WEYL ENTERING THE 'NEW' QUANTUM MECHANICS DISCOURSE†

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Abstract. Early in 1925 H. Weyl finished his great series of publications on the representation of Lie groups and started the studies for his *Philosophie der Mathematik und Naturwissenschaften* delivered to the editors in summer 1926. He was in touch with M. Born and got to know of the developments in the Göttingen group around Born, Heisenberg and Jordan in early summer 1925. After a conversation with Born in September 1925 he started to develop ideas of his own how to quantize the mechanical observables of a system and communicated them to Born and Jordan in October 1925. In these letters he proposed the basic idea of a group theoretic approach to quantization, which he presented to the scientific public in his 1927 paper *Quantenmechanik und Gruppentheorie*. This paper had a long and difficult reception history for several decades.

1. Introduction

There are many stories to be told about Hermann Weyl's involvement in quantum mechanics. Among them:

– 1918–1923, Weyl's rising awareness of the role of quantum structures in the constitution of matter during his phase of a dynamistic matter explanation in the frame of the Mie-Hilbert-Weyl program,[1]
– 1925–1927, backstage involvement in the new QM leading to his published contribution to the topic (Weyl 1927) quoted as QMG in the sequel,
– 1927/28 lecture course and book publication on *Groups and Quantum Mechanics*, quoted as GQM,
– 1929, Weyl's contribution to the general relativistic Dirac equation (Fock-Weyl theory),
– 1930ff. study of the role of spin coupling for molecular bounds, second edition of GQM, and later contributions.

This is too much for a conference talk. Here I shall concentrate on the second item. For items 4 see (Scholz 2005), for 3 and 5 (Scholz 2006), for item 1 with a link to 4 (Scholz 2004*a*, Scholz 2004*b*).

Date: August 3, 2007 .

† Conference contribution *History of Quantum Physics 1*, Berlin, July 2-6, 2007.

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[1]This relates to Weyl's role in the *Forman thesis*. P. Forman's version should not be taken literally, however, und has to be reconsidered and drastically corrected.

The reason for this choice is that Weyl's contributions to QM during the period 1925 – 1927 may be of particular interest for this conference. It contains a direct communication between him and two of the main protagonists of the 'new' quantum mechanics, Max Born and Pascual Jordan, in late summer and early autumn 1925. Moreover we find here a *very early* formulation of a structural approach to quantization. This may be helpful to understand historically, and perhaps more widely, a mathematician's view of the relationship mathematics — physics in foundational aspects.[2]

During the years 1924 and 1925 Weyl worked hard on his great series on the representation theory of Lie groups, beautifully described in (Hawkins 2000). It was finished in April 1925 (Weyl 1925/1926). Then he started intense reading work for his contribution to *Philosophie der Mathematik und Naturwissenschaften* in *Handbuch der Philosophie*. He took this task very seriously; it occupied him well into the year 1926. In his letter to Born of Sep. 27, 1925, he characterized himself as "fettered to the deep swamp of philosophy (gefesselt an das tiefe Moor der Philosophie)". This did not hinder him, on the other hand, to follow very closely what was going on inside the Göttingen group of theoretical physics during 1925, with its great step towards a new kind of mathematized quantum mechanics.

Already earlier in the 1920s, Weyl had found two topics in modern physics, in which group representations became important. The first topic was in general relativity and differential geometry. The representation theory of the special linear group $SL_n\mathbb{R}$, showed that there is a *mathematical reason for the structural importance of tensors in differential geometry.*[3] The second point became clear to him, when Elie Cartan proved that the algebraic part of Weyl's *analysis of the space problem* could be answered by the use of group representations more easily.[4]

When Weyl learned from M. Born in September 1925 of the recent Göttingen work in quantum mechanics, he immediately tried to link the new theory to the representation theory of groups. Already in autumn 1925 he started to investigate inhowfar group representations might help to understand the Göttingen quantization procedure and, in particular, how they shed light on the role of the Heisenberg commutation rule (section 2 below). His approach led directly to the study of abelian ray representations. He even made first steps towards what later turned into Weyl quantization (section 3). Both ideas were first published in QMG (Weyl 1927). This contribution ends with some remarks on Weyl's indications on interacting and/or relativistic systems (section 4) and a short outlook on reception and repercussions of Weyl's proposals (section 5).

[2]Here "foundational" is understood in the sense of foundations of physics, not as foundations of mathematics.

[3]All irreducible representations of $SL_2\mathbb{R}$, arise as subrepresentations of tensor products of the natural representation with certain symmetry properties. Thus infinitesimal structures of classical differential geometry have a good chance to be expressible in terms of vector and tensor fields.

[4](Hawkins 2000, Scholz 2004*b*)

2. From commutation rules to abelian ray representations

Shortly before leaving for a visit to the US of America, Max Born visited Zürich in September 1925 and informed Weyl on the recent progress in QM made at Göttingen. This led to a short correspondence between Born and Weyl (Weyl to Born Sept. 27, 1925, Born to Weyl Oct. 3, 1925) and, after Born's departure, between Weyl and Jordan (Weyl to Jordan Oct. 13, 1925, Jordan to Weyl, Nov. 1925, and two postcards, Weyl to Jordan, Nov. 23 and 25, 1925). The correspondence took place in the time lapsed between the submission and publication of Born's and Jordan's common article on quantum mechanics (Born 1925).[5]

Apparently Born had explained the content of this paper to Weyl. The latter wrote to Born:

> Dear Herr Born!
> Your Ansatz for quantum theory has impressed me tremendously. I have figured out (zurecht gelegt) the mathematical side of it for myself, perhaps it may be useful for your further progress . . . (Weyl Ms1925*a*).[6]

In his "Zurechtlegung des Mathematischen" Weyl immediately passed over from the *matrices* p, q etc. of Born, Heisenberg, and Jordan to the "one-parameter group which results from the infinitesimal transformation $1 + \delta p$ by iteration" (the δ was introduced by Weyl to characterize the limit process δp for $\delta \to 0$),

$$P(s) = e^{ps} = 1+sp+\frac{s^2}{2!}p^2 \dots , \qquad Q(t) = e^{qt} = 1+tq+\frac{t^2}{2!}q^2 \dots \quad (s,t \in \mathbb{R}) .$$

That is, he considered the p, q as *infinitesimal generators of 1-parameter groups*. He did not touch analytical details of domains of definition etc..[7]

This move was motivated by Weyl's recent experiences with Lie groups. There he had studied the consequences of the shift from the infinitesimal group (now, Lie algebra) to the finite one (the Lie group itself) for the corresponding representations. He had found valuable new insights by such a shift from the infinitesimal to the integral (finite) point of view.

In the context presented to him by Born, Weyl realized that the Heisenberg commutation rule for the infinitesimal operators

$$pq - qp = \hbar\, 1, \tag{1}$$

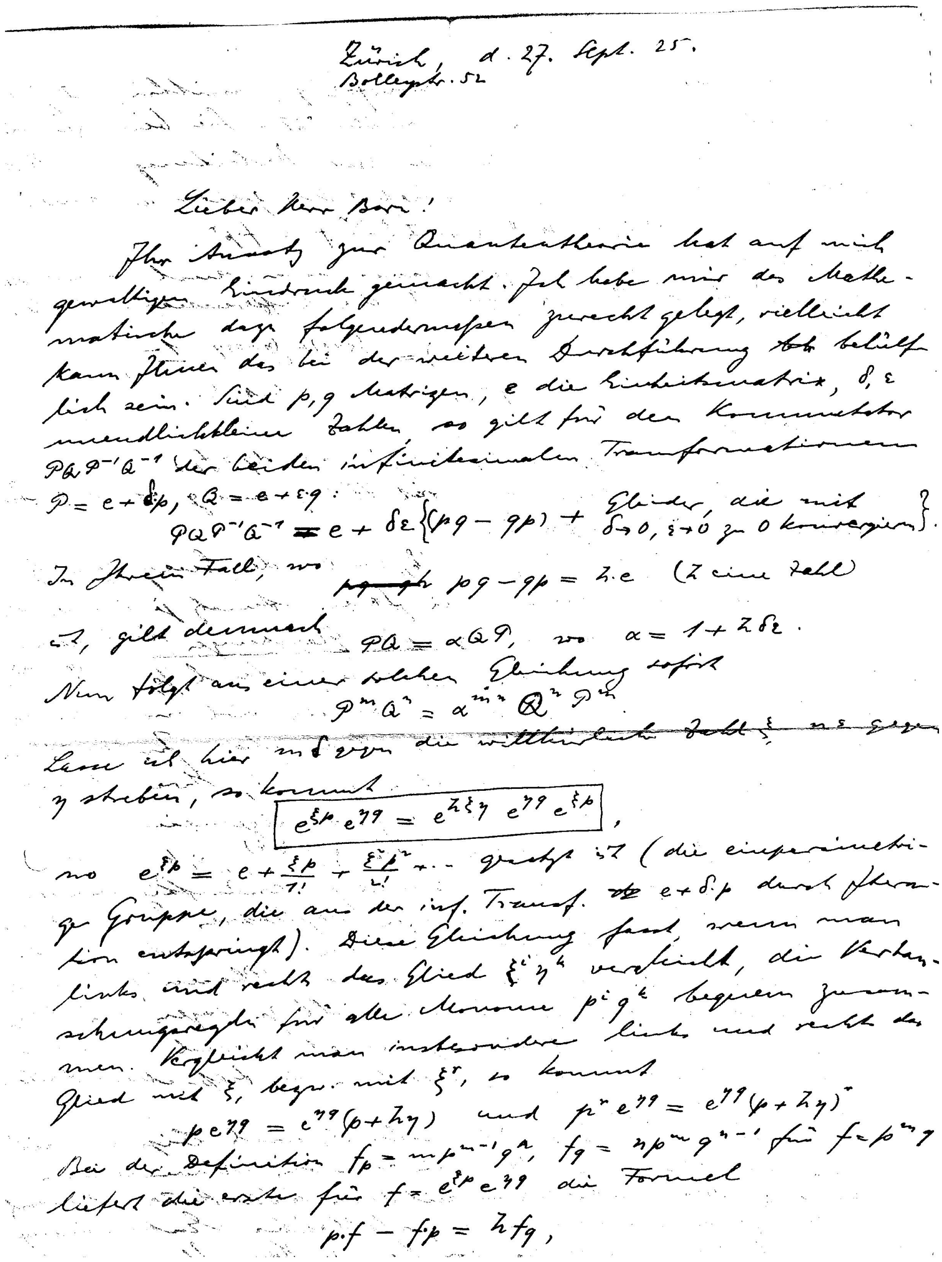
with $\hbar$ "a number" (Weyl omitted the imaginary unit i), was (and is) equivalent to the quasi-commutation rule for the integral operators $P := P(1), Q := Q(1)$

$$PQ = \alpha QP, \qquad \text{with } \alpha(s,t) \text{ complex factor.} \tag{2}$$

[5]Submission date, 27 Sept., publication 28 November 1925.

[6]The German original is even nicer: "Lieber Herr Born, Ihr Ansatz zur Quantentheorie hat auf mich gewaltigen Eindruck gemacht. Ich habe mir das Mathematische dazu folgendermaßen zurecht gelegt, vielleicht kann Ihnen das bei der weiteren Durchführung behülflich sein . . . ."

[7]This state of affairs pertained well into 1927. Even Hilbert in his lecture course in winter semester 1926/27 and (Hilbert/Nordheim/vonNeumann 1927) did not specify domains of definition (Majer e.a. 2008). This situation started only to be changed with von Neumann's first own contribution on the foundations of QM. Cf. (Lacki 2000).



Zürich, d. 27. Sept. 25.
Bolleystr. 52

Lieber Herr Born!

Ihr Ansatz zur Quantentheorie hat auf mich gewaltigen Eindruck gemacht. Ich habe mir das Mathematische dazu folgendermaßen zurecht gelegt, vielleicht kann Ihnen das bei der weiteren Durchführung behilflich sein. Sind p, q Matrizen, e die Einheitsmatrix, δ, ε unendlichkleine Zahlen, so gilt für den Kommutator $PQP^{-1}Q^{-1}$ der beiden infinitesimalen Transformationen $P = e + \delta p$, $Q = e + \varepsilon q$:

$$PQP^{-1}Q^{-1} = e + \delta\varepsilon\{(pq - qp) + \text{Glieder, die mit } \delta \to 0, \varepsilon \to 0 \text{ zu } 0 \text{ konvergieren}\}.$$

In Ihrem Fall, wo $pq - qp = h \cdot e$ (h eine Zahl) ist, gilt demnach $PQ = \alpha QP$, wo $\alpha = 1 + h\delta\varepsilon$.
Nun folgt aus einer solchen Gleichung sofort

$$P^m Q^n = \alpha^{mn} Q^n P^m.$$

Lasse ich hier $m\delta$ von ~~die willkürliche Zahl~~ gegen ξ, $n\varepsilon$ gegen η streben, so kommt

$$e^{\xi p} e^{\eta q} = e^{h\xi\eta} e^{\eta q} e^{\xi p},$$

wo $e^{\xi p} = e + \frac{\xi p}{1!} + \frac{\xi^2 p^2}{2!} + \dots$ gesetzt ist (die eingliedrige Gruppe, die aus der inf. Transf. $e + \delta \cdot p$ durch Iteration entspringt). Diese Gleichung fasst, wenn man links und rechts das Glied $\xi^r \eta^s$ vergleicht, die Vertauschungsregeln für alle Monome $p^r q^s$ bequem zusammen. Vergleicht man insbesondere links und rechts das Glied mit ξ, bezw. mit ξ^r, so kommt

$$p e^{\eta q} = e^{\eta q}(p + h\eta) \quad \text{und} \quad p^r e^{\eta q} = e^{\eta q}(p + h\eta)^r$$

Bei der Definition $f_p = m p^{m-1} q^n$, $f_q = n p^m q^{n-1}$ für $f = p^m q^n$ liefert die erste für $f = e^{\xi p} e^{\eta q}$ die Formel

$$p \cdot f - f p = h f_q,$$

FIGURE 1. Weyl to Born, 27 Sept. 1925, page 1

This looked like an easy reason for the validity of Heisenberg commutation in quantum mechanics, where states were represented by functions or vectors up to a non- vanishing complex factor only, if normalized up to a phase factor α, $|\alpha| = 1$, "...which one [could] deny any physical meaning" (Weyl Ms1925*a*).

In this sense the integral version (2) of the commutation rule seemed more basic to Weyl. He started to explore first conclusions of it for algebraic expressions of p and q, which we do not go into here (we come back to this question in the discussion of Weyl's letter to Jordan).

Born's reaction was polite,

> Dear Herr Weyl,
> It was a great pleasure for me to see that our new quantum mechanics attracts your interest. In the meantime, we have made considerable progress and are now sure that our approach covers the most important aspects of the atomic structure. ...(Born Ms 1925)

But apparently he was not particularly interested in Weyl's proposal. In the last phrase Born referred to his joint paper with Jordan, just submitted to *Zeitschrift für Physik*. Then he continued:

> It is very fine that you have thought about our formulas; we have derived these formulas in our way, even if not as elegantly as you, and intend to publish the subject in this form, because your method is difficult for physicists to access. (ibid)[8]

So Weyl was left alone with his proposal to pass over to the integral version of the infinitesimal transformations.

He saw the opportunity to come back to the question in a direct communication with the younger colleague a little later. On 13 Nov. 1925 he received proofs of Born and Jordan's paper, "against ackowledgement of receipt and by express mail! (eingeschrieben und durch Eilboten !)", as he remarked with some surprise in his first letter to Jordan. He did not go into the details of the paper but referred to his letter to Born, in which he had done some of the calculations in his own approach, and added some other comments.

Jordan replied in November (no day specified in the date) answering that he had seen Weyl's letter to Born. But also he did not take up the idea of passing to the one-parameter integral groups.[9] He added that in the meantime he and Born had found their own way to establish the Heisenberg

[8] "Lieber Herr Weyl,
daß unsere neue Quantenmechanik Ihr Interesse erregt, hat mir große Freude gemacht. Wir sind inzwischen sehr viel weiter gekommen und sind jetzt ganz sicher, daß unser Ansatz die wesentlichen Züge der Atomstruktur richtig trifft. Daß Sie sich selbst mit unsern Formeln beschäftigt haben, ist sehr schön; wir haben diese Formeln uns auch, wenn auch nicht so elegant, hergeleitet und werden wohl in dieser Form die Sache veröffentlichen, weil Ihr Verfahren für die Physiker wohl zu schwer ist. ..." (Born Ms 1925)

[9] "Ihren Brief an Prof. Born habe ich seinerzeit mit Interesse gelesen ..." (Jordan Ms 1925). Apparently Born had handed over the letter to Jordan during the final preparation of the manuscript. It remained in Jordan's hand and is still in his Nachlass (Staatsbibliothek Berlin).

commutation relation "*without* any other precondition" from the "equations of motion"

$$\dot{q} = \frac{\partial H}{\partial p}\ , \quad \dot{p} = -\frac{\partial H}{\partial q}$$

for a Hamilton operator H which could be expressed algebraically by a polynomial in p and q .

In a footnote he added

> When Born talked to you, we still believed that $pq - qp = \frac{h}{2\pi i}\,1$ is an independent *requirement*.[10]

So, even if Weyl's proposal to consider integral versions of 1-parameter operator groups and their natural quasi-commutation rule (2) did not immediately enter the Göttingen discourse on the foundation of QM, he seemed to have triggered second thoughts of the Göttingen physicists on how to derive Heisenberg commutation from basic principles of QM ("without any other precondition"). Born and Jordan succeeded by referring to the Hamilton operator of the system.

On the other hand, it appeared unnatural for Weyl to consider only polynomial expressions in the basic momentum and localization operators p, q for H. In his postcards Weyl mentioned an idea to Jordan, which kind of functions for H (classical Hamiltonian) might be taken into consideration for a quantum analogue of H.

> I conclude that the domain of acceptable functions H is characterized by the Ansatz
>
> $$\int\int e^{\xi p + \eta q}\varphi(\xi, \eta)d\xi d\eta\ . \tag{3}$$
>
> This is less formal than $\sum p^m q^n$ (Weyl Ms1925*c*) (equ. number added, E.S.).[11]

The formula has to be read with the imaginary unit in the exponential, $\int e^{i(\xi p + \eta q)}$. Weyl was used to omit these to "facilitate reading", as he felt.[12] Thus the integral in the postcard to Jordan indicated something like an inverse Fourier transform of φ. It contained the starting point for Weyl's idea of quantization by using operator Fourier integrals. He later explored and extended the idea and published it in 1927 (QMG). We will come back to this point in the next section.

Weyl's idea to look at the operator relations of QM from an integral point of view lay dormant for more than a year. In autumn 1927, shortly before his lecture course on group theory and quantum mechanics started, Weyl finally prepared his s article QMG, (Weyl 1927).[13] There he discussed some basic principles of the representation of physical quantities in QM by Hermitian forms (in particular simultaneous diagonalizability) as well

[10] "Als Born Sie sprach, glaubten wir noch daß $pq - qp = \frac{h}{2\pi i}\,1$ eine unabhängige *Voraussetzung* sei." (Jordan Ms 1925) (emphasis in original)

[11] "Ich komme darauf, den Bereich der vernünftigen Funktionen H durch den Ansatz $\int\int e^{\xi p+\eta q}\varphi(\xi, \eta)d\xi d\eta$ wiederzugeben; das ist weniger formal als $\sum p^m q^n$."

[12] In a slightly different denotational form: "Um der Leserlichkeit willen schreibe ich oft $e(x)$ statt e^{ix}." (Weyl 1927, below equ. (35)). In the postcard he even used the exponential form of denotation $e^{\xi p+\eta q}$ itself.

[13] Submitted October 13, 1927.

as the difference of *pure states* (eigenvectors of a typical observable of the system under consideration) and *mixtures* (compositions of pures states in any mixing ratio).[14]

We are here more interested in part two. Weyl announced that this section

> . . . deals with deeper questions. [. . .]. It is closely connected to the question of the essence and correct definition of a canonical variable. (Weyl 1927, 92)

He continued by criticizing Jordan's paper (1927) which left "completely unclear" how to assign a matrix $f(Q)$ to a function $f(q)$ of position coordinates q. Moreover Weyl considered Jordan's presentation of the concept of canonical variables "mathematically unsatisfactory and physically unfeasible".

> Here I believe to have arrived at a deeper insight into the true state of affairs by the use of group theory. (ibid.)

This insight was gained from extending the approach he had already proposed in his letter to Born in September 1925.

Starting from a Hermitean matrix A, Weyl associated the corresponding anti-Hermitean

$$C := i\,A$$

and considered the unitary 1-parameter group generated by it,

$$U(s) = e^{isA} = e^{sC} \ , \quad s \in \mathbb{R} \ .$$

For an abelian group $\tilde{G} =< C_1, \ldots, C_k >$ generated freely by k such matrices $C_1, \ldots, C_k$, he accordingly got a k-parameter unitary group with typical element

$$U(s_1, \ldots, s_k) = e^{i \sum\limits_{\nu} s_\nu C_\nu} \ , \quad s \in \mathbb{R} \ .$$

In QM the commutation of the C-s and U-s may be weakened. The weakening of the commutation relation for the unitary group elements

$$U(s)U(t) = U(t)U(s) \ , \qquad s, t \in \mathbb{R}^k$$

by admitting phase factors

$$U(s)U(t) = e^{i\alpha(s,t)}U(t)U(s) \ , \qquad \alpha(s,t) \in \mathbb{R} \, ,$$

corresponded to commutation relations for the generators of the form

$$C_j C_l - C_l C_j = i\, c_{jl} \cdot 1 \qquad (4)$$

with skew-symmetric real coefficients (c_{jl}) (*commutator form*).

Weyl argued that for an irreducible group the commutator form is non-degenerate ($|c_{jl}| \neq 0$). By a change of generators it could be normalized to matrix blocks

$$\begin{pmatrix} 0 & 1 \\ -1 & 0 \end{pmatrix} ,$$

i.e., to the normal form of a symplectic matrix.

[14]Von Neumann characterized mixtures in the same year more precisely by a positive Hermitian operator A with sum of eigenvalues $\sum a_\nu = 1$ (trace class operators of trace class norm 1).

For even k, $k = 2n$, the new generators (after change of base) can be written as

$$iP_\nu \ , \quad iQ_\nu \quad (\nu = 1, \dots n)$$

with P_ν, Q_ν Hermitian and

$$i(P_\nu Q_\nu - Q_\nu P_\nu) = c \cdot 1 \ , \quad c = 1, \hbar \ . \tag{5}$$

All other commutators are 0. By obvious reasons Weyl called $P_1, \dots P_n, Q_1, \dots Q_n$ a *canonical basis* for the representation of $\tilde{G}$.

For operators

$$A(s) = e^{i \sum s_\nu P_\nu} \ , \quad B(s) = e^{i \sum t_\nu Q_\nu}$$

$$W(s,t) := A(s)B(t) \ ,$$

the commutation relations acquire the form

$$A(s)B(t) = e^{ic \sum_\nu s_\nu t_\nu} B(t)A(s) \tag{6}$$

The commutative addition for (s,t), (s',t') in $\mathbb{R}^n$ reappears here slightly deformed as:

$$W(s+s', t+t') = e^{-ic<s,t>} W(s,t) W(s',t') \ , \tag{7}$$

where $\quad < s,t > := \sum_\nu s_\nu t_\nu$.

Weyl called the 'deformed' representation

$$\begin{array}{rcl} \tilde{G} := \mathbb{R}^{2n} & \longrightarrow & \mathcal{U}(\mathcal{H}) \\ (s,t) & \mapsto & W(s,t) = A(s)B(t) \ , \end{array} \tag{8}$$

with $\mathcal{U}(\mathcal{H})$ the unitary group of the Hilbert space $\mathcal{H}$, an "irreducible group of abelian ray rotations". Later authors would prefer the terminology *projective* (or *ray*) *representation of* $\tilde{G}$.

Weyl realized that he had found a structural reason, based on group theoretic considerations, for the canonical pairing of basic observables

$$P_\nu, \ Q_\nu \ ,$$

satisfying the Heisenberg commutation relations

$$[P_\nu, Q_\nu] = -i \, \hbar 1 \ . \tag{9}$$

The latter arose naturally as the infinitesimal counterpart of the integral version for the unitary 1-parameter groups

$$e^{isP_\nu} e^{itQ_\nu} = e^{i\hbar st} e^{itQ_\nu} e^{isP_\nu} \ , \tag{10}$$

with its commutator phase shift $e^{i\hbar st}$. In the sequel (10) will be called, as usual, the *Weyl commutation relations*.

Weyl showed that in this situation the spectrum of the localization operators Q was the whole real continuum, $\mathbb{R}$, and the pure states could be characterized by square integrable complex-valued functions on $\mathbb{R}^n =: G$ ($\tilde{G} = G \times \hat{G}$), with $\hat{G}$ dual of G)

$$\psi \in \mathcal{L}^2(\mathbb{R}^n, \mathbb{C}) \quad \text{of norm} \ \ |\psi| = 1$$

on the space of q localizations. Then the operators (8) of the kinematical group $\mathbb{R}^{2n}$ were represented by translations, respectively phase multiplication operators of the form

$$A(s)\,\psi(q) = \psi(q-s)\ , \qquad B(t)\,\psi(q) = e^{i<t,q>}\psi(q)\ , \tag{11}$$

and the canonically paired basis operators became

$$P_\nu : \psi \mapsto i\frac{\partial \psi}{\partial q_\nu}\ , \qquad Q_\nu : \psi \mapsto q_\nu \cdot \psi\ . \tag{12}$$

Weyl commented:

> *We have thus arrived at Schrödinger's version* [of quantum mechanics, E.S.]. (Weyl 1927, 122, emphasis in original)[15]

In the end, the Schrödinger characterization of a free particle turned out to be nothing but a well chosen basis description of the irreducible ray representation of the non-relativistic kinematical group $\mathbb{R}^{2n}$. Moreover, this argument showed that *every irreducible ray representation* of $\mathbb{R}^{2n}$ was *isomorphic to the Schrödinger picture* of a free particle. Weyl concluded:

> *The kinematical character of a physical system is expressed by an irreducible Abelian rotation group the substrate of which* [the set on which it operates, E.S.] *is the ray field (Strahlenkörper) of 'pure cases'.* (Weyl 1927, 118, emphasis in original)[16]

The "kinematical character" of a non-relativistic quantum system with n continuous non compactified degrees of freedom turned out to be of a rather simple nature and universally given by the uniquely determined irreducible unitary ray representation of $\tilde{G}$.

3. Weyl's approach to the quantization problem

We have seen that already in his postcard to P. Jordan Weyl indicated that a Fourier transform kind approach might be helpful to delimit the "domain of reasonable functions H" (3) or, in slightly more generalized terms, for functions which could be considered as candidates for observables. In his 1927 article Weyl came back to this idea and worked it out in some more detail.

After having arrived at the irreducible ray representation of $\tilde{G} \cong \mathbb{R}^{2n}$ with canonical basis

$$iP_\nu,\ iQ_\nu\ , \qquad 1 \le \nu \le n\ ,$$

he turned to the interrelation between the quantum system characterized by it and the classical system with n continuous degrees of freedom, which could be assigned to the former in a natural (structurally well determined) way. The latter had the classical momentum and location observables $p_1, \ldots, p_n$, $q_1, \ldots q_n$. Weyl remarked:

[15] *"Damit sind wir bei der Schrödingerschen Fassung angelangt."*

[16] *"Der kinematische Charaktter eines physikalischen Systems findet seinen Ausdruck in einer irreduziblen Abelschen Drehungsgruppe, deren Substrat der Strahlenkörper der 'reinen Fälle' ist."*

> A physical quantity is mathematically defined by its functional expression $f(p,q)$ in the canonical variables p,q. It remained a problem, how such an expression had to be transferred to the matrices. (Weyl 1927, 116)[17]

Weyl reminded the reader that the transfer from classical to quantum observables was clear only for pure monomials of the form $f(p,q) = p^k$ or q^l. Already for mixed monomials of the type p^2q it was no longer uniquely determined how the quantum analogue should be characterized, because of the non-commutativity of Hermitian operators, P^2Q, QP^2, PQP etc.. To solve this problem he recommended to use Fourier integrals.

Weyl considered the Fourier transform ξ of f, normalized like

$$\xi(s,t) = (\frac{1}{2\pi})^n \int e^{-i(ps+qt)} f(p,q) d\xi d\eta \; , \quad \text{in short} \quad \xi = \hat{f} \; ,$$

and represented f as the Fourier inverse of ξ,

$$f(p,q) = \int e^{i(ps+qt)} \xi(s,t) dsdt \; , \qquad f = \check{\xi}. \tag{13}$$

It appeared rather natural to pass over to the operator analogue

$$\mathcal{F}(f) := \int e^{i(Ps+Qt)} \xi(s,t)\, dsdt = \int W_{s,t}\, \xi(s,t)\, dsdt \; . \tag{14}$$

For a real-valued square-integrable f, the Fourier transform ξ is itself square-integrable and satisfies the reality condition

$$\overline{\xi}(s,t) = \xi(-s,-t) \; ,$$

which again implies Hermiticity of $\mathcal{F}(f)$ (Weyl 1927, 116f.). Weyl therefore considered the resulting $F := \mathcal{F}(f)$ as a naturally defined quantum mechanical version of the physical quantity related to f. In the sequel $\mathcal{F}(f)$ will be called the *Weyl quantized* observable corresponding to f.

He added:

> The integral development (42) [our (13), E.S.] is not always to be understood literally. The essential point is only that one has a linear combination of the $e(p\sigma + q\tau)$ on the right hand side [σ, τ correspond to our s, t, $e(x) = e^{ix}$, E.S.], in which σ and τ take on arbitrary real values. If, e.g., q is a cyclic coordinate which is to be understood mod 2π (...), the integration with respect to τ becomes a summation over all integer numbers τ; then we have the case of a mixed continuous-discrete group. (Weyl 1927, 117)[18]

[17] "Eine physikalische Größe ist durch ihren Funktionsausdruck $f(p,q)$ in den kanonischen Variablen p,q mathematisch definiert. Es blieb ein Problem, wie ein derartiger Ausdruck auf die Matrizen zu übertragen war."

[18] "Die Integralentwicklung (42) ist nicht immer ganz wörtlich zu verstehen; das wesentliche ist nur, daß rechts eine lineare Kombination der $e(p\sigma + q\tau)$ steht, in denen σ und τ beliebige reelle Werte annehmen können. Wenn z.B. q eine zyklische Koordinate ist, die nur mod. 2π zu verstehen ist, so daß alle in Betracht kommenden Funktionen periodisch in q mit der Periode 2π sind, so wird die Integration nach τ ersetzt werden m üssen durch eine Summation über alle ganze Zahlen τ; wir haben dann den Fall einer gemischten kontinuierlich-diskreten Gruppe."

That is, Weyl envisaged the possibility of the torus group and its dual

$$G \cong (S^1)^n =: T^n\,, \quad \hat{G} \cong \mathbb{Z}^n\,, \qquad \tilde{G} \cong T^n \times \mathbb{Z}^n \tag{15}$$

as an example for a mixed continous-dicrete group and considered Fourier integral quantization on it.

For the existence of the Fourier integral (13) Weyl could refer to recent papers by N. Wiener, S. Bochner, G.H. Hardy and J.E. Littlewood on trigonometric integrals.[19]

He had thus arrived at a theoretically *satisfying solution of the quantization problem for classical observables* depending only on the kinematical variables p and q.

Weyl had now at hand two structurally well defined types of composition of observables on a "kinematical" system, defined by an irreducible ray representation of $\tilde{G} = \mathbb{R}^{2n}$

(i) composition of classical "physical quantities" $f(p,q)$, $g(p,q)$ (real valued functions on G) by multiplication, $f \cdot g$,
(ii) composition of Weyl quantized observables $\mathcal{F}(f) \circ \mathcal{F}(g)$, with

$$\begin{aligned} \mathcal{F}(f) &= \int e^{i(Ps+Qt)}\xi(s,t)\, dsdt \\ \mathcal{F}(g) &= \int e^{i(Ps+Qt)}\eta(s,t)\, dsdt \quad \text{for} \quad \eta = \check{g}\;. \end{aligned}$$

Of course these compositions differed essentially, as $\cdot$ is commutative and $\circ$ obviously non-commutative. Weyl might have easily transported the operator composition back to the functions on the abelian group, defining $f * g =: h \Longleftrightarrow \mathcal{F}(h) = \mathcal{F}(f) \circ \mathcal{F}(g)$. But he did not. He was not so much interested in the arising new algebraic structure itself, as in the quantum physical context to which his investigations belonged. The next most pressing problem after the derivation of the Schrödinger representation of a free quantum mechanical system (see above) seemed to be the question, how to characterize interactions, the "dynamical problem" as Weyl called it.

4. Reflections on the "dynamical problem"

Section III of Weyl's article dealt with the *dynamical problem.* While the "kinematics" of a system characterized by a continuous group $G \cong \mathbb{R}^n$ was uniquely determined by its number n of degrees of freedom, the same did not hold true for the dynamics, taking interactions into account.

> Up to now the approach claims general validity. The situation is less comfortable for the *dynamical problem* which is closely bound to the role of *space and time* in quantum physics. (Weyl 1927, 123, emph. in original)[20]

Weyl immediately hit on a strict limitation for contemporary quantum physics, which was bound to the different roles played by space and time in Galilean and in relativistic quantum physics. In non-relativistic QM, time

[19](Wiener 1926, Bochner 1927, Hardy/Littlewood 1926)

[20]"Die bisherigen Ansätze beanspruchen allgemeine Geltung. Nicht so günstig steht es mit dem *dynamischen Problem*, das eng mit der Frage nach der Rolle zusammenhängt, welche *Raum und Zeit* in der Quantenphysik spielen."

was an *independent variable*, and even the only one, of a system in the following sense:

> Independent variables are no measurable quantities, they are a cognitive spider web of coordinates arbitrarily spread out over the world. The dependence of a physical quantity on these variables can therefore not be controlled by measurement; only if several physical quantities are in play, one can arrive at relations between the observable quantities by elimination of the independent variables.(Weyl 1927, 124)[21]

Now Weyl indicated a critical difference between field physics and relativistic quantum mechanics on the one side and Galilean QM on the other. Field theory deals with *state quantities (Zustandsgrößen)*, i.e., observables, which are "spread out in space and time", while in particle mechanics time may be considered as an independent variable. A relativistic quantum description of the electron, e.g., has to consider spatial coordinates and time as state quantities, "really marked space and, of course, also really marked time" and thus as observables represented by Hermitian forms (or operators).

> In contrast to this state of affairs, non-relativistic mechanics is in the comfortable situation to be able to ignore time as a state quantity, while relativistic mechanics needs measurable time coordinates of the particles together with measurable space coordinates. (Weyl 1927, 124)[22]

The dynamical law of non-relativistic QM could therefore be given in the Schrödinger picture by

$$\frac{d\psi}{dt} = \frac{i}{\hbar} E \cdot \psi$$

(with this sign!), where " iE is the infinitesimal unitary mapping coupled to the Hermitian form E which represents energy" (ibid, 124).[23]

For relativistic quantum physics the situation appeared still rather inconclusive, and Weyl indicated only the direction of research one had to pursue:

> If one wants to remove the criticized deficiency of the concept of time in the old pre-relativistic quantum mechanics, the measurable quantities time t and energy E have to be included as another conjugate pair. This can also be seen from the action principle of analytic mechanics; the dynamical law disappears completely. The relativistic treatment of

[21] "Die unabhängigen Veränderlichen sind keine gemessenen Größen, sie sind ein willkürlich in die Welt hineingetragenes gedachtes Koordinatenspinngewebe. Die Abhängigkeit einer physikalischen Größe von diesen Variablen ist also auch nicht etwas durch Messung zu Kontrollierendes; erst wenn mehrere physikalische Größen vorliegen, kommt man durch Elimination der unabhängigen Veränderlichen zu Beziehungen zwischen beobachtbaren Größen."

[22] "Diesem Sachverhalt gegenüber ist die nicht-relativistische Mechanik in der glücklichen Lage, die Zeit als Zustandsgröße ignorieren zu können, während die Relativitätsmechanik parallel mit den meßbaren Raumkoordinaten auch die meßbaren Zeitkoordinaten der Teilchen benötigt."

[23] Apparently Weyl followed here (Schrödinger 1926, 112) = (Schrödinger 1927, 142).

> an electron in the electromagnetic field by Schrödinger e. a. corresponds to this point of view.[24] A more general formulation is not yet available. (Weyl 1927, 127)[25]

"Inclusion" of time and energy as another canonical pair of variables for a relativistic approach would surely imply to take care also for the relativistic transformations between different observers, i.e., the consideration of a ray representations of the Poincaré group with conjugate pairs of translation variables $(\mathbb{R}^4 \times \mathbb{R}^4) \rtimes SO(1,3)$ (with $\rtimes$ for the semi-direct product). However, Weyl left it with the indication quoted above. It would be taken up only much later by E. Wigner and G. Mackey.

In 1927, and still in summer 1928, Weyl apparently hoped that the group theoretic approach might be a guide to field quantization also, at first in the non-relativistic case, but then perhaps even in the relativistic one. At the end of §44 of the first edition of GQM in which he sketched the quantization of the wave equation according to Jordan and Pauli, he expressed confidence in the method to quantize the electromagnetic and the electron wave. Then he continued (curly brackets { . . . } denote passages which were omitted in the second edition 1931 and the English translation, angular brackets < . . . > an addition in the second edition):

> We have thus discovered the correct way to quantize the field equations (. . .) defining light waves and electron waves {The exact execution is the next task of quantum physics. The preservation of relativistic invariance seems to offer serious difficulties [reference to (Jordan/Pauli 1928) and (Mie 1928), E.S.] }. Here again we find <, as in the case of the spinning electron,> that quantum kinematics is not to be restricted by the assumption of Heisenberg's specialized commutation rules. {And again it is group theory which furnishes us with the natural general form, as is shown in the next section. . . . } (Weyl 1928, 1st ed. 1928, 203), (Weyl 1931, 253)[26]

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[24] Weyl quoted (Schrödinger 1927, 163ff.), reprinted from (Schrödinger 1926, 133ff.).

[25] "Will man den gerügten Mangel des Zeitbegriffs der alten vorrelativistischen Mechanik aufheben, so werden die meßbaren Größen: Zeit t und Energie E, als ein weiteres kanonisches Paar auftreten, wie ja bereits das Wirkungsprinzip der analytischen Mechanik erkennen läßt; das dynamische Gesetz kommt ganz in Fortfall. Die Behandlung eines Elektrons im elektromagnetischen Feld nach der Relativitätstheorie durch Schrödinger u. a. entspricht bereits diesem Standpunkt.[Fussnote mit Hinweis auf (Schrödinger 1927, 163ff.)] Eine allgemeinere Formulierung liegt noch nicht vor."

[26] "Damit ist der Weg gezeigt, wie die Licht- und Elektronenwellen umfassenden Feldgleichungen in richtiger Weise zu quantisieren sind. {Die genaue Durchführung ist die nächste Aufgabe der Quantenphysik; die Wahrung der relativistischen Invarianz scheint dabei noch ernste Schwierigkeiten zu bereiten. [Verweis auf (Jordan/Pauli 1928) and (Mie 1928), E.S.]} Es hat sich hier von neuem <, wie beim Spin der Elektronen,> die Notwendigkeit herausgestellt, die Quantenkinematik nicht an das spezielle Schema der Heisenbergschen Vertauschungsrelationen zu binden." Robertson's translation of the first sentence in (Weyl 1931) has been corrected by obvious reasons (" . . . electron waves and matter waves . . . " is non-sensical and not in agreement with the original).

The "next section" of the first edition comprised the content of (Weyl 1927).[27] Thus even at the time when he finished his book on *Gruppentheorie und Quantenmechanik* (GQM), Weyl apparently had the impression that the study of irreducible ray representations and a group theoretically founded approach to quantization ought to be helpful for a full solution of the"dynamical problem" of quantum physics, i.e., the study of interactions and for relativistic systems. His colleagues in physics started to attack such problems by introducing the method of field quantization.

By the early 1930s Weyl became more cautious. His physics colleagues had embarked even more strongly on the program of field quantization, including the relativistic case. The great problems of divergent field expressions, even for perturbation developments, were accumulating. Weyl did no longer try to pursue his own approach against the mainstream of the (still very small) quantum physics community; he may have felt that he should no longer insist on the superiority of the group approach to the foundations of quantum physics, if he himself did no longer continue to work along these lines.[28] In the second edition of GQM, and thus in the English translation, the (curly) bracketed sentences no longer appeared.

Notwithstanding this shift at the turn to the 1930s, Weyl had good reasons in the late 1920s to be content with his group theoretic approach to the foundations of QM. He had arrived at a convincing structural characterization of what he called the "quantum kinematics" of physical systems. For n continuous degrees of freedom, the quantum kinematics was even uniquely determined by n. Not so, however, for discrete systems in which the non-commutative product structure of the algebra of observables might become more involved. Weyl indicated very cautiously that such structures might perhaps be useful for the understanding of atomic systems; but he was far from claiming so (Weyl 1928, 207) (Weyl 1931, 276).

5. Outlook and repercussions

Weyl's approach to quantization was so general that for decades to come it did not attract much attention of physicists. At the beginning it even attracted very few successor investigations inside mathematics and was not noticed in the foundation of QM discourse, which was exclusively shaped by the Hilbert and von Neumann view until the 1950s. Although the immediate reception of Weyl's early contributions to QM until about 1927, in particular his (Weyl 1927), was very sparse, its repercussion turned out to be *remarkably strong in the long range*. Of course, this question touches a difficult matter and deserves much closer and more detailed scrutiny. Here I can give only a very rough first outline. It will be given in form of a provisional list of investigations which seem to count as follow up stories to the proposals made by Weyl between 1925 and 1927.

[27]In the second edition and in the English translation new sections on the quantization of the Maxwell-Dirac field and on relativistic invariance were inserted before the section on quantum kinematics (Weyl 1931, §§ 12, 13).

[28]The non-uniqueness problem for irreducible unitary representations of infinite dimensional degrees of freedom, and thus for quantum field theory, was realized only in the 1950s (Summers 2001); it seems unlikely that Weyl expected a problem in this respect already at the turn to the 1930s.

(i) A first and immediate next step was made by *Marshall Stone* and *John von Neumann*. They both took up Weyl's statement of a uniquely determined structure of irreducible unitary ray representations of $\mathbb{R}^{2n}$ and proved it for $n = 1$ in $\mathcal{L}^2(\mathbb{R}, \mathbb{C})$. The result of this work is (for finite n) the now famous *Stone/von Neumann representation theorem*: Up to isomorphism there is exactly one irreducible abelian ray representation of $\mathbb{R}^{2n}$ by unitary operators (Stone 1930, von Neumann 1931). As we have seen its content and a sketch of proof, generously passing over the functional analytical details in silence, goes back to (Weyl 1927).

Only much later a critical analysis of functional analytic preconditions for the equivalence of Heisenberg commutation (9) and Weyl commutation (10) started. Sufficient conditions were established by (Rellich 1946) and (Dixmier 1958). The breakdown of uniqueness for infinite degrees of freedom (and thus for quantum field theory) started with seminal work by Kurt Friedrichs and Rudof Haag in the 1950s. Construction of "pathological" counter-examples, disregarding the conditions of Rellich and Dixmier, even for the finite dimensional case ($n = 1$) followed (Summers 2001).

(ii) A second line of repercussions may be seen in that part of the work of *E. Wigner* and *V. Bargmann*, which dealt with unitary and *semi-unitary ray representations*. In particular Wigner's now famous work (at the time among physicists completely neglected) on the *irreducible unitary ray representations of the Poincaré group* (Wigner 1939) looks like a next step beyond Weyl's non-relativistic quantum kinematics from 1927. It established a basis from which investigations of relativistic dynamics might start from. But is has still to be checked in which respect, or perhaps even whether, Wigner was motivated by Weyl's work. Wigner surely knew the latter, but he may have developed his research questions autonomously, in communication with von Neumann, Dirac and others which stood closer to him than Weyl.[29]

(iii) A third impact is clearly to be seen in *George Mackeys's* work. Mackey expressedly took up Weyl's perspective (Mackey 1949) and developed it into a broader program for the study of *irreducible unitary representations of group extensions*, $H \lhd G$, induced from representations of a normal subgroup H in G, by what he called *systems of imprimitivity*. Starting at first from abelian subgroups H, he realized that the dual group $\hat{H}$ led to a pairing characteristic for Weyl's analysis ($H \cong \mathbb{R}^n$, $H \times \hat{H} \cong \mathbb{R}^{2n}$) and generalized it to non-abelian normal subgroups. His later commentaries on the *foundations of QM*, among them (Mackey 1957, Mackey 1963, Mackey 1993*a*), were

[29] Wigner expressedly acknowledged the importance of Dirac's and von Neumann's communications for his work (Wigner 1939, 341/156); whereas he quoted Weyl only in questions of technical details and (Weyl 1927) not at all. On the other hand, Mackey is certainly right in the characterization of (Wigner 1939): "This kind of application of the theory of group representations to quantum mechanics is much more in the spirit of Weyl's 1927 paper in the *Zeitschrift für Physik* than that of most of Wigner's work up to this point" (Mackey 1993*b*, 265).

seminal for bringing to bear the Weylian perspective in the domain of foundations of quantum physics. They were so deeply influenced by the Weylian view, that Mackey even considered his work as the true successor line of Weyl's foundational perspective (Mackey 1988*b*, Mackey 1988*a*). His work was influential among mathematicians (Varadarajan 1970), although apparently not so much among physicists. It seems to contain unexhausted potential.

(iv) Finally, *Weyl quantization* was taken up by mathematical physicists from the later 1960s onwards with the rise of *deformation quantization* (Pool 1966). Here the starting point was the idea to translate the operator product introduced by Weyl's own quantization

$$f, g \rightarrow H := \mathcal{F}(f) \circ \mathcal{F}(g)$$

back to the function space:

$$f * g =: h \Longleftrightarrow \mathcal{F}(h) = H = \mathcal{F}(f) \circ \mathcal{F}(g)$$

Today, this noncommutative product of functions is usually considered (slightly anachronistically) as *Weyl quantization*. Weyl's mixed continuous-discrete group (15) developed into the noncommutative torus. This was one step into the newly rising field of noncommutative geometry and deformation quantization, which is a very active subfield of present day mathematical physics.

The last two points lead straight into very recent developments of mathematical physics and far beyond the scope of this talk (and my competences). Nevertheless it seems quite remarkable that at least two of Weyl's ideas developed in the first two years after the transition to the 'new' quantum mechanics, turned out to bear fruits in so diverse directions in the long run. They inspired highly original work for more than half a century and perhaps contain the potential to continue to do so.

Both ideas happen to have been mentioned at the very beginning of this phase in Weyl's correspondence with Born and Jordan, in late summer and autumn 1925. In this correspondence Weyl contributed to the new quantum physics discourse in a more personal form, before he turned toward published expression of his views two years later.

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