Session 1

History

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1.1 Rapporteur talk: Solvay Redivivus, by Peter Galison

1.1.1 *Three Miracles*

It seems impossible: that knowledge-transforming confrontations occurred not just once but several times within a series of small, highly-planned scientific meetings. Yet this is what made the "Solvay Councils" - a few of them at least - turning points in the history of modern science. Of course such luminous moments are not all that the Solvay meetings accomplished. These assemblies served as sites for powerful reviews of the field, and catalysts for intellectual and social networks that helped advance research. Importantly, the Solvay meetings also played a crucial role in setting and maintaining international scientific exchange after some of the worst times of a deeply troubled twentieth century.

But now, in these first moments of a revived Conseil Solvay, we do not need the spirit or structure of this century-long tradition to propel forward either international scientific exchange or the comprehensive reviews of specialized domains. We have other, larger and more efficacious means to advance these goals. There are journals like Physics Reports or Reviews of Modern Physics that commission effective, easily accessible summaries of fields. We have international meetings that effectively disseminate the current state of action in the many subfields of physics. And of course, in successful pursuit of coordinated international scientific activity, we have regional and worldwide agencies, institutes, and laboratories. Assembling fifty or so participants, however remarkable they may be, would not, in any case, be the best way to advance the flow of talent and ideas from country to country and continent to continent.

No, when, as a historian of modern science, I look back on the Solvay phenomenon, what is so remarkable is that, once in a while, something powerful happened in the concatenation of differing points of view, something that might not be anticipated. By way of historical introduction to this 23rd Solvay Conference, I would like to call a few of those moments to mind [1]. Of course, pace Santillana, history is not going to repeat itself - whether we study it or not - but it might be worthwhile to see just how remarkable a few of those moments were: a few days in October-November 1911, and then several more in the Octobers of 1927 and 1930. First, a few words about the beginning.

The successful inauguration of the Solvay meetings required at least three (pre-string) miracles. A first miracle demanded a precise balance between two philanthropic forces. On one side, this required that the powerful Belgian industrialist Ernest Solvay was passionate enough about science (including his own pet theory) to put astonishing resources into the Conseil. On the other side, again because he had a favored theory, it is all the more impressive that he was willing to step aside intellectually, and to leave the physics to the leading physicists of his time with no interference. Without resources - or with interference - the convocation could never have succeeded.

Solvay's boundless hopes for science built on his central enterprise: his development - and industrial prosecution - of a new way to synthesize soda using limestone, salt and ammonia. Soda Ash (sodium carbonate) had a vast range of uses, from the manufacture of glass and medicines to soaps and photography. What Solvay discovered was a method to produce the sodium carbonate from salt (sodium chloride) and limestone (calcium carbonate) - this replaced an earlier method that required the same inputs along with sulfuric acid and coal, a more wasteful and expensive product. Until the massive Wyoming deposits were discovered years later, the Solvay method, applied to a far-flung network of production and distribution, garnered a significant share of the world market and won him his fortune.

Solvay was a liberal, in its several senses, someone who held a scientific hope for a new world in which individualist politics, technically-based nutritional reform, and a novel sociology would transform the whole and alleviate suffering. He created institutes (in physiology [1892], and sociology [1902], school of commerce [1904]) that he saw as instrumental to ground the movement of societal change.

Despite the range of these ambitious pursuits, none of these institutionalized programs got at the depth of what Solvay sought. He also wanted a physics. His own theory, of which he was quite proud, carried the title "Gravitique" (1887), and put gravity at the source of all processes; it was to be more basic than energy. It would embrace the historical contributions of Kepler, Newton; it would include molecular contact and ethers ... If one had to form a slogan to capture Solvay's ambition, it might be this: he wanted both a politics of physics (that would use the physical sciences as the basis for a reformation of society), and a physics of politics (in which conduct of politics would be stripped down to its law-like, technical aspects).

Through a mutual friend, Solvay met Walther Nernst (then director of the Second Chemical Institute at the University of Berlin) in the spring of 1910 and told Nernst of his ideas about physics and his hope that it would be possible to assemble some of the greats of the discipline to discuss how things stood [2]. Nernst liked the idea and let Solvay know that he (Nernst) had already tried to interest colleagues in a discussion of the new quantum ideas - unfortunately, Max Planck had demurred, suggesting that such an assembly was premature. No doubt encouraged by Solvay, Nernst reiterated to the industrialist that far from being premature, "there seems that there could hardly have been a time as the present when such a Conseil could more favorably influence the development of physics and chemistry ... " [3] Nernst's idea was to focus on a set of seven problems that included (among others): the derivation of the "Rayleigh formula" of radiation; the Planck radiation law, the theory of quantized energy, and the relation of specific heats and the theory of quanta [4].

The first Conseil de Physique gathered in the luxurious Hotel Metropole in Brussels on Monday 30 October 1911 for a meeting that lasted through Friday 3 November-call it Solvay-1. Ernest Solvay welcomed the assembled luminaries with his theory (every delegate had already received a reprint of his views). Even if Nernst had not chosen the topic, as he had, Solvay himself might have : he expected to produce on his own an "exact and therefore definitive" account of the finite fundamental elements of the active universe. Be that as it may, the combination of Nernst and Solvay put the program in good stead-funded and scientifically connected.

The second miracle began when Solvay concluded his preamble with the words: "I am now happy to cede my place to our eminent president, M[onsieur] Lorentz." From that instant forward, Hendrik-Antoon Lorentz took charge of the meeting. Lorentz was gracious about Solvay's support and intervention, and gently but firmly guided the conversation even when the consensus violated Lorentz's most deeply-held convictions about the direction of the field. Indeed, to understand Lorentz's response to the new quantum physics, it is foolish to represent him as a reactionary, but instead we need to read his response to the new through the lens of his earlier achievements.

When Lorentz entered the electrodynamic world, it was saturated with complex theories of the ether, in which this most subtle of substances could be dragged, moved, compressed, sheared, and spun. There was a long-standing tradition that sought, since the time of Maxwell, to derive the existence of charged particles from stable flows in the ether-like smoke rings in the air. Ether models proliferated - mathematical models, physical models, analogy models. From this baroque and confusing mix, Lorentz extracted a theory of extraordinary simplicity: a rigid ether in which particulate electrons moved by a simple, (now eponymous) force law coupled to Maxwell's equations. The Dutch theorist produced miracles from this combination: he could explain myriad effects from reflection and refraction to the magnetic splitting of spectral lines in the Zeemann Effect; for the last of these he received the Nobel Prize in 1902. As an encore in 1904, he continued his reasoning about electrons, electrodynamics, and the ether, producing first an approximate and then the exact form of his transformations.

I'll come back to this notion later, but to anticipate - what Lorentz did was to present a principled, focused vision of what physics might be based on a kind of "radical conservatism" - a pushing of the electron plus rigid-ether program that he himself had followed with such stunning results. But both earlier (and at Solvay) his own commitments never stopped from encouraging views that were orthogonal to it. By doing so, the participants never had far to look to catch a glimpse of where physics was taking on a new complexion, and where it had been.

In fact, I do not think that anyone else but Lorentz could have guided these first Solvay Councils beginning in October 1911. Einstein certainly could not have - at the time of the first Council, at age 32, he was far from presenting an ecumenical, sage-like demeanor to the world. Remember, at that point he'd held a "real" academic job for just two years. He was driven, impatient, biting in his sarcasm, and wouldn't or couldn't hide his disdain for bad or wrong-headed approaches. The great mathematician-physicist Poincaré certainly couldn't have guided the 1911 discussions. True, by then, at various times, he had executed high-level administrative functions with great aplomb, true too he was learned beyond measure even in this illustrious crowd. But Poincaré had a blind spot toward young Einstein (not facilitated by Einstein's refusal to cite any of Poincaré's relativity work), and toward Einstein's new, heuristic quantum ideas. Max Planck, of course, had launched reasoning about the quantum discontinuity when he proposed, back in 1900, his conditions on the energy oscillators could have in the walls of a black-body cavity; but he had already shown himself uneasy with various aspects of the quantum phenomena - and indeed found premature Nernst's very idea for the Solvay event. Perhaps Nernst himself could have taken the lead but, as great a scientist as he was, in 1910-11 Nernst commanded neither the scientific authority of Lorentz (Nernst's best quantum work was just coming into view, his Nobel Prize a decade away), nor the personal admiration so many scientists had for the Dutch theorist [5].

The third miracle, of course, was the presence and prior contributions of Albert Einstein. Remember, this was not the Einstein of world-historical fame - that Einstein did not yet exist, and wouldn't until Einstein had finished his general relativistic work and the public had gazed over the large-type headlines of November 1915, announcing the results of Arthur Eddington's eclipse expedition. Nor was the "Solvay Einstein" the "molecular Einstein" - the Einstein who had cracked the Brownian motion problem, extended the Boltzmannian science of statistical mechanics, and provided a remarkable analysis of molecular dimensions. For these accomplishments he was, in the physics community, quickly and widely hailed. Finally, surprising as it might be in retrospect, the Einstein of Solvay-1 was also not the Einstein of special relativity. Relativity, and its second cousins the electron theories, and the ether theories were not on the intellectual order of the day that Nernst had circulated. Instead, the relevant Einstein, the Einstein whose work set much of the agenda of Solvay-1 was the light-quantum Einstein.

Back in 1905, Max Planck and Wilhelm Wien were as well known as Albert Einstein was obscure. After a period of quite painful professional marginality (one friend wrote his father that Einstein was half starving), in 1902, Einstein very happily shed his unemployment [for] a job in the Bern patent office. It was from there that Einstein wrote his friend Conrad Habicht in May 1905: "So, what are you up to, you frozen whale, you smoked, dried, canned piece of soul, or whatever else I would like to hurl at your head ... !" Why haven't you sent your dissertation, Einstein demanded. " ... I promise you four papers in return, the first of which I might send you soon ... The paper deals with radiation and the energy properties of light and is very revolutionary." (For the second and third, Einstein told Habicht he would report on atomic sizes using diffusion and dilute solutions, and the third would analyze Brownian motion.) "The fourth paper is only a rough draft at this point, and is an electrodynamics of moving bodies which employs a modification of the theory of space and time ... ." [6]

Einstein's remark about the "very revolutionary" light quantum was the only time he ever referred to any part of his own work in such strong terms. For startling as his contribution to relativity (the electrodynamics of moving bodies) was, there was something deeply disturbing about the light quantum - far more disturbing even than Max Planck's ambivalent stance toward the quantization of oscillator energy levels. Reaction to Einstein's work on molecular sizes and Brownian motion came quickly, responses to the light quantum were slower and much more reserved. But by the end of the decade, the idea had begun to catch fire. Nernst wrote to the English physicist Arthur Schuster on 17 March 1910: "I believe that, as regards the development of physics, we can be very happy to have such an original young thinker, a 'Boltzmann redivivus'; the same certainty and speed of thought; great boldness in theory, which however cannot harm, since the most intimate contact with experiment is preserved. Einstein's 'quantum hypothesis' is probably among the most remarkable thought [constructions] ever; if it is correct, then it indicates completely new paths [for the ether and molecular theories;] if it false, well, then it will remain for all times 'a beautiful memory'." [7] It was but a short time after penning this encomium that, in July 1910, Nernst wrote to Solvay: "It would appear that we currently find ourselves in the middle of a new revolution in the principles on which the kinetic theory of matter is based ... As has been shown, most notably by Planck and Einstein ... contradictions are eliminated if ... the postulate of quanta of energy ... [is] imposed on the motion of electrons and atoms. [It] unquestionably mean[s] a radical reform of current fundamental theories." [8]

In 1909-1910, the quantum discontinuity finally hit home among experts - and it was then that Einstein took on a new stature within the physics world. Nernst played a key role in that recognition, but he was not alone. Here then was the third miracle: at just the time that Solvay was willing to fund, Nernst to organize, and Lorentz to lead, Einstein had vastly deepened the quantum discontinuity, and in so doing had launched a research program that during the organization of the first Solvay Council, was on the cusp of recognition by many of the world's most illustrious theoretical physicists [9].

Shuttling between these high-ranking theorists, Nernst's set agenda of the first Solvay meeting. It was Nernst who early and powerfully recognized the importance of the "Boltzmann redivivus" who had just emerged from the patent office; it was Nernst who served as the lead contact with Solvay, and it was Nernst who induced Lorentz to preside over the whole. But once the conference began, the exchanges among physicists threw into relief the novelty of what was afoot. I want to focus on a few of those pivotal interactions, those involving (in the main) Einstein in conversation with Lorentz, Poincaré, and Bohr.

1.1.2 *SOLVAY -1: Einstein-Lorentz, Einstein-Poincaré*

In relativity theory we rightly see a transformation of space and time, a shift from absolutes of space and time to the quasi-operationalized concepts of ruler - measured distances and light-coordinated clocks. But seen from another angle, Einstein's greatest contribution in the paper was his introduction of a way of thinking, an invitation to reason toward symmetries in the explanatory structure of the theory that he demanded match the symmetries of the phenomena. If the phenomena were symmetric with respect to changes in the inertial frame of reference (magnet moves toward coil versus coil moves toward magnet) then the theory should show that same invariance. Similarly, we rightly attend to the quantum discontinuity of Einstein and Planck as a founding document in the history of quantum mechanics. But again, looked at from the point of view of the history of physical reasoning, we can see Einstein's paper differently: not just as a contribution to the nature of light but to the broader idea that in physics sometimes what is needed is not a full-blown theory but instead a heuristic, a provisional step, one that might not even appear consistent with other dearly-held tenets.

Einstein himself put it this way at the Council: "We all agree that the so-called quantum theory of today, although a useful device, is not a theory in the usual sense of the word, in any case not a theory that can be developed coherently at present. On the other hand ... classical mechanics...can no longer be considered a sufficient schema for the theoretical representation of all physical phenomena." [10]

Precisely this "incoherence" that did nothing to stop Einstein struck the great mécanicien, Henri Poincaré, as disastrous. It fell to Poincaré to summarize one session in Brussels. And having heard Einstein and his colleagues pronounce on the quanta - having heard them try to navigate a corpuscular as well as wave-theoretical notion of light - his view was dim indeed: "What the new research seems to put in question is not only the fundamental principles of mechanics; it is something that seems to us up to now inseparable from the very notion of a natural law. Can we still express these laws in the form of differential equations? Furthermore, what struck me in the discussions that we just heard was seeing one same theory based in one place on the principles of the old mechanics, and in another on the new hypotheses that are their negation; one should not forget that there's no proposition that one can't easily prove through the use of two contradictory premises." [11] Poincaré's points are two. First, differential equations give the moment-to-moment unfolding of phenomena that Poincaré took to define the very object of physics. This was what Newtonian gravitational physics had bequeathed us. But Poincaré surely also had in mind the world that issued from Maxwell's electrodynamics and all its subsequent modifications. For out of that mix had come the "new mechanics" embracing the electrodynamics of moving bodies that Lorentz and Poincaré himself had fought so hard to create - along with (though Poincaré never much liked his contributions) young Einstein. But this newfangled quantum hypothesis was something else. Just insofar as it was not representable in terms of a differential equation it threatened to depart from "the very notion of a natural law."

Second, and in some ways even more distressing, Poincaré pointed in his peroration to the flat-out contradiction that seemed to be the rule in the discussions he'd just heard about the quantum hypothesis. On the one hand his colleagues were happy to invoke "the principles of the old mechanics" - namely electrodynamics and the wave theory of light alongside Lorentz's law for the motion of particulate charges. On the other hand, Einstein and those who were following him were invoking "the new hypotheses that are their negation" - the quantum of light. As logic dictates, from contradictory premises follows anything at all. This invocation of light-as-wave and light-as-particle threatened not only to undermine itself but the very idea of science.

What to make of Poincaré's response to the deliberations? All too often he is depicted as a crusty reactionary, but the characterization is far from helpful. He was perfectly willing to accept, even to celebrate quite radical changes in physical theory; he helped invent and embraced the "new mechanics" of a modified electrodynamics that included Lorentz's hypothesis of contraction - and Poincaré's own version of the "local time." In thinking about the three-body problem, Poincaré helped usher in what became non-linear dynamics. But he would not countenance a mechanics that defied representation in differential-equation form, nor embrace simultaneously what he considered to be the proposition A and the proposition not-A.

It is telling, for example, that, in the discussion at Solvay, Poincaré was perfectly willing to consider modifying the very foundation of the electrodynamics of moving bodies: "Before accepting these discontinuities which force the abandonment of our usual expression of natural laws through differential equations, it would be better to try to make mass depend not only on speed as in electromagnetic theory, but also on acceleration." Poincaré took his suggestion to heart and set to work. Not long after the meeting adjourned, he reported back to his colleagues, as printed in the minutes, that "on my return to Paris, I tried calculations in this direction; they led me to a negative result. The hypothesis of quanta appears to be the only one that leads to the experimental law of radiation, if one accepts the formula usually adopted for the relation between the energy of resonators and the ether, and if one supposes that the exchange of energy can occur between resonators by the mechanical shock of atoms or electrons." It is a remarkable concession, indicative not just of his stance toward the particular physics question, but also of the engagement that occurred in Brussels [12]. Perhaps Poincaré's reversal on such a central matter could be a model for our and future Solvay Councils: fight hard, calculate hard - and concede defeat when the work demands it.

It was not just Poincaré who understood and recoiled at the upset heralded by the quantum of light. Lorentz too registered the conundrum faced by physics: "At this moment, we are far from a full [spiritual] satisfaction that the kinetic theory of gases, extended to fluids and dilute solutions and to systems of electrons, gave ten or twenty years ago. Instead, we have the sense of being at a dead end, the old theories having shown themselves more and more impotent to pierce the shadows that surround us on all sides. In this state of things, the beautiful hypothesis of energy elements, put forward for the first time by M. Planck and applied to numerous phenomena by Einstein and Nernst and others, has been a precious glimmer of light." However hard it was to grasp fully the implications of the physics of quanta, he agreed that they were not contradiction with older ideas like actions or forces. Yet "I understand perfectly that we have no right to believe that in the physical theories of the future all will conform to the rules of classical mechanics." [13] Not classical mechanics, but some kind of mechanics. For Lorentz insisted that some "mode of action" be uncovered that would explain the discrete acquisition of energy - only such an understanding would lead to "the New Mechanics which will take the place of the old one." [14]

Lorentz and Poincaré were flexible enough to consider another "new" mechanics. That mechanics might have the form of a mechanics reflecting the dynamics of the electrodynamics of moving bodies. Or it might be an as-yet uncovered system of mechanics appropriate to the quantum steps of energy allowed in the molecular oscillators. What was clear, however, was that they wanted above all a mechanics, some mechanics that would be expressed through a definite, visualizable microphysics (Lorentz) or differential equations (Poincaré). Not unreasonably, both wanted a theory. In fact, back in 1903, when Poincaré addressed the graduates of Ecole Polytechnique he had rhetorically asked his audience what they, this extraordinary group of scientists, military men, industrialists, and national leaders had in common. The answer: mechanics. Mechanics, modifiable, improvable - not at all frozen the age of Newton - was the centerpiece of reason about the world [15].

Knowing the importance that Lorentz and Poincaré attached to mechanics (new or old), it is instructive to register what may be the only "conversation" ever recorded between Poincaré and Einstein. It went like this:

Poincaré to Einstein: "What mechanics are you using?"
Einstein: "No Mechanics."
de Broglie: "[This] appeared to surprise his interlocutor."

"Surprise" wasn't the half of it. Poincaré had modified his mechanics for his take on the electrodynamics of moving bodies, he had altered mechanics to make room for his work on the three-body problem. And he was willing to contemplate a transformation here, in the excruciatingly difficult domain of the quantum. But Einstein's answer, "no mechanics" was, for Poincaré, an impossible one. For it was precisely the heuristic non-theory that marked one of the key aspects of Einstein's intervention. Here was an aspect of light - so Einstein was telling his contemporaries - that was not yet built into anything worthy of the name theory. And yet this discrete aspect must, in the long run, become part of our account of the physical world. How it should be incorporated remained an open question, but after reasoning about scattering, photoelectric, and specific heat effects that the quantal aspect of the world would stay remained for Einstein, and an expanding circle of others, a deep conviction.

After Friday 3 November 1911, the participants scattered. Einstein wrote to one of his friends, "H. A. Lorentz is a marvel of intelligence and tact. He is a living work of art! In my opinion he was the most intelligent among the theoreticians present." But of Poincaré, Einstein had a much dimmer view: " ... Poincaré was simply negative in general, and all his acumen notwithstanding, he showed little grasp of the situation." But for all their disagreements, amicable with Lorentz, more strained with Poincaré, Einstein left an indelible impression. A formal, rather distant Lorentz wrote to Einstein in February 1912 about the invitation he was extending to Einstein to succeed him as professor of theoretical physics at Leiden: "Personally, I cannot tell you how tempting the perspective to work in constant contact with you would be. If it were granted to me to welcome you here as a successor and at the same time as a colleague, it would fulfill a wish I have cherished in silence for a long time, but unfortunately could not express earlier. As one becomes older and the power of creativity slowly fails, one admires even more the good spirits and enthusiastic creative impulse of a younger man." [16] Nothing in the clash of ideas entered into the domain of the personal - on the contrary, it led Lorentz to appreciate Einstein's distinctive approach all the more.

What did Poincaré make of Einstein? Despite their non-meeting of minds, the older physicist wrote to Pierre Weiss in November 1911, recommending Einstein to the Swiss Federal Institute of Technology. Einstein, Poincaré wrote, was "one of the most original minds I have known." He had "already taken a very honorable rank among the leading scholars of his time. ... He does not remain attached to classical principles, and, in the presence of a proble of physics, is prompt to envision all the possibilities." Not all of Einstein's ideas would bear fruit, Poincaré, added, but if even one did, that would suffice. "The future will show more and more the value of Mr. Einstein", Poincaré ended, "and the university that finds a way to secure this young master is assured of drawing from it great honor." [17] As they weighed the discussion unfolding before them at Solvay-1, Lorentz and Poincaré were shocked at what was happening to physics, to the physics that they had done so much to develop. But they were listening - as few have done any time in the face of something so disturbing to everything for which they stood.

1.1.3 *Ignoramus, Ignorabimus at Solvay-5 and Solvay-6*

The calamity of World War I crashed through the physics community. French and Belgian scientists in particular dug a trench between them and their German homologues, neither forgetting nor forgiving. During the Great War, Max Planck, Ernst Haeckel, and Wilhelm Roentgen (joining ninety other luminaries) had issued a fierce defense of the destruction at Louvain, the invasion of Belgium - and linked German high culture to the iron will of the military. Einstein and two colleagues responded with a blast of their own with their plea for European civilization. At some risk, Einstein struggled to maintain relations between belligerents. As late as 1927 there were difficulties inside the Council itself as Solvay-5 entered its final planning stages - one handwritten note in the Solvay archives noted one participant would not be there "puisqu'il y a des allemands"; another memo, just before Solvay-5 opened, came to Lorentz on 14 October 1927: "I know that patriotism is intransigent, as much in those who attack as those who defend; these are territories of an infinite sensitivity." [18]

If the postwar political scene was overheated, the scientific one was as well. After two years of extraordinarily intense work, by mid-1927, its creators were celebrating a triumphant quantum theory. Werner Heisenberg had extended the Niels Bohr's work into his "matrix mechanics," eschewing the visual elements that he found too redolent of a dead classical physics. Erwin Schroedinger had hoped to counter the Heisenbergian anti-visual with his wave mechanics - and Max Born had but recently offered the probabilistic interpretation of the theory's wave function.

Lorentz's commentary as Solvay-5 advanced offers us an extraordinary ring-side seat, not because he was a participant in assembling the new quantum mechanics - but precisely because he shows us what one of the great theoretical physicists of all times thought of a theory that had departed so radically from the microphysics of fields and electrons that he worked so hard to put in place. Indeed, in many ways the new quantum theory departed from Lorentzian precepts even more dramatically than Einstein's heuristic light particle had back at Solvay-1 in 1911. And now, in earnest, Einstein leveled his own criticisms at the new theory in one of the greatest dialogues that has ever taken place in the history of science - the battle between Einstein and Bohr. Though in brief compass one cannot possibly do justice to this long story, it is worth recalling some of what happened as Lorentz and now Einstein faced a very different kind of physics.

To set things in perspective, it is useful to begin with Lorentz's reaction to

Bohr and his young quantum mechanicians' work. But one has to see it from Lorentz's angle of vision - and for that one has to recall Lorentz's extraordinary several decades of success with electron theorizing as the charged particle moved, oscillated, radiated. Only then can we grasp just how sensible it must have seemed for him to urge caution before abandoning the tools that so very recently had yielded such extraordinary structures. Microphysics was new, visualizable, calculable: it taught us how to think all the way down to the micro-structures that explained the splitting of spectral lines in the presence of a magnetic field. It gave a clear picture of what was happening in the reflection and refraction - it was at long last a way to put paid to the promise Maxwell's theory made to join optics to electrodynamics. This was a theory and a way of doing science worth fighting for. Here's Lorentz as he reflected at Solvay-5 on the new work by his young quantum colleagues: "We want ... to make an image in our imagination [esprit]. Until now we have always wanted to form images through ordinary notions of time and space. These notions may be innate; in any case, they were developed by our personal experience, by our everyday observations. For me, these notions are clear and I admit that I cannot form an idea of physics without these notions. The image that I want to form of phenomenon must be absolutely distinct and defined, and ... we cannot form such an image except in space and in time. " [19] To read these words of Lorentz is to see the innovation of Bohr, Schrödinger, and Heisenberg from the shadow it cast on turn-of-the-century electron physics. "What Mr. Bohr does is this: after an observation he limits anew the wave packet in a way that will represent for him that which the observation taught us on the position and movement of the electron. Then begins a new period during which the packet diffuses again, up to the moment when a new observation permits us to effect the reduction once more. But I would like an image of all this during an unlimited time." [20] Yet it was exactly this image that the new quantum mechanics would not - could not - provide. Lorentz looked at the physical description and said: ignoramus - we do not know (but we could). Bohr and Heisenberg replied, essentially, ignorabimus: we cannot know.

Einstein too sought a way out of a description of nature that to him seemed too impoverished to catch nature in the fullness that it, in principle, should allow: "In my opinion, the difficulty can only be resolved in this way: one does not only describe the process using Schroedinger's wave, but at the same time one localizes the particle during propagation. I think that de Broglie is right to look in this direction. If one works only with Schroedinger's waves, in my opinion, the second interpretation of [psi squared] implies a contradiction with the postulate of relativity." [21]

Lorentz reckoned that if one wanted to have an idea of an electron at one moment and then at another, one had to think of its trajectory, "a line in space." "And if this electron encounters an atom and penetrates it, and after several adventures it leaves the atom, I make a theory in which this electron maintains its individuality; that is I imagine a line following which that electron passes through the atom."

Now it could be very difficult - but it should be possible. Could electrons suffer transformations? Fine. Could an electron melt into a cloud? Fine too. But then as far as Lorentz was concerned it was our duty to figure out how that transformation takes place. But one could not, a priori, forbid ourselves to conduct research into such questions. Put another way, Lorentz could perfectly well allow that we could not answer the question now - that was acceptable. But banish it forever? That seemed to him absurd. "If we abandon old ideas one can always maintain the old names. I want to conserve this ideal from older times of describing everything that happens in the world by distinct images." Lorentz welcomed new theories (be they wave-like or particulate) - so long as it was possible to keep clear and distinct images of the underlying process. He was never one to say, as some physicists had, that the older knowledge was "in principle" complete. That we don't know (ignoramus) was fine. That we can't know (ignorabimus) was too much.

Lorentz was even willing to have that probability calculable by the square of the wave function. "But the examples given by Mr. Heisenberg teach me that I would have attained all that experience allow me to attain." This limitation was what was at stake for the Council's leader. It was the idea that this notion of probability should be put at the beginning of our physics that bothered Lorentz. At the conclusion of our calculations, a probabilistic result would be no more consequential for the meaning of physics than other results that issued from a calculation. But make probability part of the axiomatic, the a priori, and Lorentz bridled: "I can always guard my determinist faith for fundamental phenomena ... .Couldn't one keep determinism in making it the object of a belief? Must we necessarily establish indeterminism in principle?"

Though they may have split in various particulars, Lorentz and Einstein both were bothered by in-principle ignorance. And here, toward the end of Solvay-5, Einstein advanced a picturable thought experiment (first figure). Imagine particles entering the device from point 0 and then spreading toward a circular screen. Einstein then posits that there are two imaginable roles that the theory might play. Possibility 1: the theory with its psi-squared only claims to describe an ensemble of particles, not each particle one-by-one; Possibility 2: "the theory claims to be a complete theory of single processes. Every particle which moves towards the screen has a position and a speed, insofar as they can be determined by a packet of de Broglie-Schroedinger waves with small wavelength and angular opening." Bohr rejected Einstein's choice, noting that the particles could not be considered in isolation - it is only permitted for us to consider the system as a whole, diaphragm 0 and particles. The position, (and therefore the momentum and momentum transfer to the particles), of the diaphragm matters in what we can say about the particle and its subsequent path [22].

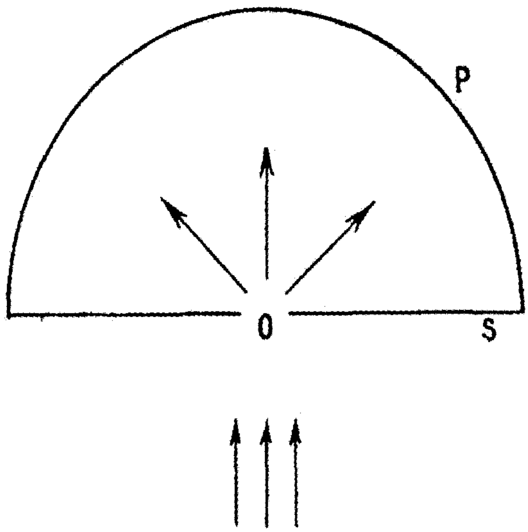

Fig. 1

Poincaré died not long after the first Solvay Council; Lorentz just a few months after Solvay-5. Marie Curie wrote for the volume in honor of what "the soul of our meetings" had meant to the assembled physicists: "The illustrious master teacher [maître] and physicist, H.-A. Lorentz, was taken from us [4] February 1928 by a sudden sickness - when we had just admired, one more time, his magnificent intellectual gifts that age had not diminished." He had, in Curie's view, brought to the meetings diplomacy, students, followers, and collaborators - he was in all senses master of the field. Lorentz was "the real creator of the theoretical edifice that explained optical and electromagnetic phenomena by the exchange of energy between electrons and radiation in accordance with Maxwell's theory. Lorentz retained a devotion to this classical theory. It was therefore all the more remarkable that his flexibility of mind was such that he followed the disconcerting evolution of quantum theory and the new mechanics." [23]

Langevin took over for Solvay-6 following Lorentz's death; it was Langevin who guided discussion in October 1930. By then, the themes we have been discussing split Einstein ever further from Bohr, and their struggle continued long after they left the Metropole - through Einstein-Podolsky-Rosen and beyond. Throughout, Einstein famously pressed ahead in his quest to show that the problem with contemporary quantum mechanics was that it was incomplete - that our state was one of ignorance in fact, not in principle: ignoramus not ignorabimus. One Gedankenexperiment followed another, for which just one, perhaps the most remarkable, must stand for many.

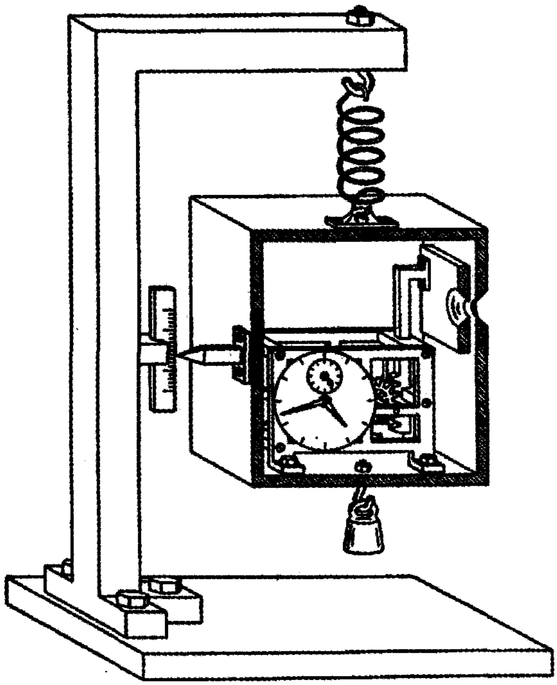

Fig. 2

Einstein: imagine a clock-like device like that shown in the second figure. Einstein: Bohr and his quantum mechanics forbid knowing both time and energy. What if we measure the time a photon is released (using a clocklike release mechanism of figure 2) and we weigh the source of light before and after photon released? Then we would have the time of the photon's launch and its energy using the weight change of the mechanism and E = mc2. Wouldn't that outsmart the uncertainty principle showing we could measure quantities more accurately than the theory allows? In other words, ignoramus: we can be clever, and get both quantities where the theory tells us we must choose.

This, one contemporary observer recorded, left Bohr miserable. It did indeed look as if Einstein had shown the theory to be incomplete because it could not fully represent physically measurable quantities. "During the whole evening [Bohr] was extremely unhappy, going from one to the other and trying to persuade them that it couldn't' be true, that it would be the end of physics if Einstein were right; but he couldn't produce any refutation." [24] Finally Bohr found a solution: To weigh the box is to fix its position vertically. But the uncertainty principle then requires the box have an uncertainty in vertical momentum. Reweighing the box requires a time T for the box to settle, and a corresponding uncertainty in height. But this uncertainty in height corresponds, by the gravitational red-shift, to an uncertainty in the clock speed (as you, Einstein, proved!). So because we fixed photon energy we cannot know exactly when the photon is launched. Ignorabimus!, Bohr, in essence, replied. One cannot, in fact, have both the time of photon release and its energy. The theory is not incomplete.

Einstein's and Bohr's debate did not end there, of course. A proper account would wend its way through the rest of their Solvay debate, to the Einstein-Podolsky-Rosen thought experiment and eventually up to Bell's theorem. But even this one snapshot captures the great stakes involved for each of the antagonists as they faced off at the Solvay Council: the shape and even the existence of physics. [25]

1.1.4 *Solvay Redivivus*

The casualties of World War I were so terrible, the furor over militant nationalism so deep, that it took all of Hendrik Lorentz's - and Albert Einstein's - force and good will to repair the damage to international science. The meetings of the early 1920s, but especially of Solvay-5 (1927) and Solvay-6 (1930) were a salve to these wounds. No doubt the collective achievement of quantum mechanics, and the role that the Solvay Councils played in its interpretation, were and were seen to be lasting, international accomplishments. The composite nature of the work was visible down to the traces any physicist made as he or she calculated anything: Heisenberg's matrices, Schroedinger's wave equation, Dirac's notation and relativistic extension, Bohr's twin doctrines of complementarity and correspondence.

But where World War I only provisionally damaged the Solvay Councils, the rise, rule and ruin of Nazism did much worse. Putting together a serious Continental European conference in physics after Hitler's election in January 1933 became almost impossible. And by the time the war ended, twelve and a half bloody years later, the return address of physics had changed. The great institutes of Born and Bohr lay shattered. Many of the younger European physicists were gone - deported, killed, or driven into exile. In the United States the physics community was entirely restructured by the vast war projects of radar and the atomic bomb; theoretical physics underwent a tremendous expansion. All this meant that at just the time European physics lay most devastated, American physics stood poised on a vast armamentarium of theoretical and experimental technique, accompanied by a budget beyond anyone's wildest pre-war dreams. Working conferences like that of Shelter Island (June 1947) or Pocono Manor (March-April 1948) stood as exemplars for the new generation of theorists now beginning to take their skills to civilian issues: Richard Feynman, Julian Schwinger among them [26].

Consequently, by the time the Solvay Council met in late 1948, the world of physics had turned. Reading the pages of the proceedings, one senses a tone more of elegy than of excitement. J. Robert Oppenheimer is there, but no one breathed a word about a transformed role for physics and physicists that necessarily accompanied a world with nuclear weapons. Meanwhile, the major figures from the pre-war era issued one cautionary note after another. Dirac worried about the infinities, hoping that they could be done away with in a formulation of the physics that would by-pass perturbative methods altogether. For his part, Bohr was turning to quantum philosophy beyond physics - epistemic worries far from the pressing concerns of younger, more pragmatic theorists who wanted to dig in, build accelerators and calculate things. Bohr's worry was to set limits to visual, and he slammed those who persisted in the search for the visual, even in a disastrous encounter at the Pocono meeting where he ripped into Feynman for trying with diagrams to visualize the unvisualizable. Heisenberg too added his warning: fundamental lengths could limit the validity of all present theory.

When Feynman did speak at Solvay-12 (1961), by which time his diagrams were to physicists what hammers and saws were to carpenters, he took the opportunity to push on his own theory of QED as hard as possible. Not for him a wished-for revolution based on fundamental length, hoped-for mathematical by-passes or philosophical introspection: "In writing this report on the present state of [QED], I have been converted from a long-held strong prejudice that it must fail [other than by being incomplete] at around 1 Gev virtual energy. The origin of this feeling lies in the belief that the mass of the electron ... and its charge must be ultimately computable and that Q.E.D must play some part in this future analysis. I still hold this belief and do not subscribe to the philosophy of renormalization. But I now realize that there is much to be said for considering [Q.E.D.] exact ... to suggest definite theoretical research. This is Wheeler's principle of 'radical conservatism'." [27] Writing for the Solvay record seems to push people to think hard about even long-held views; Feynman was no exception. But in retrospect, even Feynman's radical conservatism wasn't radically conservative enough - there was a huge amount of structure still to be plumbed in renormalization (starting with the renormalization group).

Today we begin deliberations at Solvay-23 on the quantum structure of space and time. Solvay-1 had before it the problem of the light quantum, Solvay-5 and Solvay-6 confronted the brand-new quantum mechanics. We too have our agenda before us - emergent spacetime, singularities, and new structures perched between physics and mathematics. No doubt basic and high-stakes questions will arise: what do we want from our explanations in matters of cosmology? What kind of singularities do we face and what will they mean for the current campaign in theoretical physics? What is the proper place for the anthropic principle? Is the hunt for a physics that will pick out the masses, charges, and coupling constants the right goal for physics- and is our present inability to do so a matter of our not knowing (ignoramus)? Or is it rather that it is not given to us to know these things - that our ability to ask the question presupposes that we are in this universe and no other? Is it the case that we can never know those values as deduced from first principles (ignorabimus)? These and other questions of similar difficulty about the right place of string theory are just the kind of hard task that Nernst, Lorentz, Einstein, and Bohr had in mind when they gathered in this grand site almost a hundred years ago.

Western philosophy is often said, in only part exaggeration, to be a long footnote to Plato. The physics of this last century, documented - and now re-awakened - in the Conseils de Solvay, might be similarly seen as a long elaboration of the great dialogue Einstein initiated between relativity and the quantum. It remains our ground. Colleagues: Welcome back to Solvay.

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[2] (1993): 59-82, hereafter, "Witches Sabbath"; on Nernst more broadly see D. Barkan, *Walther Nernst and the Transition to Modern Physical Science*, (Cambridge: Cambridge University Press, Cambridge, 1999).

[3] Louis d'Or and Anne-Marie Wirtz-Cordier, Ernest Solvay (Académie Royale de Belgique, Mémoires de la classe des sciences XLIV Fascicule 2 (1981), p. 50-51; citation on p. 51 (Nernst to Solvay, 26 July 1910). For more on Ernest Solvay the following are very helpful and contain many further references: Jacques Bolle, Solvay: L'homme, la découverte, l'entreprise industrielle (Bruxelles: SODI); Andrée Despy-Meyer and Didier Devriese, Ernest Solvay et son temps (Bruxelles: Archives de l'université de Bruxelles, 1997); and Pierre Marage and Grégoire Wallenborn, The Solvay Councils and the Birth of Modern Physics (Basel: Birkhäuser Verlag, 1999).

[4] D'Or and Wirtz-Cordier, Solvay, pp. 51-52.

[5] In addition to the Barkan work cited above, there is a very interesting piece by Elisabeth Crawford, "The Solvay Councils and the Nobel Institution," in Marage and Wallenborn, Birth, pp. 48-54 in which she shows how Nernst positioned the Solvay Council as a competitor to Svante Arrhenius's Nobel Institute.

[6] Einstein to Habicht, 18 or 25 May 1905, in The Collected Papers of Albert Einstein, vol. 5, transl. Anna Beck (Princeton: Princeton University Press, 1995), pp. 19-20, on p. 20.

[7] Nernst to Schuster, 17 March 1910, cited in Barkan, "Witches [Sabbath]," p. 62.

[8] Cited in Didier Derviese and Grégoire Wallenborn, "Ernest Solvay: The System, The Law and the Council," in Birth, p. 14.

[9] On the timing of recognition of the quantum discontinuity see T.S. Kuhn, Black-Body Theory and the Quantum Discontinuity, 1894-1912. (Chicago: University of Chicago Press, 1978, 1987).

[10] Einstein, Solvay-1, in P. Langevin and M. de Broglie, "La Théorie du Rayonnement et les Quanta," Paris: Gauthier-Villars, 1912), 30 October-3 November, 1911, p. 436. Hereafter Solvay-1.

[11] Poincaré: Solvay-1, p. 451.
[12] Poincaré, Solvay-1, p. 453.
[13] Lorentz, Solvay-1, pp.6-7.
[14] Lorentz, Solvay-1, p. 7.
[15] On Poincaré, Polytechnique, and mechanics, see Galison, Einstein's Clocks (New York: W.W. Norton, 2003), pp. 48-50.
[16] Cited in A.J. Kox, Einstein and Lorentz, "More than Just Good Colleagues," Science in Context 6 (1993): 43-56, on p. 44.
[17] Poincaré to Weiss, cited in Galison, Einstein's Clocks, p. 300.
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[20] Lorentz, Solvay-5 (1927), 287.
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[22] Einstein, from Solvay-5, cited by Marage and Wallenborn in Marage and Wallenborn, Birth, pp. 167-68.
[23] Institut International de Physique Solvay, Electrons et Photons, Paris: Gauthier-Villars, 1928), n.p. Hereafter Solvay-5.
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[25] There is a *nearly infinite* large literature on the Einstein-Bohr debate, but the locus classicus is in the contribution by Bohr and Einstein's reply in P. Schilpp, Albert Einstein Philosopher and Scientist (Northwester University and Southern Illinois University: Open Court, 1951).
[26] On postwar expansion in American physics see Galison, Image and Logic: A Material Culture of Microphysics (Chicago: University of Chicago Press, 1997), esp. ch. 4. For an excellent account of the development of quantum electrodynamics, see S. S. Schweber, QED and the Men Who Made It (Princeton: Princeton University Press, 1994); and a splendid account of the spread of Feynman diagrams, David Kaiser, Drawing Theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics (Chicago: University of Chicago Press, 2005).
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1.2 Discussion

T. Damour I was surprised by what you said concerning the only conversation between Poincaré and Einstein. My understanding from the text of Maurice de Broglie was that the conversation did not concern quantum mechanics as you seem to convey, but "la mécanique nouvelle." Although later "meécanique nouvelle" meant quantum mechanics, I think this conversation makes more sense if it means relativistic mechanics, which Poincaré always called "mécanique nouvelle." This is the difference between Poincaré always having to assume some microscopic mechanics to discuss relativistic physics and Einstein making general postulates and not having to make dynamical postulates about microscopic physics. Do you really intend to challenge this view?

P. Galison I am quite certain it is not about the new mechanics. All of Poincaré's comments, both in the session and afterwards, concern the problem that the quantum goes outside the bounds of the description under differential equations. In the context of that discussion, it very directly concerns the quantum of light. It is true there was a misreading of that conversation in Banesh Hoffmann's book where it was attributed to a discussion about the new mechanics and relativity. In the context of the discussion however, not only where it's located, but in view of Poincaré's other comments about differential equations and the nature of mechanics, it is clear that what he is referring to is an absence of description under differential equations. This is what he considers to be a necessary, if not sufficient, condition for having a mechanics at all. That seems to be the basis on which that particular exchange is framed. On relativity I looked long and hard for direct exchanges between Einstein and Poincaré, but they just do not talk about that to each other. The session that Poincaré was running, and in which Einstein participated, had nothing to do with relativity, it was only about the quantum of light.

T. Damour But that quote by Maurice de Broglie is separate from the Solvay context. It is in a text which is not in the Solvay proceedings.

P. Galison It is about a confrontation that occurred at the Solvay conference in 1911. That is the only time Poincaré and Einstein met.

T. Damour The whole point is whether "mécanique nouvelle" is relativistic mechanics or quantum mechanics. I think it makes more sense if it is relativistic mechanics, it really gives meaning to this conversation.

P. Galison I think that if you look at the context you will see it is about the quantum, and it has to do with this question of differential equations which was so crucial. I reviewed the citations for that. I would love it to be about relativity, it would be much more interesting to me for other reasons, but it is not.

S. Weinberg In the story about Einstein and Bohr's famous argument, what bothers me is that Bohr is supposed to have won by invoking general relativity. But what if general relativity were wrong? Then, does that mean quantum mechanics would be inconsistent? I suspect, although I have not studied the debate in detail, that the issue actually arises only within a framework in which there is a gravitational redshift and it does not really depend on the validity of general relativity.

P. Galison That seems right. That is to say, what is required for the clock speed to depend on the gravitational potential is much less than the full structure of general relativity.

G. Gibbons Basically, it is energy conservation and nothing more. In fact in that little thought experiment, you are not using gravity, it could be any force that is holding up the clock. I think it is completely decoupled from general relativity, I agree entirely with what Weinberg has said.

P. Galison Rhetorically what Bohr profits from is that he is referring back to Einstein's own work.

G. Gibbons It is certainly true that Einstein discovered the gravitational redshift, but it is decoupled from general relativity.