Theoretical Practice: the Bohm-Pines Quartet

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Quite rightly, philosophers of physics examine the theories of physics, theories like Quantum Mechanics, Quantum Field Theory, the Special and General Theories of Relativity, and Statistical Mechanics. Far fewer, however, examine how these theories are put to use; that is to say, little attention is paid to the practices of theoretical physicists. In the early 1950s David Bohm and David Pines published a sequence of four papers, collectively entitled, 'A Collective Description of Electron Interaction.' This essay uses that quartet as a case study in theoretical practice. In Part One of the essay, each of the Bohm-Pines papers is summarized, and within each summary an overview is given, framing a more detailed account. In Part Two theoretical practice is broken into six elements: (a) *the use of models,* (b) *the use of theory,* (c) *modes of description and narrative,* (d) *the use of approximations,* (e) *experiment and theory,* (f) *the varied steps employed in a deduction. The last element is the largest, drawing as it does from the earlier ones. Part Three enlarges on the concept of 'theoretical practice,' and briefly outlines the subsequent theoretical advances which rendered the practices of Bohm and Pines obsolete, if still respected.*

> Were a kind of semiotic egalitarianism to direct us to regard as so many texts the papers that regularly appear in *The Physical Review,* their literary dimension must seem deeply secondary . . .
>
> Arthur Danto[1]

Preamble

My egalitarian tendencies will be all too evident throughout this essay, dealing as it does with four papers from *The Physical Review.* True, I largely

1. Danto (1986, 136).

Perspectives on Science 2006, vol. 14, no. 4

neglect their literary dimension, but the attention I pay to the narrative elements in them will betray a sadly undiscriminating taste.

Pace Danto, however plebian they may be, these papers are indisputably texts. They are written utterances that take up and respond to earlier utterances in the genre, and themselves invite responses from their intended readers. This explains why a paper can be too original for its own good, why early papers in chaos theory, for instance, were denied space in physics journals.[2] For although each paper is individuated by the original contribution it offers, the dialogic relation in which it stands both to its predecessors and its successors requires that all of them be informed by a common set of assumptions. These shared assumptions—some methodological, others theoretical, some explicit, many tacit—provide a normative framework for theoretical discourse. This in turn enables us to speak meaningfully of "theoretical practice." The details of the framework may vary from one sub-discipline of physics to another. They will also change with time; under the press of theoretical advances some elements of the framework will be jettisoned, or fall into disuse, while others become accepted in their place. Thus one should properly think of physics as involving, not theoretical practice *tout court,* but a set of theoretical practices indexed by sub-discipline and date.

The phrase itself, "theoretical practice," though not actually oxymoronic, is little used in the philosophy of physics.[3] And, in describing a theoretical advance, physicists will rarely allude to the practices that led to it. For example, in the transcript of one of Richard Feynman's *Lectures on Physics* (1964, II, 7–7) we read,

> It was first observed experimentally in 1936 that electrons with energies of a few hundred to a few thousand electron volts lost energy in jumps when scattered from or going through a thin metal foil. This effect was not understood until 1953 when Bohm and Pines showed that the observations could be explained in terms of the quantum excitations of the plasma oscillations in the metal.

Feynman is here concerned only with the result that Bohm and Pines obtained, not with the strategies they used to obtain it. In his lecture it appears as one illustration among others of the fact that "[the] natural resonance of a plasma has some interesting effects" (*ibid.*). But the work that produced it, the journey rather than the end, offers an illustration of a dif-

2. See Ruelle (1991).

3. Edmund Husserl's *The Crisis of the European Sciences* is too seldom studied by philosophers of science.

ferent kind. It displays an example of the theoretical practices of solid state physics in the mid-twentieth century.

This essay has four parts. In Part One I give synopses of the four papers in which Bohm and Pines presented a theoretical account of plasma behaviour; in Part Two I provide a commentary on these papers; and in Part Three I offer some remarks concerning theoretical practice and a brief note on the methodologies available to philosophers of science. The commentary in the second (and longest) part contains discussions of the components of theoretical practice exhibited in the quartet. They include models, theoretical manifolds, modes of description, approximations, the relation between theory and experiment, and deduction. Part Two can thus be read as an introduction to the notion of "theoretical practice" that takes the Bohm-Pines quartet for illustration. Likewise, Part Three illustrates, amongst other things, how theoretical practice evolves through time.

PART ONE
The Bohm-Pines Quartet

1.1 Introduction

Between 1950 and 1953 David Bohm and David Pines published a sequence of four papers in *The Physical Review.* They were collectively entitled, "A Collective Description of Electron Interactions," and individually subtitled, "I: Magnetic Interactions," "II: Collective vs. Individual Particle Aspects of the Interaction," "III: Coulomb Interactions in a Degenerate Electron Gas," and, "IV: Electron Interaction in Metals."[4] In this quartet of papers Bohm and Pines had two aims, one specific and the other general. The specific aim was to provide a theoretical account of the behaviour of electrons in metals; as their title announces, this account was to be given in terms of a "collective description of electron interactions" rather than "the usual individual particle description" (BP I, 625). Their more

4. I cite these papers as BP I, BP II, BP III, and P IV. Except for the abstract of BP I, the text on each page is set out in two columns. In my citations, the letters "a" and "b" after a page number designate, respectively, the left and right hand column. The fourth paper was written by Pines alone. When the first paper was published both authors were at Princeton University, Bohm as a junior faculty member and Pines as a graduate student. Over the period in which they were written Pines moved, first to Pennsylvania State University and then to the University of Illinois. Between the time BP II was received by *The Physical Review* (September 1951) and the time it was published (January 1952), Bohm had fallen foul of Joseph McCarthy's Un-American Activities Committee and had moved to the University of Sao Paolo in Brazil. See the introduction to Hiley and Peat (4, 1987).

general aim was to explore, through this analysis, a new approach to many-body problems.

The background to their investigation, roughly sketched, was this. Since the work of Paul Drude at the turn of the century, the accepted explanation of the high electrical conductivity of metals was that some, at least, of the electrons in the metal were free to move through it. On the account that emerged, the valence electrons in a metal are not attached to specific atoms; instead they form an "electron gas" within a regular array of positive ions, the "crystal lattice." This model was modified by Arnold Sommerfeld (1928). He pointed out that the electrons in a metal must obey the Pauli exclusion principle, according to which no more than one electron in a system can occupy a particular energy level; collectively they need to be treated as a *degenerate electron gas* (hence the title of BP III).[5]

In the 1930s and 40s a theory of the motion of electrons in a metal was developed using the "independent electron formulation," otherwise referred to as the "independent electron model." On this approach the electrons are treated individually. To quote John Reitz (1955, 3),

> [T]he method may be described by saying that each electron sees, in addition to the potential of the fixed [ionic] charges, only some average potential due to the charge distribution of the other electrons, and moves essentially independently through the system.

The independent electron theory enjoyed considerable success. The values it predicted for a number of metallic properties (electrical and thermal conductivity among them) agreed well with experiment (Pines, 1987, 73). However, it failed badly in one important respect: The predicted cohesive energy of the electrons in the metal was so small that, were it correct, most metallic crystals would disintegrate into their constituent atoms. Furthermore, as Pines puts it (*ibid.*), "[T]heoretical physicists . . . could not understand why it worked so well."[6] The challenge facing Bohm and Pines was to formulate a theory of the behaviour of electrons in a metal that both acknowledged the mutual interactions between electrons, and showed why the independent electron model, which ignored them, was so successful.

They looked for guidance to the research on plasmas performed by physicists like Irving Langmuir in the 1920s and 30s.[7] Langmuir used the term "plasma" to refer to an ionized gas, such as one finds in a fluorescent

5. Whereas an ideal gas obeys Maxwell-Boltzmann statistics, as a result of the exclusion principle a degenerate electron gas obeys Fermi-Dirac statistics.

6. For more details see Pines (1955, 371–74). An extended discussion of the independent electron theory is given in Reitz (1955).

7. The paper cited in BP I is Tonks and Langmuir (1929).

lamp or in the ionosphere, where electrons are stripped from their parent atoms, in one case by an electrical discharge, in the other by ultra-violet radiation from the Sun.[8] The state of matter that results resembles the one in a metal in that a cloud of free electrons surrounds a collection of heavier, positively charged ions. There are differences. The ions in a gas plasma move freely, albeit much more slowly than the electrons, whereas in a metal their motion is confined to thermal vibrations about fixed points in a regular lattice. More importantly, there are about 10^{11} times as many electrons per unit volume in a metal than in a gas plasma. This is the reason why the electron gas in a metal should properly be treated quantum-mechanically, as a degenerate electron gas, whereas the electron gas in a gas plasma may be treated classically (see fn. 5).

Despite these differences, Bohm and Pines saw Langmuir's investigations of plasmas as "offering a clue to a fundamental understanding of the behavior of electrons in metals" (Pines, 1987, 67).[9] They set out to show that the electron gas in a metal would manifest two kinds of behavior characteristic of gas plasmas: that high frequency "plasma oscillations" could occur in it, and that the long range effect of an individual electron's charge would be "screened out" by the plasma. Paradoxically, both these effects were attributed to the Coulomb forces between pairs of electrons, the electrostatic forces of repulsion that exist between like charges. "Paradoxically" because, on this account, the plasma screens out precisely those long range effects of Coulomb forces that are responsible for plasma oscillations. Note, however, that the screening effect, if established, would go some way towards explaining the success of the independent electron theory of metals.

Within the quartet a mathematical treatment of plasmas is interwoven with an account of its physical significance and a justification of the methods used. Synopses of the four papers are given in the next four sections of this essay. They have been written with two aims in mind: first, to present a general overview of each paper; secondly, to provide more detail about those parts of the quartet that I comment on in Part Two of the essay. The two aims pull in different directions. To alleviate this tension, in each synopsis I distinguish the overview from the more detailed material by enclosing the latter in brackets ("**((**" and "**))**"). This allows the reader to skip the bracketed material on a first reading to attend to the overview, and to go back later to browse on the amplified version.

8. Matter also enters the plasma state at the temperatures and pressures associated with thermonuclear reactions. Interest in controlled fusion has prompted much of the research on plasmas since the Bohm-Pines papers were published.

9. A brief summary of previous work along these lines, together with some critical observations on it, is given in BP II, 610b.

1.2 P I: Magnetic Interactions

The Introduction to BP I is an introduction to the whole quartet. Its first six paragraphs introduce the reader to the independent electron model and its shortcomings, and to the phenomena of screening and plasma oscillations. These phenomena, the authors tell us, occur in an electron gas of high density. In a footnote (BP I, fn. 1) they point out that "The [negatively charged] electron gas must be neutralized by an approximately equal density of positive charge." In a metal this charge is carried by the individual ions of the metal. But "in practice the positive charge can usually be regarded as immobile relative to the electrons, and for the most applications [*sic*] can also be regarded as smeared out uniformly throughout the system." (*Ibid.*) The presence of this positive charge (whether localized or uniformly distributed) effectively screens out short range interactions beyond a very small distance. At long range, however, plasma oscillations, a collective phenomenon, can occur. These are longitudinal waves, "organized oscillations resembling sound waves" (BP I, 625a), in which local motions are parallel to the direction of propagation of the waves. A plasma can also transmit organized transverse oscillations, in which local motions are at right angles to the direction of wave propagation. They may be triggered by an externally applied electromagnetic field, a radio wave passing through the ionosphere, for example. This applied field will produce oscillations of the individual electrons, each of which will in turn give rise to a small periodic disturbance of the field. Only if the cumulative effect of all these small disturbances produces a field in resonance with the original applied field will the oscillations become self-sustaining. As we learn at the end of the Introduction (BP I, 627a), transverse oscillations like these are the main topic of the first paper.

Bohm and Pines treat these oscillations in two ways. In their first treatment they use the techniques of classical physics; in the second those of quantum mechanics. So that the results of the first can be carried over to the second they use "Hamiltonian methods," i.e. they describe the system comprising the electrons and the field by its *Hamiltonian,* an expression that specifies its total energy.[10] The authors write (BP I, 626b),

> This Hamiltonian may be represented schematically as
>
> $$H_0 = H_{\text{part}} + H_{\text{inter}} + H_{\text{field}}$$
>
> where H_{part} represents the kinetic energy of the electrons, H_{inter} represents the interaction between the electrons and the electromag-

10. I say more about the Hamiltonian and its significance in Sections 2.3 and 2.4 of this essay.

netic field, and H_{field} represents the energy contained in the electromagnetic field.

This Hamiltonian is expressed in terms of the position and momentum coordinates of the individual electrons and field coordinates of the electromagnetic field.[11] Bohm and Pines then introduce a new set of coordinates, the "collective coordinates,"[12] and, as before, a distinction is made between particle coordinates and field coordinates. A mathematical transformation replaces the original Hamilton, H_0, expressed in the old variables, by another, $H_{(1)}$, expressed in the new ones.

((Given various approximations,

[T]he Hamiltonian in the collective description can be represented schematically as

$$H_{(1)} = H^{(1)}_{\text{part}} + H_{\text{osc}} + H_{\text{part int}}$$

where $H^{(1)}_{\text{part}}$ corresponds to the kinetic energy in these new coordinates and H_{osc} is a sum of harmonic oscillator terms with frequencies given by the dispersion relation for organized oscillations. $H_{\text{part int}}$ then corresponds to a screened force between particles, which is large only for distances shorter than the appropriate minimum distance associated with organized oscillations. Thus we obtain explicitly in Hamiltonian form the effective separation between long range collective oscillations, and the short-range interactions between individual particles. (*Ibid.*)

The "effective separation" that the authors speak of shows itself in this: Whereas in H_0 the term H_{inter} contains a mixture of particle and field coordinates, in $H_{(1)}$ the term H_{osc} contains only collective field coordinates, and $H_{\text{part int}}$ contains only collective particle coordinates. Bohm and Pines gather together the approximations that allow them to write the Hamiltonian in this way under the title "The Collective Approximation" (BP I, 628a-b); I will say more about them in my comments (Section 2.5). Among them the "Random Phase Approximation" plays a particularly important role in the authors' project. The "dispersion relation" they refer to relates the fre-

11. The field coordinates appear when the vector potential of the electromagnetic field is expanded as a Fourier series. For more on Fourier series in this context, see fn. 13 and the synopses of BP II and BP III.

12. An elementary example of a move to a collective coordinate is the use of the vector $\mathbf{X}$ to specify the position of the centre of gravity of a system of masses, $m_1, m_2, \ldots, m_n$. If the positions of the masses are given by $\mathbf{x}_1, \mathbf{x}_2, \ldots, \mathbf{x}_n$, then $\mathbf{X} = \Sigma_i m_i \mathbf{x}_i / \Sigma_i m_i$.

> quency of the oscillation to its wavelength. This relation must hold if sustained oscillations are to occur (BP I, 625b).))

In both the classical and the quantum mechanical accounts, the authors claim (BP I, 634b), the move to collective variables shows that, within an electron gas,

> [T]he effects of magnetic interactions are divided naturally into the two components discussed earlier:
>
> (1) The long range part [given by H_{osc}] responsible for the long range organized behaviour of the electrons, leading to modified transverse field oscillations [. . .]
>
> (2) The short-range part, [. . .] given by $H_{part\ int}$, which does not contribute to the organized behaviour, and represents the residual particle-interaction after the organized behaviour of the system has been taken into account.

BP I is essentially a preamble to the papers that follow. The magnetic interactions that produce transverse oscillations are many orders of magnitude weaker than the Coulomb interactions that produce longitudinal plasma waves, and consequently "are not usually of great physical import" (BP I, 627a). The authors investigate them "to illustrate clearly the techniques and approximations involved in our methods," since "the canonical treatment of the transverse field is more straightforward than that of the longitudinal field" (*ibid.*).

1.3 P II: Collective vs. Individual Particle Aspects of the Interaction

BP II and BP III both give theoretical treatments of longitudinal oscillations. A classical treatment in BP II is followed by a quantum mechanical treatment in BP III. In both these papers, as in BP I, the authors' chief concern is the relation between the individual and the collective aspects of electronic behaviour in plasmas.

In BP II Bohm and Pines analyze this behaviour in terms of the variation of the electron density (the number of electrons per unit volume) within the plasma. Because of the forces of repulsion between electrons, these variations act like variations of pressure in air, and can be transmitted through the electron gas as plasma oscillations, like sound waves. To analyze the resulting variations in electron density no transformations of the kind used in BP I are required, since the electron density is already a collective quantity. The authors work with Fourier components ρ_k of this

density.[13] The component ρ_0 represents the mean electron density of the gas, but the other components ρ_k (with $k > 0$) represent density fluctuations of different wavelengths.

((Bohm and Pines use elementary electrostatics, together with the random phase approximation, to obtain an expression for $d^2\rho_k/dt^2$ that can be divided into two parts (BP II, 340b). One part shows the contribution of the interactions between electrons, and the other the contribution of their random thermal motions.

The authors show that, if thermal motions are ignored, sustained oscillations of frequency ω_P can occur; ω_P is the so-called *plasma frequency.*[14] If thermal motions are taken into account, the frequency ω of oscillation is no longer independent of the wave number k, as in the case of $d^2\rho_k/dt^2$, the expression for ω^2, has two parts. For small values of k (i.e., for long fluctuation wavelengths), the first term in the expression for ω^2 predominates, and the electron gas displays its collective aspect. Conversely, for high values of k and short wavelengths, the second term predominates, and the system can be regarded as a collection of free particles. In the general case both aspects are involved. **))**

Bohm and Pines show (BP II, 342b-343a) that in the general case each component ρ_k of the electron density can be expressed as the sum of two parts:

$$\rho_k = a_k q_k + \eta_k$$

Here a_k is a constant, q_k is a collective coordinate that oscillates harmonically, and η_k describes a fluctuation associated with the random thermal motion of the electrons. Two general conclusions are drawn: (i) Analysis of

13. A simple example of a Fourier decomposition occurs in the analysis of musical tones; the note from a musical instrument can be broken down into a fundamental, together with a set of overtones. More abstractly, let $f(x)$ be any continuous function, such that $f(x_1) = f(x_2)$ and $f(x)$ has only a finite number of maxima and minima in the interval $[x_1, x_2]$. Then $f(x)$ can be represented in that interval as the sum of a set of sine waves: $f(x) = \sum_k a_k e^{ikx}$. The index k specifying each component runs over the integers, 0, 1, 2, . . . It is the *wave number* of the component of the function. i.e. the number of complete wavelengths of that component in the interval $[x_1, x_2]$. Thus k is inversely proportional to the wavelength λ ; the greater the wave number the shorter the wavelength (and vice versa). Messiah (1958, 471–78) gives a useful mathematical account of Fourier transformations.

14. The plasma frequency is given by $\omega_P^2 = 4\pi ne^2/m$, where e and m are the electron's charge and mass, respectively, and n is the electron density. An elementary classical derivation of this equation is given by Raimes (1961, 283–84).

η_k shows that each individual electron is surrounded by a region from which the other, electrons are displaced by electrostatic repulsion. That region, however, like the entire volume occupied by the electron gas, is assumed to contain a uniformly distributed positive charge. The result is that the Coulomb force due to the individual electron is screened off outside a radius of the order of λ_D, the so-called "Debye length."[15] In a metal this region is about 10^{-8} cm. in diameter; it moves with the electron, and can itself be regarded as a free particle.[16] (ii) It turns out (BP II, 343a) that there is a critical value k_D of k such that, for $k \geq k_D$, $\rho_k = \eta_k$, i.e. there are effectively no collective coordinates q_k for k greater than k_D. Since, by definition, $k_D = 1/\lambda_D$, the physical import of this is that there are no plasma oscillations of wavelengths less than λ_D. As in BP I, the use of collective coordinates allows Bohm and Pines to predict that long range interactions in the electron gas give rise to plasma oscillations (in this case longitudinal oscillations), and that at short ranges the normal electrostatic forces between electrons are largely screened off.

The authors go on to show how oscillations may be produced by a high-speed electron moving through the electron gas (BP II, 344b-347a). A "correspondence principle argument" (on which I will comment in Sections 2.3 and 2.6) is then used (BP II, 347a) to explain the experimentally obtained phenomenon, that, when high energy electrons pass through thin metallic foils of the same element, they suffer losses of energy that are all multiples of a specific value. These are the results cited by Feynman; they were obtained by G. Ruthemann and by W. Lang independently in the early 1940s (see BP II, 339a, and Pines, 1987, 77).

1.4 BP III: Coulomb Interactions in a Degenerate Electron Gas

BP III is the longest and most intricately argued paper of the four. The quantum mechanical analysis it presents uses the theoretical strategy deployed in the latter part of BP I alongside the physical insights achieved in BP II. As in BP I, a Hamiltonian for the system electrons-plus-field in terms of individual particle coordinates is transformed to one in terms of collective coordinates. In this case, however, the collective coordinates are those appropriate for describing longitudinal, rather than transverse, oscillations, and the procedure is far from straightforward. A sequence of

15. The *Debye length* λ_D was first introduced in connection with screening processes in highly ionized electrolytes (see Feynman, 1964, 7–9). It is the thickness of the ion sheath that surrounds a large charged particle in an electrolyte (BP II, 341b, fn.).

16. A similar result appears in BP III. Here Bohm and Pines anticipate what became standard practice in the 1950s and 1960s, whereby a particle together with its interactions with its immediate environment was treated as an elementary system, and was referred to as a *quasi-particle.* I return to this topic in Section 3.1.

five modifications, which include coordinate transformations, algebraic manipulations, and a variety of approximations (some relying on non-trivial assumptions) takes the reader from the initial Hamiltonian (which I will refer to as "H^1") to the final Hamilton H_{new} .[17]

H^1 is itself a standard Hamiltonian for "an aggregate of electrons embedded in a background of uniform positive charge" (BP III, 610a). It contains three terms, one for the kinetic energy of the electrons, another for the energy due to the pair-wise electrostatic interactions between them, and the third for the sum of the individual self-energies of the electrons. The last two are slightly adjusted to take into account the background of positive charge. The first four modifications of the Hamiltonian are effected in Section II of the paper. By means of them Bohm and Pines introduce a set of "field variables," and analyze their "approximate oscillatory behavior" (BP III, 611b). The final modification, made in Section III, enables them "to carry out the canonical transformation to the pure collective coordinates" (*ibid.*).

> ((In the first modification H^1 is transformed into another Hamiltonian H^2, expressed in terms, not just of individual particle coordinates, but also of the longitudinal vector potential $\mathbf{A}(\mathbf{x})$ and the electric field intensity $\mathbf{E}(\mathbf{x})$ of the electromagnetic field within the plasma. The transformation does not affect the first and third terms of H^1. The difference between H^1 and H^2 is that the energy that in H^1 was attributed to Coulomb interactions between electrons is now represented as an energy of interaction between individual electrons and the electromagnetic field engendered by the electron gas as a whole. Like the density ρ in BP II, $\mathbf{A}(\mathbf{x})$ and $\mathbf{E}(\mathbf{x})$ are both written as Fourier series (see fn. 13), and the coefficients of the components p_k and q_k of the two series (both indexed by k) serve as field coordinates of the plasma, as in BP I. The authors claim (BP III, 611a) that H^2 will "lead to the correct equation of motion," when supplemented by a set of "subsidiary conditions" on the allowable states Φ of the system. Each member of the set has the form, $\Omega_k\Phi = 0$, where each condition Ω_k isa function of a component of the Fourier decompositions of p and ρ (the electron density).

17. The Hamiltonians that I refer to as $H^1, H^2, \ldots, H_{new}$ appear in BP III as follows: H^1 is the Hamiltonian on p. 610b; H^2, the Hamiltonian H on p. 612a; H^3, the Hamiltonian H on p. 612a; H^4 is a Hamiltonian whose last four terms appear explicitly on p. 612b, and whose first term, H_{part} , is the sum of the first and last terms of H^3, where they represent the kinetic and self energies of the electrons, respectively; H^5 is the Hamilton H on p. 616b; H_{new} appears on p. 618b.

These subsidiary conditions also serve other functions. They ensure conformity with Maxwell's equations, and also achieve a more technical purpose, that of reducing the number of degrees of freedom allowed by the transformed description (H^2 plus the conditions) to that allowed by H^1.[18] In addition, as the expression for Ω_k makes clear,

> [They] introduce in a simple way a relationship between the Fourier components of the electronic density ρ_k and a set of field variables p_k . [. . .] [T]here is in consequence a very close parallel between the behaviour of the ρ_k, as analysed in [BP] II, and the behaviour of our field coordinates. (BP III, 611b)

Bohm and Pines extend the parallel further. They anticipate that, just as in the classical theory of plasmas developed in BP II there is a minimum wavelength λ_c of organized oscillations, and a corresponding maximum wave number k_c, so in the quantum theory a similar (but not identical) cut-off exists (BP III, 611b). Accordingly, they use a modified version of the transformation that yielded H^2 to obtain an operator H^3. The effect of this modification is to eliminate field coordinates with wave vector k greater than some critical wave vector k_c, and so confine attention to k-values between $k = 0$ and $k = k_c$. Of H^3, Bohm and Pines remark (BP III, 616a),

> There is a close resemblance between [this] Hamiltonian, which describes a collection of electrons interacting via longitudinal fields, and the Hamiltonian [H_0] we considered in BP I, which described a collection of electrons interacting via the transverse electromagnetic fields. [. . .] [O]ur desired canonical transformation is just the longitudinal analogue of that used in BP I to treat the organized aspects of the transverse magnetic interactions in an electron gas.

Algebraic manipulation shows that H^4 can be represented schematically as:

$$H^4 = H_{\text{part}} + H_{\text{I}} + H_{\text{osc}} + H_{\text{s.r.}} + U$$

Here H_{part} represents the energy of particles (electrons) due to their motion and self energy; H_{I} "represents a simple interaction between electrons and the collective fields"; H_{osc} is "the Hamiltonian appropriate to a set of harmonic oscillators, representing collective

18. The fact that the transformation allowed the system too many degrees of freedom gave Bohm and Pines considerable trouble; see Pines (1987, 75).

fields"; and $H_{s.r.}$ "represents the short range part of the Coulomb interaction between the electrons [i.e., the effective Coulomb interaction once the effects of screening have been taken into account]" (BP III, 612b). The authors use (and justify) the random phase approximation to show that the remaining term U can be disregarded (BP III, 613b-614b), and then rewrite H_{osc}, dubbing the result "H_{field}"[19] In this way they arrive at the Hamiltonian H^5:

$$H^5 = H_{part} + H_I + H_{field} + H_{s.r.}$$

The "subsidiary conditions" on the allowable states F of the system now appear as a set of k_c conditions:

$$\Omega_k \Phi = 0 \qquad (k < k_c)$$

and a new expression for the operators Ω_k is given (BP III, 616b).

Following the strategy of BP I, Bohm and Pines apply a canonical transformation to H^5 and to the Ω_k operators. The transformation is designed to "eliminate H_I to first order" by distributing most of its effects among the (transformed versions of) other terms of H^5, and so redescribe the system in terms of "pure collective coordinates."[20] That is to say, no term of the transformed Hamiltonian contains both particle and field operators. The modifications have done their work. **))**

The final Hamiltonian, which Bohm and Pines christen "$H_{new,}$" differs markedly from the Hamiltonian H^1 from which they started. It is expressed schematically as:

$$H_{new} = H_{electron} + H_{coll} + H_{res\ part.}\ .$$

19. H_{field} is obtained (BP III, 616a) by replacing each occurrence of q_k in H_{osc}, by the expression $(\hbar/2\omega)^{1/2}(a_k - a_k^*)$. The reason for making this move will be made clear in Section 2.7. q_k represents a component of the Fourier expansion for the longitudinal vector potential of the electromagnetic field $\mathbf{A}(\mathbf{x})$, and a_k and a_k^* are, respectively, the creation and annihilation operators for the longitudinal photon field. (BP III, 616a). The equation $q_k = (\hbar/2\omega)^{1/2}(a_k - a_k^*)$ is an identity in the operator algebra used in quantum mechanics. For an introduction to creation and annihilation operators, see Messiah (1958), 438–39 and 963–66. For the mathematical definition of a transformation in this context, see the examples 3, 6, and 7 in Section 2.3.

20. What is left of H_I is then discarded, and the resulting Hamiltonian symbolized by "$H_{new}(0)$ " to mark the fact that it is a lowest-order approximation. In Section III of BP III Bohm and Pines then show that this residue of the (transformed version of) H_I may be neglected, and so the superscript "(0)" is dropped.

Like $H_{(0)}$ in BP I, this Hamiltonian contains just three parts. H_{electron} contains terms referring only to individual electrons; field coordinates appear only in H_{coll}, which describes independent longitudinal oscillations of the field; and $H_{\text{res. part.}}$ represents an extremely weak "residual" electron-electron interaction; at short range the electrons are effectively screened off from each other. Thus the quantum mechanical treatment of the plasma has replicated the conclusions of the classical treatment in BP II.

> **((** The oscillations described by H_{coll} are independent in the sense that, under the canonical transformation, (i) the subsidiary conditions that guarantee conformity with Maxwell's equations no longer relate field and particle variables, and (ii) H_1, which represented field-particle interactions in H^3, H^4, and H^5, has now disappeared; effectively, the final transformation has distributed it between two terms of the Hamiltonian,[21] Part of it reappears as $H_{\text{res part}}$; the electron-electron interaction this operator describes is negligible in comparison with the short range interaction described by $H_{\text{s.r.}}$ in H^4. The other part has been absorbed into H_{electron}, where it appears as an increase in the "effective mass" of the electron. Bohm and Pines interpret this (BP III, 620a-b) as "an inertial effect resulting from the fact that these electrons carry a cloud [of collective oscillations] along with them." (See fn. 16.) **))**

In this way the program announced in BP I has been carried out. Bohm and Pines have demonstrated "explicitly in Hamiltonian form the effective separation between long range collective interactions, described here in terms of organized oscillations, and the short range interactions between individual particles" (BP I, 626b).

1.5 P IV: Electron Interaction in Metals

In P IV, the last paper in the series, Pines applies the quantum mechanical account of plasmas developed in BP III to the behaviour of the valence electrons in metals (otherwise known as "conduction electrons"). He begins by drawing attention to the assumptions this involves (P IV, 626a). In BP III the system described by the initial Hamiltonian H^1 consisted of cloud of electrons moving against a uniform background of positive charge. In a metal, however, the positive charge consists of positive ions localized on the nodes of the crystal lattice; in addition, this lattice under-

21. Note that in BP III the expressions H_{part} and $H_{\text{s.r.}}$, denoted components of H^4. Since H_{new} is a transformed version of H^4, it would have been better to denote the corresponding terms in H_{new} by H^C_{part} and $H^C_{\text{s.r.}}$, in order to reflect the fact that pure collective coordinates are being used. Instead, in P IV (627a) they appears, misleadingly, as H_{part} and $H_{\text{s.r.}}$.

goes vibrations. Thus, when the results of BP III are carried over into P IV, the first assumption made is that the periodicity and the density fluctuations of the positive charge in a metal can be ignored. The second is that "the only interactions for the conduction electrons in a metal are those with the other conduction electrons" (P IV, 626).

An important goal throughout P IV is to show that, despite the simplifications and idealizations involved, the theoretical results of BP III hold for the electron interactions in (at least some) metals.[22] The first item on Pines's agenda is to show how the collective account can explain why, for many purposes, the independent electron model (described here in Section 1.1) worked as well as it did. His argument (P IV, 627a-b) is very simple. He reminds the reader that two important mathematical results have been obtained in BP III: an expression for the Hamiltonian H_{new} for the system, and the subsidiary conditions O_k on its states. The collective description of the electron gas that H_{new} provided included a term H_{coll} which summarized, so to speak, the effects of the long range Coulomb interactions between electrons. They were "effectively redescribed in terms of the collective oscillations of the system as a whole" (*ibid.*). But, given the dispersion relation for these oscillations, which relates their frequency to their wavelength,

> It may easily be shown that the energy of a quantum of effective oscillations is so high [. . .] that these will not normally be excited in metals at ordinary temperatures, and hence may not be expected to play an important role in metals under ordinary conditions. (*Ibid.*)

Pines goes on,

> The remainder of our Hamiltonian corresponds to a collection of individual electrons interacting via a comparatively weak short-range force, $H_{\text{s.r.}}$. These electrons differ from the usual "free" electrons in that they possess a slightly larger effective mass [see Section 1.4 of this essay], and their wave functions are subject to a set of [subsidiary] conditions. However, both of these changes are unimportant qualitatively (and in some cases quantitatively). Furthermore, since the effective electron-electron interaction is so greatly reduced in our collective description, we should expect that it is quite a good approximation to neglect it for many applications.

22. Pines remarks (P IV, 626), "This assumption should be quite a good one for the alkali metals [,,,], and we may expect it to apply generally for any metallic phenomena in which the periodicity of the lattice plays no important role."

> Thus we are led directly to the independent electron model for a metal.

Like the arguments of BP III, this argument arrives at a theoretical conclusion about an electron gas through an examination of its theoretical description, the Hamiltonian H for the system.

> **((** The Hamiltonian Pines uses is a modified version of the Hamiltonian H_{new} obtained in BP III. Whereas at the end of that paper Bohm and Pines wrote,
>
> $$H_{new} = H_{electron} + H_{coll} + H_{res\ part}$$
>
> in P IV Pines rewrites $H_{electron}$ as $H_{part} + H_{s.r.}$, and neglects $H_{res\ part}$ since "it will produce negligible effects compared with $H_{s.r.}$, and this latter term is small." (P IV, 627a) The resulting Hamiltonian is:
>
> $$H = H_{part} + H_{coll} + H_{s.r.}\ .\ \textbf{))}$$

In the remainder of P IV Pines examines the quantitative results that this Hamiltonian yields. In Section II he uses it to calculate the energy e_0 of the system in its ground state, and compares the result with those reached by other approaches. In Section III he points to a problem encountered by the independent electron approach, and suggests how it could be accounted for by the BP theory. In Section IV he shows how that theory can also explain the behaviour of high energy particles as they pass through metal foils.

> **((** Pines begins Section II by pointing out that, given the Hamiltonian for the system, the direct way to obtain ϵ_0 would be to solve the time-independent Schrödinger equation,
>
> $$H_{new}\Psi_0 = \varepsilon_0\Psi_0$$
>
> in which ε_0 appears as the lowest eigenvalue corresponding to the eigenfunction Ψ_0. (See fn, 33.) Instead, for ease of calculation, he discards the smallest term $H_{s,r.}$ of H_{new} , and works with a wave function Ψ_0 which is both an exact eigenfunction of the resulting Hamiltonian ($H_{part} + H_{coll}$), and an approximate eigenfunction of H_{new} .[23] He argues (P IV, 630a-b) that, since the contribution of the

23. Note that, even though he works with an approximate eigenfunction, Pines still denotes it by "Ψ_0".

neglected term $H_{s.r.}$ is small, it can be treated later as a small perturbation in H_{new} , giving rise to a small short-range "correlation energy" ε_{corr} .[24]

Here the BP theory and the independent electron model part company. When ϵ_0 is compared with the energy E as calculated on (one version of) the independent electron model, the two values differ by a small amount which Pines calls a "correlation energy." This can be broken into two parts, corresponding to a long range and a short range interaction between electrons. Symbolically,

$$\varepsilon_0 - E = \varepsilon_{corr.} = \varepsilon_{corr}^{l.r.} + \varepsilon_{corr}^{s..r.} .$$

In Section III Pines shows that, when this "correlation energy is taken into consideration, the independent electron model faces a problem. When all electron-electron interactions are neglected, and the energy of the electron gas is taken to be the Fermi energy E_F ,[25] many of the results obtained using the model agree quite well with experiment. Quantum mechanics, however, decrees that one should allow for an additional "exchange energy" E_{exch},[26] and when the energy is "corrected" from E_F to $E_F + E_{exch}$, the agreement with experiment, far from being improved, is rendered worse than before. Pines illustrates this with two examples. The first (P IV, 631a) concerns the specific heat of an electron gas. According to the "corrected" version of the independent electron model, this quantity will vary as $T/\ln T$ (where T is the absolute temperature).[27] Experiment, however, shows a linear dependence on T. The second example (P IV, 632a-b) concerns the magnetic properties of an electron gas. According to the "corrected" account, the electron gas in cer-

24. I say more about perturbation theory in example 4 (and fn. 44, which accompanies it) in the discussion of the use of theory in Section 2.3, and also in Section 2.7.

25. According to the Pauli exclusion principle, no more than one electron can occupy a given state. To each state there corresponds a certain energy level. In a gas of n electrons at absolute zero the first n levels will be filled, and the gas will have a corresponding energy. This is called the *Fermi energy.* Because the energy levels are close together, for modest values of T the energy will not change much, since every electron that jumps to a level just above the n^{th} level vacates a level just below it.

26. If two similar quantum systems—in this case, two electrons—are close together, quantum mechanics tells us that their combined state must be such that it would not be altered if the two systems exchanged their individual states. The effect adds a small energy E_{exch} to the energy of the composite system.

27. Here Pines draws on theoretical work by James Bardeen and E.P. Wohlfarth; see P IV (631a).

> tain metals—cesium, for example—would display ferromagnetic behaviour (i.e., the spins of the electrons could become aligned). But no such behaviour is observed. Neither anomaly arises with the simpler version of the independent electron model.
>
> In contrast, on the BP model, the long range Coulomb interactions that lead to a large exchange energy E_{exch} are replaced by effective screened short range Coulomb interactions (see Section 1.4), with a corresponding reduction in energy contributions. The net result is that the effect of exchange energy on the specific heat of the electron gas is comparatively slight, and the model never displays ferromagnetic behaviour (P IV, 623a-b). **))**

In the final section of P IV Pines returns to a phenomenon discussed in BP II, the excitation of plasma oscillations by high energy charged particles passing through a metal. To describe the motion of the particle and its interaction with the electron gas, he adds two terms to the Hamiltonian H^5 of BP III; in order to rewrite these terms in collective coordinates, he then applies to them the same canonical transformation that took H^5 into H_{new} in BP III; lastly, he uses the random phase approximation, the dispersion relation, and the subsidiary conditions of BP III to obtain the three-term Hamiltonian H_{add} (P IV, 633a-b). The first term describes "a short range screened Coulomb interaction between the charged particle and the individual electrons in the electron gas" (P IV, 633a); the second "the interaction between the charged particle and the collective oscillations of the system" (P IV, 633b); the third may be neglected. Analysis of the second term (P IV, 633b-634a) shows that the interaction generates forced oscillations in the collective field, which at a certain frequency ω_P will produce resonant oscillations in the electron gas. By the same argument as was used in BP II, since the energy associated with an oscillation of frequency ω_P is $\hbar\omega_P$ (where $\hbar$ is Planck's constant), the total energy loss suffered by a high energy particle in exciting such oscillations should be some multiple of $\hbar\omega_P$. In this way, energy is transferred from the particle to these oscillations in discrete quanta. The quantum of energy loss sustained by the particle in each transfer can be calculated, and the results agree well with the experimental findings of Ruthemann and Lang mentioned earlier.

Finally, Pines uses the first term of H_{add} to obtain an expression for the "stopping power" of a metal, the loss of energy by a charged particle per unit length of the distance it travels through the metal (P IV, 635a-b). He compares his results with those predicted by Aarne Bohr (1948) and H.A. Kramers (1947), each using a different theoretical approach, and with those obtained experimentally for lithium and beryllium by Bakker and

Segré (635b).[28] All four sets of results coincide, within the ranges of error of their respective theoretical and experimental practices.

PART TWO
Observations on the Bohm-Pines Quartet

2.1 Introduction

The physicist John Ziman offers this description of "the intellectual strategy of a typical paper in theoretical physics" (1978, 3–4):

> A model is set up, its theoretical properties are deduced, and experimental phenomena are thereby explained, without detailed reference to, or criticism of, alternative hypotheses. Serious objections must be fairly stated; but the aim is to demonstrate the potentialities of the theory, positively and creatively, "as if it were true."

Individually and collectively, the four Bohm-Pines papers all conform to this description; indeed it might have been written with them in mind.[29] My comments on them are grouped under six headings: the use of models, the use of theory, the modes of description offered, the use of approximations, the connection with experiment, and the nature of deduction as it appears in the quartet. My aim is to provide descriptions, rather than evaluations, of the theoretical practices that Bohm and Pines engaged in. But before I embark on this project, a few remarks about Ziman's capsule summary are called for. Like the papers in theoretical physics it describes, the summary itself takes a lot for granted. A typical paper may well begin by setting up a model, but this is not done in a theoretical vacuum. Physicists inherit the achievements of their predecessors, and yesterday's models become today's physical systems, waiting to be modelled in their turn. Bohm and Pines, for example, take for granted, first, the account of a plasma as an electron gas that contains enough positively charged ions to neutralize the electrons' charge, and secondly, the assumption that a metal is a special case of such a system, whose distinguishing feature is the systematic spacing of the ions. Both assumptions are implicit in the references Pines makes to "the ionic field," to "laboratory plasmas," and (in a particularly telling phrase) to "the actual plasma" in the quotations with which the next section begins.

28. Kramers used "a macroscopic description in which the electrons were treated as a continuum characterized by an effective dielectric constant." We will meet it again in Part Three.

29. In point of fact, Ziman had read these papers carefully; see Ziman (1960, 161–68).

2.2 The Use of Models

In the quartet Bohm and Pines reserve the word "model" for the independent electron model. Yet their own approach also relies on a highly simplified model of a metal, one which enabled them to treat it like a gas plasma. By 1955 Pines describes it in just those terms (1955, 371): "[W]e shall adopt a simplified model for a metal in which we replace the effect of the ionic field by a uniform background of positive charge." And, whereas in 1951 this theoretical move is relegated to a footnote (BP I, fn. 1), Pines subsequently accords it much more importance. He writes (1987, 68),[30]

> In any approach to understanding the behaviour of complex systems, the theorist must begin by choosing a simple, yet realistic model for the behaviour of the system in which he is interested. Two models are commonly taken to represent the behaviour of plasmas. In the first, the plasma is assumed to be a fully ionized gas; in other words as being made up of electrons and positive ions of a single atomic species. The model is realistic for experimental situations in which the neutral atoms and the impurity ions, present in all laboratory plasmas, play a negligible role. The second model is still simpler; in it the discrete nature of the positive ions is neglected altogether. The plasma is then regarded as a collection of electrons moving in a background of uniform positive charge. Such a model can obviously only teach us about electronic behaviour in plasmas. It may be expected to account for experiments conducted in circumstances such that the electrons do not distinguish between the model, in which they interact with the uniform charge, and the actual plasma, in which they interact with positive ions. We adopt it in what follows as a model for the electronic behaviour of both classical plasmas and the quantum plasma formed by electrons in solids.

This paragraph is very revealing. Pines tells us that:

1. The theorist's task involves choosing a model.
2. In dealing with a complex system, that is the only option available.
3. The model can help us to understand the behaviour of a system; in other words, it can provide explanations of that behaviour (a claim echoed by Ziman).

30. This paragraph was comes from Pines' contribution to a *Festschrift* in Bohm's honour (Hiley and Peat, 1987). Fittingly, Pines goes on to describe his work on electron interaction in metals.

4. The model is to be "simple, yet realistic."
5. The model will involve some simplification; in the first model Pines describes, impurities and neutral atoms are to be disregarded.[31]
6. A model may misrepresent aspects of the system; a regular array of positive ions may be represented as a background of uniformly distributed positive charge.
7. More than one way of modelling may be used.

We may also observe that:

8. The components of the models—electrons, positive ions—would be described in standard philosophical parlance as "theoretical entities."
9. The two models Pines describes are at odds with one another.
10. The model he adopts involves a greater degree of misrepresentation than the other.

In the paragraph I quoted, Pines deals with a particular physical system and the ways in which it can be modelled. In contrast, among the aspects of modelling I have listed, the first eight are very general, and analogues of aspects 9 and 10 appear frequently in theoretical practice. Some amplification of these points is called for.[32]

Note first that, in addition to the simplifications already mentioned, there are interactions that the jellium model cannot accommodate. Because it ignores the fact that ions form a regular lattice, the model cannot take into account interactions between electrons and lattice vibrations (which are instrumental in bringing about superconductivity).[33] In addition, the only electron-electron interactions considered are between conduction electrons; interactions between conduction electrons and core electrons (those immediately surrounding the positive nuclei of the ions) are assumed to have no importance.[34]

Despite these simplifications, and items 5–10 above, Pines tells us that a model should be "realistic" [4], and claims that, for certain experimental situations, the model he uses meets that criterion. A contrasting view is expressed by Conyers Herring. In commenting on a theoretical account of

31. What I have called "simplification" some philosophers refer to as "abstraction." See (e.g.) the footnote on p. 38 of Morgan and Morrison (1999).

32. In the remainder of this section a numeral placed in parentheses (e.g. "[9]") draws attention to the specific point being discussed.

33. I return to this interaction in Part Three of this essay.

34. Pines himself draws attention to these simplifications at the start of P IV (626a).

the surface energy of a metal (Ewald and Juretschke 1952) he observes (1952, 117),

> It is to be emphasized that the wave mechanical calculation of the surface energy given in the paper applies not to a real metal, but to a a fictitious metal [. . .] The fictitious metal consists, as has been explained, of a medium with a uniform distribution of positive charge—we may call it a "positive jelly"—and a compensating number of electrons. This metal [. . .] we may call "jellium" to distinguish it from real metals such as sodium.

The same model that Pines describes as "realistic" is here described by Herring as "fictitious." There is no direct contradiction between these descriptions; works of fiction can be realistic. And, while the jellium model is not realistic in quite this sense, jellium itself is endowed by Herring with physical properties: "A rough calculation [. . .] has indicated that jellium of an electron density equal to that of sodium should have a binding energy only about two thirds that of sodium." (*Ibid.*)

By using the term "realistic," Pines marks a distinction between two types of model: the models he himself works with and *analogue* models like the liquid-drop model of the nucleus proposed by Niels Bohr in the late 1930s. An analogue model relies on a correspondence between the behaviours of two otherwise radically different types of physical systems (nuclei and liquid drops, for example). A "realistic" model, on the other hand, is defined by taking a description of the physical system to be modelled, and modifying it by a process of simplification and idealization [5, 6]. These descriptions will be in a well understood vocabulary that includes terms like "electron" and "positive ions." The philosopher may regard such entities as merely "theoretical" [8], but when Pines used them they had been familiar and accepted elements of the physicist's world for forty years.

But why are such models *necessary,* as Pines insists [1, 2]? They are needed because without them it would be impossible to apply our theories to the physical world. In the first place, given a "complete description" of a natural system, we still need a principle by which irrelevancies can be winnowed out from salient information. Secondly, from Galileo onwards, our theories of physics have treated only ideal entities (point masses, rigid bodies, frictionless planes), items that are absent from the physical world. As we have seen, a realistic model of the kind Pines envisages is an entity defined in terms of simplifications and idealizations [5, 6]. Effectively, the model's definition allows it to act as a principle of selection, and the idealizations built into it make it amenable to treatment by our theories. A

model of this kind functions as an essential intermediary between the theories of physics and the physical world.[35]

How does a model help us to understand the behaviour of a physical system [3]? The short answer is that models are things we can play with. A model's resources are gradually made available to us as we come to see how much can be deduced from its theoretical definition, and how aspects of its behaviour are interlinked. As a variety of phenomena are successfully represented by the model, it becomes progressively more familiar to us; increasingly, we come to see the physical system in terms of the model, and vice versa.[36]

Because a model of this kind is a defined entity, pragmatic considerations can influence what simplifications and idealizations are made [6]. The theorist has to make choices [7, 9]. An additional idealization may make his problems more tractable, but at the cost of making his predictions less accurate. Luckily these choices are not irrevocable. In making that idealization, the theorist will be opting for the model that involves the greater degree of misrepresentation [10], but he can always rescind it in the search for greater empirical adequacy. Pines did just that. Throughout the quartet he and Bohm opted for the jellium model, and treated the positive charge of metallic ions as uniformly distributed. Three years later Pines (1956) treated the charge more realistically, as one that varied regularly within the metal, in order to make more accurate estimates of the energy loss of high energy particles in metal foils.

2.3 The Use of Theory

After a model has been set up, Ziman tells us, "its theoretical properties can be deduced." But, as we learn from Pines, the same model can be adopted "as a model for the electronic behaviour of both classical plasmas and the quantum plasma formed by electrons in solids." Which is to say (i) that the behaviour of the model is assumed to be governed by a foundational theory, and (ii) that in some cases it is appropriate to use classical mechanics, and in others, quantum mechanics. We have seen this happen, first in BP I, which provided both classical and quantum mechanical treatments of magnetic interactions between electrons, and then in BP II and BP III, which provided, respectively, classical and quantum mechanical

35. This point has been made by many writers, including Nancy Cartwright (1983 and 1999), Ernan McMullin (1985), Ronald Giere (1985) and (1988), Mauricio Suarez (1999), and Margaret Morrison and Mary Morgan (1999). Morrison and Morgan go on to emphasize that models act as mediating instruments in a great variety of ways in addition to the one I discuss here.

36. For an extended account along these lines, see Hughes (1993).

accounts of electrostatic (Coulomb) interactions. In addition, the use of a "correspondence principle argument" in BP II can be seen as an appeal to the old (pre-1925) quantum theory. (See Section 1.3.) A wholly classical analysis is given of the energy loss suffered by a high velocity charged particle when it excites plasma oscillations; this analysis is then supplemented by the assumption that the energy loss is quantized, and that the energy E per quantum is given by the Planck formula, $E = \hbar w$. Here $\hbar$ is Planck's constant, and ω is the frequency of the plasma oscillation which it excites.

Early quantum theory was notorious for its reliance on *ad hoc* procedures. To quote Max Jammer (1966, 196),

> In spite of its high sounding name [. . .] quantum theory, and especially the quantum theory of polyelectronic systems, prior to 1925 was, from the methodological point of view, a lamentable hodgepodge of hypotheses, principles, theorems, and computational recipes rather than a logical consistent theory.

The introduction in BP II of the Planck formula within an otherwise classical treatment of plasmas is a case in point. The formula functions simply as a useful tool in the theoretician's workshop. In contrast, the theories the authors use in BP III are post-1925 orthodox quantum mechanics and (occasionally) standard electromagnetic theory and quantum field theory. Each of these theories is a foundational theory, in the sense that it is undergirded by a powerful mathematical theory.[37] While not conceptually wrinkle-free,[38] none of them would normally be described as "a lamentable hodgepodge." Nonetheless, in one respect, each of them resembles early quantum theory in the first quarter of the 20th century. They too provide sets of ready-to-hand tools for the theoretician's use. To extend the metaphor, the difference is that all the tools from a given theory now come from the same tray of the toolkit.[39]

I will illustrate this use of theory with nine examples from the Bohm-Pines papers, the first very general, the rest specific. In all four papers foundational theory provides a template for the mathematical description

37. A paradigm example is the mathematical theory set out in John von Neumann's *Mathematical Foundations of Quantum Mechanics* ([1932] 1955).

38. Orthodox quantum mechanics was not a wholly unified theory, and no-one has yet solved the "measurement problem."

39. I was first introduced to the image of a theory as a set of tools by Paul Teller, in conversation. It is drawn by Nancy Cartwright, Tofic Shomar, and Mauricio Suarez in the paper, "The Tool-Box of Science" (1994). The earliest use of it that I can trace is by Pierre Duhem in the series of articles in the *Revue de Philosophie* in 1904 and 1905 that later became his *Aim and Structure of Physical Theory* (Duhem [1914] 1991, 24).

of a system, and specifies how that form can be given content. Whether provided by classical physics or quantum mechanics, these descriptions are given by the Hamiltonian H for the system, which represents its total energy.[40] H may be written as the sum $H_1 + H_2 + \ldots + H_n$ of terms, each of them representing a different source of energy (kinetic energy, electrostatic potential energy, and so on). The mathematical nature of H is not the same in the two theories; in classical physics H is a function, in quantum mechanics it is an operator.[41] There are, however, standard procedures for obtaining a quantum mechanical Hamiltonian from a classical one. To take a particular case, wherever a momentum p appears in a classical Hamiltonian function, one substitutes the operator $-i\hbar(\delta/\delta x)$ to obtain the corresponding quantum mechanical operator; thus the kinetic energy term represented classically by the function $p^2/2m$ appears as the quantum mechanical operator $-(\hbar^2/2\mathrm{m})\delta^2/\delta x^2$.

Specific instances of the use of theoretical tools—results, strategies, and technical manoeuvres—which theory provides and whose use needs no justification, are supplied by eight examples from BP III. Here as elsewhere, Bohm and Pines are considering "an aggregate of electrons embedded in a background of uniform positive charge" (610b).[42]

1. (610b) The authors write down the Hamiltonian H^1 for the system. It contains three terms: The first is the operator provided by the theory to express the kinetic energy of the electrons; the second and third are standard expressions for the energy due to Coulomb attractions between electrons, and for their self energy; each of them is slightly modified to take into account the uniform background of positive charge, and the second term is expressed as a Fourier series (see fn. 13).
2. (610b-611a) When this Hamiltonian is rewritten in terms of the longitudinal vector potential $\mathbf{A}(\mathbf{x})$ and the electric field intensity

40. In classical physics an alternative mode of description, in terms of forces, may be used, but this is not available in quantum mechanics.

41. In classical physics the Hamiltonian H of a system is a function $H: \Omega \rightarrow \mathbf{R}$, where Ω is the set of states of the system. For any state $\omega \in \Omega$, the number $H(\omega)$ is the total energy of the system in that state. In quantum mechanics the Hamiltonian H of a system is an operator $H: \Psi \rightarrow \Psi$, where Ψ is the set of states of the system. A state in quantum mechanics is represented by a wave function ψ. For any state $\psi \in \Psi$, there is a state ψ_H (not necessarily distinct from ψ) such that $H(\psi) = \psi_H$. If ψ is an *eigenfunction* of H, then there is a real number E such that $H(\psi) = E_\psi$ (which is to say, E is an *eigenvalue* of H), and E is the energy of the system in state ψ (as in the synopsis of P IV). A fuller comparison of classical physics and quantum mechanics is given in Hughes (1989, ch. 2). Jordan (1969) provides a comprehensive yet concise treatment of the operators used in quantum mechanics.

42. All page references in this subsection are to BP III. With one exception, the synopsis of BP III in Section 1.4 shows where each of the tools was used.

E(**x**), each of these quantities is expressed as a Fourier series involving one of the field coordinates q_k or p_k (for the position and the momentum associated with the field). Both of these series are supplied by electromagnetic theory.

3. (611a-b) To show that the resulting Hamiltonian H^2 is equivalent to H^1, Bohm and Pines use the method quantum mechanics prescribes: they display a unitary operator S, such that $H^2 = SH^1S^{-1}$.[43] As they note (611a, fn.), this operator is supplied by Gregor Wentzel's textbook, *Quantum Theory of Wave Fields.*
4. (614b) The "perturbation theory" of quantum mechanics is called on to estimate what corrections would have to be applied to compensate for the neglect of the terms U and H_I in H^4.[44]
5. (616a) As I noted in example 2, at an early stage of BP III Bohm and Pines introduce field coordinates q_k and p_k . Now, "in order to point up the similarity [between the transformations used in BP III and those used in BP I] and to simplify the commutator calculus," they help themselves to the fact that in quantum mechanics these operators can be expressed in terms of creation and annihilation operators a_k and a_k* (see fn. 19).
6. (616b-617a) The last of the transformations performed on the Hamiltonian in BP III takes H^5 into H_{new}. Like the transformation of H^1 into H^2 mentioned in example 3, this transformation is performed by a unitary operator. The authors' goal is to find a unitary operator U such that $U^{-1}H^5U = H_{\text{new}}$. A basic theorem of the algebra of operators is that any unitary operator U is expressible as an exponential function of another (non-unitary) operator S, the so-called *generator* of U; we may write $U = \exp(iS/\hbar)$.[45] Hence Bohm and Pines set out to obtain U by finding a suitable generator S.

43. For more on unitary operators see Jordan (1969, 18–22). S is a unitary operator if there is an operator S^{-1} such that $S\,S^{-1} = I = S^{-1}S$, where I is the identity operator: for all ψ, $I(\psi) = \psi$. S^{-1} is the *inverse* of S; for any wave function ψ_1, if $S(\psi_1) = \psi_2$, then $S^{-1}(\psi_2) = \psi_1$.

44. The use of perturbation techniques in physics goes back to Isaac Newton. In quantum mechanics, perturbation theory is invoked when a term that makes a very small contribution to the Hamiltonian is neglected to simplify calculations. Its function is to estimate the correction that would have to be applied to the result obtained in order to allow for the effect of that term. A concise account of the principles of first and second order perturbation theory in quantum mechanics is given by Cassels (1970, 780–80); for more details see Messiah (1958, chs. XVI–XVII). To estimate corrections for U and H_I in H^3, Bohm and Pines use second order perturbation theory.

45. See Jordan (1968, 52). Also, two remarks on notation: (*a*) In example 3, I used the letter *"S"* for the unitary operator that transforms H^1 into H^2, but in this example I use it for the *generator* of the unitary operator that transforms H^4 into H_{new}, rather than the unitary

This was a perfectly orthodox strategy to employ; in a footnote (616b) the authors cite the second edition of P.A.M. Dirac's classic text, *The Principles of Quantum Mechanics.* As it turned out, implementing the strategy was a different matter. The authors tell us (617a),

> The problem of finding the proper form of S to realize our program was solved by a systematic study of the equations of motion. We do not have the space to go into the details of this study here but confine ourselves to giving the correct transformation below.[46]

Evidently, in this instance no ready-to-hand tool was available that would do the job.

7. (617b) To continue the narrative of example 6: Given the generator S, H^5 can be transformed into H_{new} by the rule used in example 3: $H_{new} = UH^5U^{-1}$, where $U = \exp(iS/\hbar)$, and $U^{-1} = \exp(-iS/\hbar)$. A certain amount of algebraic drudgery would then yield an expression for H_{new}. Bohm and Pines obtain considerable simplifications, however, by using a standard mathematical tool, another theorem of the algebra of operators:[47]

   Given (self-adjoint) operators A, A', and S, if $A' = UAU^{-1}$, where $U = \exp(iS)$, then

$$A' = A - i[A,S] - \tfrac{1}{2}[A,S],S] + (i/3!)[A,S],S]S\ ] + \ \ldots$$

   Here $[A,S]$ is the *commutator* of A and S, i.e. $[A,S] = AS - SA$.[48]

8. (623b) The final example comes from Appendix I to BP III, in which Bohm and Pines develop a quantum-mechanical version of the approach used in BP II, based on fluctuations of the charge density in the plasma. Here the recourse to the toolbox of theory is explicit:

> We use the electron field second-quantization formalism [. . .]. Following the usual treatments,* we describe electrons

operator itself. (*b*) I write *"U"* for the second unitary operator, even though the same letter is used elsewhere to denote a term in the Hamiltonian H^3. Within this example, *"U"* always denotes a unitary operator and *"S"* its generator. (See item 7 in the list of theoretical tools.) In both cases, (*a*) and (*b*), I am following the usage of Bohm and Pines.

46. The reader is given no further information about this "systematic study," and is left to conjecture that it involved a sophisticated form of trial and error.

47. While the theorem itself is standard, and its proof is straightforward, its use in this context required considerable ingenuity.

48. Since both the product and the difference of two operators are also operators (hence

> by the field quantities $\Psi(x)$ which satisfy the anti-commutation relations.

At the point marked with an asterisk the authors again cite G. Wentzel, *Quantum Theory of Wave Fields* (1949).

These tools are not all of one kind. They include mathematical identities (examples 6 and 7), existing results within electromagnetic theory, quantum field theory and orthodox quantum mechanics (example 2, examples 3 and 8, and example 5, respectively), well-established perturbation techniques of approximation (example 4), and standard mathematical expressions for physical quantities (example 1). As my footnotes and the authors' own citations show, all these tools are to be found in well-known textbooks.

If, as I suggested in the Preamble to this essay, the example of the Bohm-Pines quartet can be generalized, a preliminary account of theoretical practice emerges.[49] To apply, say, quantum mechanics to a particular physical situation—in Kuhnian terms, to work within the paradigm that quantum mechanics provides—a physicist must have a working knowledge of how and when to use the following elements: the simple models that the theory deals with; the mathematical representations of their behaviour that the theory prescribes; the mathematical theory within which these representations are embedded (in this case the theory of Hilbert spaces, which includes as a sub-theory the algebra of operators); and the perturbation techniques and methods of approximation associated with the theory. This congeries of elements I call a *theoretical manifold.* I do not claim that my list of elements is exhaustive. An additional requirement, fifty years after the Bohm-Pines papers were published, is a knowledge of how to use computer methods to assist the work of—or to replace—one or more of the above.

Phrases like "that the theory deals with" and "that the theory prescribes," which qualify some of the items in this catalogue, might suggest that underpinning every theoretical manifold there is a theory separable from the manifold it supports. From the perspective of the practitioner of physics, however, that assumption would be a mistake. As Gilbert Ryle said in another context (1963, 18),

> The same mistake would be made by a child witnessing the march-past of a division, who, having had pointed out to him such and such battalions, batteries, squadrons, etc., asked when the division

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the expression "the algebra of operators"), all the expressions on the series on the right hand side of the equation are well-formed.

49. I augment this account in Part Three of this essay.

> was going to appear. He would be supposing that a division was a counterpart to the units already seen, partly similar to them and partly unlike them. He would be shown his mistake by being told that in watching the battalions, batteries and squadrons marching past he had been watching the division marching past. The march-past was not a parade of battalions, batteries, squadrons *and* a division; it was a parade of the battalions, batteries and squadrons *of* a division.

Analogously, all the items in the theoretical manifold may be thought of as elements *of* the theory; to use a theory is to use one or more of the elements of the theoretical manifold. On this reading the terms "theory" and "theoretical manifold" are co-extensive. Call this the *broad* reading of "theory".

Customary usage, however, suggests that some elements of the theoretical manifold are more central than others. In the list given, the phrase "associated with the theory" carries the connotation that the items it qualifies, the theory's "perturbation techniques and methods of approximation" occupy positions peripheral to the manifold's central core. Central to the manifold, on this account, would be the elements to which the perturbations and approximations are applied, namely the simple models of the theory and the mathematical representations of their behaviour. We may reserve the term "theory" for this central cluster of elements, while pointing out that without the other elements of the manifold very little could be achieved in the way of theoretical practice. Call this the *narrow* reading of "theory".

I have so far restricted my use of the term "theory" to foundational theories, theories like classical mechanics, classical electromagnetic theory, or quantum mechanics that have a wide range of applicability. But there is also a more local, but perfectly respectable, use of the term, as when we talk of "the Bohm-Pines theory of electronic behaviour in metals." Indeed, Ziman gives this local use primacy. He writes (1964, v), "A theory is an analysis of the properties of a hypothetical model."[50] A *local theory* will employ the theoretical manifold of an existing foundational theory, but will confine its application to a single model, here the jellium model. But in addition to the standard methods of approximation and perturbation theory that come with the foundational manifold, the local theory may also introduce approximative techniques of its own. Again, the Bohm-Pines theory is a case in point, as we shall see.

50. The quotation comes from Ziman's preface to his *Principles of the Theory of Solids* (1964).

2.4 Modes of Description

The title of the B-P quartet promises a "Collective Description of Electron Interactions." In fact, two modes of description, physical and mathematical, are presented. The physical descriptions are in English, augmented by the vocabulary of physics. Couched in terms of "electrons", "fields", and so on, they are, strictly speaking, descriptions of the jellium model, but in the authors' discourse the distinction between model and physical system is rarely observed. In Pines's words, the model chosen is to be "simple, yet realistic," and the physical descriptions are clearly intended to be construed realistically.

The mathematical descriptions of the model are provided by its Hamiltonian, as we have seen. A Hamiltonian is standardly expressed as a sum of individual terms, each of them either a standard issue Hamiltonian from the tool-kit of theory, or obtainable from one by minor modifications. Recall the first example of the use of theoretical tools in BP III, in which Bohm and Pines wrote down a Hamiltonian for their model, "an aggregate of electrons embedded in a background of uniform positive charge." The Hamiltonian contained three terms; the first denoted the kinetic energy of the electrons, the second the energy due to the Coulomb interactions between them, and the third their self-energy. The last two were both slightly modified to take into account the uniform background of positive charge. The requirement that the model be "simple, yet realistic" can be read with such procedures in mind. The simplicity of the model may be judged by the ease with which the Hamiltonian can be constructed from the standard expressions for energy that textbooks provide, its realism by the degree to which the energies associated with the model match those of the system that it represents.

Once the Hamiltonian has been rewritten in collective coordinates, a comparable procedure takes place in the reverse direction. The transformed Hamiltonian is manipulated and approximations are applied until, like the original Hamiltonian, it appears as the sum of recognizable elements, each of them capable of physical interpretation. Thus, as we saw in the synopsis of BP I, the collective approximation allows the transformed Hamiltonian $H_{(1)}$ to be expressed as the sum of three parts. Bohm and Pines write (BP I, 626b),

$$H_{(1)} = H^{(1)}{}_{\text{part}} + H_{\text{osc}} + H_{\text{part int}},$$

and interpret $H^{(1)}{}_{\text{part}}$ as "the kinetic energy in these new coordinates," H_{osc} as "a sum of harmonic oscillator terms with frequencies given by the dispersion relation for organized oscillations," and $H_{\text{part int}}$ as a term that "corresponds to a screened force between particles" (*ibid.*).

Likewise, in BP III the authors write (BP III, 612b),

> Let us [. . .] neglect U, a procedure which we have called the random phase approximation [. . .]. With this approximation we see that the third and fourth terms in our [transformed] Hamiltonian reduce to
>
> $$H_{\text{osc}} = -\tfrac{1}{2} \Sigma_{k < k(c)} (p_k p_{-k} + \omega_p^2 q_k q_{-k})$$
>
> the Hamiltonian appropriate to a set of harmonic oscillators, representing collective fields, with a frequency ω_p .

In similar vein the other three terms in the Hamiltonian H^4 are interpreted as representing "the kinetic energy of the electrons," "a simple interaction between the electrons and the collective field," and "the short range part of the Coulomb interactions between the electrons" (*ibid.;* see the synopsis of BP III).

The two modes of description, the physical and the mathematical, while in many ways autonomous, are each responsive to the demands made by the other. They are not totally intertranslatable. Not every describable model can be represented mathematically; not every mathematical description can be given a physical interpretation. Models may need to be simplified; Hamiltonians may need to be mathematically massaged. In both cases the aim is the same: to bring the description closer to the canonical examples supplied by physics textbooks and vice versa. These examples license movement from one mode of description to the other: the rendering of physical descriptions in mathematical terms, and the interpretation of mathematical expressions in physical terms.[51]

This reliance on a comparatively small repertoire of examples imposes a severe constraint on theoretical practice. Or so one might think. But working within constraints may bring its own benefits. Was sonata form an impediment to Mozart? Or the form of the sonnet a hindrance to Petrarch? In the case at hand, the constraint is positively beneficial—in two ways. In the first place, it solves the *Meno* problem: "How will you enquire, Socrates, into that which you do not know? [. . .] And if you find what you want, how will you ever know that this is the thing which you did not know?"[52] Bohm and Pines know very well what they are looking

51. The role of these canonical models as "bridge principles" has been stressed by Nancy Cartwright (1983, Essay 7). She emphasizes the need for them in what she calls "theory entry," or, in my vocabulary, the move to the mathematical mode of description. As the BP quartet shows, they are also needed for moves in the opposite direction, from the mathematical to the physical mode. For Cartwright (1983, 139), the prime virtue of these examples is explanatory.

52. Plato, *Meno 80d.*

for. They seek a Hamiltonian of a recognizable kind, and the standard examples provide aids to recognition. Secondly, the canonical status of these examples allows them to be bearers of meaning. For the habitual reader of *The Physical Review* the interpretations of these mathematical expressions do not have to be re-established *ab initio* on each occasion of their use. Furthermore the expressions remain meaningful even when they are modified to fit specific circumstances, as in BP III, for example, when the terms in H^1 for the electrons' self-energy and the energy due to Coulomb interactions are modified to take into account the uniform background of positive charge.

Each of the physical descriptions I have so far considered is isomorphic to a corresponding mathematical description. It contains as many clauses as there are terms in the Hamiltonian, each attributing a particular type of energy to the model. But Bohm and Pines also furnish physical descriptions of another kind. Within the quartet they use the adjective "physical," together with its cognate adverb "physically" a dozen times, four times in BP II, seven times in BP III, and once in P IV. On only three occasions is it used in a way that tallies with the account I have given of the interplay between mathematical and physical descriptions of the model. In BP III (619a), for instance, we read,

> The physical consequences of our canonical transformation follow from the lowest-order Hamiltonian $H_{\text{new}}^{(0)}$ [. . .] and the associated set of subsidiary conditions on our system wave function.

More often—in fact on seven of the eleven occasions when the adjective is used in BP II and BP III—it appears in the phrase "physical picture." The picture in question is described in the concluding section of BP II (350b-51a).

> In conclusion we give a brief summary of our results in terms of a physical picture of the behavior of the electron gas. As we have seen, the density fluctuations can be split into two approximately independent components, associated, respectively, with the collective and individual particle aspects of the assembly. The collective component, which is present only for wavelengths $> \lambda_D$, represents organized oscillations brought about by the long range part of the Coulomb interaction. When such an oscillation is excited, each individual particle suffers a small perturbation of its velocity and position arising from the combined potential of all the other particles. The contribution to the density fluctuations resulting from these perturbations is in phase with the potential producing it, so that in an oscillation we find a small organized wave-length pertur-

> bation superposed on the much larger random thermal motion of the particle. The cumulative potential of all the particles may, however, be considerable because the long range of the force permits a very large number of particles to contribute to the potential at a given point.
>
> The individual particles component of the density fluctuation is associated with the random thermal motion of the particles and shows no collective behavior. It represents the individual particles surrounded by comoving clouds which screen their field within a distance $\sim \lambda_D$. Thus it describes an assembly of effectively free particles interacting only through the short-range part of the Coulomb force. The screening of the field is actually brought about by the Coulomb repulsion which leads to a deficiency of electrons in the immediate neighborhood of the particle. This same process also leads to a large reduction in the random fluctuations of the density in the electron gas for wavelength larger than λ_D.

A third paragraph examines further the interactions of an individual electron with the electron gas as a whole.

What is offered here, the authors tell us, is "a brief summary of [their] results in terms of a physical picture of the behavior of the electron gas." The "physical picture" is thus a supplement to the theoretical investigations that have been the main task of the paper. It provides, in a vocabulary markedly different from the one used in descriptions obtained from the system's Hamiltonian, a summary of results already achieved. Organized oscillations are *brought about* by the long range part of the Coulomb interaction. Each individual particle *suffers* small perturbations *arising from* the combined potential of the other particles. Comoving clouds *screen* the field of each particle. This screening is *brought about* by the Coulomb repulsion, which *leads to* a deficiency of electrons in the neighbourhood of the particle. The same *process* also *leads to* a large reduction in the random fluctuations of the density in the gas at large wavelengths.

Though it appears late in the second paragraph, the key word here is "process." The behaviour of the electron gas is described in terms of causal processes, whereby one thing *brings about,* or *leads to* another. As I have said, the description is presented as a supplement to the theoretical investigations pursued in the quartet. Although its themes are anticipated in the central, theoretical sections of BP II, only once (348a) does the phrase "physical picture" occur in those sections. Elsewhere in BP II it appears only in the Abstract, in Section I: *Introduction,* and Section VII: C*onclusion;* similarly, in BP III it appears twice in the first section, once in the last, but nowhere else. This points to the fact that the picture itself is relatively

independent of the theoretical approaches taken. Though drawn from the classical account of the electron gas given in BP II, it nevertheless holds good alongside the quantum mechanical account in BP III. Descriptions of this kind—"narrative descriptions," as I will call them—are used throughout physics. Although their closest affiliation is with the theoretical manifolds of classical physics, within theoretical practice they are effectively independent of "high theory," and, as in the present case, can co-exist alongside theoretical manifolds of many different persuasions. And for obvious reasons, they are a large part of the *lingua franca* of experimental practice.[53]

2.5 The Use of Approximations

In just five pages of BP I (Sections II C and II D and Section III B) the verb "neglect", variously conjugated, appears no fewer than twelve times. In these sections Bohm and Pines are examining the results of moving from individual to collective coordinates, first in the classical case and then in the quantum case. In Sections II A and III A, respectively, the transformations that will effect these moves have been specified with mathematical exactness. After they are applied to the original Hamiltonian H_0, however, approximative strategies are brought into play, so that terms that make only small contributions to the Hamiltonian disappear. In this way the otherwise unwieldy expression for the transformed Hamiltonian is presented in a form that allows each of its components to be given a physical interpretation.

Similar approximations are used throughout the quartet. Each involves the assumption that some aspect of the physical situation guarantees that corresponding terms in its mathematical description can be neglected without grossly affecting the results of the analysis. These assumptions are brought together and discussed in Section II B of BP I (628a-b) under the title "The Collective Approximation." There are four of them; I will comment briefly on each in turn.

1. Electron-ion and electron-electron collisions are ignored. I drew attention to this simplification in Section 2.2. It is of the same kind as the simplification (some philosophers of physics call it "abstraction") whereby the effect of air resistance on the motion of a pendulum is neglected (see fn. 31). In each case the effect of the neglected element is to produce a small damping of the oscillations of the sys-

53. The importance of narrative descriptions has been emphasized by several authors. Stephan Hartmann (1999) has drawn attention to the contribution they make to hadron physics. He calls them simply "stories."

tem. The neglect of electron-ion collisions is not one of the approximations used in BP I to simplify the transformed Hamiltonian; rather it is implicit in the authors' choice of the jellium model, in which the effect of the plasma ions is represented by a background of uniformly distributed positive charge.

2. The organized oscillations in the electron gas are assumed to be small. The "customary linear approximation, appropriate for small oscillations" (BP I, 628b) is then used, allowing quadratic field terms (i.e., products of field terms) in the equations of motion of the system to be neglected.
3. In BP I, the velocity v of the electrons is assumed to be small compared with c, the velocity of light, so that terms involving v^2/c^2 are negligible. (This approximation appears repeatedly; it accounts for eight of the twelve occurrences of "neglect" that I mentioned earlier.) In BP III, as we have seen, the assumption is made that there is a maximum wave number k_c (equivalently, a lowest wavelength λ_c) for organized oscillations. These assumptions are presented as two versions of a single assumption, that $(\mathbf{k}.\mathbf{v})/\omega$ is small, appropriate for transverse and longitudinal oscillations, respectively.

These first three approximations are straightforward, the last, the "random phase approximation," less so.

4. Bohm and Pines write (BP I, 628b),

> We distinguish between two kinds of response of the electrons to a wave. One of these is in phase with the wave, so that the phase difference between the particle response and the wave producing it is independent of the position of the particle. This is the response which contributes to the organized behaviour of the system. The other response has a phase difference with the wave producing it which depends on the position of the particle. Because of the general random location of the particles, this second response tends to average out to zero when we consider a large number of electrons, and we shall neglect the contributions arising from this. This procedure we call the "random phase approximation."

The claim here is not that out-of-phase responses are individually negligible—they may well be of the same order of magnitude as the in-phase responses. The assumption made is that, taken collectively, they cancel each other out. Bohm and Pines justify the r.p.a. mathematically in the classical treatment of longitudinal oscillations given in BP II (349–

50), and in "a more qualitative and physical fashion" in the quantum mechanical treatment given in BP III (621).

The second of these approximative strategies and the first part of the third are standard fare, and I will say more about them in Section 2.7. I call them "strategies" because their application takes different forms in different theoretical contexts. None of the four are specific to the particular theoretical manifolds that the authors use. That is why the authors need to provide an extended discussion of them. As they point out (BP I, 628b), this four-part collective approximation differs from an orthodox perturbation theory in that the latter would not allow for the fact that small changes in the field arising from each particle may add up to a large change in the net field—which, on the authors' account, is precisely what generates plasma waves in metals.

2.6 Experiment and the Bohm-Pines Theory

Within the Bohm-Pines quartet little attention is paid to experimental findings, none at all to experimental practice. The first paper deals with the possibility of transverse oscillations due to electromagnetic interactions between moving electrons, the other three with the possibility of longitudinal oscillations due to electrostatic (Coulomb) interactions. The role of BP I is "to illustrate clearly the techniques and approximations involved in [the authors'] methods" (BP I, 627a). Virtually no mention is made of observed phenomena or experimental results. Bohm and Pines tell us,

> The electromagnetic interactions [which give rise to transverse oscillations] are weaker than the corresponding Coulomb interactions by a factor of v^2/c^2 [where c is the velocity of light] and, consequently, are not usually of great physical interest. (*Ibid.*)

Within the papers that follow, discussion of observable phenomena and experimental results is confined to BP II and P IV. BP III is entirely theoretical; the only mention of a possibly observable phenomenon comes in an aside (BP III, 610a-b), where the authors cite the work of two other theorists (Kronig and Kramer) who "treated the effects of electron-electron interaction on the stopping power of a metal for fast charged particles."

In BP II and P IV, a related phenomenon, the loss of energy suffered by a high energy electron when it passes through a metal film, is adduced as evidence to support the authors' own analysis. In a paragraph of the Introduction to BP II the authors predict the excitation of collective oscillations in a metal by high energy particles passing through it; the paragraph ends with a laconic reference to experimental confirmation:

> Experiments by Ruthemann and Lang, on the bombardment of thin metallic films by fast electrons tend to verify our theoretical predictions concerning this type of oscillation. (BP II, 339a.)

This remark is amplified in Section IV of the paper, where the link between theory and experiment is provided by a "correspondence principle argument" (see Sections 1.3 and 2.3 of this essay). In Section IV of P IV Pines uses quantum mechanics to provide an explanation of this phenomenon, which is summarized in the penultimate paragraph of Section 1.5 of this essay. Analysis of the interaction of the high-energy electrons with the collective field shows that only at one particular frequency ω, very close to the plasma frequency ω_P, will the oscillations of the field become self-sustaining (P IV 634a). Pines points out (P IV 634b) that for aluminium and beryllium films the predicted values of energy lost (in multiples of $\hbar\omega$) by the electrons agree very well with those obtained experimentally by Ruthemann and by Lang. Furthermore, the calculated mean free path (the average distance travelled by the high energy particle between excitations) matches an empirical value based on Lang's data for aluminium films.

When gold, copper, or nickel films are used, however, the spectrum of energy loss is not discrete. Pines attributes this failure to the fact that "the valence electrons in these metals are not sufficiently free to take part in undamped collective motion" (*ibid.*), i.e., that the jellium model, in which there is no interaction between valence electrons and the individual ions of the metal, is inadequate for these elements. He notes, "Experiments have not yet been performed on the alkali metals, where we should expect to find collective oscillation and the appearance of discrete energy losses."[54] (*Ibid.*)

There are four other places in P IV where empirical evidence is referred to. Three instances occur in Section III, where Pines is discussing the surprising fact that, if the independent electron model of electron behaviour is "corrected" to allow for the so-called "exchange energy" between electrons, the result is to worsen the agreement with experiment rather than to improve it. (See Section 1.5 of this essay.) One instance involves the magnetic properties of the electron gas, another the way its specific heat varies with temperature. In neither case does Pines cite the experiments that yielded the relevant empirical results. Nor does he in the third instance (P IV, 631a). In this instance Pines describes an *ad hoc* theoretical move made by P.T. Landsberg. In order to account for an observed feature of the x-ray emission spectrum for sodium, Landsberg had found it necessary to introduce a screened Coulomb interaction between electrons. Since

54. Alkali metals (lithium, sodium, potassium, etc.) appear in Group Ia of the Periodic Table. They have just one valence electron per atom.

a screening effect of this kind is predicted by the B-P theory, one might expect Pines to present Landsberg's manoeuvre as an indirect confirmation of that theory. Instead, Pines points to a result by E.P. Wohlfahrt. Landsberg had proposed that the screened Coulomb interaction could be mathematically modelled by writing $(e^2/r_{ij})\exp[-(r_{ij}/\lambda)]$ for the electron-electron interaction potential in place of the standard expression e^2/r_{ij}, and that the "screening radius" λ was of the order of 10^{-8} cm. Wohlfahrt showed that, if both of these proposals were accepted, then the unfortunate effect of "correcting" the independent electron model to allow for the exchange energy would be greatly reduced, along with the error in its predictions concerning specific heat. The comparison implicit in Pines's discussion of Landsberg's and Wohlfahrt's work is this: On the independent electron account of a plasma there are results that can be purchased only by making *ad hoc* assumptions concerning the electron-electron interaction potential within the plasma. On the BP approach, in contrast, they come for free.

Here, as throughout the first three sections of P IV, Pines is more concerned to compare the B-P theory with other theoretical approaches than with experimental results. In addition to the work of the theorists I have already mentioned, Pines points to two separate treatments of an electron gas by Eugene Wigner (1934 and 1938), and shows how their results match those obtained using the collective approach. This form of validation, whereby a new theoretical approach gains credence by reproducing the results of an earlier one, has received little attention from philosophers of science, though instances of it are not far to seek. For example, when Einstein first proposed his general theory of relativity, one of the earliest tasks he set himself was to show how the theory could recapture, to a first approximation, the classical principle of conservation of energy as it applied to an orbiting planet.[55] In the present case, a general sense that, for Bohm and Pines, empirical justification was something of an afterthought is reinforced by an acknowledgement they offer at the end of BP II (351a): "The authors wish to thank Dr. Conyers Herring for informing us of the experiments of Ruthemann and Lang."

Be that as it may, it is certainly true that, in the B-P quartet, theory and experiment exist as almost independent disciplines, one of them barely glanced at. We read that both Ruthemann and Lang conducted experiments in which high energy electrons were directed at thin metallic films and their energy losses in passing through the films were measured.

55. It appears in Einstein (1915), the paper in which he explained the anomalous advance of the perihelion of Mercury. This paper is the third in a quartet of papers, all published in November, 1915, in which Einstein introduced the general theory.

We are told (P IV, 634b) that in Lang's experiments the initial energy of the electrons was 7.6 kev, and that the energy losses were multiples of 14.7 ev in aluminium films and 19.0 ev in beryllium films. (The calculated values are 15.9 ev and 18.8 ev respectively.) But we are told neither how these beams of electrons were prepared, not how the electrons' initial energies and losses in energy were established. It seems that the practices involved in preparation and measurement belong exclusively to the guild of experimenters, and are not the province of the worshipful company of theoreticians. But this cannot be the whole story. While the two sets of practices, experimental and theoretical, may be different, they cannot be wholly disjoint, for two reasons. The first is that experimental practices include theoretical practices—a truism which has the virtue of being true. The energy of an electron may be measurable, but it is not observable, and every measuring instrument more sophisticated than a meter rule is a material realization of a theory. When a theoretical result is compared with an experimental result, the practices involved in the theoretical side of things may or may not belong within the same theoretical manifold as those on the experimental side. Although two different theoretical manifolds are used in BP II and P IV, in both cases the conclusions reached are compared with the results of the same experiments; thus in at least one of these cases the theoretical manifold used by Bohm and Pines differs from that used by Lang. Nonetheless—and here I come to the second point—if empirical confirmation is to be possible, in both cases the set of practices on the theoretical side must mesh with the set of practices on the empirical side. Theoretician and experimenter must agree that a theoretical energy loss of 18.8 ev can be meaningfully compared with an experimental energy loss of 19.0 ev. In both the literal and the Kuhnian sense, the two sets of practices cannot be incommensurable. In Galison's phrase (1997, *passim*), the two parties need to establish a *trading zone.* How this might be done is beyond the scope of this paper.

The fourth and last instance of an occasion when empirical evidence is at issue is an instance of a slightly peculiar kind. It occurs very early in P IV (626–27) where, as in the third instance, Pines is comparing the collective account of electron behaviour with the account given by the independent electron model. On these occasions Pines is in the position of one who needs to explain the curious incident of the dog in the night time. As readers of Conan Doyle will recall, what made the incident curious was that the dog did nothing in the night time. In like manner Pines must explain the fact that the collective behaviour that he and Bohm ascribe to metallic plasmas has very little effect on the plasma's properties and behaviour, so little that the independent electron model, in which "the motion of a given electron is assumed to be independent of all the other elec-

trons," enjoys widespread success. He makes this explanation his first order of business: a qualitative explanation is given in the paper's introduction, and a quantitative account follows in its central sections. (See Section 1.5.) In certain circumstances the absence of a phenomenon may stand as much in need of explanation as would the phenomenon itself.

2.7 Deduction in the Bohm-Pines Quartet

I introduced Part Two of this essay by quoting John Ziman's delineation of the intellectual strategy of a typical paper in theoretical physics: "A model is set up, its theoretical properties are deduced and experimental phenomena are thereby explained." Taking my cue from the first clause of that account, I began my observations on the B-P quartet by describing the role played by models and modelling. Turning my attention to the second clause, I will end them by examining the notion of *deduction,* as it appears in the third of these papers.

A curious feature of Chapter 3 of Ernest Nagel's *The Structure of Science* (1961) is that, although it bears the title "The Deductive Pattern of Explanation," there is no mention in it of the steps by which conclusions follow from premises. In that chapter the actual process of deduction is taken for granted, witness this paragraph (*op. cit.,* 32).

> [A] deductive scientific explanation, whose explanans is the occurrence of some event or the possession of some property by a given object, must satisfy two logical conditions. The premises must contain at least one universal, whose inclusion in the premises is essential for the deduction of the explanandum. And the premises must also contain a suitable number of initial conditions.

This is a beautiful example of what, borrowing the phrase from Nancy Cartwright (1999, 247), I will call the "vending machine" view of theorizing Originally, Cartwright used the phrase in criticizing a particular account of theory entry, the process by which, in Ziman's words, "a model [of a physical system] is set up":

> The theory is a vending machine: you feed it input in certain prescribed forms for the desired output; it gurgitates for a while; then it drops out the sought-for representation; plonk, on the tray, fully formed, as Athena from the brain of Zeus. (Cartwright, 1999, 247).

The feature of the machine that Cartwright challenges is its mode of input, which must accord with "certain prescribed forms."[56] (*Ibid.*) I will use

56. She continues, "Producing a model of a new phenomenon such as superconductivity is an incredibly difficult and creative activity. It is how Nobel prizes are born." (*Ibid.)* The

the same metaphor to characterize the next stage of Ziman's narrative, where "theoretical properties of the model are deduced and experimental phenomena are thereby explained." In this usage, the salient feature of the machine will be the process of *gurgitation.* Nagel has talked in the previous chapter (albeit unknowingly) about that phase of the machine's working (1961, 21):

> A type of explanation commonly encountered in the natural sciences [. . .] has the formal structure of a deductive argument, in which the explanandum is *a logically necessary consequence* of the explanatory premises. [My emphasis.]

Here "logical necessity" includes "mathematical necessity," as Nagel points out a few lines later. This kind of deduction we may call *strict deduction;* it is the kind of deduction that Nagel subsequently takes for granted.[57]

Pace Nagel, adherence to strict deduction is not "commonly encountered in the natural sciences." An insistence on strictness would mean, for example, that the explanation of the behaviour of a system consisting of more than two mutually attracting bodies would be beyond the scope of classical mechanics. Starting with Newton, physicists have become adept in finding ways around this problem, and Bohm and Pines are no exception. To illustrate how they go about it, I will examine the extended argument in BP III that transforms the Hamilton for the jellium model from H^1 to H_{new} , and in so doing makes manifest the collective properties of the electron gas. I will first give a précis of the argument, and then list the individual moves within it. The order in which they are listed, however, is determined, not by the order of their occurrence in BP III, but by the kinds of justification offered for them. First in the list are moves justifiable by strict deduction; next come moves that have obvious, but not strictly deductive, justifications; at the end of the list are moves that are highly pragmatic, and are peculiar to the investigation under way. By now virtually all these moves will be familiar to the reader. My aim in setting them as I do is to draw attention to the gulf between the concept of "deduction," as the word is used is used by Ziman, a working physicist, and as it is used by Nagel, a mid-twentieth century philosopher of science.

First the précis, in six steps:

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essay from which this quotation is taken deals at length with the Bardeen, Cooper, and Shrieffer theory of superconductivity (for which they were indeed awarded the Nobel prize). I give a brief account of their achievement in Section 3.1.

57. See, for instance, the extended footnote on p. 353 of *The Structure of Science.*

Step (1) Bohm and Pines present a standard Hamiltonian H^1 for a cloud of electrons against a background of uniform positive charge.

Step (1→2) H^1 is then claimed to be equivalent to H^2, given a family of subsidiary conditions O_k on the state of the system. The claim is justified by displaying a unitary operator S such that $H^2 = SH^1S^{-1}$. Two central terms in H^2 are expressed as Fourier series, indexed by k (as was the term for the energy due to Coulomb interactions in H^1), and each of the subsidiary conditions corresponds to a component k of the Fourier decompositions.

Step (1→3) The procedure of Step (1→2) is now repeated, using a modified operator S , so that the two Fourier series in the terms of the resulting Hamilton, H^3, are truncated; neither of them has an index higher than k_C . Correspondingly, there are just k_C subsidiary conditions O_k .

Step (3→4) Algebraic manipulation of H^3 yields the Hamilton H^4, which contains five terms.

Step (4→5) This step contains just two moves. One of the terms of H^4, $U^\dagger$, is shown to be negligible, and is therefore deleted;[58] another, H_{osc} , is rewritten to produce H_{field} . The combined result is H^5.

Step (5→new) Bohm and Pines then apply a "canonical transformation" to H^5 (and to the operators O_k), and obtain the final Hamilton, H_{new} .

Now for the list of moves, categorized by justification; the page numbers cited are all from BP III:

Trivial Moves

Step (3→4) involves just one move, an elementary example of strict deduction, in which H^4 is obtained by rearranging the components of the terms of H^3.

58. Recall that within BP III Bohm and Pines are not consistent in their use of the letters *"U"* and *"S"*. In Step (5→new) the letters refer to a unitary operator and its generator, respectively. The authors have, however, previously used *"U"* to refer to a component of the Hamiltonian H^4, and *"S"* to refer to a unitary operator in Steps (1→2) and (1→3). In this section I will resolve one of these ambiguities by writing *"$U^\dagger$"* for the component of H^4.

Strictly Mathematical Moves

1. In Step (1) the authors "have used the fact that the Coulomb interaction between the i^{th} and j^{th} electrons may be expanded as a Fourier series in a box of unit volume" (610a). Though their subject matter is the physical world, the "fact" they use is a mathematical fact, belonging to the branch of mathematics known as analysis.
2. Likewise, in Step (4→5), when the field operators q_k and p_k are expressed in terms of creation and annihilation operators a_k and a_k* (616a), although this move has a physical interpretation, it is a perfectly permissible formal move, made "to simplify the commutator calculus."
3. As we have seen , Steps (1→2), (1→3), and (5→*new*) each involves a unitary transformation of a Hamilton H^a into another, H^b, In essence, a transformation of this kind is equivalent to a move from one system of coordinates to another in the (abstract) Hilbert space on which these operators are defined. Formally, these transformations employ a mathematical identity in the operator algebra of that Hilbert space of the kind $H^b = UH^aU^{-1}$. (See example 6 in Section 2.3.)
4. In Step (5→*new*), after a move of type 3, Bohm and Pines use another theorem of the operator algebra to rewrite the right hand side of that identity:

$$H^b = UH^aU^{-1} = H^a - i[H^a,S] - \tfrac{1}{2}[[H^a,S],S] + (i/3!)[[[H^a,S],S],S] + \ldots$$

Here S is the *generator* of U: $U = \exp(iS/\hbar)$; and $[A,B]$ is an abbreviation for the *commutator* of operators A and B: $[A,B] = AB - BA$. (See example 7 in Section 2.3; the identity appears on p. 617a.) The term "$[H^a,S]$" is a commutator of the first order, "$[[H^a,S],S]$" a commutator of the second order, and so on.

Standard Approximations

Physics, in particular the physics of many-body systems, is not an exact science. Approximations (in the literal sense: numerical results "very close" to the "real" values) are not only tolerated but inevitable. If a formula for a physical quantity Q contains a term t whose value is small in comparison with the value of Q, then a term t^2 of the second order will often be neglected. In quantum mechanics, a similar procedure applies to the operator representing Q. Two of the four types of approximation discussed in BP I (and in Section 2.5 above) conform to this pattern: ne-

glected are quadratic field terms and the term v^2/c^2, when v is small compares with c. Step (5→*new*) in BP III provides an example of the former kind (618a): "In obtaining [equation (54)] we have neglected a number of terms which are quadratic in the field variables and are multiplied by a phase factor with a non-vanishing argument." Another example from the same step involves the mathematical move we have just encountered, whereby a unitary transformation is unpacked as a series of commutators involving the generator S of the relevant unitary operator:

$$H^b = UH^aU^{-1} = H^a - i[H^a,S] - \tfrac{1}{2}[[H^a,S],S] + (i/3!)[[[H^a,S],S],S] + \ldots$$

Paradoxical though it may seem, the use of this identity leads to considerable simplifications. In preparation for that move Bohm and Pines have defined an *expansion parameter,* α, which is a measure of the strength of "the coupling between the field and electrons" (615b). They expected α to be small; in fact they described it as "the measure of the smallness" of a term in the system's Hamiltonian (*ibid.*). If we replace H^a in the identity above by H_I (one of the terms of H^4) it transpires that "the effects of the field-particle interaction (up to order α) are contained in the first correction term, $(i/\hbar)[H_I, S]$. The higher order commutators will be shown to be effects of order α^2, [. . .] *and may hence be neglected."* (618a; my emphasis; $\hbar$ is Planck's constant.)[59] To anticipate: In the vanishing case, when $[H^a,S] = 0$, then $H^b = H^a$, and H^a is unaffected by the transformation.

Perturbation Theory

In quantum mechanics the treatment of small terms is codified by *perturbation theory.* A basic element of quantum theory is the (time-independent) Schrödinger equation, which relates the Hamiltonian to a spectrum of values of energy. The values are determined by the solutions of the equation. A problem arises when a Hamiltonian has the form $H_a + H_b$, and H_b is small compared with H_a , because in that case the Schrödinger equation may not have exact solutions. Perturbation theory then provides a procedure whereby an approximate solution may be found.

Step (4→5) provides an example. Bohm and Pines were faced with a Hamiltonian (H^4) which contained five terms: $H_{\text{part}} + H_I + H_{\text{osc}} + H_{\text{s.r.}} + U^\dagger$. Since both $U^\dagger$ and H_I were known to be small compared with the other three terms, Bohm and Pines used second order perturbation theory to de-

59. A similar strategy was adopted in BP I. At the corresponding point in their quantum mechanical treatment of *transverse oscillations* Bohm and Pines tell us that "higher order commutators can be neglected if we restrict our attention to the lowest order terms in v/c." (632b) I will come back to the commutator $(i/\hbar)[H_I, S,]$ later.

termine just how small those effects were. Bohm and Pines concluded that they were "justified in neglecting completely the term $U^{\dagger}$" (BP III, 614b), but that, although they were "justified in neglecting H_I in order to obtain a qualitative and rough quantative understanding of the behaviour of [their] system, [. . .] the effects arising from H_I should be taken into account in a careful quantitative treatment" (*op. cit.,* 615a).[60] Note that perturbation theory does not go beyond orthodox quantum theory; it merely provides a recipe whereby selected terms in the Schrödinger equation are discarded; in first order perturbation theory terms of second order are discarded, in second order perturbation theory, terms of third and higher orders.

Formal Analogies

I turn now to analogical reasoning, specifically to the use within BP III of arguments and results from the two papers that came before it. BP I contained both a classical and a quantum mechanical treatment of transverse plasma waves, and BP II a classical treatment of longitudinal plasma waves. BP III gives a quantum mechanical treatment of longitudinal plasma waves, and so marries the second of the theories used in BP I to the phenomena discussed in BP II. It is therefore not surprising that the correspondences drawn between BP III and BP I are different in kind from those drawn between BP III and BP II.

In BP III Bohm and Pines reserve the words "analog" and "analogous" for the correspondences between that paper and the quantum mechanical treatment provided in BP I, and, on each of the eight occasions when BP I is mentioned, one at least of those words occurs. The analogies the authors refer to are formal analogies, correspondences between mathematical formulae. For instance, "H_I and H_{field} are [. . .] analogous to the transverse terms encountered in BP I, and we may expect that many of the results obtained there may be directly transposed to the longitudinal case." (617a) Likewise, the operator S which is instrumental in the canonical transformation of H^5 to H_{new} in Step (5→*new*) "may be seen to be just the longitudinal analog of the 'transverse' generating function given in BP I" (*ibid.*).

Non-Standard Approximations

As we have seen, in Step (4→5), Bohm and Pines use second order perturbation theory to justify the neglect of the term $U^{\dagger}$ in H^4. They had previously used another argument to the same end (612b):

60. Recall that the transformation that took H^5 into H_{new} was designed to distribute the effects of H_I among those terms that remained. See Section 2.4 and fn. 20.

> $U^{\dagger}$ [. . .] always depends on the electron coordinates, and since these are distributed over a wide range of positions, there is a strong tendency for the various terms entering into $U^{\dagger}$ to cancel out. Let us for the time being neglect $U^{\dagger}$, a procedure we have called the random phase approximation, and which we shall presently justify.

Physical Analogies

Though Bohm and Pines had used two distinct theories in BP I, in each case their approach had been the same. The starting point of each was a Hamiltonian for a collection of electrons in a transverse electromagnetic field, in which the electrons were represented by the momentum p_l and position x_l of the individual charges. A similar approach was taken in BP III; the only difference was that the longitudinal, rather than the transverse components of the electromagnetic field are included in the Hamiltonian. In both these papers, only after a canonical transformation was applied to the Hamiltonian was the system described in terms of collective variables. In contrast, in BP II Bohm and Pines wrote (BP II, 340a),

> Our approach to the equations of motion is aimed at making use of the simplicity of the collective behavior as a *starting point* for a tractable solution. *As a first step,* we study the fluctuations in the particle density, because [. . .] their behavior provides a good measure of the applicability of a collective description. [My emphases.]

In other words, since the particle density is a collective property, nothing corresponding to the canonical transformations used in BP I is needed in BP II. And while it is true that in BP III Bohm and Pines develop "a direct quantum mechanical extension of the methods used in Paper II," this extension is relegated to a brief appendix (623–24), and is used in Section V of the paper only to resolve some complications involving the subsidiary conditions on the quantum states of the system (see fn. 18). In the theoretical core of BP III the mathematical aspects of BP II are set to one side. Instead, Bohm and Pines emphasize the "physical picture" the paper presented:

> In the preceding paper [BP II] we developed a detailed physical picture of the electronic behavior [of a dense electron gas]. Although the electron gas was treated classically, we shall see that most of the conclusion reached there are also appropriate (with certain modifications) in the quantum domain. Let us review briefly the physical picture we developed in [BP II], since we shall have to make frequent use of it in this paper. (609a)

As we have seen, the most striking feature of the picture was that, within it,

> [T]he electron gas displayed both collective and individual particle aspects. [. . .] The collective behaviour [i.e., the plasma oscillations] of the electron gas is decisive for phenomena involving distances greater than the Debye length, while for smaller distances the electron gas is best considered as a collection of individual particles which interact weakly by means of a screened Coulomb.force. (609a-b)

I will focus on just two of the correspondences between BP III and BP II that Bohm and Pines rely on. The first is very general: it involves the demarcation marked in the classical case by the Debye length. The second is more specific: it concerns the *dispersion relation,* the relation between the frequency and the wavelength of plasma oscillations.

1. Some prefatory remarks are needed. In BP II (341a-b) Bohm and Pines present a criterion for the applicability of a collective description. The criterion involves an equation (labelled "(9)"), which has been deduced from the principles of electrostatics with the help of the random phase approximation. In this equation $d^2\rho_k/dt^2$ is equated with the sum of two terms,[61] one representing the random thermal motion of the individual particles, the other the collective oscillations of the electron gas. Thus the "rough criterion for the applicability of a collective description" is that "for most particles the collective [second] term in (9) be much greater than the term arising from the random thermal individual particle motions" (BP II, 341a-b). It turns out that, if k is sufficiently small, the effect of the first term can be neglected, and a straightforward derivation shows that the quantitative form of this condition can be written as $k^2 \ll 4\pi n\lambda_D^{-2}$, where λ_D is "the well known Debye length" (*ibid.*).[62] Conversely, for high wave number k (and small wavelength λ) collective behaviour (i.e. plasma oscillations) will not be generated.

   This result is carried over into Step (1→3) of BP III (611b-612a).

   > We found in Paper II that in the classical theory there is a minimum wave wavelength λ_C (which classically is the Debye length), and hence a maximum wave vector k_C, beyond which

61. Recall that ρ is the electron density and k is the wave number of the oscillations. It appears when Bohm and Pines represent the electron density ρ by the Fourier decomposition $\rho = \Sigma_k\, ak \cdot \rho k$. See Section 1.3.

62. λ_D is defined by the equation: $\lambda_D = \kappa T/4\pi ne^2$, where κ is Boltzmann's constant and T is the absolute temperature of the electron gas.

> organized oscillation is not possible. We may anticipate that in the quantum theory a similar (but not identical) limit arises, so that there is a corresponding limit on the extent to which we can introduce collective coordinates to describe the electron gas. [. . .] The number of collective coordinates, n', will then correspond to the number of k values lying between $k = 0$ and $k = k_C$. [. . .] The modification of [H^1] to include only terms involving (p_k, q_k) with $k < k_C$, may be conveniently carried out by applying a unitary transformation similar to [the transformation which took H^1 to H^2], but involving only [position coordinates] q_k for which $k < k_C$.

By this means H^1 is transformed into H^3, and the information about plasma behaviour obtained by classical means becomes encoded in a quantum theoretic Hamiltonian. This correspondence, however, is not quantitative. As the quotation above tells us, Bohm and Pines anticipate that, in the quantum case, the limit k_C will be "similar, but not identical" to the classical limit, where it is the reciprocal of the Debye length λ_D.

2. The *dispersion relation* of a plasma relates the frequency ω of a plasma oscillation to its wave number k (the reciprocal of its wavelength). The relation is presented on p. 618b, in the penultimate move of Step (5→*new*). Bohm and Pines show that, because of the form it takes, significant simplifications can be effected.[63] They write (619b),

> This dispersion relation plays a key role in our collective description, since it is only for $\omega(k)$ which satisfy it that we can eliminate the unwanted terms in the Hamiltonian [. . .] and the unwanted field terms in the subsidiary condition.

The fact that a particular choice for the dispersion relation leads to desirable results does not, however, justify that choice, as Bohm and Pines acknowledge. They continue,

> The frequency of these collective [longitudinal] oscillations is given by the dispersion relation [. . .], which is the appropriate quantum mechanical generalization of the classical dispersion relation derived in BP II, as well being the longitudinal

63. To be precise, several terms in the first version of H_{new} disappear if the dispersion relation takes the form suggested (618b). The terms in question are the first two terms of the commutator $(i/\hbar)\{H_1, S,\}$ and the transformed version of one of the terms of H_{field} in H^5.

> analog of the quantum-dispersion relation for organized transverse oscillation, which we obtained in BP I.

This is a double-barrelled justification, appealing as it does both to a physical and to a formal analogy.

Causal Inferences

In this catalogue of argumentative moves, all but one of those under the headings *Non-Standard Approximations, Physical Analogies,* and *Causal Inferences* (that is to say, those moves furthest away from the canons of strict deduction) are buttressed by more than one argument.[64] The two arguments under the heading *Causal Inference* both involve the term $H_{\text{s.r.}}$, which appears in H^4 and H^5 and represents "the short-range part of the Coulomb interaction between the electrons" (612b). In each case Bohm and Pines want to establish that the canonical transformation which takes H^5 to H_{new} has a negligible effect on $H_{\text{s.r.}}$; or, in mathematical terms, if U is the unitary operator associated with the transformation, then to a good approximation, $UH_{\text{s.r.}}U^{-1} = H_{\text{s.r}\ldots}$

1. Example 4 from the strictly mathematical moves shows that the desired conclusion holds, provided that the commutator $[S,H_{\text{s.r.}}]$ is negligible (where S is the generator of U). Bohm and Pines present "a typical first order term arising from $[S, H_{\text{s.r.}}]$," and label it "(74)." They point out that "the structure of (74) is quite similar to that of $U^\dagger$ [in H^4]" (621a). As we have seen, $U^\dagger$ was shown to be negligible by the use of second order perturbation theory; in treating (74), however, the authors opt for a different strategy (621a-b).

> Because of the analytic difficulties involved [in the use of perturbation theory] we prefer to justify our neglect of (74) in a more qualitative and physical fashion.
>
> We see that (74) describes the effect of the collective oscillations on the short-range collisions between the electrons, and conversely, the effect of the short-range collisions on the collective oscillations. [. . .] The short-range electron-electron collisions arising from $H_{\text{s.r.}}$ will act to damp the collective oscilla-

64. In the non-standard approximation the neglect of $U^\dagger$ was justified by the r.p.a., but further justification was provided, as we have seen, by second order perturbation theory applied to the single operator $U^\dagger$ (614); furthermore, justification of the r.p.a. in the classical case appeared in Section VI of BP II, and although it was tailor-made for a specific example readers were invited to regard it as a template to be used elsewhere. And as I pointed out at the end of the discussion of the second physical analogy, the choice of dispersion relation had a twofold justification.

> tions [. . .]. A test for the validity of our approximation in neglecting terms like (74) is that the damping time from the collisions be small compared with the period of a collective oscillation. In this connection we may make the following remarks:
>
> 1. Electron-electron collisions are comparatively ineffective in damping the oscillations, since momentum is conserved in such collisions, so that to a first approximation such collisions produce no damping. [. . .]
> 2. The exclusion principle will further reduce the cross section for electron-electron collision.
> 3. If H_I [which represents "a simple interaction between the electrons and the collective field"] is neglected, collisions have no effect on the collective oscillations. This means that the major part of the collective energy is unaffected by these short-range collisions, since only that part coming from H_I (which is of order α [the expansion parameter] relative to ω_p) can possibly be influenced. Thus at most 20 percent of the collective energy can be damped in a collision process.
>
> All of these factors combine to reduce the rate of damping, so that we believe this rate is not more that 1 percent per period of an oscillation and probably quite a bit less. [. . .] It is for these reasons that we feel justified in neglecting the effects of our canonical transformation on $H_{s.r.}$.

We have been given two reasons for taking (74) (and hence $[S,H_{s.r.}]$) to be negligible compared with other terms in H^5: first, Bohm and Pines drew attention to the existence of formal similarities between (74) and $U^\dagger$ (a term already known to be negligible); secondly, they presented a causal argument to show that the effects, represented by (74), of interactions between long-range collective oscillations and short-range collisions of electrons would be small.[65]

Even before the canonical transformation took place (616b-618b), a condensed version of this argument had been presented (616a):

> From [the expressions for the components of H^5] we see that if we neglect H_I, the collective oscillations are not affected at all by $H_{s.r.}$. Thus $H_{s.r.}$ can influence the q_k only indirectly through H_I. But, as we shall see, the *direct* effects of H_I on the collective oscillations are

65. In addition, the authors draw attention to a classical treatment of damping of collective oscillation by electron-electron collisions by Bohm and Gross (1949).

> small. Thus, it may be expected that the *indirect* effects of $H_{s.r.}$ on the q_k through H_I are an order of magnitude smaller and may be neglected in our treatment which is aimed at approximating the effect of H_I.

(The variable q_k that appears here has been introduced in H^2. It represents a generic component of the Fourier expansion of **A**, the electromagnetic vector potential in the plasma, and will be supplanted by a collective variable when the canonical transformation takes place.)

I call this argument a "causal inference" because of the vocabulary employed: "$H_{s.r.}$ can *influence* the q_k only indirectly"; "the *direct effects* of H_I"; "the *indirect effects* of $H_{s.r.}$"; "collective oscillations are not *affected* at all by $H_{s.r.}$" (emphases mostly mine). The agents portrayed as bringing about effects, directly or indirectly, are components of the Hamilton H^5. But here we should listen to the stern voice of Pierre Duhem ([1914] 1991, 20), "These mathematical symbols have no connection of an intrinsic nature with the properties they represent; they bear to the latter only the relation of sign to thing signified." In what sense, then, can a theoretical term be a causal agent? The short answer is that in this argument Bohm and Pines have moved into a figurative discourse, within which the terms, H_I and $H_{s.r.}$ play metonymic roles. The long answer, which may shed light on the short one, requires us to follow Duhem's lead, and undertake an analysis of *theoretical representation* as it appears in BP III, a project that will take us to the end of Part Two of this essay.

In the BP quartet we may distinguish two layers of theoretical representation. The first is the representation of a metal by a simplified and idealized model, in this case the jellium model, consisting of a gas of electrons moving against a uniform background of positive charge. The second involves a foundational theory. The model is represented in two ways, as a classical system and as a quantum mechanical system;[66] the quantum mechanical treatment is applied in BP III. As we have seen, because the behaviour of a quantum system is governed by its Hamiltonian, to represent it the theoretician needs to know the system's energy. In the jellium model it comes from three sources: the kinetic energy of the electrons, the Coulomb energy due to the electrostatic repulsion between them, and their self-energy. These physical quantities are *denoted* (at the second level of representation) in the language of quantum mechanics by standard formulas (slightly modified to take into account the effect of the positively charged background). The sum of these formulas, signified by "H^1" represents the total energy of the model. By a series of mathematical

66. The choice of a foundational theory need not involve an extra layer of representation; instead, we can simply distinguish two jellium models at the first level.

transformations and judicious approximations Bohm and Pines *demonstrate* that H^1 is effectively equivalent to another cluster of formulas, H_{new} , which can be *interpreted* in physical terms: $H_{electron}$ contains terms referring only to individual electrons; "field coordinates appear only in H_{coll}, and thus describe a set of uncoupled fields which carry out real independent longitudinal oscillations" (619b); and $H_{res\ part}$ "describes an extremely weak velocity-dependent electron-electron interaction" (620b).

The two-stage denotation of the physical systems by H^1 and the interpretation in physical terms of H_{new} mark off the two boundaries of the demonstration phase. Within this phase, however, there is a continuous interplay between the formulas of the second level and the physical quantities they represent. Indeed, it would not be an overstatement to say that this interplay drives the deduction from H^1 to H_{new} . Accompanying the steps that lead from H^1 to H_{new} is a commentary on their physical implications. Only one of these steps is purely formal: Step (3→4) consists solely of the *Trivial Move.* All the others have physical import. Even in Step (1), when Bohm and Pines write the energy for the Coulomb interactions between electrons as a Fourier series, the choice has two rationales. The announced rationale is that the choice allows them to "take into account the uniform background of positive charge" by the simple device of excluding from the summation over k in the Fourier series the index $k=0$ (610b). The long term rationale is that the new idiom is ideal for expressing oscillatory phenomena. The subsidiary conditions on the wave function introduced in Step (1→2) guarantee that Maxwell's equations are satisfied. Step (1→3) is prefaced by an argument which justifies by *Physical Analogy* the truncation of the Fourier series of H^2 . The remaining steps yield the Hamiltonians H^4, H^5, and H_{new} . Each of these – not only H_{new}—is broken down into clauses that are given physical interpretations. $H^4$and H^5 share three clauses, H_{part} , H_I , and $H_{s.r.}$, and a fourth clause of H^4 ($U^{\dagger}$) is discarded on both theoretical and physical grounds (*Perturbation Theory* and the *Random Phase Approximation*). (The replacement of the last clause in H^4 (H_{osc}) by H_{field} in H^5 has no physical significance; it relies on the second *Strictly Mathematical Move.*) Step (5→new) involves not only the canonical transformation of H^5 into H_{new} (an example of the third *Strictly Mathematical Move*) but the assumption that a particular equation gives us the *dispeersion relation* (the relation between the frequencies of the collective oscillations and their wavelengths, justified by *Physical Analogy,*

The clauses H_I and $H_{s.r.}$ in H^4 are, of course, the formulas which were endowed with causal powers in the second C*ausal Inference.* We are now in a position to make sense of that endowment. Bohm and Pines interpret H_I as "a simple interaction between the electrons and the collective field" (612b), and $H_{s.r.}$ as "the short-range part of the Coulomb interaction be-

tween the electrons" (*ibid.*). We may reverse the metonymy that the second *Causal Inference* relied on, and replace H_I and $H_{s.r.}$ by their interpretations to obtain a straightforwardly causal argument.

> From [the expressions for the components of H^5] we see that if we neglect *a simple interaction between the electrons and the collective field,* the collective oscillations are not affected at all by *the short-range part of the Coulomb interaction between the electrons.* Thus *this short-range part of the Coulomb interaction* can influence the q_k only indirectly through *the interaction between the electrons and the collective field.* But, as we shall see, the *direct* effects of *the interaction between the electrons and the collective field* on the collective oscillations are small. Thus, it may be expected that the *indirect* effects of *the short-range part of the Coulomb interactions*" on the q_k through "*the interaction between the electrons and the collective field* are an order of magnitude smaller and may be neglected in our treatment which is aimed at approximating the effect of as *a simple interaction between the electrons and the collective field.*

Notice that the argument is now couched in the narrative mode illustrated in Section 2.4.

The first topic of this section was the vending machine account of deduction; the last one the role of representation in theoretical physics. In Nagel's version of the vending machine, the input and the outut are statements, and the mechanisms which transform one into the other are linguistic. Its components are statements, and its workings are governed by the laws of logic. If the machine is to work, then although the referring terms are divided into two disjoint classes, observational terms and theoretical terms, and correspondence rules are needed to link the two, all these elements must belong to a single language.[67] Bohm. Pines, and their contemporaries took it for granted that, impurities aside, a metal in the solid state consisted of crystals, each comprising a regular lattice of ions surrounded by a gas of electrons, And given this description, the physicist faced a true "Theoretician's Dilemma." In Pines's words (1987, 68), which I quoted very early in this part of the essay,

> In any approach to understanding the behaviour of complex systems, the theorist must begin by choosing a simple, yet realistic model for the behaviour of the system in which he is interested.

67. The difficulties of carrying out the project are acknowledged by Nagel in ch.5 of *The Structures of Science,* and (very clearly) by Carl Hempel in his essay, "The Theoretician's Dilemma" ([1958], 1965).

The dilemma resides in the phrase "a simple, yet realistic model." For verisimilitude begets complexity, and complexity begets intractability. The dilemma is not new; the "many-body problem" was faced by Newton, for whom "many" was "three." He supplemented the deductive resources of Apollonian geometry with the first perturbation techniques of the modern age. (Ptolemy's epicycles, equants, and eccentrics were his counterparts in the ancient world.) Today it is neither surprising nor reprehensible that physicists should supplement the resources of strict deduction, and that these supplements should be adopted by others. The random phase approximation, for example, is arguably the most valuable contribution to the deductive practices of solid-state physics directly traceable to Bohm and Pines.

Though they arrived too late for Bohm and Pines to make use of them, two other supplements that appeared around 1950 should be noted. One emerged within physics, the other from elsewhere. In 1949 Feynman introduced what were soon referred to as "Feynman diagrams," and by the mid-1950s they were part of the toolkit of many theoretical physicists—including those working in solid state physics.[68] And, as early as 1956, a paper on solid state physics by Neville Mott (1956, 1205) began as follows,

> The use of electronic computers has made it possible to calculate the electronic wave functions of simple molecules with any degree of accuracy desired.

From our present perspective we can hardly regard the use of computer techniques as a "supplement" to deductive practice. Rather, virtually all the work performed in the demonstration phases of theoretical representation is out-sourced to computer programs. Paradoxically, the greatest change of theoretical practice in the last half century did not involve any change of theory.

PART THREE
On Theoretical Practice

3.1 Theoretical Practice and the Bohm-Pines Quartet

My chief aim in this essay has been to provide a case study of the theoretical practices of physics. I chose the quartet of papers that David Bohm and David Pines published in the early 1950s, and in this section of the essay I

68. I say more about Feynman diagrams in Part Three. We may note that in 1967 Richard Mattuck published *A Guide to Feynman Diagrams in the Many Body Problem,* a book that was entirely devoted to applications of Feynman diagrams.

will use this example to summarize what *theoretical practice* involves, and to sketch the ways in which this practice changes though time. Initially this will involve some recapitulation of familiar material, but some new themes will be sounded as I go on.

For most readers of the BP quartet, its importance lay in its third and fourth papers, in which the authors used the resources of orthodox quantum mechanics to investigate the behaviour of the conduction electrons in metals.[69] The mathematical foundations of that theory had been laid twenty years earlier by Paul Dirac and John von Neumann; Dirac's *The Principles of Quantum Mechanics* was published in 1930, and von Neumann's *Mathematische Grundlagen der Quantenmechanik* in 1932. The greater part of Dirac's work presented an abstract formalism of which both Heisenberg's matrix mechanics and Schrödinger's wave mechanics (published in 1925 and 1926 respectively) were realizations;[70] von Neumann's work absorbs Dirac's "transformation theory" within the mathematics of "Hilbert spaces." (Both terms were coined by von Neumann.) Each of these publications marked a major advance in mathematical physics, but I mention them only to emphasize that, on two counts, none of them provided an example of *theoretical practice,* as I use the phrase. The first is that, even though a progression can be traced from Heisenberg to von Neumann, all four were too original to be regarded as part of an established practice; rather, they were the innovations around which practices would coalesce. The second is that theoretical practices emerge when a theory is put to work; that is to say, when a mathematical theory is applied to a particular phenomenon or system, and must be supplemented by additional procedures and techniques.

Twenty years separated the publication of the Bohm-Pines quartet from the work of Dirac and von Neumann, In that period not only did physicists become thoroughly familiar with the intricacies of quantum mechanics, but they also devised a variety of stratagems and artifices to complement the bare mathematics of the theory. Bohm and Pines in their turn contributed a number of original techniques, as we shall see, but it is the *unoriginal* part of their work, the miscellany of theoretical elements which they inherited from previous investigators, and with which they and their peers were fully conversant, that allows us to speak of a "theoretical practice" shared by a community of practitioners.[71]

A partial inventory will show just how diverse the elements of this

69. The exception was Stanley Raimes, whose book (1961), aimed at students of experimental physics, presented the classical account of plasma oscillations given in BP II.

70. Schrödinger had previously (1926) shown the equivalence of the two approaches.

71. The emphasis on a community of practitioners immediately raises the question: Is it a solecism to speak of "Newton's theoretical practice"? It is not. But Newton is a unique

practice were. From the mathematical part of orthodox quantum theory Bohm and Pines took standard results; some came from the algebra of Hilbert space operators, others from the standard perturbation techniques of the theory. On the physical side, the basic structure of the system they investigated was well known. Metals in the solid state were crystalline; within each crystal, the positive ions of the metal formed a regular array, the "crystal lattice," and were surrounded by a cloud of valence electrons. To apply quantum mechanics to this system Bohm and Pines needed to model it in a way that adequately satisfied the competing desiderata of verisimilitude and simplicity. The criteria for the latter rested on the exigencies of quantum theory, in particular, on how amenable the model was to representation by the stock Hamiltonians of quantum mechanics.[72] In BP III, by choosing a model in which the discrete nature of the individual ions was ignored, the authors were enabled to write down a comparatively simple Hamiltonian for the system, H^1. It contained just three terms. Each was a standard expression for a particular component of the energy of an aggregate of electrons, slightly modified to allow for a background of positive charge. The term for the Coulomb potential was expressed as a Fourier series, and when H^1 was rewritten in terms of the longitudinal vector potential $\mathbf{A}(\mathbf{x})$ and the electric field intensity $\mathbf{E}(\mathbf{x})$ of the electromagnetic field within the plasma, each of them was also decomposed by Fourier analysis into a series of sinusoidal waves. Both moves were well established. Indeed, the latter was the most venerable procedure used in the quartet: Jean Baptiste Fourier was born in 1768, a year before Napoleon Bonaparte.

Again, of the four kinds of approximations which Bohm and Pines drew attention to in BP I (see Section 2.5), only the fourth, the random phase approximation, was original. The first approximation allowed electron-electron collisions and electron-ion collisions to be ignored. The abstraction which ignored electron-electron collisions was analogous to that which ignored atom-atom collisions in a simplified 19th century derivation of the ideal gas law, and (as I pointed out in Section 2.5) individual electron-ion collisions were effectively ignored by all theoreticians who used the jellium model. The second approximation relied on the assumption that the organized oscillations in the plasma were small, in order that the authors could use "the *customary linear approximation* appropriate for small oscillations" (my emphasis); the third, the neglect of terms involving v^2/c^2

case. No other theoretical physicist has simultaneously articulated a truly original theory and applied it to as many diverse phenomena as did Newton.

72. This point has been emphasized by Cartwright (1999, 268–78) in analyzing the Hamiltonian used by Bardeen, Cooper, and Schrieffer in their theory of superconductivity.

on the assumption that the velocity v of the electron is small compared with the speed of light, c, was hardly a novel manoeuvre (see Section 2.7).

The elements of theoretical practice are often easily identified; their use is so widespread that they have acquired names. The *jellium model* is an example. In that instance the name is descriptive of the element, but more often the name attached to an element is the name of its originator, as in "a Fourier series." In BP III alone we find eleven such names attached to thirteen elements of five different kinds: to physical phenomena [*Coulomb interactions* (609a, *passim*)]; to theoretical models [*a Fermi gas* (610b), *a Bose field* (625a)]; to magnitudes associated with such models [*the Debye length* (611b), *the Bohr radius* (615a)]; to mathematical entities [*a Fourier series* (610b), *the* n^{th} *Hermite polynomial* (613a), *the Slater determinant* (613a), *the Fermi distribution* (615a)]; and to mathematical representations of physical systems [*Maxwell's equations* (611a), *the Heisenberg representation* (623b), *Fermi statistics* (623b), and, of course, *the Hamiltonian,* which appears on virtually every page]. Two of these phrases are used in describing the kind of theoretical approach taken by others; the rest are woven into the authors' own arguments. There are no footnotes to provide glosses on them, nor are they needed. The phrases are part of the vernacular of the readers of *The Physical Review,* and the elements they denote are part of a physicist's stock-in-trade, not least because of their versatility. The "Debye length," for example, entered the physicist's lexicon in 1926, as the thickness of the ion sheath that surrounds a large charged particle in a highly ionized electrolyte. As we have seen, twenty five years later Bohm and Pines used it in their treatment of conduction electrons in a metal, at a scale several orders of magnitude smaller than the phenomenon it was originally designed to model. Because of this versatility, when Bohm and Pines wrote in the Introduction to BP II, "For wavelengths greater than a certain length ρ_D the fluctuations are primarily collective," this parenthetical aside conveyed two messages. It gave the reader information, and it told him that he was on familiar ground.

So much for the techniques that Bohm and Pines inherited. In turn, the quartet bequeathed a number of useful theoretical tools to future investigators. The most obvious is the "random phase approximation." Another is the practice of treating a system consisting of a particle and its immediate environment as a *quasi-particle.*[73] In BP III the authors observed that, when an electron is surrounded by a cloud of collective oscillations, it be-

73. Nearly a decade later, Pines included a section (1962, 31–34) on the definition of quasi-particle in the lecture notes that form the first part of *The Many Body Problem.* The remainder of the volume is an anthology of papers from previous dozen years on the physics of many-body problems.

haves as though its mass has increased, and they treated it accordingly. (See the penultimate paragraph of Section 1.4.) Subsequent examples of quasi-particles include (a) a *conduction electron,* which consists of an electron moving within the periodic potential provided by fixed lattice ions; (b) a *polaron,* which is an electron moving in an insulating polar crystal, and (c) a *quasi-nucleon,* which is a proton or a neutron surrounded by a cloud of other nucleons.[74] Another bequest was made by Pines in 1956, when he proposed that the quantum of energy lost by a high energy electron when it is scattered within a metal foil should be regarded as a particle: a *plasmon.* He summarized "the evidence, both experimental and theoretical, which points to the plasmon as a well-defined entity in nearly all solids." (1956, 184b). Plasmons are analogous to phonons. Both are quanta of energy in condensed matter, but while phonons are stored as thermal energy by atoms as they vibrate about their mean positions, plasmons are stored by the valence electrons of a metal either individually, or collectively in plasma oscillations.

But arguably the most significant legacy of Bohm and Pines' work was the contribution it made to the theory of superconductivity published by Bardeen, Cooper, and Schrieffer in 1957. The overall strategy of their paper resembled that used in BP III. To echo Ziman, in both papers a model was set up, its Hamiltonian was prescribed and its theoretical properties deduced, and experimental phenomena were thereby explained. The BCS model was the more complicated of the two. Whereas the BP model represented the charge of the positive ions as a uniform background of charge, the BCS model not only allowed for the fact that the ions formed a regular lattice, but took into account the lattice vibrations as well. These vibrations (and here I switch into narrative mode) are quantized into phonons, which mediate an attractive interaction between two valence electrons (the so-called "Cooper pairs"). Since the repulsive force of the Coulomb interaction between these electrons is attenuated by the screening effect predicted by Bohm and Pines, the net force between the members of a Cooper pair may be attractive, causing the metal to become a superconductor.[75]

74. See Mattuck (1967, 15).

75. This "criterion for the occurrence of a superconducting phase" (BCS, 1176b) had been put forward by Bardeen and Pines (1955). (Note the connecting link between the BP quartet and the BCS paper.) The achievement of the BCS paper was to show how the criterion could account for the "main facts" about superconductivity, and the authors list (BCS, 1175a): "(1) a second order phase transition at the critical temperature, Tc [. . .], (2) an electronic specific heat varying as $\exp(-T_0/T)$ near $T=0$°K [. . .], (3) the fact that a superconductor exhibits perfect diamagnetic behaviour, (4) effects associated with infinite conductivity ($\mathbf{E}=0$), and (5) the dependence of Tc on isotopic mass, $Tc\sqrt{M} = \text{const.}$" In 1972 all three authors were awarded the Nobel Prize for their work on superconductivity.

The Hamiltonian for the BCS model reflects these two opposing interactions. It contains four terms (BCS, 1179a), of which the third, H_{coul}, represents the energy due to the screened Coulomb interactions between electrons, and the fourth (oddly referred to as H_2) the energy due to the phonon interaction with the Cooper pairs. For an expression for H_{coul} the authors go to the BP theory. Recall from Sections 1.4 and 1.5 that what had started out in BP III as the Hamiltonian H^1 for a dense electron gas was successively modified until it appeared in P IV in its final version: $H = H_{part} + H_{coll} + H_{s.r.}$. In that final Hamiltonian the effects of Coulomb interactions were divided into two parts. The long-range effects were summarized in the term H_{coll}, where they were "effectively redescribed in terms of collective oscillations of the system as a whole" (P IV, 627a). In contrast, the term $H_{s.r.}$ corresponded to "a collection of individual electrons interacting with a comparatively short-range force" (*ibid.*). Since the high energies associated with collective oscillations do not occur in a superconductor, the result is that the term H_{coul} in the BCS Hamiltonian need only represent short range effects, i.e. the screened interaction represented by $H_{s.r.}$.

The kind of theoretical practice exemplified by the BP theory of plasmas and the BCS theory of superconductivity did not last. From the perspective of the early twenty-first century, these theories appear as two of the last constructionist contributions to solid state physics. I borrow the term "constructionist" from Philip Anderson (1972), who contrasts two hypotheses: constructionist and reductionist.[76] The latter is a hypothesis about the physical world. On the reductionist hypothesis the behaviour of a macroscopic system is ultimately determined by the behaviour of its sub-microscopic constituents, which in turn is governed by simple fundamental laws. The constructionist hypothesis is bolder; it is a hypothesis about the reach eventually attainable by our fundamental theories of physics. It suggests that, as and when our "final theory" has established the ontology of fundamental particles and the laws which they obey, we will have the theoretical resources to explain all the phenomena of nature. This hypothesis Anderson rejects (*op. cit.,* 393): "(T)he reductionist hypothesis does not by any means imply a 'constructionist' one: the ability to reduce everything to simple fundamental laws does not imply the ability to start from those laws and reconstruct the universe." To amplify this he continues,"[I]t seems to me that we may array the sciences linearly according to the idea: The elementary entities of Science X obey the laws of Y," and he then sketches a hierarchy whose first three entries are these:

76. Incidentally, in 1958 Anderson published a paper which drew on the work, both of Bohm and Pines and also of Bardeen, Cooper and Schrieffer. The title was "Random-Phase Approximation in the Theory of Superconductivity."

X	Y
Solid state or many body physics	elementary particle physics
chemistry	many body physics
molecular biology	chemistry
.	
.	

And he quickly adds, "But this hierarchy does not imply that science X is 'just applied Y.' At each stage entirely new laws, concepts and generalizations are necessary [. . .]."

I extend the usage of the adjective "constructionist" to cover theoretical endeavours that would, if successful, confirm the constructionist hypothesis. In this sense, the approach taken by Bohm and Pines was constructionist in many ways. This is beautifully illustrated by the transformations that I alluded to two paragraphs ago, whereby the Hamiltonian H^1 in BP III became H in P IV. Bohm and Pines started with elementary particles; the components of H^1 dealt with the properties and interactions of electrons: their individual kinetic energies, their individual self-energies, and the energy due to the pair-wise Coulomb forces between them. As we saw, the final Hamiltonian is the sum of three terms, $H_{\text{part}} + H_{\text{coll}} + H_{\text{s.r.}}$. The first term and third terms still deal only with electrons. H_{part} represents their kinetic energy and self energy (albeit as those were modified by the environment) and $H_{\text{s.r.}}$ "corresponds to a collection of individual electrons interacting via a comparatively weak short range force (P IV, 627b)." H_{coll}, however, is expressed entirely in collective variables: "The long range part of the Coulomb interaction has effectively been redescribed in terms of the collective oscillations of the system as a whole." (P IV, 627a) In this way the emergent behaviour of the system is made explicit, and the microscopic and the macroscopic are accommodated under one roof.[77]

But, as we saw in Section 3.2, the collective variable approach to many-body problems had severe limitations. It was never taken up outside David Bohm's immediate circle at Princeton. Even the authors of the BCS paper had reservations about its use; in the conclusion of their paper they wrote (BCS, 1198a), "An improvement of the general formulation of the theory is desirable," and listed half a dozen items which could be im-

77. Subsequently Pines predicted the value of an emergent property of the plasma, its specific heat (P IV, 632).

proved on, the BP "collective model" being one. From the middle of the 1950s on attention moved away from constructionist treatments of high density plasmas in favour of macroscopic treatments. For example, in 1954 Lindhard described the behaviour of plasma entirely in terms of a macroscopic property, its dielectric constant, and his example was quickly followed by others. Intertwined with the rejection of a constructionist methodology was a major change in theoretical practice: the orthodox quantum mechanics used by Bohm and Pines was supplanted by quantum field theory.[78]

Throughout the 1930s and 1940s quantum field theory had been highly problematic. It had a number of successes, like the prediction of the positron, but it was prone to divergencies; that is to say, seemingly innocuous calculations went to infinity.[79] In 1949 the problem was resolved from two directions. Sin-Ituro Tomonaga and Julian Schwinger used a generalization of operator methods, which was theoretically impeccable but very difficult to work with; Richard Feynman, on the other hand, used a "propagator approach," and showed how it could be pictorially represented by simple diagrams.[80] While the Tomonaga-Schwinger approach made quantum field theory respectable, the Feynman approach made it easy to apply. Later in the year Freeman Dyson showed the two approaches to be equivalent, and by 1955 quantum field theory had established itself as the core of a new theoretical practice, largely because the Feynman diagram had shown itself to be one of the most remarkable theoretical tools of the twentieth century. In particular, there were two major reasons why physicists found the theory well suited for treating dense electron gases. The first was "[t]he realization, that there exists a great formal similarity between the quantum theory of a large number of Fermi particles and quantum field theory" (Hugenholtz and Pines, 1959, 489/332); the second was the theoretical economy afforded by Feynman diagrams, and was as important as the first.[81] Eugene Gross wrote (1987, 47),

78. Nevertheless, despite the change of methodology and of foundational theory, the various approaches to high density plasmas in the 1950s were by no means incommensurable. For example, Philippe Nozières and Pines bridged the gap between the collective variable approach and the alternatives in papers like "Electron Interactions" (1958b), which was subtitled, "Collective Approach to the Dielectric Constant."

79. For a succinct account of the theory's successes and failures, see Howard Georgi (1989, 449). The seriousness of the problem can be judged by the language used to describe it. Michio Kaku (1993, 4) describes quantum field theory as "plagued with infinities," and for Georgi that was its "tragic flaw."

80. Not all physicists welcomed the Feynman diagram. In fact Schwinger is reputed to have forbidden his graduate students from using it, on the grounds that its use represented the triumph of theft over honest toil.

81. In an earlier version of this paper, when I discussed alternative approaches to elec-

> Feynman's introduction of diagrams freed the imagination of theoretical physicists to deal with what had been depressingly complicated formalisms in quantum field theory and many-body physics.

Concerning the latter, Gross wrote from experience. Like Pines, he was one of Bohm's students, and he too had collaborated with Bohm in papers that took the collective approach.

3.2 A Very Brief Note on Methodology

There are two respectable ways in which a philosopher of physics can approach theoretical physics. The first is to examine specific theories with an eye to philosophical issues. Examples are the usual suspects—statistical mechanics, general relativity, quantum theory—but the theories investigated need neither have an extensive domain of application nor be widely accepted; Bohmian quantum mechanics and Heinrich Hertz's version of classical mechanics are cases in point. Some of the issues will be metaphysical: The problem of *The Direction of Time; What kind of Being does space-time have?* Others will be internal to the theory: *What is measurement in quantum theory?*[82] Though a theory may be associated with a particular physicist, as Einstein is with the special and general theories of relativity, that is irrelevant to the philosophical issues the theory raises. A theory achieves a life of its own, so to speak.

The second approach consists in examining how physicists use these theories, and is more empirical in nature. Its object is, first of all, to give accurate descriptions of theoretical practices, something signally absent until recently in philosophical circles. On this approach, the philosopher regards each specific application of the theory as uniquely tied to the theorist (or theorists) who made it. The application may involve standard techniques, like the use of the Bloch Hamiltonian or the random phase approximation, but they are the choices the authors made. By its nature the material examined by the philosopher are the words, equations, and diagrams appearing in a paper in a physics journal. To amplify a theme from the Preamble to this essay, such a philosopher regards those works as texts, and her task to be analogous to that of a literary critic. Like a good literary critic, the philosopher who uses this methodology draws the reader's attention to the elements of a text and how they fit together. In particu-

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tron plasmas between 1954 and 1958, I cited fourteen papers from that period. Feynman diagrams appeared in nine of them.

82. These three issues are section headings in Lawrence Sklar's admirable *Philosophy of Physics* (1992).

lar, she shows the reader what kind of text it is and the nature of its success.

This essay is an endeavour of the second kind.

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