with the remainder staying loyally at their jobs.

The general inertia amongst the senior people set the pattern for the younger ones. The more declarations of loyalty were signed by leading people, the greater was the risk for any other man who wanted to keep out. Accordingly the courageous few who risked serious suffering felt almost greater resentment against the "good citizens" who compromised for the sake of peace and comfort, than against the few fools who were taken in, or the scoundrels who joined the party for personal gain.

Now they are nearly all back in academic life. The good citizen, who is good at signing papers and getting along with the authorities, is back in the leading job and gets on excellently with the representatives of Military Government. He has now found the courage to disassociate himself from the Nazi creed. He is beginning to discover that it is much less dangerous to contradict the Military Government than to contradict the Nazis, and he is now found to hold an opinion of his own. Of the active collaborators, some are back, too, since they have a chance of counting as "victims of Nazi persecution" if they had a row with a party official or got into a concentration camp for embezzling party funds. The decent people, who managed to keep their hands clean are back. They are much more awkward people to deal with; they think about principles and they get worried if they think the Military Government is making mistakes. They are not too easy to get along with.

The real victims are back too — if they have survived. They are rarely in leading positions; they have been out of the business for too long to have administrative experience or to be up-to-date in their professional work. So they work under the good citizen whom it makes very uncomfortable to look at them or in some cases under the old party member who proved his integrity by having a row with Nazi officials. On paper they have some privileges, they do not amount to very much.

In this atmosphere the education continues. Many of them are still influenced by their earlier education under the Nazi system. They are, to a varying degree, ready to accept new ideas and there is no doubt that the spirit of their teachers and professional leaders exercises a profound influence on their attitude, and thus may determine the attitude of the future professional men and administrators. Here is the place where it would be vitally important to encourage the democratic ideals, unprejudiced thought about the past as well as the future, personal integrity, and independence of opinion.

On several occasions I have met Germans who throughout the war have managed both to retain their self-respect in spite of temptation and their lives in spite of persecution, and they deeply regretted that the Universities were reopened too soon. They argued that compromises were justified in the case of key administrators or technicians in industry without them the economy of the country would have deteriorated even more. If reliable people could not be found for these jobs, one had to make do with more doubtful ones.

But in the Universities, the position is different. The aims they serve are long-range ones, and one has to balance the immediate effects of a shortage of professional men against the danger of allowing again the growth of a professional class whose spirit may repeat to a dangerous extent the pattern that was a vital element in the growth of Nazism. Much better for Germany, those Germans argue, to build up a few centres staffed with people whose records are known, and who would attract in the course of time, others of similar attitude. One or two such centres do, I believe, exist, but they are overshadowed by the majority of other universities.

This is the background against which we should consider our attitude to German colleagues. We much remember the prestige attached, in German eyes, to an invitation to go abroad, at a time when foreign travel is virtually impossible, the effect on morale of close personal relations with people in other countries, and above all the effect on the feelings of those few decent people who do not get the same reception.

I do not, of course, for a moment wish to suggest that we should interrupt, or fail to resume, the exchange of scientific information. If a scientist, whatever his personal record, has ideas or results to contribute, it would be foolish and contrary to the principles of scholarship to take no notice of them. Equally we would wish him to be acquainted with our own results and ideas which we publish in scientific literature so as to make them openly available to all.

Most of us are so accustomed to carry on technical discussion with our good friends that it seems almost impossible to distinguish one's personal relations from the professional ones. And evidently it is much pleasanter, and more convenient, if we are able to receive our colleagues as friends. But it is certainly not impossible to draw a distinction. Those of us who had the experience between 1933 and 1939 of meeting German scientists whom they knew to be party members and in active support of the regime, will remember that it was painful but quite possible to discuss technical matters without letting one's memory of the other person's record be blunted. We might get some advice on this from scientists in countries occupied during the war for whom it was a common experience to meet men whom they respected professionally, but from whom they wanted to keep aloof personally and socially.

It is not, of course, easy to discriminate, quite often we do not know enough about each of these men to define our attitude. It would be much more comfortable for us if this work were done by the authorities, or by the denazification tribunals. But their decisions rest on a different basis.

They are concerned with questions of expediency as well as principle and (much as we may deplore the large part expedience plays in German university plans) they do not decide the problem which I have tried to formulate.

The problem of deciding is not unsurmountable. About the senior men, who held positions of responsibility under the old regime, much is widely known, and more can be learned from colleagues in Holland, Belgium, Scandinavia and other countries who mostly had first-hand knowledge of this problem at a time when it was far less academic than it is now. Much less is known about the younger ones, but about them we can far better afford to be generous in case of doubt.

[441] Nevill Mott to Rudolf Peierls

Bristol, [date unspecified][178]

Dear Peierls,
I promised Kurti to write something on Skinner's proposals, and this led me into putting down my opinion now ab[out] control of A[tomic] E[nergy]. I hope you can read my scrawl.

Would you like to have it circulated to the A.S.A.Council as a basis for discussion? Actually, I don't think that we shall any more get agreed statements — unless we shed our left wing — if then!

Also if people want to meet and discuss our policy on this, I think we should urge them to put down their views in writing first, or else say how they agree or disagree with a statement like this — purposely rather than controversial.
P[lease] return for typing.

N.F. Mott

[178]The letter was received on 6.1.1948.

[442] Nevill Mott to Rudolf Peierls

Bristol, 2.2.1948

Dear Peierls,
Thank you for sending me your MS.[179] As you know, though I agree with your diagnosis, I do not agree with your conclusions.

I do, of course, agree with what you say about the wet behaviour of German University Professors during the war. Also that I can't imagine British Universities behaving in the same way. The question is, what to do about it? I believe your solution impossible. One cannot dig up the political past of each man whom one might invite to come here. I tried over Justi,[180] to whom Simon had strong objections. I could not find anyone else in Holland or England who knew Justi (I don't mean I wrote more than a dozen letters). What I did get was a lot of opinions, favourable as unfavourable, on Simon's judgements in such matters. The whole thing made me rather sick. I shall not do it again.

Have you read last week's leading article in the Economist? I very much agree with it. We ought now to give the Germans full authority again over their own economic and cultural affairs. To attempt to reeducate them through Military Government will probably have the opposite effect to what we intend. Then, as regards to our scientific contacts, I think they should be based on scientific achievement, and be coupled with normal friendliness. What you ask, (though your case looks strong in this instance) is that the scientific invitations should be affected by political judgements. It seems to me simply untrue that we as a body, can choose "decent" Germans (or decent Americans, those who have stood up to the Committee of Unamerican activities???) because noone will agree on who is "decent". Your proposal opens the door to such questions as "is he anti-nazi?'; "Is he anti-Soviet?"; "Is he pro or anti-Marshall ⋯?", and I would not like to see this enter our scientific relations.

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[179] Item [440].

[180] Eduard Justi (1904–1986), German electrochemist who had been working on fuel cells at the University of Braunschweig. He later became professor of low-temperature physics at Braunschweig (1946–74).

Well, like the question of whether or not we should make atomic bombs, these are emotional and ethical judgements. I don't expect to convince you; your experience is different from mine and, of course, you'll have different ideas on what is of first importance.
Yours ever,

N.F.M

[443] Rudolf Peierls to Nevill Mott

[Birmingham], 6.2.1948
(carbon copy)

Dear Mott,
As you say,[181] the problem of German scientists is one on which we shall have to agree to differ, but I would like to comment on one or two points in your letter.

I quite agree, of course, and I have experienced myself, the difficulty of finding out something about the lesser known people, but the point is the importance of the cases of the prominent people whose record is known to everybody in Germany, since what matters i[s] the effect on German people.

I also agree that there will never be unanimity on the merits of one particular person, but what I am asking is not that we should agree, but that each of us should form judgement, not of course about anybody's political views but on his personal integrity which is a rather different story.

If any scientists in this country had been convicted of robbing a bank or fraudulent bankruptcy, we would still listen to his lectures or read his papers, but if an occasion arose to ask him to dinner or shake his hand on a social occasion, we should want to know what we personally felt about his record.

To quote a relevant example, if we will have an occasion again to meet Nunn-May, no doubt, people's reactions will differ to what their

[181] Peierls refers to letter [442].

personal relations to him should be, some will decide that their behaviour will not be influenced by his actions, others will have nothing to do with him, but this is quite apart from what we think about his political views. All of us will wish to do what we can to see that he is enabled to carry on scientific work in the most favourable circumstances. All I am asking for is the things that people in Germany did or failed to do, be considered as important as robbing a bank or violating the Official Secrets Act.

There is a lot in what you say in letting Germans run their own affairs, but this would have been a lot more convincing if it had been done from the beginning. This would have led to a good deal of unpleasantness, possibly even a good deal of bloodshed, but after having intervened to restore order and to back the "respectable" people, the occupation authorities have accepted the responsibility which they cannot suddenly drop.

If my article gets published, I hope this will start some correspondence including letters from people holding views like yours and the result of that will be to help people appreciate the issues and make up their minds one way or another.
Yours sincerely,

R.E. Peierls

[444] Rudolf Peierls to Niels Bohr

Birmingham, 6.2.1948
(carbon copy)

Dear Uncle Nick,
I should probably have written before to say that I had a brief opportunity while I was in America[182] to discuss the draft of our paper

[182]Rudolf Peierls had been to the US for a conference, and on that occasion met George Placzek with whom he discussed the Bohr-Peierls-Placzek paper. Peierls appears to have left a copy of the draft with Placzek who commented on it and passed the revised manuscript on to Bohr, when the latter came to Princeton in May 1948. See R. Peierls, 'Introduction', *Bohr: Collected Works*, Vol. 9, p. 51.

with Placzek. Placzek raised a number of small points that might want amending, but it seemed to us that all these could be taken care of by alterations of a few words, they could well wait until we knew your reaction to the main outline. It has occurred to me that one of these points might be causing you difficulty and that it might save you trouble to draw your attention to it. It concerns the derivation of the Breit-Wigner formula.

The derivation which I have sketched is valid only for the part of the resonance curve for which the kinetic energy of the emerging neutron (or other particle) differs only by a small fraction from its value at resonance. It does not cover either cases in which the width of the resonance level is comparable to the kinetic energy of the neutron at resonance or the cross section for thermal neutrons ($1/\nu$ law). We tried to see whether it was easy to generalise the derivation so as to cover these cases as well, but we felt that this was not possible without spoiling the transparency of the argument, but that it was preferable, therefore, to leave the derivation as it stands and merely to make clear to which category of problem it is applicable. Since the purpose of the paper is mainly to deal with high energies, it would be quite reasonable to use an argument which is not appropriate for very low velocities.

I am afraid that local arrangements made it necessary to make a decision on our plans for the summer about which I wrote to you before, and we have decided to go ahead with a conference here in the week starting 20th September.[183] This will be a joint affair of the Physics and Mathematical Physics Departments. I very much hope that in doing so, we are not clashing too badly with any plans you have in mind.

With very best wishes to all friends in Copenhagen,

Yours sincerely,

R.E. Peierls

[183]While Rudolf Peierls was at Birmingham, he co-organised two major international conferences, one in 1948 and one in 1953. See Peierls, *Bird of Passage*, pp. 261–2.

[445] Rudolf Peierls to Werner Heisenberg

[Birmingham], 11.2.1948
(carbon copy)

Dear Professor Heisenberg,
I hear that you are spending some time in this country and I would be very glad if it were possible to visit Birmingham and talk to our Seminar. I have a group of about 15 people, most of whom are working on field theory and other fundamental problems and of whom all have heard a good deal of fundamental theory so that they will appreciate a talk on a rather advanced level. They would also be interested to hear about superconductivity, though they are much less familiar with the background there and one could not assume too much to be known. Our seminar normally meets on Thursday afternoons, but if necessary it would be easy to arrange a meeting on another day. In any case, you would have to spend at least one night in Birmingham and if you can manage it, it would be even better if you could stay for several days so that there would be more opportunity for full discussion of any problems you care to tell us about and also of the work we are trying to do here.

I have lately thought a good deal about the problems arising from the relations of scientists here and in other countries with those in Germany, and you may be interested to see what my views are from the enclosed copy of an article that I hope to get published.[184] I feel that while these problems are not easy to discuss, it is important that they be faced frankly and I hope that during your visit there would be an opportunity to have a frank talk about these things. It may well be that you will not agree with me on these matters, but that is no reason why we should not, in any case, get together over physics.

Please let me know whether it would be possible for you to come and what would be a good time. The coming week would not be convenient for us, but from 24th February almost any day would be possible. If

[184] Item [440].

you will let me know how long you are able to stay, I can make sure accommodation is arranged for you.
Yours sincerely,

R.E. Peierls

[446] Hans Bethe to Rudolf Peierls

[Ithaca], 12.2.1948

Dear Rudy:
Thanks for your letter of February 5.[185] There seems to be quite a mess about paying the fares for the Solvay Conference.[186] I saw Blackett for a few minutes and he told me he would talk to the U.S. Navy. At that time he did not seem to want any help, but if he does I shall of course stand by.

Bragg wrote me recently that "he was sure they could arrange to pay for <u>my</u> fare." I am not sure how much that means, but I shall wait for developments.

I am sorry I will be very much in a rush around the Solvay Conference because it is in the midst of our term. Moreover, I am still planning to come over for a more leisurely visit this summer. So I would rather not visit you in connection with the Solvay Conference, unless it just happens that my return plane is delayed.

It seems that our summer trip will also be made by plane so that we shall arrive quite early in June. My plans are to spend the time from then until July 15 visiting various places in England, except for about two weeks that I want to spend with my parents-in-law. That can easily be arranged at my convenience. When will it be best to come to

[185] Letter Rudolf Peierls to Hans Bethe, 5.2.1948, *Peierls Papers*, Ms.Eng.misc.b2102, C.16.

[186] The 8th Solvay Conference was to take place in October 1948. From the official photograph it appears that Bethe did not take part in the conference after all.

you and to go to other places in England? Will you have a conference during this time? This might be a great advantage to me because it would probably facilitate getting my trip paid.

Skyrme[187] is of course most welcome. The only boundary condition is that I will be in Columbia the first semester next year and at Cornell the second. I do not think that the Commonwealth people and Skyrme will mind this.

Yours sincerely,

Hans

[447] Werner Heisenberg to Rudolf Peierls

Cambridge, 17.2.1948

Lieber Peierls!

Haben Sie vielen Dank für Ihren Brief. Ich komme gerne nach Birmingham, und ich kann ja vor Ihrem Seminar über das wenige sprechen, was ich von der Theorie der Elementarteilchen zu wissen glaube.

Ich danke Ihnen auch sehr dafür, dass Sie mir offen Ihre Meinung über ein schwieriges politisches Problem geschrieben haben.[188] Es ist so, wie Sie vermuten: ich bin nicht mir Ihnen einverstanden. Aber die Tatsache, dass Sie mir so offen geschrieben haben, gibt mir Hoffnung, dass wir in einem Gespräch wenn auch nicht zu einer Angleichung der Standpunkte so doch zu einem Verständnis des anderen Standpunktes kommen können.

Zur Frage des Zeitpunktes: Vom 10.–12. März bin ich bei Blackett; ich könnte am 12. nach B[irmingham] kommen und bis zum Abend des 13. bleiben. Eventuell auch 8. und 9. März, wenn aus dem geplanten Besuch in Oxford nichts wird. Würde Ihnen das passen?

Also auf gutes Wiedersehen,

Ihr

Werner Heisenberg

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[187] Tony Skyrme, at the time university research fellow at Birmingham, spent the academic years 1948–50 in the United States, as research associate at M.I.T. and as a member of the Institute of Advanced Study at Princeton.

[188] Peierls had sent Heisenberg a copy of his memorandum [440].

[448] Rudolf Peierls to Hans Bethe

[Birmingham], 3.3.1948
(carbon copy)

Dear Hans,
Thank you for your letter of 13th February.[189] I have now at last my Solvay report finished and, while you will receive a copy through official channels, I thought I had better let you have one as soon as possible for your information.

In most English universities term finishes about 1st July, in Birmingham in particular is finishes with the Degree Ceremony on the 3rd. Between the 24th and 29th of June are our examiners' meetings when there is one or sometimes two meetings almost every day. I am hoping to go on a holiday about the middle of July, so that the best time to see you here would be either before 22nd June or between 30th June and about 12th July. I shall be away for three weeks and then again here from early August, though naturally, of course, during August other members of my department may be away at various times. Lastly, we shall have a conference here from the 14th to 18th September. I hope this is not too late for you. It will this time be a bigger affair covering experimental as well as theoretical problems, but we hope nevertheless to preserve the informal character of our last conference, at any rate in the theoretical meetings. You will receive an official invitation to that conference in a day or two.
Yours sincerely,

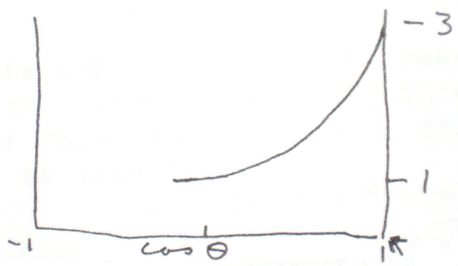
[Rudi]

[189] Refers to letter [446].

[449] Robert Serber to Rudolf Peierls

Berkeley, 4.3.1948

Dear Rudolph:
The experimental facts about the *np* scattering are about as follows: Good measurements have been made, under Segre's direction, using triple coincidence counters to detect the protons. These have been carried from 5° to 50° (perhaps 55°) in the lab system. The average neutron energy is 90 Mev. The cross section in the center of mass system looks like



There is evidence from the counter work that the curve rises again on the left. However, the conclusion about the symmetry comes principally from the work of Powell, who scatters the neutrons in a hydrogen filled cloud chamber. While the statistics of the cloud chamber results are not very good yet, they are in good agreement with the counter data for $\cos\theta > 1$, and are symmetric.

I didn't quite say that this is support for strong coupling (in fact Dancoff and I are on record in the Physical Review as saying that strong coupling theories can't account for nuclear forces).[190] What I said was that the easiest way of describing what symmetrical scattering means is to say that after the collision, the neutron forgets whether it started as a neutron or as a proton. This would be natural with strong coupling; and, although weak coupling theories may be fixed up to give the same result, it would involve apparently arbitrary and accidental choices of coupling parameters.

[190]R. Serber and S.M. Dancoff, 'Strong Coupling Mesotron Theory of Nuclear Forces', *Phys. Rev.* **63**, 143–61 (1943).

I doubt if a symmetric theory will turn out to fit the facts. At 90 Mev the Born approximation is not very bad, and although the double scattering effects are then all right, and quite visible to the naked eye, they are not big enough to do much symmetricising. This isn't by any means conclusive — the errors in the cloud chamber work are still too big to say that the symmetry of the scattering is really established.[191]

I would be interested in hearing of your new method of calculating. We have fooled around with variation methods, but finally decided the easiest and quickest way was to use a partial wave analysis, and determine phase shifts by WKB approximation. The WKB method gives remarkably good wave functions, and, if better accuracy is wanted, a single iteration of the WKB solutions in the integral equation for the wave function gives a better result than we ever need. Also, it is just as easy to get the WKB solutions including tensor forces.

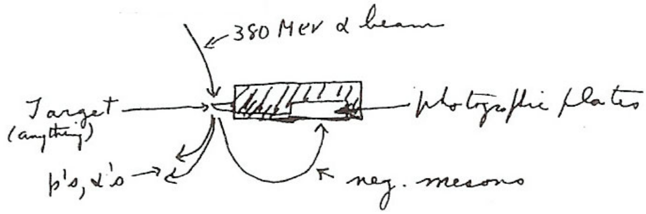
Assuming that the scattering is symmetric, we can get a pretty good description of the facts by taking a simple Yukawa potential with a range of 1.2×10^{-13} as determined by Breit from the $p - p$ scattering. This gives $\sigma(0°)/\sigma(90°) = 3$, in agreement with experiment, and a total cross section of 0.087 barns, compared to McMillan's $0.084 \pm .003$. However, if tensor forces are included, it is a real strain to get a big enough quadripole moment with such a short range force. Perhaps it is not possible at all, but we quickly found out that the tensor force would have to be so large that the calculated ratio $\sigma(0°)/\sigma(90°) = 3$ would be reduced greatly. So we are now engaged in seeing if with a larger range we can fit the data.

The $n - p$ scattering has been crowded into the background at the moment. Perhaps you have already heard that Lattes and Gardner[192] have found mesons made in the cyclotron. The experimental arrange-

[191] Serber had recently published on the subject R. Serber, 'Nuclear Reactions at High Energies', *Phys. Rev.* **72**, 1114–1115 (1947). Another important contribution was his paper, together with Fernbach and Taylor published in the following year; S. Fernbach, R. Serber and T.B. Taylor, 'The Scattering of High Energy Neutrons by Nuclei', *Phys. Rev.* **75**, 1352–55 (1949).

[192] J. Burfening, Eugene Gardner and C.M.G. Lattes, 'Positive Mesons Produced by the 184-Inch Berkeley Cyclotron', *Phys. Rev.* **75**, 382–87 (1949).

ment is like this:



This all takes place inside the vacuum chamber of the cyclotron, and the magnetic field focuses negative mesons on the plates. This density on the plates is about 10^8 what one can get in cosmic rays. Most of them are seen to end in stars, and probably all do. Their mass, determined by H_ρ and range measurements on 50 tracks, is 313 ± 16. This is all the information we have at the moment, but we look forward to a busy future.

Needless to say, I would be delighted to hear any ideas you may have concerning either scattering or the mesons.
With best regards,

Robert Serber

[450] Werner Heisenberg to Rudolf Peierls

Cambridge, 4.3.1948

Lieber Peierls!
Leider haben sich meine Pläne wieder etwas ändern müssen, da ich aller Wahrscheinlichkeit am Freitag, d[em] 12. abends in London sein muss. Könnten Sie eventuell, wenn ich nicht nach B[irmingham] kommen kann, — etwa für Donnerstag — nach Manchester kommen? Ich würde Sie wirklich gerne wieder sprechen. Vielleicht könnte ich auch Samstag früh noch nach B[irmingham] fahren. Jedenfalls will ich versuchen, Sie am Mittwoch abend oder Donnerstag früh in B[irmingham] anzurufen. Bis dahin weiss ich genau, was meine Verpflichtungen sind. Hoffentlich auf gutes Wiedersehen!
Ihr

W. Heisenberg

[451] Hans Bethe to Rudolf Peierls

Ithaca, 18.3.1948

Dear Rudy:
I suppose the enclosed letter to Bragg is self-explanatory.[193] I should really like very much to be back in time. This of course also applies to your conference.

In view of the new date for the Solvay Conference, I wonder whether your Theoretical Conference would not be more convenient in June or early July. I do not know whether that is possible. If not, could it be early in September? Otherwise the program outlined in your letter suits me very well. I intended to be in Switzerland and possibly Germany from the middle of July to the end of August which overlaps with the time you intend to be absent from Birmingham.
I shall probably come to see you in the beginning of July but maybe I shall stop for a day or two in early June.
Yours sincerely,

Hans

Thanks a lot for your Solvay report!

[452] Rudolf Peierls to Hans Bethe

[Birmingham], 23.3.1948
(carbon copy)

Dear Hans,
Thank you for your letter.[194] I am very sorry that the date of our meeting is too late for you. Unfortunately, it is impossible to change it

[193] Letter Hans Bethe to W.L. Bragg, 18.3.1948 asking Bragg to take into account the American academic calendar when rescheduling the Solvay Conference. Carbon copy in *Peierls Papers*, Mc.Eng.misc.b202, C.16.
[194] Letter [451].

now since the invitations have gone out, the accommodation has been booked (this is very short as you may imagine) and particularly as there are a number of other events in September. It begins with a[n] International Conference on Applied Mathematics and Mechanics from 6th–11th September which will overlap to a slight degree in membership with ours. There are also likely to be some business meetings involving a number of our people as well as possible foreign visitors during the later half of that same week. In the week following our conference is the Cosmic Ray Conference at Bristol which was the first one to get fixed and, in fact, our date was partly chosen to enable the overseas visitors to attend both conferences. Following that, of course, is the Solvay. I am surprised at what you say about the starting date of the American universities since last year the majority of visitors seemed to remain in Europe until the end of the Copenhagen Conference in the very last days of September. I very much hope you will be able to delay your return to take in our conference at least, if not Bristol and the Solvay.

I understand the meeting sponsored by the American Emergency Committee of Atomic Scientists will now not be held in Jamaica, but in the United States. I have said that I accept in principle, but if the date remains in June I shall probably not get away in time. I would be grateful to have your views as to whether this meeting is likely to serve a useful purpose and whether it would be worth the effort to go there, particularly if it means missing important University meetings.

With kindest regards,

Yours sincerely

[Rudi]

[453] Hans Bethe to Rudolf Peierls

Ithaca, 7.4.1948

Dear Rudy:

Thanks for your letter of March 23.[195] I am rather sad that the conference cannot be moved. The starting of American Universities has

[195] Letter [452].

always been around September 20. The trouble was only that Uncle Nick has always disregarded university schedules and chosen the dates for his Copenhagen Conference at random.

Some visitors, like Vicky, were too anxious to go to the grand reopening of the Copenhagen meetings[196] last year and therefore did not return in spite of the semester. (I appreciate that the Bristol meeting was the first to get fixed and I protested immediately to Powell about his date, but without success.)

I have pretty much decided to go to your conference but none of the later ones.

I hope that I will hear everything in Birmingham then and earlier in the summer.

The meeting sponsored by the American Emergency Committee[197] is not likely to serve a useful purpose. It was Szilard's idea to have such a meeting to bring together scientists from all countries east and west of the iron curtain. Those from the east of the curtain have already refused to attend, as was to be expected. In the present situation, it is my opinion, that nothing useful can be done from the side of atomic energy or of scientists in general and that any stress on atomic energy can only deepen the international conflict. I was very much against holding this conference and this opinion is shared by other people such as Oppenheimer and Weisskopf.

By the way, I resigned from the Emergency Committee, not because of the conference but because of my conviction of the futility of these endeavors at the present time. I have also heard some rumors that the Federation of American Scientists is giving up the idea of the International Conference. This seems sensible to me, but of course the Emer-

[196]The conference had attracted a large number of physicists who spent some time in Copenhagen during the last two weeks of September 1947. Among them were Kramers, Weisskopf, Pais, Rosenfeld, Peierls, Blackett, Placzek, Wheeler, Rossi, Ferretti, Klein. See letter Wolfgang Pauli to Otto Stern, 19.8.1947, K.V. Meyenn (ed.), Wolfgang Pauli. *Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg u.a.*, Vol. 3: 1940–1949, Heidelberg: Springer, 1993 (hereinafter cited as Pauli, *Wissenschaftlicher Briefwechsel*, with volume number), p. 471.

[197]See letter [452].

gency Committee is an independent agency. Anyway, I would much rather talk physics with you than have you go to that conference.

Thanks very much for the parliamentary debates of the House of Lords.[198] I am of course very proud.
Yours sincerely

Hans

[454] Robert Oppenheimer to Rudolf Peierls and Mark Oliphant

[Princeton], 8.4.1948

Dear Peierls and Oliphant:
Thank you both for your good notes of invitation to the Birmingham Conference. I am sending in an acceptance which I only hope will correspond to reality when the time comes. I want very much to visit with you and talk over the many wonderful developments in physics, as well as other problems with which we have had a common concern in the past.

You ask for other suggestions for invitees to the conference. There are two who have carried out essentially parallel and very beautiful developments in quantum electrodynamics, Julian Schwinger at Harvard and Sin-itiro Tomonaga in Japan. It would surely be a great addition to the conference if they could both come.

I hope that there is nothing improper in my accepting an invitation in which it is inevitable that there be a little uncertainty as to my plans. With all warmest greetings,

Robert Oppenheimer

[198] Almost certainly this refers to the parliamentary debate on the perils of atomic warfare launched by the Archbishop of York, 18 February 1948, *Hansard*, HoL, Vol. 153, cols 1178–1213.

[455] Rudolf Peierls to Robert Oppenheimer

Birmingham, 15.4.1948

Dear Oppie,
Thank you for your letter.[199] We are delighted that there is at least a chance of seeing you here, and it is, of course, quite in order to accept provisionally: we only hope you will be able to keep to it.

We had already invited Schwinger. As regards Sin-itiro Tomonaga, we do not know anything about him directly, but would be very glad to have him on your recommendation, if the administrative side of it could be managed. Presumably he could not be brought here without some help from the U.S. authorities in Japan, and I imagine you know better than we do whether, and how, they can be approached. I accordingly enclose an invitation for this purpose and we would be very grateful to you if you could have it passed along the right channel, or alternatively suggest to us the right "ansatz".
With best wishes,
Yours sincerely,

R.E. Peierls

[456] Rudolf Peierls to Robert Serber

[Birmingham], 17.4.1948
(carbon copy)

Dear Bob,
Thank you very much for your letter of 4th March.[200] I quite agree with you that the new discoveries about artificial mesons are much more exciting than n–p scattering, still, it seems worth continuing the discussion on n–p scattering. I enclose a curve giving the results of

[199] Letter [454].
[200] Letter [449].

calculations by Barker[201] here and a copy of a note to Nature[202] explaining the calculations. The method of Ferretti and Krook which he used and which is being published in the May number of the Proceedings of the Physical Society[203] goes as follows:

We need to determine the logarithmic derivative at some point $r = a$ beyond the range of the forces. Supposing we start with the right value of [...][204] we will get a solution which is regular at the origin, otherwise the solution is singular at the origin. Consider the Taylor series for [...][205] at a. For the general solution which is singular at the origin the radius of convergence of this Taylor series can at most be a and usually is just a. For the regular solution it is larger. If I consider, therefore, the ratio between two successive Taylor coefficients, it should converge to a much lower value if the parameter [...][206] is chosen in the right way. The approximation now consists in applying this criterion not to the limit, but to a Taylor coefficient of finite order. It is found in practice that for a sensible choice of a, something like the 10th coefficient will give a very good accuracy and the determination of these coefficients is very rapid, provided the potential function is such that the required successive differentiations do not give too much trouble. The method is also applicable to tensor forces and Barker is now engaged in a discussion of this problem using something like the Schwinger theory.

I am a little puzzled by your statement that at 90 Mev the Born Approximation would give only backward scattering, whereas the more likely case B of Barker's, the forward and backward intensities for exchange forces differ only by a factor three. Qualitatively Barker's curves are very similar to those published by Bethe, but we were reluctant to

[201] See also F.C. Barker and R.E. Peierls, 'On the Definition of the "Effective Range" of Nuclear Forces', *Phys. Rev.* **75**, 312–13 (1949).

[202] F.C. Barker and D.G. Ravenhall, 'Scattering of like particles at 100 MeV', *Nature* **163**, 20 (1949).

[203] B. Ferretti and M. Krook, 'On the Solution of Scattering and Related Problems', *Proc. Phys. Soc.* **60**, 481–90 (1949).

[204] Missing in carbon copy. Details of the calculation found in the above paper, note 203.

[205] Missing in carbon copy.

[206] Missing in carbon copy.

use the square well, since at short wave lengths the sharp boundaries are liable to give spurious effects. However, we need not have worried apparently, since it looks as if the wave length is not as yet very short.

As far as I can see from your account of the experiments, the statement that the curve is symmetrical rests entirely on the cloud chamber data, and it would seem a little premature to draw conclusions until one knows with what accuracy symmetry is really established.

With kindest regards,
Yours sincerely,

R.E. Peierls

[457] Rudolf Peierls to Hans Bethe

[Birmingham], 6.5.1948
(carbon copy)

Dear Hans,
You may be interested in the enclosed note which we have sent to Nature. The results bear out qualitatively what you have found independently with a square well. We were afraid of a square well, since at very short wavelength the sharp edges of the well are liable to give serious interference effects. We started, of course, like most people with the idea that 100 MeV is a high energy. The results show that it isn't, and so probably the square well is as good as anything else. It is, however, desirable that at these energies the results are insensitive to the nature of the forces, no doubt this would change at still higher energies, but even if these could be attained, they will be very much harder to interpret because of relativistic effects and the like. What is more pleasing is the rapidity with which it is possible to calculate phases by a method of Ferretti and Krook.[207]

[207] See letter [456], note 203.

The trick is as follows:

You choose a point $r = 2$ beyond the range of the forces and you imagine that you integrate the equations inwards from this point. In general, i.e. choosing the wrong slope, the answer will be singular at the origin and its Taylor series about a will have a radius of convergence at most equal to a. For the correct slope the radius of convergence will be larger and may even be infinite. Accordingly you can get the correct slope by imposing the condition that the Taylor series should converge as well as possible. As an approximation you make the nth term in these series as small as possible, i.e. equal to zero, and for quite reasonable n this gives a good approximation of the slope and hence to the phase. Since in this operation only successive differentiations are required, it is quite easy to go to, say $n = 10$ without much trouble if your potential is of a reasonably simple analytical form. The method works amazingly well and can be generalised to tensor forces. We are getting rather excited about the self-energy problem. You remember Salpeter here having a proof for the one-body problem that the self-energy is calculated exactly and without perturbation theory and was really infinite.[208] In trying to generalise this to the case of hole theory, he struck some snags, however, and there seems now the possibility that the statement is not in fact true in hole theory, but that the logarithmic infinity disappears if one calculates exactly. We are very far from having proved this, but the mere possibility is, of course, very important.

Another point that agitates me is the self-energy of the photon. In the ordinary way this, of course, is again infinite and one would expect with the usual tricks or, for example, with the kind of modification that McManus or Feynman are playing with,[209] to make it finite. However, the rest mass of the photon is not merely finite, it must be very exactly zero, and the only way to get this in any of these theories is to assume a finite mechanical rest mass for the photon which would then exactly

[208] Ed Salpeter (1924–) was completing his Ph.D. at Birmingham in 1948. After a brief period he went to Cornell to work with Hans Bethe where, apart from brief research spells elsewhere he stayed for the remainder of his career.

[209] R.P.Feynman, 'Relativistic Cut-Off for Quantum Electrodynamics', *Phys. Rev.* **74**, 1430–38 (1948).

cancel the finite (negative) dynamic mass. This is rather interesting formally and it means, amongst other things, that perturbation theory applied to this must always give utter nonsense since you can never get a finite rest mass to rest mass zero by a small perturbation.
See you in June.
Yours sincerely,

[Rudi]

[458] Max Born to Rudolf Peierls

Edinburgh, 22.5.1948

Dear Peierls,
There are two things I would like to have your opinion about. The first is this: I got to-day an information from the R[oyal] S[ociety] about the recommendations of names for election of Foreign Members. Apart from Cripps, and two names unknown to me, there are Browser and Pauling. But not Schrödinger. Dirac and I have tried for years to bring him into the R[oyal] S[ociety] The main obstacle is the letter he wrote in Graz, in which he expressed his agreement with the Nazis (or something like that; part of it was, I think, published in "Nature").[210] On account of this silly document a group of people have prohibited his election. The formal difficulty is that he is not British; hence he cannot be an ordinary Fellow. And as he lives in Eire which is regarded as part of the British Empire, he cannot be a Foreign Member. Now the latter obstacle has been removed, as far as I know, by a special decision of the Council. When I heard that Heitler was to be elected I wrote some strong letters to members of the Sectional Committee, saying that it would be an affront to Schr[ödinger], if H[eitler] would be in the Society and he not. Actually I have a very high opinion of H[eitler], but still I think Schr[ödinger] is of a higher order of magnitude. There is hardly any paper in theoretical physics in the world where not

[210] See Letter [273], note 1029 in Lee, *Selected Correspondence*, Vol. 1, Chapter 5.

the Schr[ödinger] equation is used. He has actually revolutionised our science. That he was not very lucky in recent years seems to me of less importance. Planck has also not done any fundamental work after 1900. Therefore I feel rather strongly in this matter. I should like to know what you think. It is a great pity that Dirac is not here; I think I could agree with him on some drastic action. But now I do not know what to do. I am quite aware of Schr[ödinger]'s shortcomings and of the enmity he has accumulated through his own behaviour. But I think that all this ought not to matter in the question of election to a purely scientific society.

The other point is Palestine. I am not a Zionist, not even a Jew by religion. I tried always to be as impartial as possible, on account of my ignorance of the facts. It appeared to me from the newspapers that Britain played a nasty game, arming and training the Arabs and then retiring, to leave the dirty work to the sons of the desert. But I had not proof that this is really so. To-day Manchester Guardian has a leader confirming my worst fears. I think nobody can doubt any more that this is really Bevin's politics. I think it is almost worse than Hitler, sit still and watch quietly this second mass murder arranged by our own Government? I am a member of the Labour Party — can one stay in it? Should we — you, Polany, Goldstein, I and as many others, as we can get — not publish some protest? I shall write to Einstein for advice. But let me know what you think, or better feel, and also your wife's opinion.

From the Department I could tell you some interesting things, but I am not in the mood for it.

With kind regards,

Yours ever,

M. Born

[459] Rudolf Peierls to Max Born

[Birmingham], 24.5.1948[211]
(carbon copy)

Dear Born,
Thank you for your letter.[212] As regards Schrödinger, there is no doubt whatever about his merits as a scientist and surely no member of the Royal Society can have any doubt that he is much better than practically all Fellows and Foreign Members recently elected. This issue is, however, whether scientific distinction is the only factor taken into account in the election. As regards election of Fellows that is perhaps the case. The rule is, however, that he is not eligible to be a Fellow and for the election for a foreign member it is probably true that the personal record has also to be weighed. Election of a foreign member has some of the characteristics of conferring an Honorary Degree when personal factors undoubtedly play a legitimate part. I have understood the decision of the Council to mean that they take this view.

On grounds of personal record I think there is a very strong case against Schrödinger. The famous Graz letter is only one example, but it is bad enough. To describe it just as „silly" is minimising this too much. Our distress at events in Germany was so bitter just because there were so many people who failed to understand the importance of personal integrity and the disastrous effect of men with a worldwide reputation saying what they knew to be wrong for the sake of expediency. It is true that most of us would say or sign such things under strong pressure or in acute danger. We would, however, expect to pay for it and we would disassociate ourselves from such statements as soon as the danger was passed. Schrödinger has never troubled to do so. About other facts my opinion is only based on hearsay, but it adds up to a consistent

[211] Two drafts of letters dated 24.5.1948 exist and a further draft dated 29.5.1948, which is the one likely to have been sent. The second draft dated 24.5.1948 is reproduced here to demonstrate the development of Peierls' thinking on the two issues about which Born had enquired.
[212] Letter [458].

impression of irresponsible behaviour. At a time when scientists are in the public eye and when their word counts, there is a particularly strong duty to apply severe standards.

The only case you could make out is to claim that the election of a foreign member should be based on scientific merit alone, but if this is done, it must be done consistently. Would you take the same view if Heisenberg's name were proposed?

Or in the hypothetical case that Weizsäcker had done work of comparable importance, would you take the same line in his case?

On the other matter I agree with you in being distressed over the recent events. I also agree that it is foolish for this country not to recognize the new state. This, however, is only foolish and short-sighted. I do not believe it is particularly immoral. The moral question has been confused ever since, in a spirit of wishful thinking, promises were made to both Arabs and Jews which at least in the interpretation given to the words by each side were outright contradictory.

I think it is a tragedy that this whole Palestine adventure was ever started. The numbers that can be accommodated there are pitifully small to make any difference to the wider problem and the trouble arising from it will only make more difficult the problem of settling homeless Jews elsewhere and of creating a tolerable situation for others in the places where they are now living.

I believe that nationalism is always bad and Jewish nationalism is no exception.

This does not diminish the distress one feels about the violence which is abundant there now and which, as always, must hit harmless people. I do not think, however, one has any right to protest in public, unless one also protested at the violence from Jewish terrorists when this was hitting harmless civilians on the other side.

What one can argue about is expediency in the long run. I do not believe that your opinion or mine on this point will be particularly welcome, but I believe that political pressure from America which is undoubtedly being exerted is much more likely to change the position.

If you hear from Einstein it would, of course, be interesting to know his views, but one should not follow his example which is to give his

name to any cause for which it is requested regardless of whether or not he has any knowledge of the case and thereby achieving a position where nobody takes any notice of his signature under any appeal.
Yours sincerely,

R.E. Peierls

[460] Rudolf Peierls to Max Born

[Birmingham], 29.5.1948
(carbon copy)

Dear Born,
Thank you for your letter.[213] I am sorry it has taken me so long to reply, but I have been away from Birmingham for several days.

As regards Schrödinger, there is, of course, no doubt whatever about his merits as a scientist, and surely no member of the R[oyal] S[ociety] Council can have any doubt that he is much more eminent than practically any Fellow or Foreign Member recently elected. The issue is, however, whether scientific distinction is the only factor to be taken into account in the election. As regards Fellows that is perhaps the case. Schrödinger, however, does not appear to be eligible as a Fellow and election of a Foreign Member is a rather different story. It is in many ways analogous to conferring an honorary degree, where the personal record is most certainly taken into account. I have understood the decision of the Royal Society to mean that this is their interpretation.

On grounds of personal record I think there is a very strong case against Schrödinger. The famous Graz letter is only one example, but it is bad enough. I would not pass it over by just describing it as ,,silly". Our distress at events in Germany surely was so bitter just because there were so many people who failed to understand the seriousness of the issues, and the importance of personal integrity. If a man with a worldwide reputation says what he knows not to be true for reasons of

[213] Letter [458].

expediency, it is not merely silly. Admittedly most of us would say or sign such things under strong pressure or in acute danger. But we would expect to pay for it, and in any event would presumably disassociate ourselves from what we said as soon as it was safe to do so. To my knowledge, Schrödinger has never troubled to explain this letter was written under pressure. (As far as I know it was not very severe pressure at that). About other similar things I only know from hearsay, such as his retaining his name on the books of the German Legation in Dublin practically as long as such Legation existed. It all adds up to a consistent impression of irresponsibility. At a time when scientists are so much in the public eye and when their words count more than ever, standards of behaviour must be particularly severe.

The only case you could make would be to claim that the election of Foreign Members should be based exclusively on scientific standing. But this would have to be applied consistently. Would you take the same view if Heisenberg's name were proposed? Or in the hypothetical case that Weizsäcker had done work of comparable importance, would you take the same line in his case?

On the question of Palestine, I agree with you in feeling acute distress. Of course, Britain ought to recognize the new state. Their not doing so is short-sighted and foolish, but not particularly wicked. The moral question is incredibly confused and has been confused ever since promises were made both to the Zionists and to the Arabs which, at least in the sense in which each side understood them, were outright contradictory. I have no particular liking for the Arabs, but one must admit that there is considerable justification in their claims. True the Arabs are backward. In the view of the Americans, England is a backward country, too, but noone would admit that this entitles the Americans to take over the country and run it. True also the Arab population is not dense enough to work the land efficiently (at least I imagine it is true) but their refusal to allow others to come in is as justified as the refusal of the British agricultural labourers to allow foreigners to work on the land, even with the present shortage of labour. True, Palestine was a Jewish country in Biblical times; but you cannot reverse history, and hand over America to the Red Indians, or the French, or England to the Celts (if you can find enough).

With all this I do not mean to say that the Arabs are right and the Zionists wrong, but merely that the question is confused and that we have here one of those situations in which each side genuinely had the conviction of being in the right, and where nobody can prove them wrong.

This could, of course, have been the perfect situation for UNO to prove its worth. I believe, however, that the utter failure of UNO (which may finish it in the same sense in which Manchuria finished the League of Nations) cannot be blamed on the British Government, it is due to quite irresponsible behaviour on the part of America.

I believe it is a pity that this Palestine adventure was ever started. The numbers that can be settled there are pitifully small compared to the number of displaced Jews. The trouble arising over Palestine is bound to make more difficult the problem of finding acceptance for the Jews in countries capable of taking large numbers, or creating tolerable conditions for them in the countries in which they live now. Nationalism is bad anywhere, and Jewish nationalism is no exception.

All this, of course, is not the fault of the unfortunate victims who are suffering now in Palestine. Unfortunately, things have gone so far that there is bound to be suffering and violence in Palestine whatever is done. If the British Army moved in again to take over the country and restore order, the violence from terrorists on both sides would not be less severe than the present war. We tend to overestimate the scale of this war, for example a recent statement that a hundred Jews were killed in Jerusalem since the beginning of the fighting shows that the matter is serious but has hardly reached the scale of warfarer.

I am sure the government are genuinely anxious to find a solution that would avoid further violence. They probably believe that to recognize the new state, and to withdraw support from the Arab state would cause perhaps fewer victims amongst the Jews but more among the Arabs. Much as our sympathy may be on the side of the Jews, can we really insist that it makes any difference whether men, women and children are killed who speak one language or another? It might make quite the wrong impression if we made public our indignation at attacks on Jewish civilians by the Arab armies, unless we have previously protested against attacks on Arab civilians by Jewish terrorists.

In the end the problem boils down to expediency, and of what would actually happen if British help to the Arab states was withdrawn. I do not believe that on this question your opinion or mine would carry much weight — or, for that matter, whether it would be based on a particularly sound knowledge of the facts. It is very sad to have to watch these things happening without taking any action, and I can understand your impatience, but I believe that in this particular situation action would not make particularly good sense, nor would it be likely to assist in getting a satisfactory solution of the problem.
Yours sincerely,

R.E. Peierls

[461] Leon Rosenfeld to Rudolf Peierls

Manchester, 4.6.1948

Dear Peierls,
I enclose the proof of the last chapters of my book containing among others a short account of Ramsay's argument concerning Schwinger's theory.[214] I need not say that I shall be grateful for your criticism.

Blackett showed me your article on the German scientists.[215] As you would expect I agree entirely with your views and I think that it would be extremely useful if it could be published. I would like, however, if I were to write such an article myself, to lay perhaps more emphasis than you do on the detrimental influence of the present situation on the youth and I would specifically stress the fact that most German intellectuals are now taking an entirely passive attitude, and instead of contributing their share to the material and moral reconstruction of their country, they pin their hopes on the advantages to be gained from the conflict between the two occupying powers.

I would also allude to the regrettable fact that after the liberation there hardly was any spontaneous manifestation from the part of the

[214] Leon Rosenfeld, *Nuclear Forces*, Amsterdam: North-Holland Pub. Co., 1948–9.
[215] Item [440].

Germans of any regret for the past and desire to do better in the future. I feel also that you should perhaps try to formulate more firmly and more concretely the conclusions of your essay, since as it is now it gives the impression that the problem is hopeless and that there is hardly anything to do about it. Perhaps after all this is true, but still we are confronted with the urgent practical problems such as whether to invite the Germans and so on, on which we have to make up our minds.

There is one small point needing correction: the situation in Holland to which you allude on page 3 was a bit more complicated. The so called loyalty declaration was in itself a harmless document that could be signed and was actually signed by all state functionaries, including University teachers. But the conflict arose when the Germans tried to enlist the students for work in Germany. They tried to make the authorization to continue the study dependent on signing similar loyalty declarations. The refusal of most students to do so had nothing to do with the declaration itself, but was a clear protest against the attempts to force them to work for the benefit of the Germans. This protest led to wholesale arrests and deportations of students and also to the persecution of a small number of professors who had been prominent in supporting the students' action.

From this you may see how to modify your text at that particular place in order to avoid inaccurate statements.

The cautious way in which the nazis manoeuvred with us in Holland, seems to support your opinion that "even the nazi regime would have hesitated to carry on without the majority of German University teachers".—

With best wishes,
Yours sincerely,

L. Rosenfeld

I enclose the report on the examination for your signature.

[462] Robert Oppenheimer to Rudolf Peierls

Princeton, 16.8.1948
(carbon copy)

Dear Peierls:
Your good note of August 5th has just reached me.

I shall be glad to give the introductory Thursday afternoon talk on the field theory although I have no doubt that a number of people on your list could do a better job.[216]

We shall probably come up to Birmingham on Monday, September 13th. Kitty will be with me, and we both look forward very much to seeing you both. Am I right in assuming that there will be some place where both she and I can stay? We plan to be with Bohr in Copenhagen the first ten days in September and if you have messages you can best reach us there.
Sincerely,

[Robert]

[463] Rudolf Peierls to Robert Serber

Birmingham, 8.10.1948
(carbon copy)

Dear Bob,
In trying to remember what I had learned at the Solvay, I came across the following point: I believe you said that the curve for the $n - p$ scattering at 90 MeV looked so symmetrical as between 0°–180° that you were led to the assumption that there is no scattering at all in the states of odd angular momentum and that you had obtained a good fit with a cental force of such a description. Now, Mr. Preston here points out, in my view correctly, that such a force would not be compatible with the saturation of nuclear forces, since for saturation you must not

[216] This refers to the Birmingham Conference to take place in September 1948.

have more "ordinary" force than is contained in the symmetric mixture of Kemmer.[217]

Of course, all this depends on there being approximate charge independent and negligible many-body forces, but it would be very important to have evidence which forces us to abandon one or the other of these assumptions.

The tensor force in itself cannot save the saturation, though, of course, it may alter the nature of the central force that one derives from the Berkeley data.

These points have probably occurred to you, but I would be glad to know your reaction and also in view of this it is particularly important to examine how well the symmetry of the pattern is proved from the available data. From what I remember of your slide the points at angles below 90° seemed to show more scatter than those above and one could, perhaps, tolerate some deviation from this symmetric pattern. No doubt, you are now examining your suggestion of a non-exchange tensor force and if this works, which I find hard to believe, it may also have some effect on the nature of the central force that you have to assume.[218]

Yours sincerely,

[R.E. Peierls]

[217]N. Kemmer, 'The charge-dependence of nuclear forces', *Proc. Cam. Phil. Soc*, **34**, 354–64 (1938); N. Kemmer, 'Quantum Theory of Einstein-Bose particles and nuclear interaction', *Proc. Roy. Soc* A**166**, 127–153 (1938).

[218]This point is addressed in S. Fernbach, R. Serber and T.B. Taylor, 'The Scattering of High Energy Neutrons by Nuclei', *Phys. Rev.* **75**, 1352–55 (1949).

[464] Robert Serber to Rudolf Peierls

Berkeley, 4.11.1948

Dear Rudolph:
I am enclosing prints of the slides I showed at the Solvay Conference.[219] The curve labelled II is for the potential $\frac{1}{2}(1+P_x)\frac{e^{-Kr}}{r}$, $\frac{1}{K} = 1.2\times10^{-13}$. Curve I is for 40 Mev.

For charge symmetric forces with this Yukawa potential the cross section at 180° would be about 40×10^{-27}, so to get a more reasonable looking curve, the results for charge symmetry with a square well of range 1.8×10^{-13} were plotted as curve IV. (With a square well one can get considerably smaller cross sections than with a smooth potential.) III was intended to be the curve for the Yukawa potential with tensor forces included. However, III is not right, according to our latest results. The trouble was that in plotting III the phase shifts for the ${}^3D_3 +{}^3 G_3$ states were taken from a Born approximation (a fact I wasn't aware of when I last saw you). Although the phase shifts are small, the weight factors are big, and using the correct phase shifts makes a quite appreciable difference in the differential cross section. We now think that the proper curve, including tensor forces, is still quite close to II, though some checking of this conclusion is necessary. If true, the difficulties concerning tensor forces have disappeared.

As to the symmetry of the scattering: the counter data indicates some asymmetry, though a rather small one, while the cloud chamber data seems to show quite appreciable asymmetry developing at small angles (if the last point can be believed). However, the curve still appears not as asymmetric as one would predict from, say, the "charge symmetric theory". A small repulsive force in states of odd angular momentum could be invoked to account for the asymmetry, i.e. slightly more exchange than ordinary force. The argument for keeping the forces in the odd states as small as possible in that zero force gives the minimum total cross section from the calculations.

[219] See also letter [463].

To explain saturation, one would certainly have to give up charge independence — repulsive forces would have to appear in the like particle interactions. I don't believe the work which has been done on $p-p$ scattering is much help here; the limit on the amount of repulsive p wave force compatible with the observation is so sensitive to range, and, as far as I know, calculations with short-range forces haven't been made. There are no results to report yet from our 32 Mev $p-p$ scattering experiments.

The latest result on the meson mass ratio is $1.32 \pm .0-2$, so zero mass for the third particle seems pretty certain. The geometry of the apparatus for measuring the π lifetime has been checked by running α particles through it. The present figure is $\tau_{\frac{1}{2}} = 0.9\, {}^{+.25}_{-.15} \times 10^{-8}$ (half life).

With best regards,

Robert Serber

[465] Hans Bethe to Rudolf Peierls

New York, 10.12.1948

Dear Rudy:

Thanks for your several letters. I shall be very happy to have Salpeter[220] and I have just written to Wilson about giving him an appointment. I am reasonably sure that we can give him a position as a Research Associate at a salary of \$3500 to \$3800. This, I think, should be adequate and will correspond approximately to his current salary plus traveling expenses to this country and back. We cannot make any separate arrangement for travel expenses.

I hope Dyson will actually decide to come to you.[221] He gets better every day. However, he has so many offers that he has not yet made

[220] Ed Salpeter moved to Cornell after completing his Ph.D. at Birmingham.

[221] Freeman Dyson (1923–), had studied at Cambridge between 1941 and 1943. After war service in the research division of the R.A.F., Bomber Command, he undertook research at Cambridge (1946–47) and Cornell (1947–1948) before becoming research fellow at Birmingham.

up his mind where to go in England. He has some excessive loyalty to Cambridge; I told him he would waste his time if he went there.

I have sent your Letter to the Editor to The Physical Review.[222] I knew this derivation or a rather similar one; in fact, I am planning to write a paper in which a still more general and exact relation is derived and used.[223] Blatt[224] has used still another derivation, but for practical purposes has used essentially your relation. Everything about the scattering of neutrons and protons by protons is getting to be exceedingly simple. Especially the complicated calculations by Breit[225] are now quite unnecessary. I have also taken care of your application for membership in The Physical Society.

I am enjoying my stay at Columbia.[226] I find that I have much more time than usual to do some work. There is quite a lot of progress in quantum electrodynamics and the people most concerned, namely Schwinger, Feynman and Dyson, have been busy writing up their knowledge. At the moment, Rose is visiting me but without the children.
Regards to all of you.
Yours sincerely,

Hans

[222] F.C. Barker and R.E. Peierls, 'On the Definition of the "Effective Range" of Nuclear Forces', *Phys. Rev.* **75**, 312–12 (1949).

[223] H.A. Bethe, 'Theory of the Effective Range on Nuclear Scattering', *Phys. Rev.* **76**, 38–50 (1949).

[224] John M. Blatt and J. David Jackson, 'On the Interpretation of Neutron-Proton Scattering Data by the Schwinger Variational Method', *Phys. Rev.* **76**, 18–37 (1949).

[225] G. Breit, H.M. Thaxton and L. Eisenbud, 'Analysis of Experiments on the Scattering of Protons by Protons', *Phys. Rev.* **55**, 1018–64 (1939); L.E. Hoisington, S.S. Share and G. Breit, 'Effects of Shape of Potential Energy Wells Detectable by Experiments on Proton-Proton Scattering', *Phys. Rev.* **56**, 884–890 (1939), and G. Breit, L.E. Hoisington, S.S. Share and H.M. Thaxton, 'The Approximate Equality of the Proton-Proton and Proton-Neutron Interactions for the Meson Potential', *Phys. Rev.* **55**, 1103 (1939).

[226] Hans Bethe was visiting professor at Columbia University in the autumn of 1948.

[466] Rudolf Peierls to J.R. Oppenheimer

[Birmingham], 16.12.1948
(carbon copy)

Dear Oppie,
I am writing to ask about two problems which were mentioned in discussions here at the Solvay. The first is that you drew attention to the results by Finkelstein, according to which the lifetime of τ-mesons for the decay into [...][227] with the emission of one or two photons was sure to be much shorter than observed. The published paper by Finkelstein covers only some special cases.[228] We have looked into this problem more generally. It is easier, of course, to survey the case of the emission of one photon. In this case it seems possible to choose the spin and parity of the two mesons and type the coupling in such a way that this process is forbidden. We are proposing also to study what happens in three cases to the emission of the two photons and to see whether there are cases in which this also can be reduced. I imagine that you may have similar calculations going and, while a certain amount of overlap does no harm, the job of listing all the possible combinations and compiling the results of such a high-order process is one that is scarcely worth duplicating. If, therefore, you think that this problem has been completely explored, or if this is being done, could you let me know?

The second point also concerns work by Finkelstein; in this case the question of non-linear theories. You explained that the classical wave equation for two scalar fields, one with, the other without mass, and with a coupling term proportional to the square of the first and the first power of the second wave function has a non-singular stationary solution. We have played a bit with these equations and we have found it very hard to decide whether they have only one solution or an enumerable set. Here again the explanation involves a good deal of hard work and if

[227] Missing in carbon copy.

[228] R. Finkelstein, 'The *gamma* Instability of Mesons', *Phys. Rev.* **72**, 415–22 (1947); Oppenheimer and Finkelstein had collaborated on this work. E.g. S.T. Epstein, R.J. Finkelstein and J.R. Oppenheimer, 'Note on Stimulated Decay of Negative Mesons', *Phys. Rev.* **73**, 1140–41 (1948).

Finkelstein knows the answer to this question we would not want to do it again. According to our notes of the meeting you said that there was "at least one" non-singular solution, but our notes may not be accurate.

Another point is that you mentioned the question of finding an operator representing the coordinate of the centre of this structure in the quantized equations. I have not yet succeeded in seeing the purpose of this; surely if there exists in quantum theory something corresponding to a structure of finite extent in this equation, its position can be defined in an obvious way to an accuracy given by its size and to that accuracy it must also satisfy the commutation laws with a momentum, since the momentum operator corresponds to an infinitesimal displacement of all fields together. I see no reason, however, why one should expect to have an exact coordinate operator which is defined to a better accuracy than the size. One would be in a position rather similar to that of the ordinary Dirac equation without coupling in which the existence of precise coordinate operator is rather a mathematical luxury and goes by the board as soon as one used pair theory.

The direction in which it would be nice to explore such a theory is to find what one can about the existence, or otherwise, of stationary states of finite energy in the quantum equations, but this, of course, is not easy.

We greatly enjoyed reading all about you in Time[229] and I thought that, with some allowance for the habits of journalists, the article was excellent, except for the remarks about Einstein to which you objected. We were rather amused by the somewhat backhanded compliment to the state of European physics, though, of course, it is easy to reconstruct the remark that you presumably made which got translated in this fashion.
Yours sincerely,

R.E. Peierls

[229]On November 1948, TIME magazine published 'The Eternal Apprentice', a cover story on Robert Oppenheimer.

[467] J.R. Oppenheimer to Rudolf Peierls

Princeton, 30.12.1948

Dear Rudi:
Thank you for your fine letter.[230] It was good to hear from you and made me homesick for a long talk. Let me try to answer your questions.

To the best of my knowledge, no systematic search for selection rules to lengthen the lifetime of τ-mesons has been made. Of course examples of selection rules can be found in Finkelstein's paper;[231] and Furry's theorem gives one other examples. I must say that this last strikes me as somewhat unpromising. Surely in pursuing it one must also investigate stability with regard to electron-positron pairs. I gather that we are looking for a factor which is something like 10^{-9}, since certainly the characteristic decay process is not a gamma instability or an electro-positron instability. Of course, I do not have anything against pursuing this, but am only trying to explain why we have not ourselves done it. There is just one point: If one does not wish to question the materialization processes for nucleons, one may believe, as far as today's evidence goes, that the production of the τ meson is always accompanied by the production of another particle of equal or greater mass. This does not seem too likely either; but I do not believe that there is any real evidence against it. I meant really only that we should keep open-minded about the materialization process and this might be one of the ways that we might learn about them in the not too remote future.

As far as the non-linear theories, I profoundly agree with every word you write. The business of defining center of mass coordinates is a rather trivial, rather formal and quite sterile one, and in any case has been pretty well looked into both by Pryce and by Møller. Finkelstein says that there is more than one non-singular stationary solution, but whether there is an infinite number and how they are related, I do not know, and I doubt whether he does. It seems to me that the

[230] Letter [466].
[231] See letter [466], note 228.

essential questions, and this is being worried about a little locally, is to understand the relation between the classically coupled fields and the quantum theory; to understand this, that is, when one cannot use the principle of super-position and the theory of normal modes. I will promise to keep you informed if we get anywhere with this, and I would hope that you would let us know as things become clearer to you. One reason which may not be too good a one for my own interest in this problem is the following: One can integrate the Tomonaga equation formally as follows:

$$\Psi(\sigma) = \left\{ \text{``}e\psi p\text{''}\, i/\hbar l \int_{-\infty}^{\infty} T_{\nu}^{mt\mu}(x)\eta^{\nu}\eta_{\mu} d_4 x \right\} \Psi(-\infty)$$

The essential point in all electrodynamic treatments so far is that in order to define the "exponential" one must approximate to it by a power series so that one can order the non-commuting factors properly; i.e., with the latest point to the left, etc. These difficulties of the order do not occur in a classical field theory; and if one should solve the classical problem, one would have a starting point for a perturbation theory based on the smallness of h and not of e. To date this is a program only; and I suspect that there are some deep reasons why it will not work, because a number of us have been trying without success.

One piece of news which you need to know is how very very good Dyson is. He wants to return, and in fact must return, to England for the next years, but we have made a flexible arrangement with him to come back here for as many semesters as he can spare. I think he likes the arrangement and we are all delighted by it.

He has gone a long way towards answering the questions about the finiteness and uniqueness of electrodynamics and has established strong presumption that in going to more complicated processes and higher order corrections nothing worse than the Lamb shift will turn up.[232] The Lamb shift itself, after agonizing fluctuations, appears to be settling

[232] F.J. Dyson, 'The Electromagnetic Shift of Energy Levels', *Phys. Rev.* **73**, 617–26 (1948); F.J. Dyson, 'The Interactions of Nucleons with Meson Fields', *Phys. Rev.* **73**, 929–30 (1948).

down to 1052 meg, a result which was not obtained by the highbrow Feynman-Schwinger-Tomonaga methods, but by much more pedestrian ones.[233] There is still a real question as to the complete uniqueness of procedures; but as for the moment — things have been changing rapidly — it does not appear that this has contributed to the uncertainty of the result; and it does not appear that only rather arbitrary and odd methods of following the integrations, which have not, in fact, been used, can give a different answer.

You are most kind about "Time". I think it stank, and suffered very much for a couple of weeks until I could forget it. It would be very good to have a talk with you about many things in physics and some in politics and the atom. I hope that business will bring you to this country soon and that you will stay with us for a while.

We both send you both our warmest, friendliest wishes for the New Year,

Oppie

[468] Freeman Dyson to Rudolf Peierls

Princeton, 16.1.1949

Dear Professor Peierls,
I am sorry to be so slow in answering your very nice letter of November 30th.[234] During this interval I have been trying to get my own mind firmly made up about what I want to do and I have now finally decided that Birmingham is the place, and I will take up your offer of a Research Fellowship. The salary will not be of crucial importance to me, to start with at any rate, and so I leave it to you to get what you can for me. More important will be the question of finding somewhere pleasant to

[233] F.J. Dyson, 'The Radiation Theories of Tomonaga, Schwinger and Feynman', *Phys. Rev.* **75**, 486–502 (1949).
[234] Peierls had invited Freeman Dyson to join his department as a research fellow. Letter could not be located.

live, and I should be very grateful for any help or advice you can give about this; I shall be arriving in England about July 15th, and so I shall have two months to make arrangements before actually moving in.

The "usual particulars" about me are:

Age, 25
Degree, Cambridge B.A. (1st class) in mathematics, 1945
British subject by birth,
Academic history; Research Fellow of Trinity College, 1946–49; Member of the Institute for Advanced Study, Princeton, 1948.

If they want to know further details concerning the colour of my hair, membership of the Communist Party, etc. I will be glad to supply them.

For your own information (this is a little confidential), Oppenheimer has very generously arranged for me a 5-year membership of the Institute, on the understanding that I make use of it for no more then $1\frac{1}{2}$ years between now and 1954. This means that I shall be able to come here at intervals and keep abreast with what the U.S. is doing, and it is an arrangement which would combine well with a Fellowship at Birmingham carrying no departmental responsibilities. Of course I should stay at Birmingham for the whole of the first year at least. I think it is extraordinarily thoughtful of Oppenheimer to have so anticipated my needs.

I was interested in the remarks you made in your letter about physics (probably by now you have completely forgotten what you said). The problem of dealing with the hydrogen atom rigorously as a two-body system rather than as an electron in a given field has been thought about a great deal, and nobody has yet proposed a simple way of doing it; however I am convinced that it is not a fundamentally difficult matter or a gap in Schwinger's theory. One can see quite well that the second-order radiative corrections arise from the same kind of processes as in the Lamb-shift, only you have processes in which the proton takes part in addition to those involving the electron. The difficulty in carrying out a complete calculation seems to be due mainly to the fact that people have not yet found a decent method of treating the hydrogen atom as a two-body problem, even without radiative corrections. This is said, with all due apologies to Breit.[235]

Concerning the more general questions of the validity of electrodynamics à la Schwinger and Feynman, I hope soon to be able to send to you a long paper of mine in which these things are discussed.

The paper is finished and is at the moment in the process of being mimeographed; with luck it will be published some time in the summer (the first paper appears in the Feb. 1 *Physical Review*).[236] In the paper I have dealt with scattering problems exclusively, and shown that for them at least the theory always gives finite and sensible results. Also, we know now (Schwinger found it out) that there is no real discrepancy in the Lamb-shift calculations; everybody now agrees on 1052 as the right value, and the mistake in the old "highbrow" calculation was not a question of principle but was just a wrong neglect of terms at the low-energy end. It seems to me now not at all likely that the bound-state problems will give worse troubles than the scattering problems, though of course the calculations will always be tougher.

You will see from my paper that I do not believe in a program of making infinite self-energies finite. The reasons for this are too long to be talked about here. I agree with you in not taking seriously Feynman's cut-off methods; he himself does not take them very seriously either. His big paper on electrodynamics will unfortunately not be appearing for some time.

With many thanks for your letter,

Freeman J. Dyson

[235] Gregory Breit at Yale was working on related problems. In the late 1940s he published several important papers about the fine and hyperfine structure of hydrogen. G. Breit and G.E. Brown, 'Effect of Nuclear Motion on the Fine Structure of Hydrogen', *Phys. Rev,* **74**, 1278–84 (1948); G. Breit and G.E. Brown, 'The Effect of Nuclear Motion on the Hyperfine Structure of Hydrogen', *Phys. Rev,* **76**, 1299–1304 (1948). But Breit's relativistic two-body interaction had limitations in that it could only be used as an expectation value. Gerry Brown, Breit's collaborator at Yale, later tackled these limitation when he was working as a research assistant at Peierls' institute at Birmingham.

[236] F.J. Dyson, 'The Radiation Theories of Tomonaga, Schwinger, and Feynman', *Phys. Rev.* **75**, 486–502 (1949).

[469] Rudolf Peierls to Freeman Dyson

Birmingham, 21.1.1949
(carbon copy)

Dear Dyson,
I am very glad that you are coming here and I shall now set the machinery in motion to offer you an appointment. From what preliminary enquiries I have made I am now doubtful whether this will be at the top of the salary range I mentioned before, but from what you say you would not make that a condition. As I have vacancies now which I do not expect before the end of the session your appointment could run from any date you choose and if, for example, you would like to start immediately on arrival or, for example, on the 1st of September, will you let me know? It will make no difficulty if you want to visit Princeton from time to time, on the contrary, this will obviously increase your value to us. I would, of course, expect that the dates of such visits be discussed and that you would meet me if I was anxious to have you here at some specific time because I want to be away or for other reasons. Equally obviously, your salary would not continue during your absence, if during that period you are getting a full salary from another source (subject to anything special we might be able to arrange about travelling expenses).

The information in your letter is quite adequate, luckily we have not reached the stage in this country where a university would consider it reasonable to ask questions about the political views of their staff.

To avoid delaying this letter I shall not comment on your remarks about physics which were most interesting beyond saying that we are looking forward to seeing your paper[237] from which we hope to learn a lot of things that we have not yet managed to understand.
Yours sincerely,

R.E. Peierls

[237] Letter [468], note 236.

[470] Rudolf Peierls to Hans Bethe

[Birmingham], 25.1.1949
(carbon copy)

Dear Hans,
I am writing again, this time about a number of assorted points, instead of sending you three of four letters in succession.

The first point is that Dyson has now decided to come to Birmingham next session, which, of course, cheers me very much. It remains now to get the authorities here to fix his salary and I am trying to get for him a reasonably high salary which may be a little difficult in view of his age. Would you be good enough to write me a letter with a frank statement of your opinion of him; this should cover not merely his ability and promise but also his actual achievement to date, since the point at issue is the extent to which he can be regarded as of fairly senior standing. For example, it would help, if you could state what sort of job he could expect to have in an American university if he was an American, or if you were able to compare him to the general run of people holding senior jobs in this country (if this can be done without being rude).

The next point is a question of physics. I don't remember whether I have mentioned to you before some work which Swiatecki[238] here has done recently trying to get to the bottom of the magic numbers, 20, 50 and 82, in nuclear structure. The occurrence of these numbers smells of the existence of "shells", i.e. single particle states to some approximation, but there are two factors against this. (1.) that on a shell model you would have to explain why you get only the breaks at these numbers and none at the intermediate numbers which should correspond to closed shells. (2.) you cannot get a complete shell at 50 unless you omit the 2s level, whereas you cannot complete 20 unless you include it. Both these difficulties can be met in principle by observing that with increasing weight the shape of any potential well would have to change both on

[238] Wladek J. Swiatecki (1918–), completed his Ph.D. under Peierls in 1949 working on nuclear structure. He later went to Copenhagen, Uppsala, and Berkeley where he spent the rest of his career.

account of the predominance of the surface effects for light nuclei and because of the increase in the Coulomb forces which tends to decrease the density near the centre as compared to the edge. In particular, the latter effect will make the sequence 2s, 3p $\cdots$ go up with increasing weight relative to 1s, 2p $\cdots$ It is, therefore, not impossible that when Y or $N(= A-2)$ is about 20 the 2s level should still be fairly low, whereas near Z or $N = 50$ it might have moved up above 3d. At the same time, this would explain why intermediate shells do not appear to be marked when two levels are just about passing each other one would not get any clear break. On this view one would be dealing with a situation analogous to the rare earths in atomic structure.

The question whether the reduction in the density near the centre is big enough to do this depends on what one assumes for the compressibility of nuclear matter. There exists on this only a paper by Feenberg[239] in which one of the two required parameters fixed from the observed mass defect curve, whereas the other is fixed from fishy theoretical treatment. We have been unable to rule out the possibility that the compressibility might be somewhat larger than is usually assumed and it might even be possible that heavy nuclei are almost hollow. We have thought of means of deciding this question approximately; in principle it might be done by observing nuclear radii, but this is very insensitive. One interesting possibility is to use the diffraction of electrons by nuclei using energies of 30MeV or more. Now the diffraction patterns to be observed have been calculated by Rose using the Born approximation and if one does this both for a solid and a hollow nucleus the difference is small but not altogether negligible. Now Born approximation is, of course, very poor, particularly for heavy nuclei and, while it can hardly be expected to give a long order of magnitude, it would be nice to have a better theory. I believe this is in principle contained in the work of Smith[240] which you mentioned when you were here and the main object in telling you this long story (apart from getting your general comments) is to see whether Smith's work, when complete, will cover the answer to this question, or

[239]E. Feenberg, 'Nuclear Shell Structure and Isomerism', *Phys. Rev.* **75**, 320–22 (1949).
[240]Presumably Jack H. Smith at Cornell.

if not whether his method can be used for the purpose. If you do not intend to have this done we would very much welcome a chance to see Smith's work in advance so that we could adapt it for this purpose.

My third problem arises from the planning of the 1950 volume of the Physical Society Progress Reports which you know, as you wrote an article for it in the past.[241] We were wondering whether we could have an article on nuclear forces; would you be interested in writing this? The date of publication is not fixed yet, but probably the MS. would have to be received during the late summer for publication early next year. You will remember that these articles are not generously paid but serve a very useful purpose. Compared to the Review of Modern Physics they are rather less specialised and less high-brow. If you cannot do this, can you suggest someone else? Equally, of course, other suggestions about articles in the series would be welcome.

Lastly, we understand that the Fulbright Scheme is coming into effect next session and that, as you probably know this will allow research students and more senior people to come and work here. We have been asked to say whether we would accept research students, which, of course, we would, if they are suitable, and what kind of more senior people we would like. This information was required at such short notice that it was impossible to find out who would be interested. I have mentioned a few names at random including Feynman and Placzek, though these are long shorts, but if you hear of anybody who might like to spend a year or so in Europe, will you let them know or get directly into touch with the Fulbright people. I have also mentioned to them that I would quite like to have someone who works on solids and/or metals to arouse some interest in this here, though of course he could not learn very much on this subject from us. Under this heading I have not mentioned any names at all, but would be grateful for suggestions. Yours sincerely,

[Rudi]

[241] H.A. Bethe and R.E. Marshak, 'The physics of stellar interiors and stellar evolution', *Reports on Progress in Physics* **6**, 1–15 (1939).

[471] N. Kemmer to Rudolf Peierls

Cambridge, 31.1.1949

Dear Peierls,
There are two things I should like to consult you about.

The first concerns the Schwinger, Tomonaga etc. stuff. I have 2 1/2 men here studying these things and now that Schwinger has published Pt 1[242] there is hope that we shall eventually learn publicly all that has been done. In the meantime, however, we have played with the obvious generalisations to meson theory and though (using later work by Tomonaga) the general lines of a consistent extension to these cases are clear, we are still by no means clear about the details. I myself have unfortunately only very little time to devote to thinking about these things. So I wanted to ask you whether you have had any more recent news of progress on this front. We possess the things duplicated at Birmingham, and Vol. III 1& 2 of the Japanese Journal (Prog. Th. P.[243]) also of course the Pocorvo notes. I should be most grateful to learn of any other information. So far we cannot report any results, though the lads have done masses of exploratory paperwork.

The second matter is of a different nature. Do you know Felix Adler[244] who at various times was in Switzerland, Paris, Montreal and now Wisconsin? If you do, you will remember that he is an almost pathologically shy person, but has improved in recent years. At Wisconsin he has, I understand, been quite a success as a lecturer, but he doesn't like life in the States and, partly under the influence of relatives, I believe, he wants to get over here. He realises that he doesn't stand much of a chance to be offered an attractive post and that, although

[242] Julian Schwinger, 'Quantum Electrodynamics, I. A Covariant Formulation', *Phys. Rev.* **74**, 1439–61 (1948).

[243] *Progress in Theoretical Physics*, monthly journal founded in 1946 by Hideki Yukawa and now published for the Yukawa Institute for Theoretical Physics and the Physical Society of Japan.

[244] Felix T. Adler (1911–1979), theoretical nuclear physics who later moved to Illinois, where he was instrumental in establishing the university as a national centre for reactor science and engineering.

there are quite a few poorly paid posts at small universities going one hardly would import a foreigner from Wisconsin for one of those. So his only chance would be to come here for a year or so on some grant and then look around.

I was wondering what your attitude would be to having him around for a year, if possible with some financial assistance provided by some source you can tap. I think he is quite a good man when in the right atmosphere and his yield of scientific work has been small. Apart from early work under Wentzel, which wasn't very independent there have been some things on neutron diffusion and now, I hear, on theoretical chemical lines. But he does know quite a lot.

I am asking you about this mainly because Mrs Burkill to whom he is related, turned to me for advice. Don't hesitate for a moment if you feel you must give a definite and not encouraging reply.

I have just thought of another thing I wanted to mention, and that is that I expect to have a really good young man in search of a post by October so that I should be glad to hear from you if you know of anything going.

Sorry to bother you.

Best regards.

Yours sincerely,

N. Kemmer

[472] Rudolf Peierls to N. Kemmer

[Birmingham], 2.2.1949
(carbon copy)

Dear Kemmer,

On the question of information about Schwinger theories, Schwinger has completed Part II of his paper which is being published in the Physical Review.[245] He sent me a duplicated copy of this which is at

[245] Julian Schwinger, 'Quantum Electrodynamics, II. Vacuum Polarization and Self Energy', *Phys. Rev.* **75**, 651–79 (1949).

present in enormous demand here. You might try to see whether he can spare you another copy of this. It is much more readable than the earlier Part II which you have. Another paper on the same subject has just been completed by Dyson in which the equivalence of Schwinger and Feynman's method is proved and in which is also shown how to generalize the method beyond the second approximation.[246] I have just received a duplicate copy of this and besides Dyson, Weisskopf has also had copies made. You might be able to get a copy from either (Dyson is now at Princeton).

As regards the Japanese journal, I have had correspondence with the American Scientific Advisor in Tokyo and also directly with Tomonaga as a result of which we are just putting into effect an exchange scheme. The American Office has informed me that besides Tomonaga also Dr. Kobayasi is interested in such an exchange; I am not clear whether this is an alternative to the arrangement with Tomonaga or in addition. I expect to hear shortly from Kobayasi and I also expect to hear which British journal Tomonaga wants from us. Perhaps the best thing would be to wait until I have both these replies and I shall let you know.

We are, of course, also studying the Schwinger papers intensely and perhaps in a little while it would be a good idea if one or two of the people in your team who are interested could come across for a day or so to compare notes. We would, of course, be delighted if you could join in this. I think, however, it would be best to wait another month or two when everybody will have digested things somewhat further and when we can get more out of such discussions.

As regards Adler, it would, no doubt, be nice to have him around, but competition for the Research Fellowships I have available here is likely to be keen and my impression both from what I know and from what you say is that he would not be a very strong candidate. The only other type of grant I can think of is under the Fulbright Scheme which does make it possible to pay for American scientists to spend a year or so in this country. I do not know very much as yet how one goes about getting such a grant in practice and I would not commit myself at this

[246] See letter [468], note 236.

stage to asking Adler unless I knew whether this would spoil the chances of getting someone more senior. Meanwhile it may be possible for him or the head of his department to apply directly from the American end.

Jobs at smaller universities are not always badly paid and finding a more attractive grant he could do worse than accepting a teaching or research job at a smaller university which will certainly pay a living salary, provided this is geographically within reach of bigger centres so that he can make contacts and get himself known.

In this connection I heard recently that ter Haar also is interested in a job in this country.[247] He is, of course, much more senior and he would give up a full professorship at Purdue to come here so that he would obviously not consider a very junior job. If you hear of any vacancy you might like to bear this in mind. As regards your other bright young man, could you tell me more about him, particularly whether he would be suitable for a D.S.I.R. Maintenance Award or a Senior Research Award or, of course, a University Research Fellowship. It is a bit early yet to consider the final staffing for next session, but I would like to consider him as a possibility.
Yours sincerely,

R.E. Peierls

[473] Hans Bethe to Rudolf Peierls

Ithaca, 2.2.1949

Dear Rudy:
I think the enclosed letter on Dyson is strong enough but the strange thing is that it is all true and sincere. He is really incredibly good.

Concerning your calculations on nuclear structure, I just have on my desk 2 papers, one by Nordheim[248] and the other by Feenberg,[249] about

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[247] Dirk ter Haar eventually took up a post as Reader at Oxford University in 1956.
[248] L.W.Nordheim, 'On Spins, Moments, and Shells in Nuclei', *Phys. Rev.* **75**, 1894–1901 (1949).
[249] Eugène Feenberg had published a paper on nuclear shell structure in early 1949

the same subject. Feenberg especially uses a model similar to yours in which he takes into account that the nucleus may be something like a hollow shell.

Having just returned to Cornell I have not had time yet to find out about the recent calculations of Jack Smith. I think he has calculated the effect of the distribution of protons on the nucleus and came to the conclusion that the effect is hardly observable if the distribution is that assumed by Feenberg in previous papers. However, if the nucleus were actually a hollow shell, this should be observable in electron diffraction. Nobody as yet has experimented on this but this will probably soon be done. The Berkeley synchrotron is going and ours is beginning to go, and electron diffraction is one of the points on our program. I will write more about this when I have checked with Smith.

Concerning the Physical Society progress reports, I should like to think it over a little more. At the moment I would rather say no, because I still have a good deal of writing to do for the books of Segre[250] and Schein[251] on nuclear physics and cosmic rays respectively. I will ask Phil Morrison if he wants to do it; he would do it very well. Weisskopf may be another possibility.

Concerning exchange students, I had a very good boy at Columbia by the name of Slotnick[252] who would like to go to Europe next year. His first choice is Pauli, but he is also very much interested in coming to you. Possibly he will want to divide his time between Birmingham and Zurich. He will write you directly. I am writing to Eyges[253] about possible jobs for next year for him.

(see letter [470], note 239.) and had submitted another paper on nuclear shell structure to be published later in 1949. E. Feenberg and K.C. Hammack, 'Nuclear Shell Structure', *Phys. Rev.* **75**, 1877–93 (1949). A few weeks after Bethe's letter to Peierls, Feenberg, Hammack and Nordheim submitted a joint note on the same theme. E. Feenberg, K.C. Hammack and L.W. Nordheim, 'Note on Proposed Schemes for Nuclear Shell Models', *Phys. Rev.* **75**, 1968–69 (1949).

[250]Emilio Segrè (ed.), *Experimental Nuclear Physics*, New York: Wiley, 1953.

[251]D.J.X. Montgomery (ed.), *Cosmic Ray Physics: based on lectures given by Marcel Schein at Princeton University*, Princeton: Princeton University Press, 1949.

[252]Murray Slotnick, in fact went to Princeton in 1950, before moving to the University of Michigan, Ann Arbor.

[253]Leonard Eyles went to Berkeley, M.I.T. before joining the Air Force Cambridge Research Laboratory in Massachusetts.

I had a rather fruitful semester at Columbia and will soon send you a paper on the effective range of nuclear forces.[254] I am also sending you a copy of the first paper by Dyson which will soon appear in print.[255]
With best regards to you and the family,
Yours sincerely,

Hans

[474] Wolfgang Pauli to Rudolf Peierls

Zürich, 14.2.1949

Lieber Herr Peierls!
Ich habe mich soeben entschlossen, eine freundliche Einladung des "Faculty Board of Mathematics" in Cambridge anzunehmen und dort die sogenannte "Rouse Ball lecture" zu halten.[256] Da der Term dort am 11. März schließt, habe ich vorgeschlagen, die letzte Woche dieses Terms als Zeit der Vorlesung zu wählen. (Meine Korrespondenz läuft mit dem Sekretär der Faculty Board Dr.A.J.Ward, Emmanuel College, Cambridge). Das Thema der Vorlesung ist "Physical and mathematical aspects of recent developments in quantum electrodynamics".

Vor etwa 2 Wochen habe ich einen langen Brief an Schwinger geschickt.[257] Leider habe ich nur eine begrenzte Zahl von Kopien zur Verfügung, hatte aber Ma[258] (zur Zeit in Dublin) gebeten, Ihnen eine Kopie zukommen zu lassen. Wenn Sie sie noch nicht erhalten haben, wird sie sicher bald zu Ihnen gelangen.

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[254]H.A. Bethe, 'Theory of the Effective Range in Nuclear Scattering', *Phys. Rev.*, **76**, 38–50 (1949).
[255]See letter [468], note 236.
[256]These were lectures about new developments in quantum field theory, which took place between 8 and 19 March 1949.
[257]Letter W. Pauli to Julian Schwinger, 24.1.1949, Pauli, *Wissenschaftlicher Briefwechsel*, III, pp 609–19.
[258]Shih-Tsun Ma, Chinese physicist who was working with Walter Heitler at the Dublin Institute for Advanced Studies.

Es handelt sich mir darum, versteckte Annahmen, die in Schwingers "Beweisen" implizit enthalten sind, an's Tageslicht zu bringen. Das gilt nicht nur für die Fragen der Polarisation des Vakuums, sondern auch für das interessantere Problem der Korrektur des magnetischen Momentes des Elektrons. Die in meinem Brief an Schwinger zum Schluß erwähnte Rechnungen von Villars[259] sind inzwischen im wesentlichen fertig geworden,[260] auch haben wir die erste Hälfte von Schwingers "Part III"[261] inzwischen erhalten.* Die dort angeführten Rechnungen über das magnetische Moment des Elektrons sind formal richtig, ich betrachte sie aber als definierende Regel, wie die betreffenden bedingt konvergenten Integrale auszuwerten seien und nicht als hypothesenfreie Beweise. Dies kann ich auf Grund von Villars Rechnungen näher begründen.

Ich bin jetzt an einem Punkt angelangt, wo ich gerne mit anderen über diese Fragen diskutieren möchte. Denn ich verstehe zwar die Mathematik, bin aber von der Physik darin nicht befriedigt. Die ,,Regulatoren" können dem Wesen der Sache nach nur eine provisorische Formulierung sein.

Ist Herr Salpeter noch bei Ihnen? Was haben Sie sonst für Leute in Ihrer Gruppe? Ich würde eigentlich gerne noch einige Tage länger in England bleiben und auch andere Orte als Cambridge sehen. Da ich an die Konferenzen in Birmingham so oft abgesagt habe, könnte ich diesmal kommen. Es liegt mir aber nichts an populären Vorlesungen, sondern mehr an "technical talks" in Seminaren über theoretische Physik. Ist Rosenfeld an solchen Fragen interessiert?[262]

*Dagegen habe ich von ihm noch keine Antwort auf meinen Brief. It seems that he thinks it over.

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[259] Felix Villars (1921–2002), studied physics and mathematics at the ETH where he completed his Ph.D. in 1946, before working as Pauli's assistant between 1946–49. He became research associate and later lecturer and professor at the MIT where he stayed until his retirement.

[260] W. Pauli and F. Villars, 'On the invariant regularization in relativistic quantum theory', *Rev. Mod. Phys.* **21**, 434–44 (1949); F. Villars, 'On the energy-momentum tensor of the electron', *Phys. Rev.* **72**, 122–28, (1950).

[261] This refers to the draft of J. Schwinger, 'Quantum Electrodynamics III. The electromagnetic properties of the electron-radiative corrections to scattering', *Phys. Rev.* **76**, 790–817 (1949).

[262] Rosenfeld spent the academic year 1949/50 at Peierls' institute in Birmingham.

Vielleicht können Sie die Nachricht meines Kommens nach England auch sonst dort verbreiten und sich über Daten mit Dr. Ward in Cambridge verständigen. (Vor dem 7. oder 8. März kann ich nicht nach England kommen, habe aber Zeit hinterher.)
Viele Grüße

Ihr W. Pauli

[475] Rudolf Peierls to Wolfgang Pauli

[Birmingham], 17.2.1949
(carbon copy)

Dear Pauli,
Thank you for your letter. If after your visit to Cambridge you could manage to spend a week or so in Birmingham this would suit us both very well indeed. We would not expect you to give any lectures to a big audience but we would get most out of your visit if you could give us one talk (or more) in our Seminar and for the rest just discuss things informally. Besides myself, Salpeter is very interested in the Schwinger theory and has studied it extensively, also a Pole, Rzewuski,[263] (a friend of Rayski) who is working now on the coupling between nucleons and the meson field à la Schwinger. McManus is also very interested in field theory,[264] though he has as yet spent less time with the Schwinger technique and there are three or four others who know enough about the problems to ask intelligent questions.

The period from 11th–19th March is quite free as far as I know, except that we are arranging for Dirac to come over on the 17th to give a talk in our Seminar. However, it would add to the attraction to have both you and Dirac here at the same time and there is no reason why our Seminar should not meet several times during the same

[263] Jan Rzewuski had come to Birmingham on a fellowship of the Polish government and returned to Gdansk where he continued to teach physics.
[264] See letter [396], note 80.

week. Our term does not end until the 19th and I, therefore, shall have to give a few lectures but not enough to interfere seriously with our discussions. If your time is limited and you want to spend only a few days in Birmingham, I think it would be better if you came during the early part of the week, since Dirac's talk will be about monopoles and presumably this is not what you want to discuss in the first place.[265]

We can take care of your fare from Cambridge and back to Cambridge or London and we can pay your hotel expenses while you are in Birmingham, but apart from this I am afraid we cannot offer you a fee for your lectures.

I am writing to Ward in Cambridge so that he can keep us informed about the dates of your visit there.

A copy of your letter to Schwinger has arrived here a few days ago from Heitler; we have not yet, of course, digested it completely, but I tend to agree with you that this represents a considerable advance in our understanding of the mathematical structure of the theory, but leaves it physically in an unsatisfactory state. One interesting question would be to see the relation between your method and Dyson's treatment of the higher approximations.[266] In particular, one would like to know whether in the treatment of higher order treatments each order has again to be treated by using appropriate "regulators" or whether the same trick will do for all orders. I suppose you have seen Dyson's paper, if not we could probably lend you a copy.

I think it would be very important to let Dyson see your note if that has not yet been done. As you know he is now at Princeton.
Yours sincerely,

R.E. Peierls

[265] Pauli had initially been very doubtful about magnetic monopoles (P.A.M. Dirac, 'The theory of magnetic monopoles', *Phys. Rev.* **74**, 817–30 (1948)), but after discussing the idea in detail with Dirac at the Solvay Congress in 1948, he revised his views. See letter Wolfgang Pauli to Arnold Sommerfeld, 1.2.1949, Pauli, *Wissenschaftlicher Briefwechsel*, III, p. 624.

[266] F.J. Dyson, 'The radiation theories of Tomonaga, Schwinger and Feynman', *Phys. Rev.* **75**, 486–502 (1949).

[476] Wolfgang Pauli to Rudolf Peierls

Zurich, 23.2.1949

Lieber Herr Peierls,
Vielen Dank für Ihren Brief vom 17.[267] Zeit und Bedingungen für meinen Besuch in Birmingham passen mir ausgezeichnet so wie Sie es vorschlagen. (Inzwischen habe ich von Kramers auch eine Einladung nach Holland erhalten. Ich soll ab 21. März dort sein, werde also von Birmingham nach Leiden fahren.) Die Vorlesung in Cambridge ist am 10. März, ich rechne also damit, etwa vom 14. bis 19. März in Birmingham zu sein.

Daß ich auf diese Weise einen Vortrag von Dirac über die Monopole hören werde, ist mir auch ganz recht. Diese Theorie von Dirac hat eine gewisse Schönheit in sich und es ist möglich, daß die Monopole tatsächlich in der Natur existieren. Wenn man sie aber ernst nimmt, müßte man viel <u>mehr</u> über sie wissen als was in Diracs Arbeit steht. 1) Was ist ihre Masse? 2) Wie beeinflußen sie die <u>Kernkräfte</u>? Diracs Theorie kann richtig sein, aber sie ist zu <u>arm</u> an Aussagen. Da alle Theorien über die magnetischen Momente von Photon und Neutron falsche Resultate geben, könnte man wohl auf den Gedanken verfallen, die Neutronen als aus zwei Diracschen Monopolen zusammengesetzt zu denken.

Ich möchte gerne von Ihnen wissen, ob in Birmingham Kopien von Schwingers "Part II" und der ersten Hälfte von "Part III" available sind.[268](Wenn ja, könnte ich nämlich meine Exemplare Herrn Villars hier lassen, der sie gut brauchen kann.)

I have a copy of Dyson's paper[269] here and also a letter of his in which he announced a second paper, which, however, has not yet arrived. I also made an arrangement that a copy of my letter to Schwinger should

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[267] See letter [475].

[268] J. Schwinger, 'Quantum Electrodynamics II. Vacuum polarization and self-energy', *Phys. Rev.* **75**, 651–79 (1949) and J. Schwinger, 'Quantum Electrodynamics III. The electromagnetic properties of the electron-radiative corrections to scattering', *Phys. Rev.* **76**, 790–817 (1949).

[269] See letter [475], note 266.

be sent to Princeton. Dyson seems to be a good man, his letter was in some respect enlightening for me. We shall talk more about it.
Schwinger's deep silence is continuing.
Meanwhile my best regards.
Yours sincerely,

W. Pauli

[477] Rudolf Peierls to J.R. Oppenheimer

Birmingham, 7.3.1949

Dear Oppie,
I wrote to you before that we were looking a little more quantitatively into the lifetime of the τ-meson following the process discussed by Finkelstein. This has not been completed and as you predicted the result is quite independent of the type of particle and the type of interaction. What we had been looking for was not so much a selection rule which, as you say, would not alter things as it would be broken in the next approximation, but rather a factor depending in a different way on the various dimensionless mass rations which occur.

I enclose a copy of a paper on this subject which is still in the draft stage and may still need some editing. Since the whole problem goes back to a suggestion you made here, I would be glad to know if you had any objections to being quoted on this and also whether you have, in the meantime, made this point in any other publication to which reference should be made.
Yours sincerely,

R.E.P

I am just getting Dyson's appointment here confirmed. There will of course be no difficulty in the way of his coming to Princeton whenever it suits him and you and I am very glad of this further link with the Institute.

[478] Robert Oppenheimer to Rudolf Peierls

[Princeton], 16.3.1949

Dear Rudi,
Thank you for your good letter and for sending Van Wyk's paper,[270] which I was glad to get, and with the results of which I have no quarrel. There is not yet in published form a discussion of this question on my part, but in the records of the Solvay Congress, there is to be, I think, a brief summary of my views on the alternatives, in which the connection of this difficulty with the materialisation process is raised, and the possibility of other coupling schemes briefly indicated. I do not know whether it is worth referring to this document, which is not yet out and which may never be widely available.

In the meantime, our thoughts have not matured much. Powell's wonderful picture, of course, raises the possibility of $\tau \to 3\omega$, where the lifetime, barring some mystical selection rule, should be even shorter than for gamma rays. If one assumes that $\tau \to 2^{\overline{\omega}+\mu}$ is made impossible by the conservation laws, then it appears that the lifetime of $\tau \to \omega + 2^{\mu}$ may well be of the [r]ight order of magnitude to fit observation. This point is in fact being explored by Sheila Power.[271] We will let you know when and if we have anything that looks as though it had any connection with the real world.

We sent you Dyson's second paper[272] and also, I believe, some long works of Case[273] on the nucleon-meson problem. It is turning out to be a very tough thing to digest these developments of the last two years and maintain any sort of perspective, the more so because at the moment one can neither get any sensible results with the mesons, nor devise

[270] Presumably C.B. van Wyk, 'On the Decay of the τ-Mesons', *Proc. Phys. Soc.* **A62**, 697–709 (1949).
[271] S. Power, 'Decay of a Heavy *tau*-Meson into Three Lighter Mesons', *Phys. Rev.* **76**, 865–66 (1949).
[272] F.J. Dyson, 'The Interaction of Nucleons with Meson Fields', *Phys. Rev.* **73**, 929–30 (1948).
[273] K.M. Case, 'On Nucleon Moments and the Neutron-Electron Interaction', *Phys. Rev.* **76**, 1–13 (1949).

methods sufficiently powerful to justify expecting them. It would be good to have a chance to talk again, and I hope that one or another pretext will bring us together in one or another country before very long. I have high hopes that on the matter of our more effective collaboration the situation will begin to clear long.

Kitty joins me in sending the warmest greetings to you and Genia. We think of you often with great gratitude for the wonderful time in Birmingham.

Robert Oppenheimer

[479] Freeman Dyson to Rudolf Peierls

Princeton, 31.3.1949

Dear Professor Peierls,
Thank you very much for your last letter.[274] The salary you are mentioning is really astonishingly generous, as also is your anxiety not to tie me down to Birmingham. I enclose herewith the particulars you wanted for the Royal Society; I am very much obliged to you for undertaking yourself to handle the details of applying for the Royal Society grant.

The Institute has not made me a specific travelling allowance for my expenses on future visits here, but the funds it provides are so ample as to cover all such expenses very comfortably. So you need not bother about that.

I will now briefly reply to the physical half of your letter. Incidentally, I have spent the last month, and shall spend the next, travelling around from place to place and giving talks and consuming excessive quantities of food and liquor. While this sort of life is good for acquiring general knowledge about what is going on in physics, it is certainly not conducive to serious thinking. For this reason my thoughts have not progressed substantially beyond the stage at which they were when

[274] Letter could not be located.

I wrote the S-matrix paper.[275] I believe I am not likely to be smitten with any new ideas until I have taken a long and complete holiday, which will not be until the midsummer at the earliest.

I am glad you have had a talk with Pauli, who seems to be the one member of the "old gang" who takes the trouble to thoroughly understand the new methods. We are all very pleased with his regulators;[276] especially the programme of using regulators harmonizes very well with the programme of systematic segregation of renormalisations outlined in my S-matrix paper. The regulators just make it clearer why the rules of procedure I have proposed are sensible, and vice versa. To demonstrate the equivalence of my rules with Pauli's it is only necessary to show that all the convergent operators, which appear in my method as the physically real effects after separation from the renormalisation, actually tend to zero with increasing electron rest-mass. That this is so seems to follow just from dimensional arguments; since the real effects always begin by being proportional to the particle momenta to some positive power, they must also have some positive power of the electron mass in the denominator. However, I have not yet tried to make a rigorous argument out of this.

You will see, if you read the last section of the S-matrix paper, where the physical meaning of the theory is discussed, that I differ very much from your view of these matters. Of course, one is here arguing about things which are matters more of taste than of judgement, and either or both of us may turn out to be completely wrong. However, I will here summarise my point of view for your consideration.

I regard the method of regulators as a method of making predictions of electrodynamics mathematically precise. As such it introduces into the theory a few additional physical hypotheses which were left vague in the old electrodynamics but these new hypotheses do not assert any physical significance for the method of regulators itself. In other words,

[275] F.J. Dyson, 'The *S*-Matrix in Quantum Electrodynamics', *Phys. Rev.* **75**, 1736–55 (1949). The paper was published in June 1949 but had been submitted in February 1949.

[276] See W. Pauli and F.Villars, 'On the Invariant Regularization on Relativistic Quantum Mechanics', *Rev. Mod. Phys.* **21**, 434–44 (1949).

the physics of the method is to be found in the formulae which are obtained after going to the limit and letting all additional masses tend to infinity, and not at any earlier stage. The hope which is expressed in the last section of the S-matrix paper and which I believe is a promising hope, is that electrodynamics can now be put into a consistent and divergence-free shape by a purely mathematical reformulation, without any additions to its physical content. The method of regulators is a rather modest step in this direction.

I certainly envisage the necessity later on of making alterations in electrodynamics of a more fundamental and physical kind. But I think the nature of such alterations cannot be guessed at, for example, by proposing to take seriously the formulae which arise in the method of regulators before passing to the limit of infinite auxiliary masses, you are making such a guess. I am less ambitious and confine myself to squeezing all I can out of the existing theory.

Again with many thanks,

Freeman Dyson

[480] Rudolf Peierls to Robert Oppenheimer

[Birmingham], 13.4.1949
(carbon copy)

Dear Oppie,

Things have changed a little as regards the calculation of van Wyk,[277] about which I wrote to you before. We had not noticed before that the list of cases he had covered in his paper did not contain the case in which both mesons have zero spin. In this case the emission of one photon is forbidden by conservation of angular momentum. The next process one would think of is then the emission of a positron-electron pair which might give an intensity only 137 times smaller. We have not studied this process but this is also forbidden if the two mesons have opposite parity, i.e. if one is scalar and the other pseudoscalar. In that

[277] See letter [478], note 270.

case the most likely allowed process is the emission of two photons. This has now also been calculated by von Wyk taking only the "g" coupling terms, i.e. coupling terms without derivatives. The result is: [...][278] which would not seem to contradict any known evidence. I suspect that taking f type coupling instead, the result would only differ by a small numerical factor, but this must be confirmed.

Admittedly this is a somewhat artificial explanation because it would give a [...][279]-meson the same status as an isomeric nucleus, but it might exist in nature for some reason that isomeric nuclei are found. It might well be true that in principle mesons are possible with all sorts of different masses and spins and that they all have very short lifetimes and are, therefore, never seen except the [...][280] which is metastable and therefore just visible.

Until we have evidence about the spin and parity of these mesons we should therefore bear in mind this rather simple possibility which does not require a drastic breakdown of current theory.

Von Wyk's paper has not been sent off for publication as yet, and he has amended his calculations accordingly. I would be interested in your reaction to this development.
Yours sincerely,

R.E. Peierls

[481] Rudolf Peierls to Freeman Dyson

[Birmingham], 22.4.1949
(carbon copy)

Dear Dyson,
Thank you very much for your letter.[281] In the meantime I had a chance to digest your second paper[282] a little more and I think I now

[278]Missing in carbon copy.
[279]Missing in carbon copy.
[280]Missing in carbon copy.
[281]Letter [479].
[282]Ibid., note 275.

understand its ideas, though, of course, I have not yet mastered every detail of technique.

I think, as Bohr would say, "we agree much more than we think".

You have shown in your paper that as long as the expansion in powers of the coupling constant is justified, one can find the prescription which will make the coefficient of any power unambiguous and finite. The new point which I had not appreciated when I last wrote was that only two or three divergent intervals recur and that everything is unique once one has made up one's mind what value to attribute to them. Up to this point, of course, there can be no possible controversy. However, the penalty for using an arbitrary prescription imposed after constructing the fundamental equations is that the procedure is not convincing as regards consistency with a different approach. If one could calculate the whole thing from a theory which would give only finite answers either by introducing a fundamental length or by using auxiliary masses of the Pauli type, one would at each stage get a definite answer and could then go to the limit of the point charge or of infinite auxiliary masses. I have little doubt that by proceeding in this limit your prescription for identifying and discarding self-energy terms would justify itself and that the questionable divergent integrals would in all orders take the values that you require. However, what is doubtful is the part played by the series expansion in this procedure. One may, I think, learn a little from the analogy with the classical case. The nonsense one gets in the Dirac equation with runaway solutions is directly due to the fact that this theory assumes the infinite positive self-energy to be offset by an infinite negative mechanical rest mass. At high acceleration the field lags behind and hence the negative mechanical energy provides an unlimited store for further acceleration. If the use of a cut-off in the Weisskopf theory of the self energy is any guide, it is likely that in a finite theory the self-energy will again be positive, though, of course, only varying as a logarithm of the cut-off energy. If you then proceed to the limit of zero radius you will reach a point at which the mechanical mass changes sign. Now in the classical theory the mischief brough[t] by this is not apparent if you only look at solutions which are power series in the electric charge and trouble arises from the appearance of new solutions which cannot be so expanded. In the quantum theory it is likely that the corresponding trouble, if it arises will take the form that no solution corresponds to a convergent series. Whether or not this difficulty arises in your cases is not easy to say and there are two separate questions: (a) whether the series obtained with your existing method converges. This would seem rather likely. (b) the other question is whether a theory in which a self-energy is made finite would converge for all values of the self-energy so that one could go to the limits. If (a) is all right but (b) is not, in other words if the elimination of the self energy and the limit of zero radius are not interchangeable, one could begin to have doubts whether your theory would be logically consistent with a treatment of the case where one deals with the case of discrete levels.

It is perhaps not quite fair to attack you on this point because you admit yourself that you have not covered the case of stationary states, but my point is that if one can succeed in rendering your prescription as plausible from a physical point of view, one would gain increased confidence that it must hang together with a reasonable treatment of stationary states. If it is regarded merely as a mathematical procedure justified by its consistency and by its results, one is, of course, left with the job of proving the consistency fully for all conceivable combination of cases.

I was also rather interested in your discussion of observability and I very much like the tendency to say that because one cannot detach particles from their fields, the range of possible observations is rather more limited. This fits on to what Landau and I tried to do many years ago[283] but did not do right. However, I think this must be studied very much more deeply because from the way you put it one does not get a clear distribution what the limits of observation are and what physical factors in them are responsible for the limitation. You will have an amusing controversy on your hands as soon as Bohr reads that section of your paper because Bohr is very sensitive to anything being said on this subject that he does not fully approve. The controversy, however, will be both entertaining and instructive.

[283]R.E. Peierls and L. Landau, 'Erweiterung des Unbestimmtheitsprinzips für die relativistische Quantentheorie', *Z. Phys.* **69**, 56–69 (1930).

I always feel that there is something inconsistent in the whole present approach because we take as a starting point the wave equation, i.e. the relation between energy momentum and spin, for free particles which is about the only thing we must be sure of. However, when one proceeds with the theory it turns out that the wave equations we have written are not really the equations for real particles, but they are the equations for ficticious particles uncoupled from the surrounding field about which we ought not to postulate anything. One could, of course, start differently by postulating in the first place that the wave equations for one real electron or one real photon should be simple and in that way would make the variables used by Schwinger after the first transformation the real basis of the theory. However, in these variables the equations, of course, are not linear but contain coupling terms of any arbitrary order giving direct matrix elements for processes in which an arbitrary number of particles are created, provided you have at least two to start with. Obviously it is hopeless to expect that each of these treatments of arbitrary order should be derived from the new physical principles, but it might be possible that there exists some common functional form for all these terms taken together which could be justified by reference to general principles. If this could be done one would have a basic formulation in which the "bare" particle never enters and in which accordingly the question of self-energy never arises. This would, I think, be very close to the spirit of your treatment but would differ from it by the fact that the self-energy is not ever written down and then eliminated. But whether such a treatment is possible I have no idea.

Yours sincerely,

R.E. Peierls

[482] Rudolf Peierls to Wolfgang Pauli

[Birmingham], 10.6.1949
(carbon copy)

Dear Pauli,
Following our discussions here[284] we have thought more about some of the problems arising from regularization and some more quantitative work has been done by Rzewuski. I think you might be interested to know the position we have reached at present, particularly since Rzewuski is going back to Poland now and for some time our own further progress here will not be rapid.

The first things that Rzewuski tried was to see whether in place of using particles of integer spin one could obtain your regularization also if the auxiliary particles were Fermi particles of higher spin. It seems somehow more satisfactory to couple electrons with other Fermi particles, though, of course, in regularizing such things as the electron self-energy one has to use "auxiliary photons" and one would like those to be Bose particles.

Rzewuski has limited himself to auxiliary electrons of three-halves and he has looked at the problem of vacuum polarisation.[285] In this case, it turns out, however, that the terms which give the same singularity as that for spin one-half, also have the same sign and therefore cancellation is impossible. Moreover, one obtains singularities of higher order because of the occurrence of higher derivatives in the commutation laws and these cannot be cancelled unless perhaps one introduces particles of still higher spin, but at this point we give up.

The situation, however, is still worse in which it is a condition on the Schwinger formalism that the local interaction Hamiltonian should

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[284] Wolfgang Pauli had visited Birmingham in March 1949. See letters Peierls to Pauli, 17.2.1949, Pauli to Peierls, 23.2.1949 in Pauli, *Wissenschaftlicher Briefwechsel*, III, pp. 631–2 and 638–39.

[285] J. Rzewuski, 'Some cut-off methods for the electron self-energy', *Proc. Roy. Soc.* A**62**, 386–91 (1949). He later took up this work in a joint paper with Rayski. J. Rayski and J. Rzewuski, 'On a system of fields free of divergences of the mass renormalization type', *Acta Physica Polonica* **10**, 159–72 (1951).

have the property that its value at two points on the same space-like surface should commute. Schwinger discusses this condition for his case and he claims that it is all right, but as far as we can see the proof involves the same kind of doubtful treatment of products of singular functions which you have criticised. This difficulty does not arise in Schwinger's case, if the electro-magnetic potentials are not quantized. In the case of spin three-halves this further goes wrong, already for a given external field and therefore one cannot really draw any conclusion at all, but this difficulty in itself seems to me to offer a ray of hope. It has been shown by Tomonaga that a similar difficulty arises in the case of meson fields or in other cases where the interaction Hamiltonian contains derivatives of the field variables and Tomonaga has succeeded in adding a term to the interaction which restores the Lorentz invariance as well as the commutability. One might try to do the same for the general Schwinger case and the extra terms would then have to be as ambiguous as the term Schwinger uses, but in such a way that the total expression becomes unambiguous and therefore for its evaluation one can use any reasonable representation of the D-function. We have not yet seen, however, how to start looking for the right kind of term.

In case of spin one-half one can write the commutator between different local Hamiltonians including field quantization and if one takes only the vacuum expectation value, which certainly ought to vanish, it actually becomes somewhat identical with your K[$\cdots$][286], but with the function [$\cdots$][287] $x - x'$ replaced by $D[\cdots]$[288]$(x - x')$.

One other point that we notice and which shows how sloppy the Schwinger mathematics can be arises from the zero-point energy. In equation 1.46 of Schwinger's second paper[289] he claims, for example, that the vacuum expectation value of the energy density of the electromagnetic field is zero. This, however, is also equal to the expectation value of $E^2 + H^2$ and it is rather a tall order to be asked to believe that this can be zero. Of course, the zero-point energy can always be

[286] Missing in carbon copy.
[287] Missing in carbon copy.
[288] Missing in carbon copy.
[289] Letter [476], note 268.

subtracted in an invariant way and will not give trouble, but to claim that a calculation of this quantity can give zero shows how dangerous the Schwinger methods are.

This makes us feel that a decent solution of the difficulties which you have pointed out in connection with vacuum polarization is really essential even at the very beginning and even the basic equations of the theory hang in the air until this problem has been faced.

I would be very interested in your reactions to these various points. Yours sincerely,

R.E. Peierls

[483] Wolfgang Pauli to Rudolf Peierls

Zürich, 10.7.1949

Lieber Herr Peierls!
Ich beantworte erst heute Ihren Brief vom 10. Juni,[290] da ich hierzu noch einige Resultate abwarten wollte. Zu den von Ihnen aufgeworfenen Fragen wollte ich folgendes sagen:

1. Ich weiß jetzt, daß alle Zweideutigkeiten betreffend Kommutatoren von physikalischen Größen (Viererstrom, <u>Dichte</u> der Wechselwirkungsenergie) in Punkten mit raumartiger Verbindungslinie fortfallen in einer Mixtur aus "wirklichen" geladenen Elektronen und geladenen Spin 0-Bosonen, wenn die Bedingunen erfüllt sind
$$\sum C_i = 0, \sum C_i M_i^2 = 0$$
(Dies ist demnach genügend für Eichinvarianz der Resultate.) mit den <u>speziellen Werten $C_i = 1$ für alle Spin 1/2-, $C_i = -1/2$ für alle Spin 0-Teilchen</u>.*

*Dieses Resultat stammt (für die erste Näherung in $e^2/\hbar c$ unabhängig von Rayski und Jost [Jost und Rayski (1949)], von Uhlenbeck und Pais [Pais und Uhlenbeck (1949)] und von Umezawam Yukawa und Yamada [(1948)](nach brieflicher Mitteilung von Tomonaga).

[290]Letter [482].

Dieser “realistische” Standpunkt dürfte auch genügend sein, um die Nullpunktsenergie zu kompensieren, da diese ja negativ ist für Elektron-Positronpaare.** (Man soll Kompensation der Nullpunktsenergie-dichte in allen Raum-Zeitpunkten verlangen. Es handelt sich um Ausdrücke der Form $\sum_i C_i(\frac{\partial^2\Delta^{(1)}}{\partial x_\mu \partial x_\nu})_{x<0}$ mit verschiedenen Massen.) Ich weiß allerdings nicht auswendig, ob auch hier die Zahlenwerte $C_i = 1$ für Spin 1/2, $C_i = -1/2$ für Spin 0 die richtigen sind, aber ich vermute das.

2. Aber: Dieser “realistische” Standpunkt versagt für die Selbstladung. Diese (logarithmisch unendliche) Konstante hat dasselbe Vorzeichen für Spin 1/2 und Spin 0-Teilchen (aus physikalischen Gründen vermute ich dasselbe für alle Teilchen) und eine Kompensation ist unmöglich. Dieser Sachverhalt macht mir die größte Sorge (siehe jedoch unten sub 5). (Jost nennt die Selbstladung den ‘top-nonsense’ der jetzigen Theorie.)

3. Was Teilchen mit höherem Spin betrifft, so habe ich bisher immer vermutet, daß für geladene Teilchen mit höherem Spin im äußeren (nicht quantisierten) elektromagnetischen Feld die Komponenten des Viererstromes in Punkten mit raum-artiger Verbindungslinie wirklich nicht kommutieren werden (und nicht nur zweideutig sind). (Wissen Sie etwas bestimmtes darüber?) Ich war deshalb geneigt, geladene Teilchen mit Spin > 1 auszuschließen (seit etwa 1940).

   Einer meiner Schüler hat mich darauf aufmerksam gemacht, daß dieser Schluß nicht zwingend sei, da eine Mischung geladener Teilchen mit höheren ganz- und halbzahligen Spins existieren könnte, für welche der Vakuums-Erwartungswert des Kommutators der Totalstromkomponenten in Punkten mit raumartiger Verbindungslinie wirklich Null ist. Vielleicht werden wir das hier noch weiter untersuchen.

**Das ist eine alte Idee von Pais und Bohr (1946).

4. Ein Schwede in Lund namens Källén, der jetzt in Zürich ist, hat das von Ihnen bei meinem Besuch in Birmingham angeregte Problem der <u>höheren Näherungen</u> (d.h. nicht linear im äußeren Feld) <u>der Vakuumpolarisation</u> mit großem Erfolg behandelt. (Dabei wurde zunächst das elektromagnetische Feld nicht quantisiert.) Für Elektronen kam dabei heraus, daß die Divergenzen in den sukzessiven Näherungen immer <u>schwächer</u> werden. In der nächsthöheren Näherung ist dann nur noch eine (die Eichinvarianzstörende) logarithmische Divergenz vorhanden, die mit $\sum C_i = 0$ formal weggeht und alle folgenden Näherungen (von e^6 angefangen) konvergieren von selbst. Nun rechnet Källén dasselbe auch noch für Bosonen (Spin 0). Ich <u>vermute</u>, daß die oben beschriebene Mixtur — <u>bis auf die unendliche Selbstladung! — in allen Näherungen konvergiert.</u>[291]

5. Dabei blieb, wie gesagt, das elektromagnetische Feld <u>un</u>quantisiert. Das andere Problem, wo man sich umgekehrt auf die im <u>äußeren</u> Feld <u>lineare</u> Näherung beschränkt, dafür aber die von der Quantisierung des elektromagnetischen Strahlungsfeldes herrührenden Korrekturen in höherer Näherung berechnet, wird hier von den beiden Polen Rayski und Weyssenhof behandelt.[292] (Rayski ist eben nach Hause gefahren, aber Weyssenhof — der bessere Physiker von den beiden, der auch sehr eifrig ist — bleibt noch bis Herbst hier.) Bei diesem Problem gibt es natürlich etliche Korrekturen zur Selbstladung. Ich bin sehr neugierig, welches Vorzeichen diese Korrekturen in nächst höherer Näherung haben werden und hoffe, daß es negativ sein wird.

   Es schwebt mir nämlich — als letzter Ausweg beim Selbstladungsproblem — etwas vage eine Art Bestimmung von $e^2/\hbar c$ aus der Bedingung des Verschwindens der Selbstladung vor. Natäurlich ist es dann sehr unschön, daß man nach Potenzen

[291] G. Källén, 'Higher approximations in the external field for the problem of vacuum polarization', *Helv. Phys. Acta* **22**, 637–654 (1949).

[292] See J. Rayski, 'Polarization of the vacuum', *Phys. Rev.* **75**, 1961 (1949).

von $e^2/\hbar c$ entwickelt, {Das denke ich mir aber nur als Ersatz für die Behandlung einer strengen Gleichung $f(e^2/\hbar c, m_1, m_2, \cdots) = 0$ wobei man eben $f(.)$ nach der Variablen $e^2/\hbar c$ entwickelt.} Zunächst muß man natürlich sehen, ob auch die Terme nächster Näherung in $e^2/\hbar c$ für die Selbstladung bei Elektronen und bei geladenen Bosonen kompatibel sein sollen. (Natürlich werden auch die zur Kompensation bei der Selbstenergie der geladenen Teilchen benötigten neutralen skalaren Hilfsfelder mit $m \neq 0$ in dieser Näherung Beiträge zur Selbstladung geben.)

Es würde mich interessieren, nun Ihre Kritik zu meinem momentanen Standpunkt zu hören. Es ist merkwürdig, daß ich aus Ihrer Kritik immer sehr viel mehr lernen kann als aus Ihren eigenen Arbeiten (oder Arbeiten Ihrer Schüler).

Meine allgemeine Idee ist die, daß die in beliebig kleinen Raum-Zeitgebieten definierbaren physikalischen Größen (Total-Energie-Impulsdichte, Total-Vierer-Strom) keine Spezifizierung der Massen und Spins der beteiligten Teilchen erlauben dürfen (siehe oben sub 1 des über die Nullpunktsenergie Gesagte).

Mit vielen Grüßen and Sie selbst und Ihre Familie (how is the baby?)
Stets Ihr

W. Pauli

P.S.
1) Der Inder Vachaspati muß sich erst an die europäische Umgebung gewöhnen und hat vorläufig noch nichts geleistet. Er soll, finde ich, vorläufig ruhig hier bleiben und so weit deutsch lernen, daß er Vorlesungen hören kann, statt schon wieder den Ort zu wechseln. Sollte er bis Herbst noch immer nichts verstanden haben, so müßte man ihn dann heimschicken, aber ich habe die Hoffnung für ihn noch nicht aufgegeben.

2) Ein Schüler von Schwinger hat mir einen langen, aber keineswegs inhaltsreichen Brief betreffend meiner Kontroverse mit seinem verehrten Lehrer geschrieben. Die Diplomatie dabei besteht darin, daß Schwinger ihm zwar gestattet hat, mir zu schreiben, sich aber geweigert hat, den

Brief seines Schülers selbst zu lesen!— Inhaltlich konnte ich nichts anderes aus der langen Rede entnehmen als daß Schwinger eine Art Offenbarung (auf irgendeinem Berg Sinai) gehabt hat: Und der Herr sprach: ,,Setze immer $\frac{\partial\Delta^{(1)}}{\partial x_\nu} = 0$ für $x - 0$, tue aber nicht so für $\frac{\partial}{\partial x_\nu}\partial^{(4)}(x)$ trotz gleicher Symmetrieeigenschaften."— Was Schwinger bei der Nullpunktsenergie macht (im Gegensatz zu meinem Kompensationsversuch) ist natürlich ganz ähnlich.

[484] Rudolf Peierls to Hans Bethe

[Birmingham], 27.7.1949
(carbon copy)

Dear Hans,
It looks almost as if I have not written to you since your letter of 7th February.[293] Meanwhile your letter about Dyson has done just what I wanted it to do, namely not merely persuaded the university to offer him a good job, but also got the Royal Society to appoint him to a Warren Fellowship which has advantages. He has been here already for two days and it will make all the difference to the department to have him here next year.

I assume that you have not done any more about the Physical Society Progress Reports and that is just as well, because with all the experiments going on now it is likely that a report written now would be obsolete before it appeared (the volume now in preparation will not come out before July 1950). Things may have settled down a little more in a year's time. The closing date for manuscripts for the following volume is going to be about September 1950 and it is much too early to talk about an article for that volume. However, I need hardly say that if you actually have started to write an article and if you are prepared to let us have it by September, everybody will be delighted to see it go in to the forthcoming volume.

[293] Letter Hans Bethe to Rudolf Peierls, 7.2.1949, *Peierls Papers*, Ms.Eng.misc.b.202, C16.

On the question of nuclear structure it seems indeed that Feenberg and Swiatecki here have been thinking along very similar lines, but for some time, Swiatecki has been working on a decent derivation of the surface tensions of a nucleus which is essential for the question under discussion and he had discovered that nothing said on this subject in the literature is any good. He has now developed a generalization of the Thomas-Fermi method which is suitable to treat potential gradients and, while the numerical work is not complete as yet, he seems to be able to derive a fairly good value for the surface term in the semi-empirical formula and one can then have some confidence in applying the methods to internal density gradients. I have not heard anything from Slotnick,[294] but I had some correspondence with Gerald Brown of Yale [295] who makes a very good impression and who is coming here probably with a Fulbright Grant. He was coming in October, but he has to finish some job for Breit and will not be free before January.

The last session has not been very productive, largely, though not entirely, because we were always trying to catch up with the work on field theory by Schwinger, Feynman and Dyson etc. We are now preparing for a determined attack on discussing meson theory by the new techniques without the use of perturbation theory; most of what has been said in the literature seems just to be perturbation theory run riot.

Dyson told us the sad story of your car; I hope it is making a good recovery and that in spite of it you are by now all reunited.

Yours sincerely,

[Rudi]

[294]Murray Slotnick, at the time at Columbia University then moved on to Princeton postdoctoral fellow.

[295]Gerry Brown had worked with Gregory Breit at Yale. He joined the Birmingham group in 1950.

[485] Niels Bohr to Rudolf Peierls

Copenhagen, 22.8.1949

Dear Peierls,
It has been a pleasure to me that it has now been arranged that Lindhard[296] will be with you next year. I am sure that it will be a great experience to him and I also hope that it will mean a still closer cooperation between our groups. As a small beginning I reckon that Lindhard's stay with you will be helpful in completing our old work with Placzek. In the last weeks I have gone through the old manuscripts with Lindhard and discussed with him the latest progress as regards nuclear constitution and in particular the success of the method of considering the binding energy of the nucleons separately in the nuclear field. I realize that one sometimes has taken the drop model too literally, and to clear my thoughts, I have written down a few tentative comments[297] of which I shall be very glad to hear your opinion. They do not contain much new, but I feel that the development gives a simple basis for the treatment of the problems of nuclear reactions and removes doubts as regards the conclusions to be drawn from dispersion theory and detailed balancing. As soon as I get time, I will try to incorporate such views in our old manuscript and will, if not before, give it to Lindhard when he leaves. This summer I have been busy with the preparation of a series of lectures on general topics, which I shall deliver at Edinburgh in the autumn [298] and have also worked with Rosenfeld on the completion of our work on the measurability of field and charge quantities.[299] It has come out that the situation is just as required by Schwinger's formalism, and that it is simpler than assumed by Heisenberg in that respect that charge fluctuations are well defined in sharply limited space-time

[296] Jens Lindhard (1922–), student of Niels Bohr's, later professor of theoretical physics University of Aarhus.
[297] Manuscript 'Tentative Comments on Atomic and Nuclear Constitution', (1949), reproduced in *Bohr. Collected Works*, Vol. 9, pp. 523–55.
[298] See letters [486], [490].
[299] N. Bohr and L. Rosenfeld, 'Field and Charge Measurements in Quantum Electrodynamics', *Phys. Rev.* **78**, 794–98, (1950).

extensions, just like field fluctuations. Also, this work I hope to complete in the autumn months. As you may understand it will be quite a busy time for me and, if it is not too inconvenient to you, I should be glad if Lindhard could stay here and leave for Edinburgh in the middle of October or in any case till the end of September.

With kindest regards and best wishes to your family and yourself from us all,

Uncle Nick

[486] Rudolf Peierls to Niels Bohr

Birmingham, 26.8.1949
(carbon copy)

Dear Uncle Nick,
Thanks you for your letter.[300] It will be all right for Lindhard to come here in the middle of October. I gather from Born that your lectures in Edinburgh will have longish intervals between them and this makes me wonder whether there might be a chance of you spending a little while in Birmingham while you are in this country. It would, of course, give us the greatest pleasure, if that were possible and we would be able to look after your expenses arising from such a visit. However, you need not decide this now.[301]

I have read your note with great interest,[302] but I am afraid I do not agree with some of the points. If I understand correctly the argument on the second page, you deduce from the large indeterminacy of position that it is possible to describe the motion of each particle as if it were moving in a smooth field of force. I do not believe that this conclusion is correct. At least if one assumes the forces between the nucleons to be of the type usually assumed (i.e. two-body forces, partly of exchange character, and compatible with the properties of light nuclei)

[300] Letter [485].
[301] Bohr did go to Birmingham and Peierls also met him in Edinburgh during the summer. See letter [490].
[302] See letter [485], note 299.

then the attempt to find the best potential to represent the motion has been carried out by Euler[303] for a nuclear force obeying a Gaussian law and by Huby[304] for the "meson potential". Both have calculated the higher approximations which take into account the correlations between individual nucleons and find that these higher terms are by no means small and severely alter the magnitude of the total binding energy. This tends to prove that, while the potential energy of the particle does not depend much on its position relative to the centre of the whole nucleus, it does depend decisively on the position relative to the neighbouring nucleons. It is in the nature of exchange forces that this kind of correlation becomes particularly strong since each nucleon tends to be coupled strongly with only three others.

Now in the last few months we have seen evidence that properties of nuclei could be described very well by means of a "shell model" which would seem to contradict the conclusion about the importance of correlations. Supposing that this evidence is really conclusive, it would mean either that the nuclear forces are not of the kind which are now generally accepted, or that there exists some other way of describing the motion in which correlations are not neglected and in which nevertheless, the energy values can be put in correspondence with the shell model.

I think it is important to face this difficulty and to recognize that with at least the usual assumptions about nuclear forces the uncertainty in the position is not sufficient to make the shell model a good approximation.

For the same reason I am not very happy about the view you take at the end of the second page, in which the capture of a particle into the nucleus proceeds first by way of a stationary state in a smooth potential. In a formal way one can, of course, always consider such states with limited life-time due to the possibility of exchange of energy between the nucleons. I should expect, however, that in the energy region corresponding to the capture of a neutron of few MeV, the life-

[303] H. Euler, 'Über die Art der Wechselwirkung in den schweren Atomkernen', *Z. Phys.* **105** (1937), 553–75.

[304] R. Huby, 'Investigations on the Binding Energy of Heavy Nuclei', *Proc. Roy. Soc. London* A**62**, 62–71 (1949).

time of such a state would be so short that it would not be very helpful in describing nuclear processes. However, at much higher excitations energies it may well be that such states would help to understand the maxima and minima in the excitation curves found, for example by Pollard and his collaborators at Yale.[305]
Kindest regards, from all of us and also to Mrs. Bohr,
Yours sincerely,

R.E. Peierls

[487] Rudolf Peierls to Robert Oppenheimer

Birmingham, 16.10.1949

Dear Oppie,
I am afraid I vanished from Princeton without trace, and without saying again how much I enjoyed the day; I was very conscious of having come at a time when you could have used a day's quiet much better than any visitor, and if I had known before that I could afford a few days in U.S. after the conference, I would have suggested coming later. The Chalk River Conference turned out to be much more fruitful than I expected, and we had an opportunity of saying what ought to be done next and why.[306] Whether this will get us anywhere, I do not know, of course, but the discussion was at least interesting.

I have been very much intrigued by your idea that the nuclear forces (apart from tensor interactions) might, in fact be contact forces, and that the "effective range" might, in fact, be due to damping. There is in this, a mathematical problem, whether damping effect of the kind we know now can result in a finite range, while not, at the same time, also contributing to the $p-, d-, \cdots$ scattering about as much as a "real"

[305] E.C. Pollard (1906–1996), had studied at Cambridge under Chadwick. In 1933 he joined the Yale faculty and later became the first professor of biophysics there. In the late 1940s he worked on exchange forces and he carried out experiments involving the deuteron bombardment of various elements.

[306] Peierls had attended a declassification conference at Chalk River. While on the other side of the Atlantic, he had spent a day at Princeton.

potential of the same effective range. No doubt you will be pursuing this further; I am trying to look into this for my own satisfaction, but I would be very glad to know if you reach a definite conclusion.

I would have written before, but on my return we discovered that our last baby has achondroplasia, which means she is one of those dwarfs with normal bodies and very short arms and legs. We were naturally in great distress, but it makes it a little easier to know that she is likely to reach 4 or $4\frac{1}{2}$ ft. and that people of this type are usually very happy and well-adjusted. We are now trying to find some adult people with this condition, so as to learn better the sort of life for which to educate her; but the condition is quite rare.

Dyson has been here for a week or so; he makes a great difference to the place.

With many thanks again and greetings to Kitty.

Yours sincerely,

R.E. Peierls

[488] Rudolf Peierls to Raymond Priestley

[Birmingham], 9.11.1949
(carbon copy)

Dear Vice-Chancellor,

I believe that the next meeting of the Senate Executive Committee will have before it again the proposal concerning the method of deciding promotions at the efficiency bar.

As no doubt you know, this was discussed by the Faculty of Science Executive in a long and heated debate ending in a close vote in favour of the proposal that the discussion be limited to Professors.

I do not wish to be disloyal to my colleagues in raising again a question on which I voted with the minority, but I have very strong doubts whether the decision was constitutionally possible. The presently accepted constitution of the Faculty of Science provides clearly that questions of salary are to be decided by Faculty Executive which includes the Chairman of Faculty Boards. I do not believe this constitution can be altered by one of the bodies of which Faculty consists, though, of course, Senate can over-rule it. The responsibility, therefore, rests with Senate and is not settled by the decision of Faculty Executive.

Turning now from the legal form to the substance of the programme, it seems to me a retrograde step to exclude non-professorial members of staff from such discussions. For some years we have had in the Faculty of Science the Chairmen of two Boards present and I have never seen any signs of embarrassment resulting from their presence or any contribution from them which was not as constructive or as responsible as from Professors. I believe we have a lot to gain from the presence of men who, while they cannot repeat outside the meeting the arguments that were used, will be able to assure their colleagues that each problem was discussed fairly and on the basis of available facts and without personal prejudice. One hears stories of cases in the past where members of non-professorial staff behaved irresponsibly. I think we must recognise that whatever may have been the quality of our staff twenty years ago we would have failed badly in our duty if this was possible now and in any event if such people were appointed Chairmen of Faculty Boards or in other ways members of Faculty.

In the particular case of the Faculty of Science the proposed constitutional change would make a very illogical position since it would leave promotion to Grade I, the withholding in exceptional cases of routine increments, the temporary re-appointment of members of staff beyond retiring age and similar matters involving delicate personal matters, in the hands of the full Faculty Executive with the sole exception of the increase at the efficiency bar. The only logical step to take if this is passed would be to exclude the Chairman of the Boards also from the discussion of all thee matters, leaving them in practice merely to present the report of their Boards and depart. This would completely wreck the spirit of the present experiment in the Faculty and give offence to the non-professorial staff. I hope Senate Executive will bear these points in mind when making their decision.

Yours sincerely,

R.E. Peierls

[489] Ed Salpeter to Rudolf Peierls

Ithaca, 4.12.1949

Dear Peierls,
A. Physics

(i) "Rochester Star". You've probably heard of Brodt and Peter's famous star by now, but just in case you haven't, here it is: In a star with 18 heavy-fragment prongs (Ag or Br nucleus), 23 minimum-ionisn. particles were emitted in a cone making $\sim 2.5°$ (total projected angular spread) with forward direction and 33 at larger angles. In 1/4 " radiation length of glass & emulsion 15 to 20 more charged p[articles] were produced in this forward cone. (These are presumably $e^+ - e^0$ pairs created by the γ-rays given off by the decay of the neutral mesons. $\therefore$ most probable number of neutral mesons ~ 20). Av[erage] energy of these neutral mesons estimated (from spread of the pairs, I think?!) to be 10BeV each. The conclusion is (a) Must be multiple production in a single act (at least the forward cone), (b) evidence for neutral mesons and either (c) Meson production in C.G. systems is isotropic, but only fraction of energy converted into mesons or (d) Most of energy converted, but mesons produced in cone ($\sim \pi/10$) around the direction of each of the nucleons.

(ii) Electron Sprays: Exp[erimen]ts of Oppenheimer & Ney[307] (*Phys. Rev.* 76, 1418 (1949)) have been separated and confirmed: Up to 50 particles of mass $< 10m_e$ and kinetic energies not much greater than their rest mass (since almost all absorbed in one radiation length of Pb without starting showers) have been observed. According to one report these sprays have been so frequent, that every second primary at 100.000ft must create one!! Noone here has an explanation as yet (if the exp[eriments] are really correct).

[307] F. Oppenheimer and E.P. Ney, 'Wide Angle Sprays of Minimum Ionization Particles', *Phys. Rev.* **76** 1418–1419 (1949).

(iii) I don't know of any spectacular results I can report on but here are some topics on which people are working: (a) Multiple meson production (Princeton Inst. Rochester). Attempts to modernise the 1949 paper of Lewis, Oppenheimer and Wouthuysen[308] and rescue what can be secured of it. Interest (mainly scepticism) in Heisenberg's view (Z.P. 126, 569, (1949)[309] that mesonic interaction at high energies is so strong that methods like L.O. & W's (which assume "statistical independence" of meson emission) are entirely unjustified.

(b) Investigations for all types of meson interactions which nucleon-mesonic field interactions, give finite results with mass- and charge renormalisation alone — which can be rescued by a finite number of additional renormalisations (i.e. adding infinite quadruple moment coupling terms in original Hamiltonian etc.) (Princeton Inst., Cornell, etc.)

(c) Difficulties of finite cut-offs (Pais & Uhlenbeck, Feynman[310]): With finite order diff[erential] equations on P[ais] & U[hlenbeck]]s theory (equiv[alent] to discrete values of masses of auxiliary "photons" on F[eynman's] theory) the theories seem to be mathematically self-consistent but lead to predictions which are physical nonsense (emission of negative-energy auxiliary particles, etc.) On F[eynman's] theory of continuous distribution of auxiliary masses it seems possible to make prob[abilities] of emission of these negative energy p[artic]les zero. (F[eynman]'s dodge is to calculate the excitation of a far-away atom, say, by means of these auxiliary p[artic]les — instead of calculating their emission pr[obability] in the orthodox way — so that he adds amplitudes and not pr[obabilities] and the contributions of the different masses cancel.) But in this case the theory is self-inconsistent, e.g. the

[308]H. W. Lewis, J. R. Oppenheimer, and S. A. Wouthuysen, 'The Multiple Production of Mesons', *Phys. Rev.* **73**, 127–140 (1948).

[309]W. Heisenberg, 'Über die Entstehung von Mesonen in Vielfachprozessen', *Z. Phys.* **126**, 569–82 (1949).

[310]See A. Pais and G.E, Uhlenbeck 'On Field Theories with Non-Localized Action', *Phys. Rev.* **79**, 145–65 (1950).

life-time of an excited atomic state (imaginary part of Lamp-shift) does not tally with emission pr[obability] of real photons (as calculated by the effect of a far-away atom). On P[ais] and U[hlenbeck]'s theory with infinite number of orders the positrons is more obscure, but seems to be similar.

B: Gossip:
Believe it or not, I have tracked Tony Skyrme down to earth! In fact I spent Thanksgiving with the Skyrmes. They live in one of the Institute Housing Project apartments, own a car (ancient, but sturdy); still have their English accents without any trace of contamination and generally maintain a little oasis of England. Tony seems to prefer the Institute to MIT and is working on some rather abstract topics of field theory.

The Princeton Institute seems to have more Theoreticians this year than ever before — the only ones who haven't arrived yet are Jost & Pauli (and they're probably here now). There have been a few changes at Cornell — the only other addition to the Theor. Dept. is Fritz Rohrlich[311], an Austro-Israelien (ex-Jerusalem, ex-Harvard, ex-Princeton).

I haven't seen very much of the Bethes or Wilsons yet, so I can't report any news there, but they seem to be fine and send their regards.

I am sharing an apartment with Darcy Walker at the moment (and have managed to fill it with junk). I have little to report about myself except that I, quite inexplicably, have lost my giant appetite on arrival in this country and eat no more than an average American (in my present form I just can't touch Bethe).
With best regards to you, the Peierls household and the Dept.

Ed Salpeter

P.S. I am enclosing a cheque for phone calls, etc. which I owe the Dept.

[311] Fritz Rohrlich (1921–) who had previously been at Harvard, joined the Institute of Advanced Studies in 1949; he later went to the University of Iowa (1953–1963) and Syracuse University (1963–1991).

[490] Rudolf Peierls to Niels Bohr

Birmingham, 7.12.1949
(carbon copy)

Dear Uncle Nick,
I have made provisional arrangements to fly to Copenhagen on the morning of 2nd January (there is no suitable plane on the 1st) and if this arrangement is still convenient to you I shall arrive at Copenhagen about 3.30 p.m. I shall be needed again in Birmingham on the 9th January and will probably have to leave on the 8th.

It may help if I put down a few points that I did not have time to explain adequately either here or at Edinburgh.[312]

It seems to me that the contents of the paper as at present drafted are largely, if not entirely, independent of the model one makes of the nucleus, though the values one would tend to guess for the various constants occuring in the equations do, of course, depend very much on the model. In the past there has been a tendency to confuse these matters, i.e. to identify the model that you first proposed of the nucleus, with the mathematical formalism developed to investigate this model, which, however, is far more general. For this reason I entirely agree that it would be desirable in the introduction to explain this and to say also that one should now have an open mind about the model and that the experimental facts about "magic numbers" and the success of the theory of Jensen[313] and Goeppert-Meyer[314] are vital pieces of evidence.

However, the question of what exactly one must conclude from these things is, to my mind, essentially unsolved. In earlier correspondence I insisted that it was not correct to regard each particle as moving in the average field of the others, if our present view about these forces were anything like correct. This, however, does not prove that one cannot

[312]Bohr had given a series of lectures at Edinburgh, and on this occasion, he had met Peierls and visited him in Birmingham. See letter [486].

[313]O. Haxel, J.H.D. Jensen and H.E. Suess, 'On the Magic Numbers in Nuclear Structure', *Phys. Rev.* **75**, 1766 (1949).

[314]Maria G. Mayer, 'On Closed Shells in Nuclei. II', *Phys. Rev.* **75**, 1969-70, (1949).

get a shell structure out of the present forces. It might be possible, at least in the case of a single nucleon outside a closed shell, to find a picture describing it as a particle moving in a suitable field of force which would, however, not be the average potential of the others. The situation is reminiscent of that in field theory, where one gets large errors (and indeed infinities) if one regards the disturbance caused by the electron in the field as small. We are now learning how to take into account the disturbance which inevitably accompanies an electron and in some sense this is the meaning of "renormalization". One might hope in the nucleus equally to think of the momentum of a nucleon in an otherwise saturated nuclear fluid, taking into account the disturbance it will locally cause in it, and this might lead to a reasonable one-body picture.

I am, therefore, not sure that there is enough evidence on which you say we must abandon our present picture of the forces, but equally it is not certain that we can retain this picture and, while I am most anxious to discuss these problems with you and see what progress one can make, I feel that for the present paper it would be wiser to admit the existence of unsolved problems than to attempt a complete answer in this context.

Yours very sincerely,

R.E. Peierls

We greatly enjoyed your brief visit. I am looking forward to the days in Copenhagen, but please say quite frankly, whether this is still convenient. If you would rather leave it until you pass through this country, this would be very nice for me, too.

[491] Rudolf Peierls to James Chadwick

[Birmingham], 8.12.1949
(carbon copy)

Dear Chadwick,
I believe I have not yet written to thank you for your second letter on the problem of Oliphant's successor.[315] For various complicated reasons our Committee did not, in fact, meet until this week and I was glad to be able to put your views before them.

We have not made very much progress yet beyond ruling out a number of names on the grounds either that they were no[t] suitable or that it was known that they would not accept. We have, in particular, ascertained that Dee could not be persuaded to change his mind. We are now left with the following names as possibilities: (1) Moon; I have explained the position as regards him already. (2) Devons[316]; this was one of the names you mentioned and it was also mentioned by Cockroft. The Committee felt that before ruling out Devons as a candidate for the Poynting Chair, they would want to be quite clear about his merits. They might face the possibility of offering him the appointment in preference of Moon if there was a really strong case for doing so. This means judging his merits as a physicists as regards past performance and future promise and his suitability from the point of view of administration and teaching. I gather that his reputation is of being somewhat intolerant with people and of expecting too much from students, but I do not know how seriously one ought to take this. I would expect, personally, that to appoint Devons would mean loosing Moon and apart from my personal regret at such a step one would want

[315] Mark Oliphant was planning to take up an appointment as first director of the Australian National University Research School of Physical Sciences in Canberra in 1950.

[316] Samuel Devons (1914–), had studied at Cambridge where, after war work as senior scientific officer for the Air Ministry, he became a lecturer after the war. He later became Professor of Physics at Imperial College (1950–1955), before taking up an appointment as Langworthy Professor of Physics at Manchester. He later became Professor of Physics at Columbia University (1966–1984).

to be sure that in the end we are gaining by the transaction. (3) Powell; this name has not previously been mentioned, partly because we always took it for granted that the appointment should bear some relation to the operation of the big machines. However, it may be possible to delegate the day-to-day supervision of the machines to some of the first-rate people who are now with Oliphant on this job, subject, of course, to a supervision on policy and overall responsibility in the hands of one or both of the Professors. On the experiments done on the machines as opposed to their operation, one would feel that the proton synchroton should, in fact, come into very close contact with Powell's work. I believe it may be that Powell would, in fact, prefer, if he accepted at all, to hold the second chair, but it might still be the correct course to offer him the more senior job and let him choose.

I am sorry to trouble you again over this, but you will appreciate that this is a most difficult problem for us and it would be most helpful if you could let us have your comments on these three possibilities.

We have not yet considered the problem seriously of who would be appointed to the Second Chair, if Moon were to be Poynting Professor, but (unless Powell would be interested in that position) an interesting suggestion would be Pontecorvo, but this is by no means clear and need not be settled at this stage.

Yours sincerely,

R.E. Peierls

[492] Rudolf Peierls to Ed Salpeter

[Birmingham], 26.1.1950
(carbon copy)

Dear Salpeter,

Thank you very much for your long and most welcome letter and the various bits of information in it. I have no particular comments on them, except that I have now understood why the results of Pais[317] seem to

[317] Abram Pais was working on field theories with non-localised action. See letters Peierls-Pais, *Peierls Papers*, Ms.Eng.misc.b212, C.230.

be rather different from ours, in particular, Irving's.[318] The answer is that the work of Pais is based on the theory by Bopp[319] which leads to equations for a free electron in which the integral goes over the past but not the future motion. Such theories are not equivalent to McManus's form of the equation.[320] They arise only if the field allows a photon of finite mass as well as zero mass and I believe it follows from Pais's arguments that to get the self energy finite, one must then assume negative energy densities for these auxiliary masses. McManus's theory leads to integrals going over past and future and can always been arranged so as to give a finite energy but Irving has demonstrated the danger of runaway solutions. We have not yet found a form function which avoids this and it may well not exist. Whether an oscillatory runaway solution in the classical theory is necessarily fatal is, of course, another question. We have made no progress about quantisation. Rumour has it that Feynman can quantize equations having only a Lagrangian and not a Hamiltonian. I wonder whether this rumour is correct, or whether he can do this perhaps only for the Wheeler-Feynman formalism which is special in the sense that one has started from a differential theory of two fields and then eliminated one of the fields.

Other current work: we are having a beautiful course of lectures from Dyson which, amongst other things, help to show how little field theory we knew before, but a few people are now beginning to learn how to use it.

Of these Dalitz has completed his calculation on the corrections to angular distribution of pairs ejected by a 0-0 nuclear transition as in oxygen. The correction is several percent but it varies rather slowly with angle and it is not easy to observe experimentally. It is of the wrong sign as well as too small for explaining results which Devons appeared to find, which, however, in any case, were still uncertain.

Ravenhall found the problem about complex eigenvalues in Feynman theory too vague and in view of what your letter says in this context I see

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[318] J. Irving, 'Applications of the Peierls-McManus Classical Finite Electron', *Proc. Phys. Soc. London* A**62**, 780–790 (1949).

[319] F. Bopp, 'Eine lineare Theorie des Elektrons', *Ann. Phys.* **38**, 345–84 (1940).

[320] See letter [396], note 80.

now that the problem is much deeper than I had thought. He has just completed a calculation of the production of pairs by electrons which had never been done decently and which is of interest in connection with the Bristol experiments.[321] He is now going on to look at the problem of bound states in field theory.

Gunther got here finally after waiting two months for his British visa and is still living mostly on air and he is now waiting for his money from Poland. He is making rather heavy weather of the derivation of the Breit terms from field theory (so far only the order e^2, nothing to do with Lamb shift etc.). He is tying to do it by means of a $4n$ dimensional configuration space. I thought at first that this would lead to unsurmountable trouble. He has convinced me that it can be done but not yet that there is an advantage in doing it.

Moorhouse has shelved field theory for the time being and has worked out the theory of scattering neutrons by ferromagnetism. It appears that one can give a very direct and instructive interpretation to such experiments which will not teach us much about neutrons, but may throw light on models of ferromagnetism.

Barker has sent his paper on the Schwinger model off for publication[322] and is now worrying about nuclear models, investigating, in particular, a two-particle problem which may give a lead to estimating the extent to which the existence of shells may be compatible with the general Bohr picture.

Wroe has turned over to cosmology and the origin of the elements. It now looks as if the general idea of Teller and Meyer[323] can be rescued assuming that the universe was at one time so small that it was completely filled with matter and nuclear density and low temperature and then expanded. This leads to condensation more or less as in the cloud chamber and the rest proceeds as in Teller's picture. There are,

[321] R.H. Dalitz, and D.G. Ravenhall, 'On the Tomonaga method for intermediate coupling in meson field theory', *Phil. Mag.* **42**, 7 (1951).

[322] F.C. Barker, 'Schwinger Potential in Nuclear Forces', *Proc. Phys. Soc.* A**63**, 898–909 (1950).

[323] Maria G. Mayer and E. Teller, 'On the Origins of the Elements', *Phys. Rev.* **76**, 1226–31 (1949).

of course, very many complications to be allowed for and one cannot yet be sure of the answer.[324]

Butler extended Wroe's work on the three-body problem and set out a programme of carrying out the necessary integrations numerically. This is not prohibitive but too long for Butler to do himself and we have shelved this until we can find a suitable computer.

Lindhard, whom you don't know, has settled down very well in the department. He is interested in the degree of ionization of nuclear fragments and in nuclear models. I am also working with him and van Wieringen on an old problem in the theory of metals in which, after fifteen years, we have now made one step forward, but seem to be unable to take the remaining step necessary to get an answer.

The synchrotron is getting along well but, of course, it is much too early to make any predictions. The cyclotron is now behaving well at a D voltage, somewhat below the design figure and hence giving a beam that does not quite get to the edge. It has now been decided to reduce the magnetic field a little and thus to get certainty of a particle beam at a slightly lower voltage rather than hopes of a hypothetical beam at the design energy.

You are not the only ex-member of the department who writes letters. Several people heard from Skyrme and we had a letter from Rzewuski saying that he got married recently. Gardner is settling down at Harwell but largely is continuing the kind of problem he was working on here.

I forgot to mention Swiatecki. He is still engaged in mopping up operations resulting from his work on nuclear surface tension.[325]

We are at last getting round to issuing a list of papers of which reprints are available. I do not know whether your reprints will be sent out from Cornell, but if you like us to put your papers on the list could you let me know more or less by return as I would like to get the list out within the next week or two. In that event you should either mail

[324]The results were later published as R.E. Peierls, K.S. Singwi and D. Wroe, 'The Polyneutron Theory of the Origin of the Elements', *Phys. Rev.* **87**, 46–50 (1952).

[325]See W.J. Swiatecki, 'Density distribution inside nuclei and nuclear shell structure', *Proc. Phys. Soc.* A**63**, 1208–18 (1950); and W.J. Swiatecki, 'Nuclear Compressibility and Fission', *Phys. Rev.* **83**, 178–9 (1951).

us a supply of reprints or else we should send you from time to time a list of names of people who have asked for your papers. If I do not hear from you I shall assume that you wish to stand on your own feet in this matter.
Yours sincerely,

R.E. Peierls

[493] Genia Peierls to Klaus Fuchs

Birmingham, [date unspecified][326]
(carbon copy)

Dear Klaus,
Rudi just came home from London, and I am writing to you in front of our sitting room fire, where we so often talked about so many things. This is a hard letter to write, perhaps even a harder one to read, but you know me well enough not to expect me to mince my words.

I am taking it all much easier than everybody else, because my Russian childhood and youth taught me not to trust anybody and to expect anyone and everyone to be a communist agent. Twenty years of freedom in England softened me somewhat, and I learned to like and trust people, or at any rate some of them. But early attitudes are deeper and after the first half hour I feel I can take it. I certainly did trust you. Even more, I considered you the most decent man I knew. I do that even now. This is the reason why I am writing to you.

I understand that you have now changed you views and want the best of our civilisation to go on. The best is trust in human beings, friendship, this bit of freedom and fresh air which is still lingering here and there in the world and makes life worth living, and bringing up children a joy. Your actions have tremendously endangered just these things. And this in two ways: one is directly and as was your intention, and one cannot do much about that now. But one can and one must do something about the other.

[326]The letter was probably written on the 4th of February 1950, the day that Rudolf Peierls visited Klaus Fuchs in prison.

Do you realize what will be the effect of your trial on scientists here and in America? Specially in America where many of them are in difficulty already? Do you realize that they will be suspected not only by officials but by their own friends, because if you could why not they?

For your "cause" you did not <u>have</u> to be on such warm personal relations with them, to play with their children and laugh and drink and talk. You are such a quiet man that you could have kept yourself much more aloof. You were enjoying the best of the world you were trying to destroy. It was not honest.

In a way I am glad that you failed in this, because [???] you the value of humanity, of warmth of freedom, what did you do to them, Klaus? Not only that their faith in decency and humanity is shaken, but for years to come they will be suspected to be involved in this with you. Perhaps you did not think about it at the time, but you must think now.

[???] to say who were your real connections. It is awfully hard, perhaps the hardest thing of all to do. But you went all the way in one direction, don't stop half way now. You are not soft, and not one for an easy way out. You are a mathematician. This problem has no rigorous solution. Try to find the best approximation.

Rudi told me that you don't want to give the impression that you want to ease your own position. Klaus, don't be a child. This is the schoolboy code of honour. Impressions don't matter. You personally do not matter. The issues are too important for that, and you know it, otherwise you would have taken the only easy way out for you personally — to take your life. Thank you for not doing that. You could not leave all this terrible mess for others to sort out. This is your job, Klaus. And you know you never shirked.

No even the washing up!!

Oh Klaus, my tears are washing away the ink. I was so very fond of you, and I so much wanted you to be happy, and now you never will be.

I still think that you are an honest man, it means that you do what you think right, whatever the cost. Do the right thing. Try and save as much as you can of this decent and warm and tolerant, this free community of international science which gave you so much these last ten years.

This letter is just a sea of ink, I am asking Rudi to copy it. Would you like me to come to see you? You are now going through the hardest time a man can go through, you have burned your god.
God help you!

[Genia]

[494] G.I. Taylor[327] to Rudolf Peierls

Cambridge, 5.2.1950

Dear Peierls,
Like everyone I have come across who knows Fuchs I was shocked and astounded when I heard what has happened. I cannot imagine that our police would have acted unless they had overwhelming evidence because they must have understood how terribly bad their action would be for Anglo-American co-operation and even then I cannot understand why they did not get Fuchs to resign quietly.

Then the thing is quite outside anything I could imagine. It is just impossible to imagine anyone person acting as the police allege Fuchs did. But Frisch suggested to me the only possibility that I have heard that has any chance of being conceivable. He tells me that Fuchs came over from Nazi Germany early as a refugee and that he was interned at the beginning of the war and was in a group that was so badly treated that there was in fact an enquiry into the matter. Frisch suggested Fuchs might have an insane resentment and has for years been stiring up a punishment which has now been let loose. If this is indeed the case, it is indeed an extraordinary case for resentment against bad individuals has been turned into a desire to punish all people who have since treated him extremely kindly. People like Cockroft & you and me for instance, as well as the whole body of the scientific workers. Of course one looks

[327] Geoffrey Ingram Taylor, (1986–1975), studied mathematics and physics at Trinity College, Cambridge, where he became a fellow, lecturer and later professor of physics. Member of the British mission at Los Alamos.

against all reason that a mistake has been made, but I feel that's the least probable of the possibilities. I write to you because you know F[uchs] better than I do + you may have some light to shed on the psychological aspects of the question. Don't answer if you feel you can't. I wrote because I feel I am one of a small group who have been dealt a shattering blow and that some comfort may come from a feeling of solidarity with other members of that group.
Yours sincerely,

G.I. Taylor

[495] Klaus Fuchs to Genia Peierls

London, 6.2.[1950]

Dear Genia,
It was wonderful of Rudi to visit me on Saturday, although I could not do anything to cheer him up. On the contrary. It is up to you.

Do you mind if I talk of other things? Sometime I shall try and describe to you what went on in my mind. But you will have to be very patient. I have been sitting here for an hour, trying to think what to write next, when your letter arrived. I have told myself almost every word you say, but it is good that you should say it again. I know what I have done to them and this is why I am here. You ask: Perhaps you did not think about it at the time. Genia, I didn't, and this is the greatest horror I had to face when I looked at myself. You don't know what I had done to my own mind; and I thought I knew what I was doing. And there was this simple thing, obvious to the simplest decent creature, and I didn't think of it.

I had used my god to make myself into this, and that was the point where it finally crashed down. Controlled schizophrenia is the nearest description I can give to it; but I didn't control the control; it controlled me.

I know that it is my job to try and clear up the mess I made. I am afraid I did shirk it at first, and that made the mess even worse. They gave me a much easier way out. I could have left Harwell to go to a university a free man, free from everything, free from friends, with no faith left to start a new life. I could even have stayed at Harwell if I had admitted just one little thing and had stayed quiet about everything else. I bungled the "take your life" stage; yes I went through that too, before the arrest. The elaborate precautions taken after my arrest, I am glad to say, were quite unnecessary, though a trifle inconvenient. I was only afraid they would discover the safety pins which held my pants together. In that case my appearance in court the following morning might have been somewhat undignified.

I suppose you would almost enjoy the kind of things I am learning about here. All these people [are] in their way kind and decent. Even the chap who apparently made prison his home by occasional excursions to pick up a few hundred pounds and have a few riotous weeks on them. He grew quite sympathetic when I admitted that I hadn't made any money out of it. Nothing could shake him from the belief that I had been double-crossed.

Many many thanks for your letter. Funny that women see such thin[g]s so much clearer than men. And that they are so much kinder by saying hard words straight out.

Klaus

Sorry I have not got anybody to type this out for me. I hope you can read it. And don't worry if you can't see the tears, I have learned to cry again. And to love again.

K[laus]

[496] Rudolf Peierls to G.I. Taylor

Birmingham, 7.2.1950
(carbon copy)

Dear Taylor,
Your letter reached me only now, as it went to an old address, owing to an error in the Royal Society Year Book.

I very much appreciate your writing to me at once, and I entirely agree with all you say about the consequences this is likely to have. You can imagine that this came as an even greater shock to us than to anybody else, after ten years of the closest personal and scientific association with Fuchs.

At the present moment I cannot say very much for reasons that you will appreciate, but I can say that I do not think the picture Frisch has given you is at all likely to be accurate. One must still keep an open mind, as long as the facts are not clear, and they are certainly not clear to me. But assuming the facts to be as alleged, the only explanation is that for him his political views took, as these particular views do so often, the form of almost religious convictions and psychologically the situation would be rather like that of a Jesuit, who may feel free to act against the ordinary standards of morality in a higher cause. I also have reason to think that, always the facts are as alleged — which I do not yet accept — he must have gone through a process of development in which he abandoned these views but after the date of what is said to have happened. I felt I should tell you this at once because, while it would not for a moment excuse the action, it would at least raise it to a somewhat higher level than the kind of personal grudge that you speak of.

However, I have put all this in such a conditional form for a definite reason, and I can yet see a possibility of an explanation which would be less distressing. This may be wishful thinking, and I cannot know until I have more of the evidence than I can obtain at the present. If this should be true, it would become clear quite soon, and meanwhile one has to be patient.

Your sincerely,

R.E. Peierls

On re-reading the last paragraph, I find it sounds more definitive than I intended. It is a very long shot.

[497] Rudolf Peierls to Niels Bohr

[Birmingham], 14.2.1950
(carbon copy)

Dear Uncle Nick,
I feel I should write to you about Fuchs, though I have really very little to say that you do not know already. This has come as a distressing blow to very many people and I am sure that it must also have caused you terrible distress. I do not know whether from the American papers it was possible to sort out fact from imagination, but you will have seen a report of the Police Court Proceedings last week in which statements by Fuchs himself were described. If one takes these statements as genuine, and it is very hard to believe anything else, he has lived all these years hiding his real allegiance, yet at the same time acquiring a genuine and almost passionate interest for his job and building up personal relationships and friendships which were kept quite separate from his secret contacts. One can believe that a man should hold political views of such strong, almost religious, conviction that he should let them override all other considerations, but it is incredible that, at the same time, a man who had never thought for himself and who was always ready to go to enormous lengths in the interest of others, should allow himself to become so attached to the people and to allow other people to become so attached to him without seeing what he was doing to them.

According to the statements quoted in court, this was really what broke him in the end, and because it was the trust and confidence shown him by his friends which convinced him in the end that there was something wrong with the cause, but it was, of course, then too late to undo the damage.

The whole picture is so unbelievable that we continue to ask ourselves whether he has really done all the things to which he is reported to have confessed, or whether some, or all of them, are perhaps hallucinations created by a very great mental strain. It is impossible for us to judge this because the answer depends, of course, on what other evidence exists besides his own statements and quite properly the authorities will not tell us whether such evidence exists and how strong it is, but until then I know I shall continue to regard this at least as a possibility.

There is no doubt that this whole case will have disastrous effects, quite apart from personal relations on the political atmosphere and the positions of scientists both here and particularly in America. It is, of course, quite illogical if all security clearance and investigations have missed such a case to seek a remedy in submitting people to further checks and clearances. Nevertheless this will, of course, be done. We are beginning to wonder whether the real lesson is not that it is impossible to maintain secrecy in a project involving so many people without creating the atmosphere of a totalitarian country in which everybody is ready to suspect his best friend of being an informer. Russia has found how to stop leakages very effectively. If this is the only effective solution do we want to go that way ourselves or should we not say that at that price security is not worth having.

With kindest regards,
Yours sincerely,

R.E. Peierls

[498] Rudolf Peierls to E.C. Bullard[328]

[Birmingham], 15.4.1950
(carbon copy)

Dear Bullard,
As you may know, I had some correspondence with Womersley[329] about the possibility of employing a first-rate man who is graduating this summer and there is a point of principle arising from this which I would like to put before you.

[328] Edward Crisp Bullard (1907–1980), studied physics at Cambridge under Blackett, and later turned to geophysics. In 1949 he became director of the National Physical Laboratory, Teddington Middx.

[329] John R. Womersley (1907–1958), Superintendent of the Mathematics Division of the National Physical Laboratory.

It emerges that the only thing this man could do would be to apply to the Civil Service Commission for the general entry to the Scientific Civil Service though he could state a preference for the N[ational] P[hysical] L[aboratory] but I gather from Womersley that it is not possible to conduct direct negotiations about the type of work he might be taken in for.

I have often felt from previous experience that the centralised method of recruiting for scientific work was bad. It is, of course, modelled on the procedure invented for the administrative class where presumably a man decides in the first place that he wants to become a Civil Servant and does not mind very much, even though he may have some preference, whether he collects taxes or issues building permits. In the case of scientists I find the best people are usually those who have some concrete ideas as to what they would like to do although they may later change their ideas, and I have never come across a case which makes it as obvious as the present one to what extent the present system discourages applications from such people.

In writing to you I do not want in the least to press the particular case since the man was somewhat doubtful about it anyway. I had offered him a place as a research student and he is quite keen to stay on except that his age gives him some reason not to prolong his training excessively. I was making enquiries about possible jobs mainly to help him make up his mind whether he wants to stay on here and it is likely that he may stay on anyway.

I thought, however, that the particulars of this case might interest you and in case your views about the Civil Service recruiting are similar to mine it might provide useful ammunition.

Yours sincerely,

R.E. Peierls

[499] Egon Orowan to Rudolf Peierls

Cambridge, Mass., 20.4.1950

Dear Peierlses,
You would not believe how often, in certain situations, the question occurs to me: How would you expect me to behave in this (mess, quandary, tight spot, problem, and the rest of Roget's). I am afraid, however, just at the moment, there is no real problem on the table for you; so this is just a sign that I am thinking of you not only when strictly utilitarian reasons demand it. Also, I should like to thank you for sending me to Weisskopf; he was very useful indeed. He said: 1) he would not go from MIT to Princeton if he were offered an equivalent job; 2) Cambridge Mass. is far better than Princeton; 3) apart from Bethe and Wigner, he could not tell the name of another physicist whose salary exceeds 10,000. This is his own salary, too.

With the MIT, the main problem is merely to discover the snags and the flies in the ointment. So far, the whole place appears so improbably nice that it seems to me the less good spots are reserved for later discovery. As a university and research place, it seems Paradise after Cambridge – Eng. The people are incredibly nice; partly it is the attitude of the couple before the marriage ceremony which cools down when they leave the church. The surroundings are pretty, one can recognise it even now when the trees hardly begin to bud.

As you see, therefore, there is a definite recession with Canberra and Princeton. However, I have not arrived at the zero line yet, and I have duly sent to Hugh S. Taylor a project he asked from me about the new group for applied physics of solids (or materials engineering) that they want to set up at Princeton.

Having penetrated into the skin of American life, it seems to me far more attractive than it appeared from the distance. Of course, this place is a bit highbrow and too full of intelligentsia as far as I am concerned, but I can easily learn a few words like repression, existentialism etc. to fit into the picture.

I hope you are well; since you are very busy, I do not suggest that perhaps once one of you might take pity of a poor exile to the extent of a few lines.

Between this line and the previous one I had a visitor belonging to the wire-pulling class who broached pointedly the question of the Harvard metallurgy department which they want to re-open when they find somebody to put in. At the same time, he was so positive that you cannot live in the U.S. a life better than a dog's with less than 20,2000 or 30,000 p.a., that I am beginning to think Canberra is better.
With the kindest regards to you all,
Yours ever,

E. Orowan

[500] Memorandum Rudolf Peierls
The Lesson of the Fuchs Case

[Birmingham, around March 1950]

To all those who were associated with Dr. Fuchs during his work on the atomic energy project the disclosures at his trial have caused great distress. One could wish nothing less than to go on talking about this, particularly in public. However, the case will do such serious harm and there seems to be so much contradiction and confusion about it that I feel it necessary to write up the picture as I see it. The main point will be the conclusions to draw (or not to draw) and I shall describe the past events only as far as they have a bearing on this.

For me the story starts in 1941 when a small team was then working on atomic energy in this country. I was mainly responsible for theoretical physics and more help was needed on this side. Most people of suitable ability were then already on high priority work but when I heard that Fuchs was available I knew he was a man of the right scientific qualifications. I knew he had left Germany because of his opposition to the Nazis and I respected him for this. I knew of his connection with left-wing student organisations in Germany since at that time the communist controlled organisations were the only ones putting up any active opposition. It was natural for a young man who wanted to fight the Nazis to work with any available allies, as indeed this country did later during the war.

Approval for his appointment had, of course, to be given by the authorities. I do not know what their methods of investigations were, and what was disclosed, but I assumed that they had to weigh the value of his help (at a time of great shortage of scientific manpower) against any risk of his having retained from his early contacts in Germany (8 years earlier) a loyalty to a party that owed allegiance to a foreign power.

During all these years we saw much of him. Shy and retiring at first he made many friends and in many conversations politics was, of course, a frequent topic. His views seemed perhaps a little to the left of ours, but he seemed to share the attitude to Communism — and to any kind of dictatorship — of most of his friends. I remember an occasion when he talked to a young man who was in sympathy with communism and in the argument Fuchs was very scornful of the other's dogmatic views.

When I heard of his arrest I regarded it as quite incredible that anyone should have hidden his real beliefs so well. Looking back it seems that at first he shared in the life of his colleagues and pretended to share their views and attitude only in order to hide his own convictions. But gradually he must have come to believe what was at first only pretence.

There must have been a time when he shared one attitude with his colleagues and friends and another with the agents to whom he then still transmitted information, and when he was himself in doubt which of the two was conviction and which was pretence.

I do not want to enter into speculations about the state of his mind during all this time. Some have described it as an abnormal case of a split personality, others tend to regard it as a superb piece of acting, but either way it is certainly quite exceptional.

In the past his close friends were mostly amongst people who shared his extreme views. Of course, the case for the democratic way of life must have been made to him also by many people who felt a genuine conviction for it, but apparently this had not converted him. The years spent here and in America on the project brought him more and closer association with new friends, and it is one of the most unusual features of his case that a man who was not selfish should, in spite of his position, allow these close associations to form on false pretences. But as a result there was something new that grew on him. Nobody ever argued the case because nobody knew that he needed convincing, but he discovered that implicitly all shared principles which gave him a strength that his ideals were losing.

From his point of view this is perhaps the most tragic: that he does not now even have the satisfaction of suffering for a cause in which he believes. But it contains a slight piece of comfort: the story has shown up a weakness in the defence of democratic countries beca[u]se the atmosphere of mutual confidence that is so essential a part of our life, makes this kind of betrayal harder to guard against. Yet, it also shows the strength of our system which in time won over such a strong supporter of a different ideology, though, in his case, only too late. Our problem must be how to reduce the risk of further cases of this kind, while yet preserving those features that make us so sure (and that ultimately convinced Fuchs) that we are right.

How, then, can we avoid further leakages: As an ordinary mortal I do not presume to know the methods of the security services but broadly speaking they can work in three ways; by "counter-espionage", i.e. by infiltrating into the espionage organisation which they are trying to frustrate, by "clearance", i.e. by investigating the background of people employed or to be employed on secret work and by "supervision" of the conduct of the men on the job.

The first is obviously a good method if practicable, but one would not imagine it to be a complete safeguard in itself.

"Clearance" investigations are, of course, employed in connection with secret work. In the case of Fuchs, they would have had to probe very deeply to disclose his continued adherence to the communist cause and that would have required a depth of human insight that is very hard to achieve. Anything that could be done to raise the level of knowledge in this way would, of course, be most valuable. But, in any case, such investigations would presumably have shown that he had been a member of a left-wing organisation in his youth. Should we now exclude others of whom this is found? Fuchs was German born; should one now all be suspicious of foreign born people? Fuchs was a scientist; should one mistrust all scientists? Should one mistrust all men with the initial F?

The Fuchs case came as such a shock to the public that I would not blame anyone for advocating all these measures, except perhaps the last one. But we must not be under the illusion that they would bring safety. They would not even have prevented the case of Nunn May.[330] But they would have lost the country a great deal of ability. I believe that it is fair to say that if from the atomic energy teams in England and in America one would have excluded all foreign born scientists as well as those who in their youth had held extreme political views of one kind or another, the leakage of atomic energy would have been prevented by the fact that there would have been no atomic secrets. The work could not have continued effectively under such restrictions. This may sound an immodest statement for me, as a foreign born scientists, to make. But a glance at the names in the Smyth Report[331] which summarises atomic energy work in America will make my point obvious. I am not saying that one should take no notice of the background of the people to whom one entrusts secrets. As long as there are any secrets (and all this story increases our longing for a state of the world in which they would not be necessary) it is important to judge who can be trusted with them, but one cannot insist that the precautions should be such that they would necessarily detect a second Fuchs. We are not likely to find a second person who can for years maintain the impression of being a politically inactive but generally liberal and reasonable person. But if there should be further cases of the same kind of psychology (or of equally perfect acting) they may well be people who had never openly professed communism.

Should one then rely more on supervision? The difficulty in the large number of scientists and others on secret work. To "shadow" a person day and night takes more than one investigator. Where would one find the necessary number of intelligent investigators and how does one check their reliability? Probably this method had its best chance in the atomic bomb work in Los Alamos, New Mexico, which was located at the remote spot largely just in order to reduce the risk of leakage. While

[330] Alan Nunn May (1911–2003) had worked at the Chalk River Plant of the Manhattan Project. In 1946 he was sentenced to 10 years; hard labour for spying for passing information on the Manhattan Project in to the Soviet Union.

[331] Henry De Wolf Smyth, *The Official Report on the Development of the Atomic Bomb Under the Auspices of the United States Government*, Princeton: Princeton University Press, 1945.

the gates of this "atom city" were not actually locked, travelling by its members was discouraged and few ever travelled beyond the immediate neighbourhood. We always assumed that on our rare trips we would be watched by the efficient army security services and that this applied particularly to those employed by the British rather than the American authorities.

Yet one of the charges against Fuchs relates to February 1945, a time when he was working at Los Alamos and presumably just absent to attend some meeting or collect some technical information elsewhere. If his secret rendez-vous could pass unnoticed in these circumstances the prospects of generally keeping all people under supervision does not look promising.

If one considers these problems objectively, one sees that as long as there are large projects employing thousands of people we cannot have absolute assurance against leakage except in one way. The governments of totalitarian countries presumably find it easy to keep their secrets, and by adopting their methods we might succeed, too. If we build up an iron curtain preventing travel across the border, except in rare cases, if we suspect people who are talking to a foreigner, if we give the police the right to act on suspicion and, above all, if we build up a state of affairs in which everyone suspects his best friends of being police informers (and half of them probably are) then our military secrets might be safe, but at what price?

If this were really necessary, we would lose most of the assets of democracy including even the pleasure of convincing a man like Fuchs in the end that we are right and he was wrong — because there would not be much difference.

Nobody has yet proposed such drastic measures, but the insistence that one now hears frequently on security measures without specifying them exactly and the very understandable desire for certainty that there will be no further such cases, may logically lead us in that direction.

Must we then choose between helplessly tolerating all foreign agents and becoming a police state? Fortunately things are not as black as that. Of course the authorities will continue to find out what they can about the people entrusted with important secrets and they will make the job of any future Fuchs or Nunn May as difficult as they can; they will not pretend that they are infallible. A good general knows he is bound to lose a battle occasionally.

The details of all military equipment such as tanks and aeroplanes have always been considered as important secrets. Nevertheless, no country ever succeeds to hide their main features indefinitely, but this does not even out the assets. The country with the better technical skill, the greater ability for research and design and the greater industrial potential will still be better off because no leakage can replace the value of the right skill and knowledge of the man actually on the job. The question of the importance of atomic weapons for the future safety of this country and of the United States is a controversial one which I do not want to raise here but accepting their importance more can be gained by assuming a positive need through efficient development work and good planning than by a frustrating attempt to seal up hermetically all possible channels by which others may get to know things which, after all, they might discover for themselves.

One fallacy that would be particularly dangerous in this context is to extend the principle of clearance to cover not merely the employment of men to be entrusted with secret work but to a wide variety of cases which it is argued that people with extreme political views might abuse their position for seditious propaganda. This is dangerous because it would lead to political discrimination and to a restriction of the freedom of expression. It is clear that in certain circumstances the spreading of extreme political opinions might be a danger, but the difference is that propaganda is something that cannot be pursued in secret. If people misuse their position to advocate their own views, this can easily be known and they can be dealt with on the basis of their actions. There is no need to suspect them in advance. In the cases where the job is concerned with non-political matters, in particular, technical information, anybody engaged in political propaganda would, in fact, not be carrying out his duties properly and could be dealt with on that basis. In jobs concerned with the discussion of such problems as international relations of political theory or practice, it is most desirable that all views should be heard and that people should be in a position to make up their minds on a full knowledge of all arguments. This means that it is, in fact, undesirable that people should be prevented from expressing any views however extreme or unpopular, provided one takes care to balance their views by having others available who would speak for the other side. This has always been the tradition of this country and it is important that the danger of disloyal acts which, as the Fuchs case has reminded us, is serious and should not be confused with the danger of extremist propaganda which at the present time is negligible and, in any case, must be fought by argument and not by prohibition.

[501] D.H. Wilkinson to Rudolf Peierls

Cambridge, 12.5.1950

Dear Professor,
I hoped to see you at the A.S.A. Council Meeting last Saturday, but I hope you will not mind being written to instead. I have been doing a series of measurements on the photo-disintegration of the deuteron at various gamma-ray energies between 6 and 18 MeV, and there is one point which I would be pleased if you would clear up. It is whether or not the state of polarisation of the gamma-rays, linear, elliptical, or anything else, has any influence on the photo-disintegration or whether it depends solely on the gamma ray energy when the deuterons are randomly oriented. I pass the gamma-rays which are derived from nuclear reactions and may be very strongly polarized, through an ionisation chamber containing ordinary deuterium gas and observe the total number of disintegrations they produce and also, effectively, the angular distribution of the photo-protons $I(\Theta)$ in the Θ co-ordinate only. Now does the number of disintegrations or $I(\Theta)$ depend on the state of polarisation? It seems instinctively obvious to me that it should not, but I do not know how to write it out properly. People who do calculations never mention this problem, so it is probably pretty trivial, but on the other hand, nobody is prepared to give me a whole-hearted yes or no answer. As you did the early calculations on this problem you can probably tell me straight away, and I would be very grateful, if you would. I measure the gamma-rays through matter and measuring the amount

of ionization they produce, and I presume that here again there is no influence of the state of polarisation?

I would also be pleased if you would let me know your views on the reliability of recent calculations such as those of Bethe and Longmire.[332] What I am most concerned about is the validity of the fundamental formula derived from perturbation theory. Is this effectively exact or are unknown approximations involved? If this formula is exact then the rest follows automatically, and one may use the formula of Bethe and Longmire for determining the effective triplet range. But are there any troubles back in the fundamentals which people never remark on nowadays? If one uses my values of the photo-disintegration cross section one derives a value for the triplet effective range almost the same as the "new" value of 1.71 derived from the liquid mirror slow neutron scattering, though the photodisintegration results are not so accurate as the others.

The results so far are:

E(MeV)	$\sigma(10^{28})$ cm^2
6.11	21.5 ± 1.2
8.45	18.6 ± 1.5
12.4	10.0 ± 1.2
17.6	8.5 ± 1.2

I hope you do not mind these questions, but I would like to be sure about the straight forwardness of the interpretation of the experiments, and that I need not worry about complications of multipolarity and so on.

Yours sincerely,

D.H. Wilkinson

[332] H.A. Bethe, 'Theory of the Effective Range in Nuclear Scattering', *Phys. Rev.* **76**, 38–50 (1949); H.A. Bethe and Conrad Longmire, 'The Effective Range of Nuclear Forces II. Photo-Disintegration of the Deuteron', *Phys. Rev.* **77**, 647–54 (1950).

[502] Rudolf Peierls to John Cockroft

[Birmingham], 15.5.1950
(carbon copy)

Dear Cockroft,
If Pryce does go to Princeton next year I shall certainly do my best to help with the running of the theoretical division at Harwell.[333]

My main difficulty in this, as you will realise, is going to be one of time and I would suggest that a little later it would be good to plan out in what way this would least interfere with running my department here. I assume that I can count on assistance with such matters as petrol supply where the use of a car makes it possible to fit in visits to the Establishment with less dislocation.

As regards the question of fees, I can not really pretend that this extra work would be carried on without loss of efficiency in the performance of my duties to the university and it may well be a reasonable suggestion that part or the whole of any further fee should be passed on to the university.

The whole arrangement is, of course, subject to approval by the university but before consulting them it would be better to know precisely how we are going to arrange my visits.
Yours sincerely,

R.E. Peierls

[333] Maurice Pryce had helped run the theoretical division at Harwell after the arrest of Klaus Fuchs.

[503] Rudolf Peierls to D.H. Wilkinson

[Birmingham], 18.5.1950
(carbon copy)

Dear Wilkinson,
Thank you for your letter.[334] The question of the polarization of the gamma rays will depend on the symmetry of the source from which they are obtained in relation to the direction of observation. For instance, if your gamma rays go in the forward direction as seen by the bombarding beam then the problem has complete axial symmetry and therefore gamma rays will be unpolarised.

Of course, each individual photon will have a polarization correlated with the direction of the fragments and your disintegration but since the direction of these fragments is statistically symmetrical about the direction of the beam, this will make no difference. One must remember in this connection that there is no physical difference in the gamma ray beam according to whether you produce an unpolarized beam by superimposing waves of linear polarization in different directions or waves of opposite circular polarization or intermediate elliptic cases, as long only as one averages over all possible orientations. Therefore, in this case of forward emission of the gamma rays it follows rigorously that polarization is unimportant.

Now, in general, you will be working at different angles and then there may be a greater likelihood of gamma rays being polarized in a direction at right angles to the plane formed by your bombarding beam and the line of travel of the gamma ray then in that plane and vice versa. Even in that case, however, your method of observation would not be sensitive to polarization since you only measure the angle between the proton and the gamma ray and any effect of polarization must disappear if you imagine your chamber rotated about the line of the gamma ray and the results averaged. This clearly would not make any change to your set-up.

[334]Letter [501].

It is an interesting question whether one could conduct the experiment in such a way as to distinguish protons forming the same angle with the gamma ray but orientated differently with respect to the plane of symmetry set up by bombarding beam and gamma ray. If the gamma ray were polarized, one would then expect a strong correlation of the protons with that direction. At low energies this correlation would be something like a $\cos^2$ distribution for the electric dipole effect. At the energies you mention the magnetic effect will be quite negligible but there may be contributions from higher multipoles and things are then a little more complicated, but this will only make the correlation stronger.

All this, of course, depends on how strongly the gamma rays are polarized and this depends on the reaction in which they are emitted. To take an example, if the reaction is such that bombarding a nucleus with no spin the proton comes in with angular moment 1 and the final state of the nucleus is again without spin, then the angular momentum has to be taken over by the emitted gamma ray which for observation at right angles would give complete polarization. This is an extreme case and unlikely to be realised in practice. Another simple case about which a simple statement is possible is that in which the protons arrive with angular momentum zero. This is usually the most important case at low bombarding energies unless it is forbidden by selection rules. In that case, the proton entering the nucleus has no memory of the direction from which it came and as a result the gamma rays are rigorously unpolarized.

If you can tell us a little more about the reactions you are using I could probably say in what cases there is a chance of observing polarization and in some cases such observations might, in fact, throw a new light on the mechanism of the reaction. The principle of the argument is very similar to the coincidence measurements made, for example, by Martin Deutsch at M.I.T.[335] or discussed in the recent paper by Gard-

[335] Martin Deutsch and his collaborators at MIT had published various papers on disintegration schemes of radioactive substances throughout the 1940s and into the 1950s, using coincidence methods.

ner in the Physical Society (LXII, 763, 1949).[336] The difference is only that here one takes as one datum the direction of the bombarding particle rather than that of another particle emitted in the interaction.
Yours sincerely,

R.E. Peierls

[504] Niels Bohr to Rudolf Peierls

Copenhagen, 25.5.1950

Dear Peierls,
I thank you for your kind letter of May 16th[337] and hasten to answer that Mr. Barker shall be most welcome indeed to work with our group for the next academic year. I was very interested in what you wrote about his abilities and about his work. We are also here just now occupied with the problems of nuclear reactions on such lines and expect in a few weeks a visit of Dr. Hill from U.S.A. who has worked with Wheeler and me on fission problems, and I hope with him soon to complete a paper just on the relationship between the drop model and an individual particle model in such respects.[338]

In Paris you probably have heard that considerable progress has recently been made in the relationship between nuclear shape and nucleon binding. This gives not only a far-reaching quantitative account of the quadrupole moments of nuclei, but implies also a coupling between the excitation of individual nucleons and the oscillations of the whole nucleus which offers a general understanding of the properties of the formation of the compound state on nuclear reactions.

Our old work with Placzek has also been much on my mind, but both Placzek and I were so pre-occupied in Princeton with other work

[336] J.W. Gardner, 'Directional Correlation between Successive Internal-Conversion Electrons', *Proc. Phys. Soc.* A**62**, 763–79 (1949).

[337] Rudolf Peierls to Niels Bohr, 16.5.1950, *Peierls Papers*, Ms.Eng.misc.b203, C.33

[338] The results were published by Hill and Wheeler in D.L. Hill and J.A. Wheeler, 'Nuclear Constitution and Interpretation of the Fission Phenomena', *Phys. Rev.* **89**, 1102–1121 (1953).

and interests that we only had a few discussions about this paper, which I hope we all can complete as soon as I have got the work with Wheeler and Hill off my hands.

It has been a great pleasure to learn that Lindhard has had such a good time in Manchester, and I look forward to see him soon again and to go to work in the charge of fission fragments, about which Lassen's experiments[339] have given such interesting results. Would you kindly greet Lindhard from me and say that I shall be glad to learn about his plans. I have not written myself to him because I found so much to do in the first weeks after my return from Princeton.

I cannot close this letter without remembering the very noble and moving letter you sent to me to Princeton about the tragic case which has brought some much anxiety into wide circles.[340] We are certainly not living in a pleasant world, but in spite of all I keep up the hope that we shall see better times before it is too late.

With the kindest regards from my wife and me to the whole family and your self.

Yours ever,

Uncle Nick

[505] Rudolf Peierls to Raymond Priestley

[Birmingham], 13.6.1950
(carbon copy)

Dear Vice-Chancellor,

Sir John Cockroft, the Director of the Atomic Energy Research Establishment of the Ministry of Supply, has asked me to give them some help

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[339] N.O. Lassen was working on the ionisation of fission fragments, and he had recently completed some experiments which were published in N.O. Lassen, 'Total Charges of Fission Fragments in Gaseous and Solid Media', *Phys. Rev.* **79**, 1016–17 (1950).

[340] Letter [497].

during the next academic year with the supervision of their Theoretical Physics Division.[341]

The position is that they have so far been unable to find a successor to Dr. Fuchs, and there is no prospect of a suitable man being man being found in the near future. At present Professor Pryce of Oxford is directing the work of the division in a part-time capacity but he has arranged to spend the next academic year at Princeton and Cockroft is asking me to take his place.

I am not looking forward to this further commitment with much pleasure since it will be extremely hard to do this and at the same time do justice to my own research team, but I do not see how in such circumstances such a request can be refused. I am, therefore, writing to ask you to bring the matter before the Council for their approval.

I understand that Pryce was spending half-a-day a week at Harwell for this purpose. I expect that I shall probably go there for one day from time to time on the average probably twice in three weeks. This depends a little on how much of the business can be done by Harwell people coming to see me here. I expect also to spend a little more time there during the vacations.

In the correspondence the question of a fee has also been raised; you may remember that I am now a Consultant for the Ministry of Supply. Until recently this meant that I was receiving a fee of £ 300 p.a. but recently when I completed my term of office on their Technical Committee, this was reduced to £ 200. Cockroft now says that if I take on the extra work they should pay an increased fee and, while the exact amount has not been fixed, a figure of £ 500 p.a. has been mentioned. My own feeling in this matter is that I could not really accept such a large payment for spare time work since I cannot fairly claim that this is a commitment I can carry in addition to my full university duties. I feel, therefore, that, while the Ministry of Supply ought of course to pay for what they get, I should return at least part of the increased fee to the university. If we can see any way to minimise the harmful effect to my department of the extra weight I have to carry, for example, by

[341] See letter [502].

temporary staff to relieve some of my other duties, this might well be regarded as first charge on the money obtained in this way, though I do not see clearly at the moment any arrangement that would really help to give me more free time.

Yours sincerely,

R.E. Peierls

[506] Rudolf Peierls to Manchester Guardian

Birmingham], 17.6.1950
(carbon copy)

LETTERS TO THE EDITOR

Professor Bohr's letter

Sir. — Your leading article today about Professor Niels Bohr's letter to the United Nations makes disappointing reading for those who respect your paper highly for its liberal tradition.[342] By all means let us, as you suggest, keep our feet on the ground, but also let us try to keep our head out of the sand.

Professor Bohr tried to show us some hope in an imaginative approach towards restoring openness in the field of international relations, particularly in scientific matters. He was trying to show the value in itself of even a small step in that direction. If I read his letter right he did not say specifically how far such a first step should go, and he certainly did not imply that the United States should forthwith publish complete blueprints of their atomic energy installations, but he talks of an "offer ··· of immediate measures towards openness on a mutual basis." No progress in international relations is possible which does not involve mutual concessions, but Bohr was stressing the advantage to be gained by reversing the present trend of increasing the height of barriers on all sides. Maybe, as you suggest, such an offer would not meet with any response, but then nothing would be lost, and a great deal gained, by the fact that the offer had been made.

[342] On 9th June 1950, Niels Bohr had sent an open letter to the United Nations, arguing for rational peaceful atomic policies. See www.galilean-library/bohr.html.

You describe the United States' "atomic mysteries" as her trump card. If this were correct, the security of the Western Nations would look very sad indeed. The Fuchs case gave proof again, if proof was needed, that in democratic countries it is impossible to keep large projects secret for long. We must guard against the danger, which is becoming evident today, of drifting more and more away from that personal freedom and freedom of knowledge which is so important a part of our way of life. We all know that an efficient way of keeping secrets is to curtail freedom of expression, freedom of movement, and the free flow of information, but this leads us surely towards the loss of what we value most in our democratic institutions as we understand them.

The real trump card of the United States lies in her resources, her industrial potential, and her scientific and technical staff of high quality and enterprise. They are of little value in the end, as the example of German scientific effort has shown, unless they will, if need be, pull their weight inspired by the enthusiasm for their system of government and by confidence in their method of government. From this point of view a genuine effort in this direction suggested by Bohr would add to the moral strength of the countries that value personal freedom. The danger of losing any part of the moral strength by related or not fully consistent attempts to protect secret information is likely to be far greater than the danger of the possible loss of some secrets that, for all we know, may already be compromised.

R.E. Peierls

[507] Rudolf Peierls to Freeman Dyson

[Birmingham], 19.6.1950
(carbon copy)

Dear Dyson,
Here is some assorted information; There has been some delay in writing up Ravenhall's note about pair creation[343] but this is now practically

[343] G.E. Brown and D.G. Ravenhall, 'On the Interaction of Two Electrons', *Proc. Roy. Soc.* A**208**, 552–59 (1951).

ready. I have asked Kolsrud[344] to look into the effect of screening on the partition of energy between positron and electron in gamma ray pair creation (King's anomaly); Brown is looking after that and they are also keeping in touch with Ravenhall so this should be all right. Dalitz has found that in his calculation on O^{16} he has omitted a term which does not influence the answer greatly but might in principle be observable since it leads to a relatively large anomaly at small angles. The reason for the error was that in the double integration of the Feynman type he had not noticed that a pole crosses the real axis in the course of the integration so that caution is needed. He is now anxious to make sure that there are no other similar troubles and this will delay the completion of his paper a little. He has, however, solved the other difficulty in the discussion of the ordinary Born approximation to Coulomb scattering and this is now perfectly reasonable and intelligible as far as it has been carried and it is not worth going further. My own paper is still held up while I search for a more satisfactory derivation which is not messed up by the presence of non-commuting factors. One can always carry out the proof for Lagrangians of the usual type but the result is so obviously of more general validity that it seems a pity to prove it in such a restrictive way. Brown has helped a good deal with sorting this out but in the last week or two examinations have been a major nuisance; thank God these are over.

Gunther is leaving in a few days and neither of his papers is ready for publication.[345] His derivation of the Breit terms seems now all right in principle but it has to be explained better and it really has not made much progress in the last few months. He also gave us a Seminar about his use of configuration space. Without pair theory this now looks all right but there are still some proofs missing for statements which are probably correct. He comes periodically to say that everything is wrong

[344] Marius Kolsrud, later Institute for Theoretical Physics, Oslo.

[345] Marian Günther later published a paper in two parts. M. Günther, 'The Relativistic Configuration Space Formulation of the Multi-Electron Problem', *Phys. Rev.* **88**, 1411–21 (1952); and M. Günther, 'The Relativistic Configuration Space Formulation of the Multi-Electron Problem. II', *Phys. Rev.* **94**, 1347–57 (1954).

because, for example, an integral which ought to vanish does not do so and in this particular case it turned out that he had merely shown that the integrand did not vanish identically as he had thought. As regards the use of this method, with pair theory everything is in a complete mess because he had carefully assumed that the S_+function (summing over positive energy states only) vanishes outside the light cone, which, of course, is not true. This means one needs a new idea and whether it can be done at all is now doubtful.

Schonland has now evaluated the Scott and Snyder formula[346] for the case in hand instead of improving the 7% discrepancy it increases it to about 20%. This may be due to some misunderstanding about density and composition of the emulsion and we are checking up on this now, because I cannot believe that the Williams formula which the Bristol people have used, can differ that much more from the correct result. But if it is confirmed, I would begin to suspect the method of Snyder and Scott.[347]

You will now have seen the letter by Bohr which I was not allowed to talk about before. As I expected it has not made a strong impression because it was not easy for the newspapers, with the best intentions, to sum it up clearly in the space they have. In particular the Manchester Guardian had a leader which was very unreasonable and I have just sent them a furious letter on the subject.[348]

Cockroft, whom I saw the other day, seemed quite shaken by your decision to go to Cornell. He seemed worried for one thing that your reply might be taken to indicate that you would have given a different answer if you had, in fact, been offered a job which involved the full responsibility of running the division, whereas he had made the sug-

[346]H.S. Snyder and W.T. Scott, 'Multiple Scattering of Fast Charged Particles', *Phys. Rev.* **76**, 220–25 (1949); H.S. Snyder and W.T. Scott, 'On Scattering Induced Curvature for Fast Charged Particles', *Phys. Rev.* **78**, 223–30 (1950).

[347]In his paper D.S. Schonland, 'On the Utilization of Multiple Scattering Measurements', *Proc. Phys. Soc.* A**65**, 640–56 (1952), Schonland used the method of Scott and Snyder after Corson had confirmed that it was in agreement with experiment. Dale R. Corson, 'Multiple Scattering of Fast Electrons on Nuclear Emulsions', *Phys. Rev.* **80**, 303–304 (1950).

[348]Letter [506].

gestion of being free to do your own work mainly to make the thing more attractive to you. I told him that it was not my impression that you would have taken the job either way but I feel you ought to know about this. He asked me whether, in general, we could improve our p[ro]spects of keeping young theoreticians in the country if there were more research Professorships. My reply to that was that I thought in your case this would not help because even if the job with the status of the Cornell job was created somewhere in this country you were likely still to prefer Cornell and that otherwise I did not know of people who have yet reached the stage where they would expect to get a Research Fellowship, though this may well arise in a few years time. If I have misrepresented your position on this I would be interested to know even if it is now too late to do anything about it.
With kindest regards,
Yours sincerely,

R.E. Peierls

[508] Freeman Dyson to Rudolf Peierls

Princeton, 24.6.1950

Dear Professor Peierls,
Tomorrow I am leaving Princeton for Ann Arbor, and so I think I may send you a report on what I have learned here. I was lucky in arriving here just before a large number of people left.

Tomonaga says he has spent a profitable year here, not working very hard but enjoying his leisure and the relief from running a department and finding problems for numerous graduate students. The first months he spent in an attempt to combine his variational method of describing radiation fields (as he uses it in his papers on meson theory where the nucleons are treated as non-relativistic finite sources) with the covariant formulation of electrodynamics. He found this did not work out, he got into great complications of detail and could not see his way through, and so he never wrote the work up and finally abandoned it. Since then he has worked on the problem of describing the behaviour of a degenerate gas of Fermi particles with strong interactions. (It seems that everybody has to have a rest from field theory occasionally!) He has found a method like that of Bloch spin-waves which describes the state of the gas as a superposition of sound-waves which behave like simple harmonic oscillators on Bose particles. The method is however worked out for the one-dimensional case. Also it is restricted to long-range interactions and may not be at all applicable to nuclear material. He says the relation of the particles to the sound waves is very similar to that of neutrinos to photons in the neutrino theory of light. For this reason the three-dimensional case is not to be handled by any simple extension of the one-dimensional case.

He has written a paper on the one-dimensional model[349] which is now being mimeographed and you shall receive a copy of it.

Jost and Luttinger, also exhausted with field theory, have been amusing themselves about the Ising lattice problem.[350] They have only succeeded in convincing themselves that the 3-dimensional case is too difficult for them. Jost now intends to go back and work some more on field theory, and he has some ideas which are good though nebulous.

Case has written with Pais a very good paper on the analysis of the $P-P$ and $N-P$ scattering experiments in terms of the spin-orbit coupling potential.[351] He seems to think the evidence for some such a coupling is now really convincing. I am sending you also copies of this paper. It seems that now Harwell can make a valuable contribution by doing some accurate measurements of the scattering, especially the $P-P$ scattering at 150 MEV. I hope you can bring this to their notice. In particular the Case-Pais potential predicts a $P-P$ scattering at 90° which is roughly constant from 150 MEV to 350 MEV at 4 millibarns per steradian.

[349] S. Tomonaga, 'Remarks on Bloch's Method of Sound Waves applied to Many-Fermion Problems', *Prog. Theor. Phys.* **5**, 544–69 (1950).

[350] They had just submitted, together with M. Slotnick, a paper on pair production by photons. R. Jost, J.M. Luttinger and M. Slotnick, 'Distribution of Recoil Nucleus in Pair Production by Photons', *Phys. Rev.* **80**, 189–96 (1950).

[351] K.M. Case and A. Pais, 'On Spin-Orbit Interactions and Nucleon-Nucleon Scattering', *Phys. Rev.* **80**, 203–11 (1950).

Yang has been continuing to play with the calculation of the S-Matrix in the Heisenberg representation. Nothing very useful has come out of this.

Placzek said the reports of his death had been partly exaggerated. He is, in fact, walking around and working, and seems in as a good a state as last year. They are staying here through the summer and living quietly because they are afraid his kidney may give more trouble, but at present it is behaving well. Placzek has been working all this year with a young Dutchman called Nijboer[352] on the details of the interference phenomena in neutron scattering, even a lot of numerical work has been done. He and Else send you and the family all their best wishes.

Skyrme you shall soon see and he will tell you what he has been doing.

Karplus and Neumann carried through the calculation of the scattering of light by light and found it much more unpleasant than they had believed possible. It seems the results are of no possible value except as a warning to others who may want to calculate such things.[353]

Karplus gave me a report on the doings of Schwinger. He was working until 4 months ago on an attempt to formulate the whole of electrodynamics and carry through the renormalizations without expressions in α. Then Kroll found a simple mistake at the beginning of the whole work and so he gave it up in disgust. Since then he has become interested in making a rigorous formulation of the Feynman description of field theories using "sum over histories". He has apparently translated the Feynman ideas into his own language and made them work both for Bose and Fermi fields. I have not seen the details of this. But I gather it departs rather radically from the Feynman method as Feynman uses it. Schwinger uses a method in which the fields are already quantized

[352] The result of their work was published as G. Placzek, B.R.A. Nijboar and L.V. Hove, 'Effect of Short Wavelength Interference on Neutron Scattering by Dense Systems of Heavy Nuclei', *Phys. Rev.* **82**, 392–403 (1951).

[353] Karplus and Neumann had just submitted their result which was published as Robert Karplus and Maurice Neumann, 'Non-Linear Interactions between Electromagnetic Fields', *Phys. Rev.* **80**, 380–385 (1950).

before the sum over histories is written down, and I do not know what use he then can make of the sum over histories.[354]

Schwinger will lecture on this work during the summer at Brookhaven. So I will hear about it first hand from Karplus later on.

I think this is all I have to report on physics. You will be not surprised to learn that this year the Princeton group has been suffering from a feeling of frustration, just as some of us have at Birmingham. Indeed, there has been very little serious work done, and none of outstanding importance. Driving force has been entirely lacking.

Oh, I forgot. I also talked with Van Hove.[355] He has been thinking about the connection between classical and quantum mechanics and the foundations of quantum theory. He has clarified these questions quite a lot. But of course it is an abstract mathematical piece of work, and I do not know if it will have any practical consequences. Roughly he says in the classical theory define the Hilbert Space H of all square-integrable functions of the position and momentum variables. The scalar product FG is just $\iint FGdpdq$. Now since by Liouville's theorem $dpdq$ is invariant under contact transformations, every contact transformation under pq is associated with a linear unitary transformation of H. Also, to every classical function $K(p,q)$ corresponds an infinitesimal classical contact transformation in H, i.e. a Hermitian operator K in H. The correspondence classical function $K \leftrightarrow$ Hermitian operator K is 1–1. Now the process of quantization is a projection of H on to a Hilbert

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[354] Schwinger's work led to a sequence of publications throughout the early 1950s. J. Schwinger, 'The Theory of Quantized Fields. I', *Phys. Rev.* **82**, 914–27 (1951), J. Schwinger, 'The Theory of Quantized Fields. II', *Phys. Rev.* **91**, 713–28 (1953), J. Schwinger, 'The Theory of Quantized Fields. III', *Phys. Rev.* **91**, 728–40 (1953), J. Schwinger, 'The Theory of Quantized Fields. IV', *Phys. Rev.* **92**, 1283–99 (1953), J. Schwinger, 'The Theory of Quantized Fields. V', *Phys. Rev.* **93**, 615–28 (1954) and J. Schwinger, 'The Theory of Quantized Fields. VI', *Phys. Rev.* **94**, 1362–84 (1954).

[355] Léon van Hove (1924–1990), studied mathematics and physics at Brussels and received his Ph.D. in 1946. After three years of research at Brussels he went to Princeton and Brookhaven before becoming professor of theoretical physics at Utrecht in 1954. He later worked as leader of the theory division at CERN and at the Max-Planck Institute for Physics and Astrophysics, before becoming Research Director General at Cern in 1976 (1976-80).

space of functions of <u>fewer</u> variables (e.g. q or lg instead of p and q), i.e. onto a linear subspace of H.

Van Hove is about so show:

(i) that this process of projection is connected in a direct way with the "averaging over histories" of the Feynman method. And so he thinks he can make the Feynman method mathematically water-tight.

(ii) that the full space H divides in a natural way into a kind of direct product of two subspaces, either of which can be chosen as the subspace onto which to project. And in each subspace there is a set of coordinates and momenta.

 I do not understand all the details of this. But I thought you might like to know about it. I hope he will write is up before long.

My best wishes to all the students of Birmingham. For the attention of Dalitz and/or Salam. Nobody here seems to have seriously tackled the question of whether a consistent renormalization theory can be made with a $\lambda\phi^4$ term. They all thought Matthews had proved it could be done, and so they left it at that.

Now for my personal news:

I have now understood why it is that in the Lamb shift calculation one has to use a contact transformation of the form

$$S(t) = \sum_{n=o}^{\infty} \left(\frac{-2}{\hbar c}\right)^n \frac{1}{n!} \int_{-\infty}^{t} - \int_{-\infty}^{t} P(H(x_1), \cdots, H(x_n)) dx_1 \cdots dx_n$$

with the order of integration as written, in order to get an operator which really is unitary when you throw away all the finite oscillating terms at $-\infty$. The proof of this is not difficult. At the same time this clears up completely the appearance of the $Z^{1/2}$ factors in the S-matrix, by a straight-forward physical argument. So the Karplus-Kroll argument to which you objected is now unnecessary.

That is all I have done since leaving Birmingham. I will write it down and send you a copy some time.

I look forward to hearing your news and seeing your quantization paper.[356] But of course I do not expect you write at this length!

This is intended to be a news-letter and you have full permission to circulate it if you think it is worth it.
Yours

Freeman Dyson

[509] Rudolf Peierls to Robert Oppenheimer

Birmingham, 27.7.1950

Dear Oppie,
You may remember that we chatted last Autumn about our problem here of finding another Professor of Physics. In the course of this conversation you mentioned the name of Panofski[357] as a man who might possibly be interested in such a position, and who would be very suitable.

As you have probably heard, since then Oliphant has in fact departed and Philip Moon has been appointed to succeed him as Head of Department. We must now appoint a professor to succeed Moon.

Moon himself is interested more in nuclear physics proper than in building machines, and it would therefore be useful to have another man who could play a strong part in the supervision of the cyclotron, which is now operating satisfactorily, and in helping to complete the synchrotron. However, at the same time we are trying to appoint another man of the senior staff whose specific duty would be the supervision of the machines, and there are prospects of finding a suitable man in this way. It is therefore not a necessary condition that the new professor should be machine-minded, but we have to consider the two appointments as part of the same problem.

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[356] See letter [507].

[357] W.K.H. Panofsky (1919–), studied at Princeton and obtained his Ph.D. from CalTech in 1942; after wartime nuclear work, he moved to Berkeley before taking up a professorship at Stanford in 1951 where he remained for the rest of his academic career.

I am now writing to see whether it still looks to you as if Panofski might still be a candidate for our job, and if so, whether you would be kind enough to let me have a few words about him which I could pass on to our committee, or if you prefer, would give me one or two names of people who could give well-informed opinion about Panofski.

To put this matter into proper perspective I ought to say that we are still at the stage where we are making enquiries fairly widely, and it is therefore too early to say with whom Panofski would be competing if he were interested in the job.

I have an idea that a letter which I wrote to you last October never reached its destination.[358] If it didn't you must have thought me very discourteous for fading out without saying a word about the enjoyable time I spent with you at a time when you probably had many more things on your mind.

I also wrote in that letter the story about our smallest child which we had just discovered. We have now had almost a year to get used to the idea and, like so many other things, once one finds out enough about the trouble it is not as bad as it seemed at first sight.

Higginbotham has told me about your views and advice as regards the proposed conference between American and British scientists. Your remarks will be most useful, and their spirit is very close to what we had hoped would be the line taken by the meeting. I have no illusions about many concrete results to be expected from such a meeting, but I still think it would be worthwhile, and Uncle Nick's open letter[359] would provide some useful material. I only wish it could have been written in such a way that more than the select few could understand it. But then, of course, it would not be Bohr!

Yours sincerely,

R.E.P.

[358]Letter [487].

[359]See letter [507], note 348.

[510] Freeman Dyson to Rudolf Peierls

Ann Arbor, 3.8.1950

Dear Professor Peierls,
In answer to your letter of 19 June.[360]

(i) If the Williams approximation and the Snyder-Scott method differ by 20% I would certainly trust the latter rather than the former. But probably this trouble has been cleared up by now. I wish we had put more effort into these calculations while I was in Birmingham; looking back on the past year this seems to be my main mistake, I was always dividing my time between five or six problems and never sat down and concentrated upon one thing long enough to finish it. I hope you and Schoenland will now be able to do something with it.

(ii) We had a colloquium talk the other day from Chandrasekhar[361] on the multiple scattering of light in the atmosphere. Everyone agreed that this was a work of art, a masterpiece. He has solved exactly the integral equation for scattering in a plane atmosphere, including the direction and degree of polarization. And everything comes out in precise agreement with observation. I felt after this that if we had attacked our multiple scattering problem with the same determination we should long ago have reached an exact solution. Certainly our problem is not nearly so formidable.

(iii) My course of lectures is now over and the notes will be mimeographed in a few days. They contain my new ideas so far as they have yet developed, which is not far.

(iv) About my letter to Cockroft. I said "It is clear that I am too young to be given a position of real responsibility and working primarily on pure science, it is best that I go to Cornell where I

[360] Letter [507].
[361] Subrahmanyan Chandrasekhar (1910–1995), nephew of C.V. Raman, studied in Lahore and Cambridge before joining the faculty of the University of Chicago where he stayed for the remainder of his academic career.

can work best." He seems to have interpreted this to mean that I was sorry he had not offered me a job as Head of Division.

Actually, I meant two things. (a) I had already told him when I was at Harwell before the Cornell job had arisen, that if I came to Harwell I would come to work on atomic energy and not to do pure science. Therefore I naturally expected that he would have taken this into account in making me this offer. (b) It was my own estimate of the situation that I was too young for a position of real responsibility. I still think this is true.

Now he has written to me again, as you know, with the offer of the position of Head of Division. I wrote back saying that I would like to talk with Bethe before making a final decision but that the answer would almost certainly be No. I am becoming more and more convinced that I am right in not accepting the job. But if you have any criticisms to make, or advice to offer, I shall naturally be glad to listen to you.

Concerning the question of keeping young men in England, I have nothing to say. Certainly the only thing that would keep me in England would be either (a) a clear and urgent call of patriotic duty, or (b) if Cornell should get involved in political squabbles of the kind they are enjoying this year in Berkeley. I do not think that more Research Professorships would really help in this respect. I agree with what you have said about this in your letter.

(v) I am not yet married but am rapidly approaching that condition. So if all goes well I will be needing an apartment for 3 in Birmingham next January.[362] It is, however, still uncertain whether Verena's College can find a replacement for next academic year. If they can't get one, she will be rather obliged to go on teaching there.

Yours sincerely,

Freeman Dyson

[362] Freeman Dyson married Verena Esther Huber in 1950. She already had a daughter.

[511] Robert Oppenheimer to Rudolf Peierls

[Princeton], 6.9.1950
(carbon copy)

Dear Rudi,
It has been many weeks since your good letter has been here.[363] I have been away on New Mexico and I hope that my delay in writing will have caused you no trouble.

Let me turn first to your departmental question. I still think that Panofski would be a magnificent choice. I am somewhat less confident that he could be lured away from Berkeley. I believe, though it may be a breach of confidence to write this, that he has been offered positions both at Harvard and at Columbia and has turned them down. I think he is quite attached to life in California. However, at the present moment, due to a variety of serious difficulties which have turned up at the University, this may be an auspicious time and I would encourage you to move quickly, if this is the direction in which you want to move. You ask who would write recommendations for Panofski. I should think that Macmillan and Rabi would both do very well on that. If you want a detailed appreciation from a theoretical side, probably Serber knows his work in greatest detail. I don't know of anyone in this country who combines better than he does the appreciation of the problems of high-energy accelerators with a real understanding of the physics that can be done with them.

I hope very much that the conferences to take place this month will be successful, both the conference at Harwell[364] and your own undertaking.[365] You know how much I would have wished to be there, but it did not seem like the proper moment for me to leave my modest duties here. I cannot, of course, hold very high hopes for the outcome of even the most earnest collective effort with regard to the problems of peace and atomic control; but I hope very much that I shall learn if possible

[363] Letter [509].

[364] In September 1950, a nuclear physics conference was taking place at Harwell.

[365] Peierls was organising a second international conference at Birmingham in September 1950.

from you as well as from Placzek and Higginbotham what the views were. It would be good if Bohr came himself.

You must be sad that Dyson has deserted you.[366] He will have one year here, I guess, and then go to Cornell. Matthews[367] has just arrived and I have not yet had a chance to talk with him. There has been nothing in physics which seems to me to constitute a real theoretical advance, as far as the work that has been going on in this country. It would be good to be able to talk of problems in physics with you.

Both Kitty and I were deeply touched by your few words about your youngest child. We send Genia and you our love and our warmest good wishes.

[Robert]

[512] Rudolf Peierls to Philip Moon

[Birmingham], 19.9.1950
(carbon copy)

Dear Philip,
I hope that, in spite of the winterly weather, you are managing to get a good holiday and in particular you are getting a little bit of the rest you must badly need.

I am now writing first of all to say that I have now a reply from Oppie which I enclose.[368] You have probably heard about the trouble at Berkeley[369] and perhaps it is not inconceivable then even a first-rate man might get fed up and might want to leave in the present

[366] In 1951, Freeman Dyson accepted a professorship at Cornell, before, in 1953, rejoining the Institute of Advanced Study at Princeton as professor of physics.
[367] P.T. Matthews was lecturer of theoretical physics at Birmingham between 1952 and 1957. He then joined Abdus Salam at Imperial College London and later became Vice Chancellor of Bath University.
[368] Letter [511].
[369] The University of Berkeley operated what was known as the 'speaker ban', which allowed the university authorities of censor public speaking on campus. The censorship was largely, though not exclusively, directed against communists.

circumstances. The way in which Panofski's work was referred to at the Oxford Conference does make one feel that if you could get him it would be a marvellous acquisition, both from the point of view of machines and of nuclear physics. On the other hand, of course, this would be bound to cause further delay.

Since we talked at Oxford I have heard further rumours repeating the suggestion that Blackett made to you and indicating that Cockroft also seems to be thinking in that direction. I do not really believe seriously that this idea will be pressed and if it comes before the Nuclear Physics Committee of which, no doubt, you will be made a member, I think we shall have an ally in Chadwick.

It does mean, however, that one should think out the problems and the future programme rather carefully so that we have the right answers available at the right time, but all this we can discuss when you are back.
With kindest regards,
Yours sincerely,

[R.E. Peierls]

[513] Freeman Dyson to Rudolf Peierls

Princeton, 23.9.1950

Dear Professor Peierls,
I am now installed at the Institute, having acquired a house on the Institute Housing estate. With luck I shall soon start again to do some work.

I am sending you by ordinary mail a copy of some notes made by Goldberger for a lecture course on the latest Schwinger theory. These notes are very confused and difficult to read, but I believe they contain a lot of good ideas and one ought to take trouble to understand them. I think now, after 3 readings I understand them at any rate better than Goldberger does.

In this new theory Schwinger does three things which I consider of first-class importance.

(i) He translates the Feynman Lagrangian formalism with "integration over histories" into a rigorous and conventional language. And he makes it work also for Fermi-style fields. Thus the full strength of the Feynman method is now available in a more practical and convenient form.

(ii) He has a simple and convincing explanation of the connection between spin and statistics, quite different from either Pauli's or Feynman's more complicated arguments.

(iii) He has a way of treating electrodynamics which does not treat the four potentials as dynamical variables, and thus avoids all trouble with supplementary conditions.

You have to search carefully to find how (i) is hidden in the notes. Of course, the name of Feynman is never mentioned. Only I happen to know from other sources (as is also obvious when you see what Schwinger's method actually is) that Schwinger started the whole thing from the idea of making Feynman's method intelligible to himself.

As a consequence of (i), Schwinger is able to derive from the classical Lagrangian function simultaneously the equations of motion and the commutation-relations of a quantum field theory. I believe also your form of the commutation-relations will come out of this method in a natural way. Probably Schwinger will <u>not</u> avoid the difficulties you have found when the Lagrangian is not quadratic in the fields. I hope you will look into this.[370]

With the Schwinger notes is a paper by Placzek which may interest somebody at Birmingham. My own work has not progressed any further since we left Ann Arbor. I am held up by some mathematical difficulties which may not be easy to overcome. I do not think the whole method is so very valuable and important that it is worth while to present it to the world in a unfinished state. So I am letting it sleep for a few weeks while I think about other things.

[370]Peierls was working on the commutation laws of relativistic field theory.

We have had some stimulating talks with Prof. Laurent Schwartz[371] from Nancy. Probably you have heard of his ideas. He is a mathematician with a new kind of function-theory which is much better adapted to physical applications than the old kind. For example it deals with Δ-functions. So he has in fact made mathematically honest a lot of doubtful manipulations of field theory. He has not, of course, thrown any light on the main problems, real divergences and such. But he is a man with a good understanding and a lot of interest in these problems, and so he says he will look carefully and try to understand the mathematical situation lying underneath the "renormalization" theories. I hope he may do something worth while. In any case we shall continue to correspond with him.

The Institute is just now waking up from its summer slumbers. There is no further news of general interest to report. My family is in very good state.

Oppy was very pleased when I told him about your new commutation method.

We just heard that my wife Verena is free from her job in Baltimore. They were able at the last moment to find a replacement. This is very good news.

Now we shall definitely be able to come to England together about Dec. 23. We shall spend some days with my family in London and then move into something in Birmingham (I hope) in time for the start of term in January. Now of course I ought to be getting busy finding a home before the very last moment. Perhaps you would be kind enough to let me know what you think I ought to do about this. E.g. is it sensible to write at once to the University housing office? Or to put advertisements in the paper?

I do not want in any way to saddle you with the responsibility of getting us housed.

Verena is also interested in getting some part-time or occasional work in connection with the university. I do not know at all what possibilities

[371]Laurent Schwartz (1915–2002), studied at Paris and Strasbourg where he obtained his Ph.D. in 1943. In 1945 he became professor at Nancy, before moving to Paris in 1953 where he taught in different institutions until his retirement in 1983.

exist in this direction. She thinks she is in danger of getting very lazy, if she only has a house and a husband and a child to look after, and no regular job.

Another question that arises is that of Katrin's school. Here she goes to the township school, which is a good school and free, but of course she does not learn much. She is now 5 1/2, will be 6 next April. What school do you recommend us to go to in Birmingham? And should we start negotiating early to be sure of a place?

All these questions of course need not be answered, if you think it is all right to wait until Christmas before dealing with them. In that case we shall be glad to deal with them ourselves in person.

All best wishes to your family too. Thanking you very much for your letters, and for any information you may be able to give us.
Yours sincerely,

Freeman Dyson

[514] Rudolf Peierls to Freeman Dyson

[Birmingham], 25.9.1950
(carbon copy)

Dear Dyson,
I have a very bad conscience for leaving your two long and interesting letters[372] unanswered and even more for failing to send you my personal best wishes to your marriage. On the latter point I have at least the consolation that you will have taken the long letter from Genia as speaking on all our behalf and there was really nothing I could add to what she had already said. We are now all looking forward to meeting your wife and her daughter when you get them over. We hope it will not be too bad a shock to be transferred suddenly into the middle of an English winter. If you will let us know approximately when to expect you we shall look around for a flat which will at least mitigate the worst hardships.

[372]Letters [510], [513].

About the correspondence with Cockroft, it was not my intention to persuade you to change your mind but merely to report to you what had been said to make sure I was not creating further confusion.[373] I have now come to the conclusion that the best I can do to counteract the drift of young theoreticians away from this country is to try and create a visiting Professorship somewhere, I would hope Birmingham, which one could use to invite here for one year at a time the people who on the whole want to remain in America. I think there are good prospects of achieving this. If you have any ideas about people who might be interested in such an offer a little later on I would be glad to know.

Now about physics. My progress in completing the proof of my method of quantization has been very slow and this, in fact, is the main reason why I have delayed writing to you. I always hoped I could send you the complete answer. At present the position is as follows: the method is consistent only if one can prove the inversion theorem. I have got a very transparent proof of this in the classical case, but on trying to extend this to non-commuting factors I have discovered that it is not, in fact, true as generally as I thought. That is to say keeping the order of the factors unaltered the retarded and advanced solutions which should be equal differ in fact by a commutator. This commutator vanishes by virtue of the commutation laws themselves, but this means one can only prove the consistency of the whole scheme and one cannot first establish the identity and then base the commutation laws on it. In this situation it would look more reasonable to think of a proof of the kind you have given, but I am reluctant to rely on this for two reasons. (a) because one then needs the Hamiltonian formalism to prove the consistency of my scheme and one then loses its main attraction, namely that one does not have to formulate a Hamiltonian to start with. (b) it is then necessary to prove the equality of the extra term in the Lagrange function and in the Hamiltonian to first order, even when this term contains derivatives or momenta. This follows, of course, if one uses your trick regarding the infinitesimal factor as a new dynamical variable, but I find it very

[373] See letter [507].

hard to see how this procedure can be justified. I have got some ideas of how to get around this difficulty but they are still very nebulous.

Here many things are going well. Ravenhall's work is finished, probably just in time to get this thesis in and the result is that the Breit terms are not correct to the order to which they claim to be correct when applied to the ground states of the helium atom. We are not yet sure of the exact magnitude of the difference but it seems certainly large enough to explain the observed discrepancy. Brown has shown how to do the same thing in configuration space and everybody now agrees about the result. Just now Brown has discovered an even more exciting use to be made of this new correction term but I dare not write about this yet since it is only two days old and needs confirmation.[374]

Butler's work has come out very well too and he has now shown how one can use the result of the $d-p$ reaction to identify the spins and parities of nuclear levels.[375] In particular in the case of [...][376] this leads to a result different from what everybody had assumed. No doubt Pais will have told you about all this.

We also had some interesting talks with Ferretti. He has a new way of deriving the wave function renormalization terms that arise in your expansion of the S-Matrix. This is probably identical in content with what you have done recently, but it is put in a way which, to my mind, makes it[s] significance particularly clear. He has also got some nice results about how exactly the expansion in powers of the coupling constant breaks down when there are discrete states and he has derived an equation which formally at least contains the answer to this, so it remains to be seen whether it can be turned into a practical method.

Schonland is still working on the scattering problem. On the point I mentioned to you last, it turned out as one could expect that there is no substantial agreement between the older theory of Williams and the

[374]See G.E. Brown and D.G. Ravenhall, 'On the interaction of two electrons', *Proc. Roy. Soc.* A**208**, 252–59 (1951).
[375]S.T. Butler, 'On Angular Distributions from (d,p) and (d,n) Nuclear Reactions', *Phys. Rev.* **80**, 1095–96 (1950).
[376]Missing in carbon copy.

formula by Snyder and Scott.[377] The confusion has arisen because the Bristol people had evaluated the Williams theory in a manner which we still do not understand, but there is now still a disagreement in the constant of the scattering formula, not only between experiment and theory but also between the different experiments. Moreover, according to the Bristol experiments, the constant for protons seems to vary with proton energy. We are now looking to see whether there is any chance that the method used for evaluation, including the cut-off, could be reasonable for this.

Dalitz has written up his work on [...][378] for his theses and is now preparing it for publication.[379] He also has some interesting results on Born-approximation (I don't quite remember whether you knew already about these when you left; if so, I must apologise for the repetition). First of all in the formula for the scattering of electrons by nuclei he discovered that all results published as power series in Z were wrong except the paper by McKinlay and Feshbach[380] which came to our notice only recently. Dalitz has found a mistake in each of the previous papers and obtained the correct formula which agrees with McKinlay and Feshbach and with Mott's exact formula if correctly expanded.[381] The point where most people went wrong arises from the presence of both virtual and real intermediate states in second order Born approximation and he finds that all this business can be made much more transparent if one uses Feynman notation. He has also looked at the ordinary non-relativistic Coulomb scattering where it is usually believed that although first order terms give the correct answer, the second order terms do not vanish but, in fact, diverge. To this a lot of philosophi-

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[377]See letter [507], note 346.

[378]Missing in carbon copy.

[379]R.H. Dalitz, 'On higher Born approximation in potential scattering', *Proc. Roy. Soc.* A**206**, 509–520 (1951); R.H. Dalitz, 'On radiative corrections to the angular correlation in internal pair creation', *Proc. Roy. Soc.* A**206**, 521–538 (1951).

[380]William A. McKinley, Jr. and Herman Feshbach, 'The Coulomb Scattering of Relativistic Electrons by Nuclei', *Phys. Rev.* **74**, 1759–63 (1948).

[381]N. Mott, 'The scattering of fast electrons by atomic nuclei', *Proc. Roy. Soc.* A**124**, 425–43 (1929); N. Mott, 'The polarisation of electrons by double scattering', *Proc. Roy. Soc.* A**135**, 429–58 (1932).

cal discussion has been attached, but Dalitz finds that the statement is quite wrong, again because the role of real and virtual states was not properly understood. He will get rid of both these papers soon and then hopes to start on the problem you suggested, though he realises that by now someone else may already be well on the way.

Field has nearly finished his problem and the answer seems to be identical with your result obtained by low-brow methods but he is at present checking this before making a final statement.

Kolsrud is looking into the discrepancies in pair production. You remember that the Bristol people found disagreement as regards the energy distribution between positrons and electrons and there are other experiments reporting disagreements as regards the angular distribution. We are now satisfied that screening cannot be responsible for either of these and we propose to look at the effect of Born approximation but this is, of course, a long job.

I think this is most of the progress as far as will interest you.
With kindest regards,
Yours sincerely,

R.E. Peierls

[515] Rudolf Peierls to John Cockroft

[Birmingham], 16.10.1950
(carbon copy)

Dear Cockroft,
Thank you for your letter about Luffman.[382] I shall make a point on my next visit to talk to Luffman about this. Your question, however, prompts me to tell you the position of solid state work at Harwell. It seems to me that it is almost impossible to get any useful work done by having members of the present staff learn about solid state theory however good they may be. The trouble is that solid state theory is not

[382]Letter could not be located.

a very rigorous discipline but a complicated mess of different methods which work in some cases and do not in others and to say that anything sensible one must not only know the literature, but also have a sound judgement as to what statements in the published literature one should or should not believe. It takes many years for even the best men to reach that stage.

I am all in favour of allowing some suitable people to read about the subject because they will then be suitable to work under the direction of a more experienced man if and when you find one, but they will not be very much use as long as they are on their own even with such help they can get from part-time consultants such as Mott and myself.

I spent some time talking to T.M. Fry and my conclusion was that it would be well worthwhile to make every effort to get such a more senior man on the theoretical side, even though he probably will not do everything that Fry hopes can be done. Fry seems to be somewhat optimistic about the reliability of calculations from first principles and of giving absolute interpretations to miscellaneous measurements. Solid state theory will always remain a semi-empirical science and the best that can be done is to discuss the experiments intelligently in the light of responsible theoretical speculations. This may mean that once an expert in that field has acquainted himself fully with the position there may be enough work to occupy all his time, but the answer to that should be to give him some freedom to branch out in more academic problems. The main thing is to have someone who is there all the time and who would have to give first priority to whatever can be done on the problems of relevance to the work in hand.

When I last talked to you on this subject I mentioned the name of H.R.Paneth.[383] He is now here on my staff and, while I am very glad to have him and expect him to be very useful here, I do not think I should recommend him for a job at Harwell where, for several reasons, I feel he would not fit in too well. This, of course, may change but for the present I am pretty sure he should be counted out. There are two men at present at Cambridge who deserve serious consideration: they are Dingle and

[383] Heinz Rudolph Paneth (1926–2004), later known as Henry Post, son of pioneering radiochemist Friedrich A. Paneth, had worked at Montreal in Halban's group.

Sondheimer. Of the two Dingle[384] is probably in the long run th[e] better man and more used to contact with experimental work. At the present he is still a little wild in the sense that he might quite happily investigate an effect which is 10^4 times too small to matter for the problem until someone else points that out to him. It is very doubtful, in any case, whether he could be persuaded to leave Cambridge.

Sondheimer's strength is perhaps more on the side of the more formal mathematical theory, but he is very good, and he has a varied experience. You probably know that he has just returned to a fellowship at Trinity after spending a year at M.I.T.[385] Before that he also spent a year with Mott at Bristol who also speaks very highly of him. He is of German origin and I know that his appointment might therefore cause some shaking of heads, but he has just got married and that usually means that he might be more liable to succumb to temptations of a job at Harwell. I have not, of course, asked him and I may be wrong about this. I do not know him well, and you should not accept my opinion about him without support from other people, in particular, Mott and perhaps Schönberg.

It is suggested that after Mott's return from America he should meet me at Harwell to discuss the solid state problems and probably a decision would be left until after that, but it might be useful to think about the possibilities in the meantime.

Otherwise, as far as I can see now, the main need of the theoretical division would seem to be to explore what theoretical work is required for the general programme of a long-range point of view. As regards pile and reactor programme, this would seem to be well in hand, though, of course, I would like to discuss the work in detail with Davison, Rennie, etc.. More exploration is needed in connection with fundamental nuclear physics and the cyclotron work and there Skyrme is making a good start.

[384] R.B. Dingle had come from Bristol to Cambridge and worked, among others, with D. Shoenberg. He moved on to the University of Western Australia, before becoming professor of physics at St. Andrews University.

[385] While working with Nevill Mott at Bristol and at M.I.T. Sondheimer had been working questions of conductivity in metals. His most recent publication dealt with this issue. E.H.Sondheimer, 'The Influence of a Transverse Magnetic Field on the Conductivity in Thin Metallic Films', *Phys. Rev.* **80**, 401–406 (1950).

The question must be answered whether the division should do something in parallel with Risley on the isotope plant and I believe some people should also think about the programme on which G.P. Thomson and Thoneman are working.[386]

I hope to see you next week in London or later, but I though[t] it might save time to put some of these points on paper.
Yours sincerely,

[R.E. Peierls]

[516] Claude Bloch[387] to Rudolf Peierls

Copenhagen, 26.10.1950

Dear Professor Peierls,
Please find enclosed a reprint of a paper on a variation principle in non-local field theory. It is, of course, all unquantized, except for the free fields, where the quantization is rather trivial. As regards the quantization when the interactions are taken into account, I think that a method similar to that recently developed by Yang and Feldmann,[388] and by Källen,[389] can be used. I would be extremely interested in learning your opinion about this procedure.

The equations of non-local field theory can be written by means of local field functions only, if one introduces a smearing function. Thus, the interaction term in the Lagrangian reads

$$g \int dx' dx'' dx''' F(x', x'', x''') \psi^t(x') u(x'') \psi(x'''),$$

[386]For details on the work of the different divisions working on nuclear energy see R. Carruthers, 'The Beginning of Fusion at Harwell', *Plasma Physics and Controlled Fusion* **30**, 1993–2001 (1988).

[387]Claude Bloch (1923–1971), studied at Paris and received his doctorate in 1946. He continued to do research at Copenhagen (1948–51) and CalTech (1952–53), before joining the Commissariat à l'énergie atomique where he eventually became director of the physics division.

[388]C.N. Yang and D. Feldman, 'The S-Matrix in the Heisenberg Representation', *Phys. Rev.* **79**, 972–78 (1950).

[389]G. Källen, 'Formal integration of the equations of quantum theory in the Heisenberg representation', *Arkiv Fysik* **2**, 371–410 (1950).

in the case of a charged spinor field interacting with a neutral scalar field. The field equations deduced from the variation principle are

$$\left.\begin{aligned}\left(\gamma\frac{\partial}{\partial x}+M\right)\psi(x)&=g\int dx''dx'''F(x',x'',x''')u(x'')\psi(x'''),\\(\Box-m^2)u(x)&=y\int dx'dx''F(x',x'',x''')\psi^t(x')u(x'').\end{aligned}\right\}\qquad(1)$$

Assume now for a moment that the right hand side of the equations (1) are known functions, the solution of the linear differential equations thus obtained is the sum of the free field and of a particular solution of the non-homogeneous equations. The latter can be expressed by means of a Green function. This gives

$$\left.\begin{aligned}\psi(x)&=\psi^{\dot{m}}(x)+g\int dx'dx''dx'''S_+(x-x')F(x',x'',x''')u(x'')\psi)x'''),\\u(x)&=u^{\dot{m}}(x)+g\int dx'dx''dx'''D_+(x-x')F(x',x'',x''')\psi^+(x')u(x'').\end{aligned}\right\}\qquad(2)$$

Here, S_+ and D_+ are the usual retarded Green functions, $\psi^{\dot{m}}$ and $u^{\dot{m}}$ are the incoming fields, which are equal to ψ and u for $x^4\to-\infty$. (The latter statement is true only under suitable restrictions). The equations (2) are integral equations equivalent to the system (1). A similar system (2') can be obtained by means of the advanced Green functions. It will contain the outgoing fields ψ^{out} and u^{out}. Clearly, (2) and (2') define the outgoing fields as functions of the incoming fields (which may be arbitrary), or conversely.

Quantization can be introduced by <u>postulating</u> for the incoming fields (<u>or</u> the outgoing fields) the normal commutation relations of the fields. Clearly, this is consistent with the field equations, which can be used to deduce the commutation relations of the other field functions. It can be shown that the outgoing fields satisfy also the normal free field commutation relations. Hence, there is a unitary matrix such that

$$\psi^{\text{out}}(x)=S^{-1}\psi^{\dot{m}}(x)S,\qquad u^{\text{out}}(x)=S^{-1}u^{\dot{m}}(x)S,\qquad(3)$$

which can be taken as the S-matrix of the system.

Assuming that expansions in powers of g can be used, it is possible to obtain a formal extension for the n-th term in the expansion of the outgoing fields as functions of the incoming fields. In the conventional case, in which $F(x', x'', x''') = \delta(x' - x'')\delta(x'' - x''')$, it is possible, of course to go one step further, and to obtain Dyson's expression for the n-th term of the expansion of S. In the more general case, however, I have not been able to deduce an explicit expression of S, and this makes the calculations of transition probabilities more difficult than in the normal theory. In fact I do not yet know whether convergent and reasonable results can be obtained for a proper choice of the smearing function F. At any rate, the general scheme seems to be very suitable for theories containing smearing functions, still, due to the lack of causality in the strict sense, the Hamiltonian formalism or the Schrödinger equation do not seem to be very natural approaches.

I would be very grateful, if you could spare some time and let me know your opinion on the subject.
Yours very sincerely,

C. Bloch

P.S. After the 30th of this month, my address will be:
Claude Bloch, 10 Boulevard Barbès, Paris (18^e).

[517] Rudolf Peierls to Claude Bloch

[Birmingham], 1.11.1950
(carbon copy)

Dear Dr. Bloch,
Thank you very much for your letter[390] and the reprint of your paper. Since I saw you in Paris we have made an effort to understand the physical meaning of the non-localised theory of Yukawa, but, while we see the consistency of the mathematics, I am afraid we have been quite

[390] Letter [516].

unable to see what it all means and, in fact, my feeling was [that] there is not even a theory but just some equations.

We shall read your paper and hope this will help us to understand this a little better.

In your letter you write equations which seem to me to be very similar to those of McManus, except that you have a smearing function depending on three points in space instead of two. This is an important point, because one would like to have such a function in order to make certain self-energy terms finite. However, we always believed that one would get into trouble with gauge invariance if in a product such as [...][391] one took the two functions at different points in space.

We are, in fact, playing with equations for a quantized theory which are identical with the ones you set out except for the difference in the smearing function, and we believe now even that we can justify them from some general principle that leads to quantized equations directly from an action principle without formulating a Hamiltonian. There are still, however, some mathematical difficulties which are holding us up. These difficulties concern the general principle and not the results. We are fairly clear now what the results will be and they will be just that not all the infinities are removed by this method, but as I said this has very much to do with the question of whether you have a smearing function depending on two or three coordinates.

I hope soon to have the answer to the problem about the general principle and if I get that written up, I will send you a copy of it.
Yours sincerely,

R.E. Peierls

[391] Missing in carbon copy.

[518] Freeman Dyson to Rudolf Peierls

Princeton, 8.11.1950

Dear Professor Peierls,
I am sending you a copy of some work of Schwinger, and I enclose a copy of a letter to Bethe with some remarks[392] which you might find helpful, among a lot of irrelevant material.

Thank you very much for your letter to Verena which she is answering separately.

Also I was glad to get news from Dalitz and Ravenhall. Please will you thank them.

It will amuse you to see that once again Schwinger has been directly anticipated by Nambdu[393] in a paper in Prog. Theoret. Phys. Vol. 5, part 1 published 6 months ago. But of course Schwinger has done a much better job than Nambu with the new method.
Yours

Freeman Dyson

[519] Verena Dyson to Rudolf Peierls

Princeton, 8.11.1950

Dear Professor Peierls,
Thank you very much indeed for your kind letter discussing the possibilities of jobs for me. I am very sorry we are bothering you with such questions. But since we are so far away and since I should like to start working as soon as possible after arriving in Birmingham we are bound to do this. First of all you might like to know something about my education: Elementary and high school in Athens, Greece, where

[392]Letter Freeman Dyson to Hans Bethe, 8.11.1950, *Peierls Papers*, MS.Eng.misc.b202, C.17.

[393]Y. Nambu, 'The use of proper time in quantum electrodynamics', *Progr. Theor. Phys.* **5**, 83–94 (1950).

I graduated with the "reichsdeutsche Abitur" in 1940. From 1940 to 1947 regular studies at the University of Zurich. My main subject was mathematics, the first minor subject as usual physics, the second minor subject chemistry. I went through some chemistry and physics labs, and also through examinations in these fields as well as theoretical physics. I also heard some lectures in Astronomy and some of more general contents, my hobby at the time was logistics. In 1947 I made the PH.D. in mathematics with a thesis on group theory "supervised" by Speiser. Ever since then I was interested in research in abstract group theory, but I have never succeeded in doing anything worth publishing. In 1949–50 I was an instructor of mathematics at Goucher College in Baltimore. There I taught trigonometry, analytic geometry, calculus and advanced calculus. I am looking forward to having a job again, mainly because I find it healthy and satisfactory to do some honest, solid and definite more or less useful work. Furthermore I should like to be able to contribute to the financial support of our household, especially since we shall try to get a maid.

From all the possibilities you mention the most appealing seems to me applied research. I think I would be adaptable enough for such a job. Surely I would not spare any efforts to do it well. I am not afraid of the dull part of it. I would like to learn something about the back ground and the meaning of such a work and this would make it interesting.

As to teaching: I rather liked Goucher. But I understand that I would not be likely to find a place with the same advantages: small department, easy schedule, College level, and so forth. However, I still consider this as a possibility if nothing else could be found.

As to languages, I know quite a few, but none of them really thoroughly: English, German, French, some Greek, Italian, little Spanish.

On the whole I am looking forward to doing some work I have never done. Of course I wish to find a work which I start on the basis of what I have learnt, and through which I can learn some more about physics or mathematics.

I am very much looking forward to our trip to England, and to meeting you and your family.

Again very many thanks and best regards also to Mrs Peierls.
Sincerely yours,

Verena Dyson

P.S. In view of the short time left to us I shall be glad if you will accept definitely in my name any job which you think suitable, giving priority to applied research, second choice school teaching. If possible, a full-time job would be fine.

[520] Rudolf Peierls to J. Rzewuski

[Birmingham], 8.12.1950
(carbon copy)

Dear Rzewuski,
I have an extremely bad conscience because I know that I never wrote a reply to your long letter some time ago, both to send you our best wishes on your marriage which by now must be quite a well-established institution and to thank you for the very interesting paper by yourself and Rayski.[394] In fact, in a way this paper was the reason for the delay because I thought I would write when I have had a chance to read this paper and make some intelligent comments, but life was particularly hectic at that time and I never got down to it properly.

Meanwhile I have received your very interesting paper about the result of the McManus theory. We had, in fact, intended to do just the same thing and you have beaten us to it. As soon as we saw that this was a possibility of carrying the work out we also guessed what the answer would be and, in particular, that it would not remove all the infinities. I would have said that even the self energy of an electron would not come out finite, and as far as I have been able to understand your calculations that is also your result. The difference lies merely in

[394]See letter [482], note 285.

the question of terminology what exactly what one means by the self energy and what names one gives to other similarities.

One reason why we were slow over this was that we always wanted to see whether the theory applied in this manner could be part of a consistent formalism and I had just succeeded in seeing a way how one could formulate the commutation laws in a way which was derived directly from the Lagrangian and did not need the definition of a Hamiltonian. If this method is applied to the McManus theory or for that matter to any other theory in which one can expand in powers of the coupling constant, then the results must lead to just what you have done, but by my more general method it should be possible to see whether the commutation laws which one obtains in this way are consistent and satisfactory and therefore whether the operators defined by this series really exist or whether one gets into contradictions. I have struck some trouble with developing this method further and, in particular, with proving what conditions must be satisfied for consistency, but I hope it will not take long to straighten this out and I shall then send you an account of the whole thing.

The department continues to flourish: we now have a fair number of people interested in field theory who have learnt the modern techniques from Dyson when he was here last year and we are just expecting Dyson here for the remainder of the present session. He seems to have developed a new theory by means of which one can also treat bound states and therefore make a decent approach to the problem of nuclear forces. We have made one slight step towards the treatment of bound states by applying field theory to the problem of interaction of two electrons, for example in the helium atom. This has been done so far only to the accuracy of the Breit terms: already the next step beyond that looks very complicated, but it is very interesting to find that already in this order there is a correction to the Breit terms which vanishes for free electrons (i.e. for the Møller problem) but which is important in helium and which, in fact, seems to remove the discrepancy between the present theoretical values and the experiment. This is mainly Ravenhall's work and he is now trying to generalise the method and apply it to other problems.

I also enclose a circular which I have sent to all former members of the department. If by any chance you have a picture which you could let us have for the purpose if would be very much appreciated. I hope to hear from you again about your further progress. All the old members of the department send their best wishes.
Yours sincerely,

[R.E. Peierls]

[521] Robert Oppenheimer to Rudolf Peierls

Princeton, 12.2.1951

Dear Rudi:
Of all the many things of which we might write, it has come my time to write of the pleasantest. That is to ask whether in fact you wish to come to the Institute next year. I earnestly hope that your answer will be affirmative.

We would be glad to have you for the whole academic year or for either of your terms. The formal terms will run from the first of October until just before Christmas, and from mid-January to about the first of April. We always hope that our members stay beyond the formal terms. You will know so much about what sort of place this is from your many friends here that I need not burden this letter with anything more than an assurance of our hope that you will come and a cordial welcome if you do. It would be helpful for us to know when your plans are clear, whether Genia will come with you and whether you will be able to bring your family, and what sort of a grant we might make to make your visit possible. None of this is urgent; but when the time comes, you must let us try to make your visit as agreeable and fruitful as possible.
With every warm good wish to you both,

Robert

Kitty sends you both her love. R.

[522] Hans Bethe to Rudolf Peierls

Ithaca, 19.2.1951
(carbon copy)

Dear Rudy:
Thanks for your letter recommending Noyes. I would like to hear more about your trip to India[395] if you have more time to collect your thoughts. Of course I constantly feel that I owe you a long letter about science but there always seems to be too much else in the way.

This letter is to announce the visit of our family to Europe this summer, Uncle Joe permitting. If you are there and are willing to have us, we would like to visit you. As usual in our family we follow Napoleon's principles and travel separately. Rose and the children are planning to go by boat and to get to England about June 1. They would like to come to you some time in early June, and they would like to stay for about a week, if you can accomodate them. I could imagine with the increased family, that space is limited even in your house.

I am planning to come by plane about the middle of June and to spend about two weeks in England. One of these I would like to travel around and spend the other week with you. Again if this fits in with your plans. After that I was planning to go to Uncle Nick's conference[396] which I believe is about July 4 to 10. After that I would like to join the family in Germany.

Please write me what your plans are and how you are fixed up for accomodation. We would all like to see you and to replace by a personal visit what is lacking in correspondence.
With best regards,

[Hans]

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[395] Between 14 and 22 December 1950, an international theoretical physics conference on elementary particles had been held at the newly-opened Tata Institute of Fundamental Research. Rudolf Peierls attended the conference and spent some time afterwards visiting, among others, Raman's institute at Bangalore.

[396] The conference took place between 6 and 10 July 1951, see Pauli, *Wissenschaftlicher Briefwechsel*, IV/1, p. 339.

[523] Rudolf Peierls Hans Bethe

Birmingham, 28.2.1951
(carbon copy)

Dear Hans,
I am delighted to hear of your impending visit. It will be great fun to have Rose and the children with us and I am sure we can manage to squeeze them into the house somehow. In fact, Genia is planning to take the children to the seaside for a week or two during June and if should so happen that Rose's visit should fall in that period she might like to join Genia and the small children at a seaside holiday and the best, of course, would be if this were either at the beginning or at the end of that period so that Rose could also have at least a few days at Birmingham.

As regards your own visit, you will, of course, also be most welcome at any time. I am also due to go to the conference in Copenhagen, and our plan is to take the car across via Ostend and to drive up to Copenhagen. I understand that the conference was to start on the 6th. We would probably be able to leave only on the morning of the 1st and cross on the 2nd. It looks possible that we should reach Copenhagen on the evening of the 5th. What would you think about joining us in the trip? I would, of course, hope that this would not curtail the length of your stay in Birmingham since all the other people here, including Dyson, would certainly like to talk to you. However, all these plans are subject to confirmation; it may still be that Genia might prefer to take her holiday elsewhere and I would in that case, go directly to Copenhagen by boat or by plane.

Scientifically the most important result of the Bombay conference for me was that the Bristol people produced convincing evidence in favour of multiple as opposed to plural production of mesons.[397] The point is that for plural production you would expect that for a greater number of mesons there should either be also a larger number of reasonably

[397] See Report of the international conference on elementary particles, Bombay: Commercial Print Press, 1951.

fast protons ("grey tracks") or else if the energy should be completely dissipated a greater energy in the star formed by low energy tracks. They have looked for correlation of either kind and they are completely absent. On the average, for example, the energy in the star increases only by 55 MeV per meson produced and this is clearly not enough. I also learnt there for the first time about Fermi's theory of meson production which is very attractive and simple as everything else that Fermi does.

Whether it is right is another matter and I think particularly that his explanation of the angular distribution is rather fishy. Of course there was much more to India than physics, but to do justice to the many impressions there would take a pretty long letter and I shall leave it until we meet, I hope in June.

Yours sincerely,

[Rudi]

[524] Rudolf Peierls to Robert Oppenheimer

Birmingham, 10.3.1951
(carbon copy)

Dear Oppie,

I owe you a reply to your very kind invitation to visit Princeton next Session. I did not write at once because I wanted first to explore the possibilities. May I say first of all how much I appreciate the invitation.

I have considered my arrangements for next year and have come to the conclusion that I could not possibly get away for the first term. There are many reasons for this, one is that there will again be a fair number of new research students joining our group and while I have some promising young men on the staff I do not think I have yet reached the stage, where I can leave them to decide on the programme of the new men. Another reason is that our Professor of Pure Mathematics G.N.Watson, is retiring and there will be a new Professor of Mathematics starting next Session. Since the two departments are jointly concerned with the teaching of Mathematics students, I feel I should be here to take my share of the responsibility at the beginning of the Session.

However, it seems feasible for me to come for your second term which practically coincides with our Spring term, by that time the new men should have settled down and there is not much administrative work in the undergraduate teaching at that time.

If, therefore, the international situation has not deteriorated in a drastic way it looks to me as if I shall be able to accept your invitation for the second term with delight.

As regards the financial side it is almost certain that the University will grant me leave of absence with pay except that they might deduct any fees that have to be paid to lecturers that have to do my teaching while I am away, and as far as I can foresee other sources of income would continue also, with the possible exception of some fees for consulting work.

I would not expect to bring my family to Princeton, but possibly Genia might join me for a few weeks, during my stay.

You generously asked me what payment would be necessary to make the visit possible and this puts me into a difficulty; broadly speaking my commitments here, would probably use up my normal income so that what would be required would be the cost of my stay in the United States. I shall of course apply to the Fulbright Scheme for a grant to cover my passage and I assume that this will be granted, however, I have no knowledge of the present cost of living in the United States in general and in Princeton in particular, and I feel that you can probably judge this better than I can and I would therefore be grateful if you could fix whatever sum seems to you appropriate.

I am looking forward with great pleasure to the opportunity of spending a term at the Institute and to a chance of refreshing my very inadequate knowledge of modern ideas.

Yours sincerely,

R.E. Peierls

[525] Rudolf Peierls to Viscount Portal of Hungerford

[Birmingham], 9.4.1951
(carbon copy)

Dear Lord Portal,
On thinking over our recent conversation I feel I would like to send you my comments in writing, both because this will allow me to add one or two remarks that did not occur to me on the spot, and because this will make it possible for you, if you see fit, to pass the letter on to the people who raised the matter in the first place.[398]

There are several different ideas that may have been in the minds of the security people when they drew attention to the position, and I shall comment on some of them though they may not all be relevant.

The most obvious question is whether the reported accusations throw any doubt on my reliability. On this I cannot comment usefully, but I was very gratified to know, both from your direct assurance, and from the fact that you raised the matter with me, that you regard the answer to this question as satisfactory.

There may have been doubt whether I was aware of the possible affiliations of the two men, and if I was not, whether I might inadvertently disclose to them any confidential information. If such a warning was intended, I am grateful for it, but would like to say that I did know of Prof. P. that he held extreme and rather dogmatic, political views; while I did not have the same impression of Dr. B. I would have hesitated to rely on his disgression anyway. But the main point is that whatever I thought of these people, I know enough about my obligations, and about the importance of the information I have access to, I would not dream of passing confidential information to these people any

[398] As a result of Klaus Fuchs' arrest, many British scientists were facing criticism over their links with Eastern European colleagues. In Peierls' case, the allegations were particularly frequent, because of his personal contacts to Russia and his close links with Klaus Fuchs. In this case, Peierls was accused of having close contacts to two Birmingham colleagues, referred to here as Prof. P and Dr. B, both of whom were said to have communist affiliations.

more than to my many other friends who may be safe, but who are not connected to the project.

Or perhaps there may have been concern that the political thoughts of these acquaintances might sow dangerous seeds in my mind. If a warning against this danger was intended, I am grateful for it, but I am not afraid of this danger. I have lived in many countries and discussed politics and principles with many different types of people. As a result I have developed my own views and principles, and I hold these with conviction. They are not easily shaken by propaganda. In fact, I believe it to be one of the greatest advantages of the democratic way of life, and a great source of strength, that we hold our beliefs from free choice, and after mature consideration of the alternatives, and not because dangerous ideas have been kept from us.

There might also have been a suggestion (though I would like to stress that nothing in your very fair and reasonable explanation would support this inference) that my contacts with the two men in question was liable to be misconstrued, and that it would be better to disassociate myself from them. If there was any such suggestion, I would like to make it clear that I see no cause whatever to alter my personal relations with people. Almost anything one does can, in suitable circumstances, be open to misinterpretation; this is a risk one cannot escape.

To these remarks I would like to add a comment of a different kind. The points you raised worry me because, on the face of it, the implication seems to be that of a very low standard of efficiency in the security services. It is quite correct that Professor P. is an old friend of mine, though I have not, in fact, seen him for at least eight months (outside university senate meetings, &c). But my association with Dr. B. is much more tenuous than that. He stayed at our house when he first came here in 1939 as a very poor refugee. About 18 months ago when their child was born, my wife tried to help and advise them on their domestic difficulties. As a result of differences in outlook which became apparent then, the two ladies have not seen each other since then. My own contacts with B. do not go beyond exchanging, on rare occasions, a few words about some topical news item.

I mention these thoroughly unimportant facts only because they tend to show that the information available to the security services is somewhat obsolete and not terribly significant. I would have thought there were a great many facts about me that could be made to look much worse. I would naturally assume that these were known, and regarded as innocuous, but I am now beginning to wonder whether perhaps they have not yet been reported?

Is it known, for example, that, when we were in Cambridge, we were on friendly terms with D.P. Wooster,[399] and spent a summer holiday with him and his family in 1937? Or that my wife is on very friendly terms with Mrs Betty Waddington at Cambridge, whom she has known well since about 1934, and whose views since then have shifted so far to the left that I believe she is now a member of the communist party? My wife still visits her every time she is in Cambridge, and when we go to Cambridge together I usually do as well.

Is it known that I am acquainted with Prof. P.Y. Chau of Peking, from the days when we were both research student[s], and that, when he recently visited Birmingham as a member of the official "goodwill" mission on behalf of the Chinese government, he spent an evening at our house? Is it known that, when in 1949 we arranged an exchange visit which brought a Belgian girl to our house for a few weeks, and my son later to her house in Brussels, she turned out to be the daughter of the General Secretary (or similar high official) of the Belgian Communist Party? Is it known that I am well-acquainted with Dr. Gremlin in the Physics Department here, whose name appeared on the letterhead of the committee organising the Sheffield "Peace" congress? It is true that my social contacts with the Gremlins are not very frequent, but rather more so than with Dr. B.

Is it known that I was greatly disappointed when the proposal to get Professor C.F. Powell of Bristol for our physics chair fell through, in spite of his reputation as a left-winger?

Is it known that I had recently in my department two Polish scientists who came with scholarships awarded by the Polish government,[400]

[399]Peter Wooster, a Cambridge crystallographer, and his wife Nora held pronounced left-wing views.

[400]Jerzy Rayski and Jan Rzewuski.

and that we were on friendly terms with both of them, one even staying in our house for a few nights?

I suppose it is known that my brother, like myself, has a Russian-born wife (this is pure coincidence); but is it known that, before she married him in about 1930, she was a secretary in the Russian Trade Delegation in Berlin?

On the other hand, is it known that my wife is the cousin of Kannegiesser, a counter-revolutionary who assassinated Uritzky, who was then the head of the Russian secret police? With the same, very rare surname, she was never allowed to forget this connection. Is it known that her family was banished from Leningrad in 1935, partly because of this old connection, and partly no doubt because of her marriage to a foreigner. They have not dared communicate with her for several years, and we do not know whether they are still alive.

I hope you will appreciate that this letter is not written in a spirit of complaint. I appreciate the great importance of security checks, and I have great sympathy for the difficulty in the way of such investigations, in particular in the case of intellectuals who rarely present a case without complications. But with so many facts in my case that could be open to unfavourable interpretation, if the attention of the experts is caught by just the two men in question, one naturally wonders whether they have missed many of the other facts, or whether they have a rather curious sense of proportion.

Thank you once again for the frankness and courtesy with which you talked to me about all this.

Yours sincerely,

[R. Peierls]

[526] Rudolf Peierls to Herbert Fröhlich[401]

[Birmingham], 26.4.1951
(carbon copy)

Dear Fröhlich,
Thank you for your letter.[402] May I say first of all how pleased I was that your election to the Royal Society has come through at last.[403]

About the other problem, I was meaning to write to you as soon as I knew you were back.[404] The point is the following: in your treatment of the interaction of electrons with the lattice you are in fact writing down a one body problem, but because of the Pauli principle this results in terms which are essentially two-electron terms when you [...][405] a statistical problem. We have set ourselves the problem, first of all of finding out what those terms look like if one writes down the equation for two electrons and also to see whether there are any other two-electron terms.

We have already found the following facts: firstly if one transforms your terms into coordinate space they have the peculiar property that they do not seem to depend on the distance between the two electrons. In other words if your two electrons are represented by wave brackets, which are known to be 10 cm apart, the interaction term would be equally strong, as if they were close together; secondly there are terms which come only in the two electron problem and the most important of these can be described as one electron emitting a phonon and the other absorbing it. These, however, are not diagonal terms and if one restricts oneself exactly to first-order perturbation theory they are of no importance, however, one should not be pedantic about first-order

[401] Herbert Fröhlich (1905–1991), studied in Munich where he obtained his Ph.D. under Sommerfeld in 1930. He left Germany in 1933, initially working in Leningrad with Joffe, before emigrating to England to work with Mott in Bristol. In 1948 he accepted a Chair at Liverpool where he remained until his retirement in 1973.

[402] Letter could not be located.

[403] Herbert Fröhlich was elected fellow of the Royal Society on 15 March 1951.

[404] Herbert Fröhlich had been on leave at Purdue University, Indiana.

[405] Word missing in text.

theory since one is dealing practically with a continuous spectrum, so that one has to consider cases in which the electron[s] have exchanged quite small amounts of energy. I believe now that if one takes such terms into account they should remove the apparent discrepancy and in that respect it is satisfactory that the discrepancy is important particularly for large volumes of metal when of course it is also particularly important not to regard the energy levels as far apart. At present we do not claim that the result w[ould] necessarily make any difference to the application of the theory, but I am fairly certain that an exploration of these problems will help one to understand what goes on. I am fairly confident that by our methods it will be possible to solve the two-electron problem with some simplifications but without assuming perturbation theory, but it remains to be seen whether this will throw sufficient light on the many body problem so as to be independent of perturbation theory for the physically important case.
Yours sincerely,

R.E. Peierls

[527] H. Fröhlich to Rudolf Peierls

Liverpool, 1.5.1951

Dear Peierls,
Many thanks for your letter and congratulations.[406]

I was very interested to see that you have ideas which might lead to an interaction which also depends on the coordinates in ordinary space. I felt that in a better approximation than I have used this should be expected. My reasons were as follows:

Consider classically the interaction of two sources of a field. When the sources are at rest then the interaction no doubt depends on their distances only. When they move, velocity dependent terms have to be introduced which become increasingly important when the velocity of the sources approach the critical velocity of the field (velocity of sound

[406] Letter [526].

or light). In my case the velocity of the particles is large compared with the velocity of sound. The velocity dependent terms would therefore be expected to be very important. But nevertheless there should be some dependence on the distance in the approximation in which one does not neglect s/v entirely. I realise, of course, that this is merely an intuitive argument and not at all compelling. I am very glad to see therefore that you have better reasons to expect such a term.

You may be interested, in this connection, that I have seen Wentzel and he appears to feel satisfied now that the main ideas of the theory are correct. I agree with him, of course, that one ought to find improved methods. The great difficulty in this case is that metal theory is essentially based on the hypothesis of free electrons (modified by periodic potentials only). The question then arises at what point exactly should one make this hypothesis. One might, for instance, make it for a Fermi distribution at absolute zero with any number of free vibrations excited. In this case, one would be concerned with the difference of the properties of other distributions from the f_0 distribution only.

I should like to have your opinion, if possible, on a further point concerning Bardeen's interpretation of the magnetic properties. His argument is essentially based on his belief that in the case of very small effective mass the Landau-Peierls formula breaks down in such a way that homogeneous fields are not possible. I think however that his argument is wrong. The L-P formula can be derived from a calculation of the change in the energy of an electron gas due to the presence of a vector potential A; and the result can be written as

$$1/2|\chi_0|(\text{curl } A)^2.$$

Here, strictly speaking, A is a self=consistent vector potential and very nearly curl $A = b$ and $\neq$ H, for otherwise one would obtain the wrong Lorentz forces. In general the above energy is still to be corrected because it contains, twice, the terms due to the magnetic interaction. This interaction does, of course, depend on the macroscopic shape of the specimen. The simplest way of dealing with it is to consider the

case of a slab between poles of a permanent magnet, in which case the interaction is negligible. One then finds, immediately, that χ_0 is not the susceptibility χ but rather $\chi = \mu\chi_0$ or $|\chi| = |\chi_0|/(1+4\pi|\chi_0|) < 1/4\pi$ so that the contradiction which Bardeen believes to derive (negative μ) never arises.
Yours sincerely,

H. Fröhlich

[528] Rudolf Peierls to Raymond Priestley

[Birmingham], 22.5.1951
(carbon copy)

Dear Vice-Chancellor,
I have discussed with Prof. Garner[407] the question of an appointment in my department which raises a point of principle on which your opinion would be very welcome.

The vacancy arises with the resignation of Mr. H.McManus, who was a lecturer in Grade II, his present salary being £650. His appointment was only a temporary one, the reason being that I knew he wanted a change and was going to leave as soon as he could find a satisfactory job elsewhere, but the job is definitely part of my establishment. I would now like to recommend for appointment Dr. R.H.Dalitz, who is at present a research fellow, his salary being £550 p.a. I have no doubt that Dr. Dalitz is a most suitable person for the job, but the question is what his salary should be.

Dalitz is now 26. He graduated with 1st Class Honours in Mathematics at Melbourne in 1944, and with 1st Class Honours in Physics in 1945. He spent a short time doing research there, which led to two C.S.I.R. Reports on hydro-dynamic problems, and was then awarded a research scholarship that took him to Cambridge. He spent the two

[407] Frederic Horace Garner (1893–1964), Professor of Chemical Engineering at Birmingham University, 1942–1960.

years 1946–48 in Cambridge and the following session in Bristol, and came here as a research fellow in 1949. He completed last summer a thesis which got him the Cambridge Ph.D., and this work led to two papers which have been accepted by the Royal Society and will appear shortly. He is one of the best research men in my department, very mature for his age and is at the moment completely responsible for one graduate student and takes a great share in helping and advising others. In addition, under our customary arrangement by which research fellows also help in undergraduate teaching, he has for the last two years been giving all the lectures that we normally give as part of Honours Physics Course IV. These are the hardest courses in my department, because a lot of information has to be conveyed in a very short period of lectures to people not familiar with mathematical techniques, and Dalitz has acquitted himself very well; in fact, I understand from the physicists that of the five lecturers who have given these courses recently, Dalitz is the only one who has made a real success of this job.

Dalitz himself is somewhat reluctant to take on a teaching job instead of continuing in his present research fellowship, and I can see that from his point of view to take on the extra teaching duties would be a sacrifice. I still hope to be able to persuade him to accept this if we can make it worth his while. It would not be reasonable, in any case, to ask him to take on extra duties without an increase in salary, and I feel therefore, we could in any case not offer him less than £600 p.a. I would very much like, however, to be able to offer him £650.

This is still saving, since McManus, if he had stayed, would presumably have been entitled to £700 next year. In this connection it is relevant that a year ago I replaced a lecturer Grade II who was then leaving by an appointment in Grade III, so that the balance of seniority on my staff would not be unreasonable. The question of principle that arises is therefore solely concerned with Dr. Dalitz's age. Incidentally, Dr. Dalitz is married and has two children. He is, of course, entitled to family allowance both as a research fellow and as a lecturer.

I have discussed this problem also with Professor Moon, who knows Dalitz and knows in particular of the teaching work he has done. He is prepared to support the recommendation for £650 as an exceptional case.

I believe Garner is going to talk to you about this case at the next opportunity, but the purpose of this letter is to put all the facts before you to save time.
Yours sincerely,

R.E. Peierls

[529] Robert Oppenheimer to Rudolf Peierls

Princeton, 27.8.1951
(carbon copy)

Dear Rudi:
I was very glad to get your letter of August 24th. Just three days ago I was in the office of the Science Advisor of the State Department listening to the many tales about visas in general and in particular, and I am concerned that nothing should interfere with your planned stay at the Institute.[408] Your status here is that of a Member, analogous to a fellow supported by a foundation. The formal purpose of your coming is to enable you to pursue your own studies; the Institute makes and can make no formal demands whatsoever on your time. You are thus fully eligible to come on a Visitor's visa, and I am attaching a formal statement indicating that we have been designated by the Department of State as a sponsor of the Exchange Visitor Program. The grant that we make to you is not a salary; and whatever great tangible or intangible benefits we derive from your being here, our grant and your coming are in no way conditioned by their realization. Quite recently, I have been told that you are coming next month with an official passport; and it has not been clear to me that it would be wise or even possible for you to use this for the extended visit next Spring. The Department of State

[408] Rudolf Peierls, during the McCarthy era, on several occasions, had problems with obtaining the necessary visa for the United States. After problems in obtaining a visa for attending the nuclear physics conference in Chicago in October 1951, again his attempts to get the travel documents necessary to embark on his visit to Princeton in 1952 met with delays, but were approved eventually.

is aware of our great interest in having you and our desire that nothing delay or interfere with your visit, and you must let us know if there are any ways in which we can help in bringing this about.

My present plans are to attend the Chicago conference. But since I have kept almost no plans in the last months, I do not know with assurance that I will actually be there. I do very much hope to see you, both in connection with the basic problems which will be taken up in Chicago and in connection with the always important question of effective collaboration between our two countries. I hope you will arrange, in connection with this visit, to spend a little time in Princeton and to stay with us if at all possible. Allison has asked that I tell you of my willingness to join the discussion on meson theories at which you will be a chairman. You know, I expect, that I have no great clarifying light to shed; but you know me well enough to know that I am always willing to join in a discussion.

With every warm good wishes from both of us to both of you,

[Oppie]

[530] Rudolf Peierls to Claude Bloch

[Birmingham], 13.12.1951
(carbon copy)

Dear Bloch,

In Copenhagen you mentioned to me the very nice idea that an operator like $F(x_1 - x_2)\bar{\psi}(x_1)\psi(x_2)$ could be made gauge-invariant if one expressed $\psi(x_2)$ by the Taylor series and replaced in it the derivative by the gauge invariant operator, so that formally the expression becomes

$$F(x_1, -x_2)\bar{\psi}(x_1)e^{x_1 - x_2)(\frac{\partial}{\partial x_\mu} - \frac{ie}{e}A_\mu)}\psi(x_1) \qquad (1)$$

We have taken to the study of such theories again and I would therefore be glad to know whether you have published your suggestion and whether you would allow me to refer to is in a talk I have promised to give to the American Physical Society in New York in February.

The following results may interest you. (perhaps you have already obtained them yourself):

1. The above expression can be evaluated explicitly and is essentially identical with

$$F(x_1, X_2)\bar{\psi}(x_1)e^{ie\int_{x_1}^{x_2} A_\mu dx_\mu}\psi(x_2)$$

where the integral in the exponent is to be taken over the straight line joining the (four-dimensional) points x_1 and x_2.

2. It is nevertheless not possible to assume a Lagrangian of the form

$$L_e + L_f + \iint dx_1^4 dx_2^4 dx_3^4 F(x_1 x_2 x_3) A_\mu(x_3)\bar{\psi}(x_1)\gamma_\mu e^{ie\int_{x_1}^{x_2} A_\nu dx_\nu}\psi(x_2) \tag{2}$$

where L_e and L_f are the usual free-electron and free field functions, since this leads to gauge invariant functions only if

$$\frac{\partial j_\mu}{\partial x_\mu} = 0 \tag{3}$$

where $j_\mu(x_3)$ is the factor of $A_\mu(x_3)$ in the action principle (2).

3. One does, however, obtain a consistent scheme for the action principle:

$$L_f + \iint \bar{\psi}(x_1)G(x_1, x_2)e^{ie\int_{x_1}^{x_2} \tilde{A}_\mu dx_\mu}\psi(x_2)d^4x_1 d^4x_2 \tag{4}$$

where

$$G(x_1 x_2) = \left(\gamma_\mu \frac{\partial}{\partial x_\mu} - \mathrm{im}\right) F(x_1 - x_2) \tag{5}$$

and F is an invariant form factor of the usual kind. $\tilde{A}_\mu$ is a "smeared" potential

$$\tilde{A}_\mu(x_1) = \int \varnothing(x_1 - x_2)A_\mu(x_2)d^4x_2 \tag{6}$$

This second "smearing" process may not in fact, be necessary. It is easy to see that (4) reduces to the usual theory if F and $\varnothing$ are taken as δ functions.

We are rather attracted by the equation (4) and are proposing to investigate it further.

I shall be in Princeton for the Spring Term, leaving this country on 4 January, so unless you reply before Christmas could you write to the Institute for Advanced Studies at Princeton?

With kindest regards,

Yours sincerely,

R.E. Peierls