Backstory to Wave-Particle Duality

In Query 29 of his *Opticks*, Newton had famously asked:

> *Are not the Rays of Light very small Bodies emitted from shining substances?*

Thus, Newton conjectured his corpuscular view of light and was able to account quantitatively for the optical phenomena known in the 17th century, including refraction of light. It was not until the 19th century that the observation of diffraction showed conclusively the inadequacy of Newton's picture. Meanwhile, Christiaan Huygens (1629-1695) in the 1690s proposed that light "spreads, as sound does, by spherical surfaces and waves: for I call them waves from their resemblance to those which are seen to be found in water when a stone is thrown into it." [406] To support the transmission of such waves, Huygens proposed the existence of "ethereal matter," the thorny concept of an ether going back in some form to the Greeks and persisting up to applications of the electromagnetism of Maxwell and finally found to be unnecessary by Einstein's development of special relativity in 1905. However, Huygens' waves were actually longitudinal as with the sound waves of his inspiration, and not the transverse waves that we now know from Maxwell's electromagnetism. And they were considered in terms of impulses in a medium and not as periodic waves in the modern sense because methods for treating periodicity had not yet been formulated [309, p. 343]. Another property of light familiar to Newton and Huygens was interference though then only incompletely understood. Thomas Young (1773-1829) made a decisive advance by explaining the physics behind constructive and destructive interference of overlapping waves followed by Augustin Fresnel (1788-1827) who formulated the mathematics of interference as well as experiments to verify the ideas. This included the double-slit arrangement as previously discussed in Chapter 1. An important confirmation of Fresnel's theory was obtained by François Arago (1786-1853) who experimentally observed a bright spot at the center of the geometrical shadow of a circular obstruction. Formed by constructive wave interference, this phenomenon was surprising to many at the time and became known as "the spot of Arago." By the late 19th century, the success of Maxwell's approach had merged optics and electromagnetism, with light now being recognized as a type of electromagnetic wave. And so it was that light was firmly in the category of *wave* when the quantum of action was discovered in 1900.

However, Newton was somewhat of a moving target as usual. In order for Newton to be able to describe diffraction, he knew that the motion of the light corpuscles would have to be affected at a distance by the diffracting body. An example was Francisco Grimaldi's (1618-1663) observation of the fringes of light diffracted from an edge in 1665. Regarding this situation, Newton also famously said in another query:

> *Are not the Rays of Light in passing by the edges and sides of Bodies,*

bent several times backwards and forwards, with a motion like that of an Eel? And do not the three Fringes of colored Light above-mentioned arise from such bendings?

Thus, within Newton's picture, a force necessarily reaches out from the surface of the body. For Newton's theory to be successful in all applications, including diffraction, it would actually have had to be something of a corpuscular-wave! However, rather than being ahead of his time, anticipating a type of *eel-corpuscle duality*, we can see in hindsight that Newton's approach to optics did not stand a chance in 1704 when the *Opticks* was published. However, Newtonian mechanics from the *Principia* was enormously successful and changed the way science was done. Why was the *Principia* so successful and the *Opticks* much less so?

As discussed in the section *Deductive versus Inductive Thought*, the striking aspect of Newton in the *Principia* was his deductive approach in that he would "feign no hypotheses" in contrast to other scientists of the era. What sets Newton apart from Leibnitz and others is that he does not insist that nature act in certain ways just because he doesn't like it. Nature did conform accurately to his universal gravity in which a force between masses reaches out instantaneously over arbitrary distances. Leibnitz would object to Newton's theory and simply declare that "matter cannot act where it is not." But that was his loss. Newton did not understand how "matter cannot act where it is not" either. In a private letter in 1692 to a confidant, Newton stated [407, p. 52]:

See the print edition of The Quantum Measurement Problem for quotation.

Newton repeatedly did search for some explanation of how universal gravity might act. He sought to describe universal gravity in terms of other phenomena: aether particles, electrical effluvia, an aether with variable density, etc. [407, p. 40]. None of these approaches could produce a force varying inversely as the square of the distance and that acts mutually between masses. But he did not allow this to affect or prejudice the science that did. One historical study has argued that Newton did not follow his deductive approach in the *Opticks* but instead it was Huygens who more closely followed Newton's deductive method from the *Principia* in his own more successful development of optics [408]. Perhaps the paradigm of deductive versus inductive thinking also explains this aspect of Newton's research in optics.

The impact of Newton's *Principia* was so overwhelming that his universal theory of gravity, with its force instantaneously acting at distance, became the template for performing successful science. In particular, it was also applied to the laws of electricity and magnetism in the century to follow. The formulation of electrodynamics by André-Marie Ampère (1775-1836) was in terms of an inverse-square force acting instantaneously at a distance [340, p. 348], and this approach was adopted by others into the next century including Gauss, Weber, Lorentz, Lienard and Wiechert. However, the mostly self-taught but prodigious Michael Faraday introduced the concept of lines of force traversing space between conductors and made this tangible in his demonstrations using sprinklings of iron filings. This led to the view of an electromagnetic field occupying the space in the regions between current carrying conductors. The field became an independent entity that could fill space itself, even in a vacuum. Faraday's field concept was taken over by Maxwell leading eventually to his electromagnetic field equations. Ampère's action at a distance formulation would give identical results as long as the current changes were not too rapid. The concept of electromagnetic fields ultimately would be needed as experiments became more refined. However, at the time of Faraday's investigations electromagnetic waves were unknown and they were first detected by Heinrich Hertz (1857-1894) in 1888, twenty years after Faraday died. This gave concrete evidence for the field concept which would become central to much of the physics of the 20th and 21st centuries. One implication of the presence of electromagnetic fields that was later discovered was that they would require a finite time to propagate. However, Faraday actually anticipated this and he wrote a sealed letter on March 12, 1832 stating his views on the propagation of electromagnetism, intending it to be read after one hundred years. The letter was given to the Secretary of the Royal Society of London where it lay forgotten for the next century until it was finally opened by Sir William Bragg (1862-1942) on June 24, 1937 [409, p. 11]:

> *I am inclined to compare the diffusion of magnetic forces from a magnetic pole to the vibrations upon the surface of disturbed water, or those of air in the phenomenon of sound; i.e. I am inclined to think the vibratory theory will apply to these phenomena as it does to sound, and most probably to light. By analogy, I think it may possibly apply to the phenomenon of induction of electricity of tension also. These views I wish to work out experimentally; but as much of my time is engaged in the duties of my office, and as the experiments will therefore be prolonged, and may in their course be subject to the observation of others, I wish, by depositing this paper in the care of the Royal Society, to take possession as it were of a certain date; and so have right, if they are confirmed by experiment, to claim credit for the views at that date; at which time as far as I know, no one is conscious of or can claim them but myself.*

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