The Merli-Missiroli-Pozzi Two-Slit Electron Interference Experiment

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ABSTRACT

In 2002 the readers of the scientific magazine *Physics World* voted Young's double-slit experiment applied to the interference of single electrons to be 'the most beautiful experiment in physics'; this experiment, in truth, had already been carried out 30 years beforehand. The present article aims to re-examine the latter real experiment and put it into its proper historical perspective. Even though the experiment was not afforded the importance it perhaps deserved among philosophers, its philosophical implications add new arguments to the already far-reaching debate triggered off by the ideal experiment. Within the context of quantum mechanics in particular, the experiment revealed for the first time the importance that ought to be attributed at the empirical level to single-case probability, that is, to the probability of a single electron's reaching the screen. I investigate how the empirical significance of the experimental results comes within the framework of the debate over the propensity interpretation of probability in quantum mechanics. I argue that the experiment, when examined in all its technical details does in fact throw light on the evidence for the propensity, as a physical property, of micro-objects.

1 Background and history

In May 1974, Pier Giorgio Merli, Gian Franco Missiroli and Giulio Pozzi (MMP) sent the *American Journal of Physics* an article entitled 'On the statistical aspect of electron interference phenomena', which was published two years later [Merli, Missiroli, and Pozzi 1976a]. Thirty years on, and the September 2002 number of *Physics World* contained the results of a survey in which readers had been asked to name the most beautiful experiment in physics. The readers' leading choice was Young's double-slit experiment applied to the interface of electrons, followed by: 2. Galileo's experiment on falling bodies; 3. Millikan's oil-drop experiment; 4. Newton's decomposition of sunlight with a prism; 5. Young's light-interference experiment; 6. Cavendish's torsion-bar experiment; 7. Eratosthenes' measurement of the Earth's circumference; 8. Galileo's experiments with rolling balls down inclined planes; 9. Rutherford's discovery of the nucleus; 10. Foucault's pendulum. Those interested may like to look at the remarks made by R. P. Crease [2002] regarding the underlying reasons for the choices made by the journal's readers, together with other related topics.

In the May 2003 number of the same journal, the above-mentioned Italian scientists were acknowledged as being the first to conduct the aforesaid experiment, reference being made to their article published in 1976. The experiment, carried out in Bologna, enabled the authors to obtain an interference pattern through a standard electron microscope fitted with a special interferometer ('electron biprism'). The electron biprism basically consists of a very thin wire, at right angles to the beam, positioned symmetrically between two plates at ground potential; a positive or negative potential is applied to the wire so that an electron beam is split into two deflected components. The use of the electron biprism, as we shall see later on, was the first important technical and conceptual feature of this experiment. The second fundamental aspect was, of course, the chance it gave to observe the continuous arrival of electrons, one at a time, on a television screen. MMP, together with Lucio Morettini and Dario Nobili, also produced a 16mm film entitled 'Interferenza di elettroni' ('Electron interference'), which may currently be viewed on the website http://www.lamel.bo.cnr.it/educational/educational.html.[1]

However, things did not go as smoothly as they would appear to have from reading the above. Authorship of the experiment was initially attributed to Tonomura and his team [Tonomura, Endo, Matsuda, Kawasaki, and Ezawa 1989], who in 1989, at the Hitachi Advanced Research Laboratory in Tokyo, demonstrated what MMP had already demonstrated in Bologna fifteen years earlier: that is, the formation of fringes of interference as a result of the single electron build up of an interference pattern. This is an incontrovertible fact which the distinguished physicist J. W. Steeds, Head of the Physics Department of Bristol University, was fortunately aware of, as he had the opportunity to see an initial version (in Italian) of the above-mentioned film in 1975. Steeds remarked on this fact in a letter to *Physics World*, [2003, p. 20]. Steeds' very words were as follows:

> I believe that 'the first double-slit experiment with single electrons' was performed

[1]The film received the physics category prize at the International Festival on Scientific Cinematography held in Brussels in 1976.

by Pier Giorgio Merli, Gian Franco Missiroli and Giulio Pozzi in Bologna in 1974 – some 15 years before the Hitachi experiment. Moreover, the Bologna experiment was performed under very difficult experimental conditions: the intrinsic coherence of the thermionic electron source used by the Bologna group was considerably lower than that of the field-emission source used in the Hitachi experiment.

This is not all, however. Tonomura *et al*. fail to make any explicit reference to MMP's article of 1976, but only refer to the film. Moreover, the latter reference, as MMP themselves point out in a letter to *Physics World* [Merli, Missiroli, and Pozzi 2003a, p. 20], Tonomura *et al*. do not even mention the fact that the film shows the arrival, one after another, of single electrons. Evidently the referee of the *American Journal of Physics* in 1974 had forgotten about MMP's previous work (published in the very same journal!), as was indeed pointed out in a letter to the journal [Gilson 1989] following the article written by the Hitachi group. The Hitachi version of the experiment, which is undoubtedly excellent, can be seen on the website http://222.hqrd.hitachi.co.jp/em/doubleslit.html.[2]

This false belief regarding the precedence of the Hitachi team's experiment is more widespread than one would imagine. For example, in two fine books by M. P. Silverman [Silverman 1993, 1995], who was himself personally involved in the experiment carried out at the Hitachi Advanced Research Laboratory, the author's discussion of electron interference is exclusively based on the article by Tonomura *et al*., with no reference at all made to MMP. Silverman does, however, take the precaution [Silverman 1993, p. 12] of stating that: 'The Hitachi esperiment is not the first of its kind (although it was the first I had personally witnessed), but rather one of the last and most conclusive in a line of analogous experiments dating back to just a few years after Einstein proposed the existence of photons'. Of course the 'most conclusive experiment' was really the one conducted by MMP.

My dwelling here on the authorship of the experiment is not based on mere parochial considerations, but rather on the fact that this whole story seems to confirm the idea that the history of science is clearly biased towards theory, and as such fails to give due weight to the vitally important role played by experiments. I refer in particular to Hacking [1983]. In this book, Hacking tried to redress this imbalance when he stressed the importance of experimental science and criticised the ideas of philosophers which '[B]y legend and perhaps by nature [. . .] are more accustomed to the armchair than the workbench' (*Ibid*, p. 150) and believe observation to be subservient to theory. The author quotes a host of examples of the bias for theory over experiment, emphasizing 'the standard preference for hearing about theory rather than experiment' (*Ibid*, p. 152). Succinctly put, Hacking's thesis reads (*Ibid*, pp. 150–151):

[2] Akira Tonomura still appears convinced that his experiment was the first of its kind. In reply to the above-mentioned letter from MMP [Merli, Missiroli, and Pozzi 2003a, p. 20], he writes [Tonomura 2003, p. 21]: 'We believe that we carried out the first experiment in which the build-up process of an interference pattern from single electron events could be seen in real time as in Feynman's famous double-slit Gedanken experiment under the condition, we emphasise, that there was no chance of finding two or more electrons in the apparatus'.

> History of the natural sciences is now almost always written as a history of theory. Philosophy of science as so much become philosophy of theory that the very existence of pre-theoretical observations or experiments has been denied.

The following quote regarding the double-slit experiment [Rodgers 2003] seems to fully back up Hacking's line of thought:

> So who actually carried out the first double-slit experiment with single electrons? Not surprisingly many thought or gedanken experiments are named after theorists – such as the Aharonov-Bohm effect, Bell's inequality, the Casimir force, the Einstein-Podolsky-Rosen paradox, Schrödinger's cat and so on – and these names rightly remain even when the experiment has been performed by others in the laboratory. However, it seems remarkable that no name whatsoever is attached to the double-slit experiment with electrons. Standard reference books are silent on this question but a study of the literature reveals several unsung experimental heroes.

A long list of 'unsung heroes' follows the quote: this list starts with the name of Geoffrey Ingram Taylor [1909], who in 1909 obtained interference fringes using a light source that was so weak that only a very few photons at a time struck a photographic plate screen. In subsequent years, the list included Gottfried Möllenstedt and Heinrich Düker [1955], the inventors of the electron biprism, who utilised it to obtain interference fringes with an electron microscope. Some years later Claus Jönsson [1961] managed to perform the electronic version of Young's optical experiment, when he succeeded in obtaining interference through a number of (up to five) $0.3\mu\mathrm{m}$.-wide slits. Finally, MMP were acknowledged as being the first physicists to succeed in performing the experiment with just one single electron in the apparatus at any one time.

After this brief historical introduction, one could rightly question the reason for writing yet another article about electron interference when we already know everything there is to know (more or less) about this particular subject. We know that the double-slit experiment is used as the standard example of all 'quantum weirdnesses', that is, of the basic characteristics of Quantum Mechanics (QM). A check-list of the aforesaid characteristics invariably contains wave-particle duality, the uncertainty principle (or relationship), the probabilistic nature of QM, and the concept of non-locality. Moreover, even prior to the EPR (Einstein-Podolsky-Rosen) paradox, the double-slit experiment (the gedanken version thereof, of course) was chosen by Einstein and Bohr as a focal point for their debate over the completeness or otherwise of QM [see Jammer 1974]. Bohr's analysis of the experiment reveals, in a paradigmatic manner, the role played by the principle of complementarity in reconciling the wave and corpuscular aspects of electron (and photon) behaviour. The same experiment can be used to calculate Heisenberg's uncertainty relations resulting from wave-particle duality. Moreover, the double-slit experiment has also constituted the basis for the claim that complementarity is simply the consequence of uncertainty relations [Storey, Tan, Collett, and Walls 1994]. The debate over whether uncertainty relations derive from complementarity, or vice-versa, dates from the 1990s: for references to the said debate, see Rabinowitz [1995]. During that same period, Suppes and Acacio de Barros [1994a, b] proposed the derivation of the phenomena of photon interference and diffraction without resorting to wave properties, but basing their hypothesis on the existence of certain mechanisms of emission, absorption and interaction of the particles themselves, which we shall be looking at in more detail below.

The gedanken double-slit interference experiment has also been of vital importance in relation to the employment of probability in QM. According to some authors, this experiment represents the most striking proof that the axiom of the countable additivity probability is not valid in QM; according to others, however, it is this very double-slit experiment, when analyzed correctly, which confirms the validity of the 'classical' theory of probability even in the micro-world [Fine 1972, 1973]. The same is true for the interpretation of probability in terms of propensity. We know that it was the thought double-slit experiment that led to a radical change in Karl Popper's perception of QM resulting in a new interpretation of probability connected ontologically to the introduction of a new physical property – propensity (which I shall be discussing later). Others again [Milne 1985] believe that the double-slit experiment provides proof of the inadequacy of such proposals.

The notion of non-locality is intrinsic to the gedanken double-slit experiment, when it is said that the particle 'knows' whether the slit through which it did not pass is open or closed, and it 'knows' of the decision to measure, or otherwise, its trajectory even though the said decision is taken after it has passed through the slits. Furthermore, an EPR experiment can be seen as a pair of double-slit experiments in which the result of the *decision* to carry out a measurement using one microscope should immediately modify what is observed in the other microscope (the presence or otherwise of interference fringes).[3]

We could continue discussing the 'weirdness' of quantum mechanics as revealed by the double-slit experiment, but this is not the purpose of the present article. The true aim of this paper is to put MMP's experiment into its proper historical perspective. During the 1970s a number of important articles on the foundations of QM appeared in the spheres of both physics and philosophy; these articles pointed to the important role of the double-slit experiment. I shall be referring to the proposed (updated) statistical interpretation of QM – 'statistical' as shall be shortly specified (see, for example, Ballentine [1970]) – linked also to the propensity interpretation of probability. The experiment conducted by MMP a few years later failed to prompt any real re-thinking of such problems, even if from various points of view it should have been seen as a crucial experiment, with all its empirical weight, regarding the fundamental aspects of QM.

The albeit brief survey presented above shows how the double-slit experiment continues to be of great importance to this very day, when technological progress means that the experiment can now be repeated fairly easily not only with microscopic objects (electrons, photons, neutrons and atoms) but also with *mesoscopic* systems such as fullerene molecules [Facchi, Mariano, and Pascazio 2002]. The technology available to MMP at the time was clearly not nearly as advanced as what is currently available; despite this, however, certain technical aspects of their experiment can engender various forms of philo-

[3]The conditional 'should' has been used here to indicate that an experiment of this kind using two electron microscopes, but otherwise exactly the same as the original experiment conducted by MMP, has yet to be carried out.

sophical reflection. In fact, a further reason for writing the present article is that, in my own opinion, the said experimental features may arouse interest in philosophical debates. It is not easy to understand why MMP's experiment has been afforded so little importance by philosophers of science, despite its evident historical significance. While one could of course debate whether the experiment ought to be considered the most beautiful (or the second/third/fourth ... most beautiful) experiment in physics, there can be no doubt that this experiment dramatically revealed the world of QM, complete with all its paradoxical, counter-intuitive features, to a much wider public. Besides Hacking's views, which we mentioned earlier, one possible explanation could be that the actual experiment failed to add anything to what the various versions of the thought experiment had already demonstrated. In other words, the actual experiment merely illustrated in detail the forecasts previously made by theory, rather than revealing any new 'mysteries' or stimulating the submission of new questions. While this may well be true, it does not give the complete picture. Indeed, it is true, as MMP themselves point out in their articles, that the experiment is of considerable educational value; in fact, it enables people to physically experience what the standard interpretation of QM perceives as 'wave-particle duality'. It is also true that their experiment is, rather paradoxically, more 'transparent' than the thought experiment, in that it rules out various interpretations that the thought experiment fails to exclude, such as those deriving from the hypothesis about the interaction among electrons and/or between electrons and the screen. Finally, this paper aims to look more closely at certain observations MMP themselves made when writing about their experiment for diverse Italian scientific journals, and which did not receive due acknowledgment.

2 The experiment

2.1 The experimental apparatus

For a description of the experimental apparatus, see Merli, Missiroli, and Pozzi [1974, 1976b] and Missiroli, Pozzi, and Valdrè [1981]. For the purposes of the present article, we are only going to give those technical details of essential importance to the discussion in hand. Figure 1 illustrates the layout of the apparatus around the convergent electron biprism. S indicates the '*effective* electron source'; in other words, S is not the real source of the electrons, but behaves as such. In fact, the electrons are emitted thermionically by a filament positioned 363 mm. from S, and by means of a system of condenser lenses they are focused on a given area, the diameter of which may be reduced to approximately 6 mm. Thus S is effectively the *monochromatic electron point source*. The biprism's wire F is positioned at a distance of 10 cm. (a in Fig. 1) from S, equidistant from two grounded plates. The wire has a diameter of $2r = 400$ nm, and is positioned 2 mm from the plates. If a voltage V is applied to the wire, an electric field will be generated in the vicinity that is the same as the field generated by a cylindrical condenser of external radius R – slightly less than the distance between the plates – and internal radius r. In this model, a

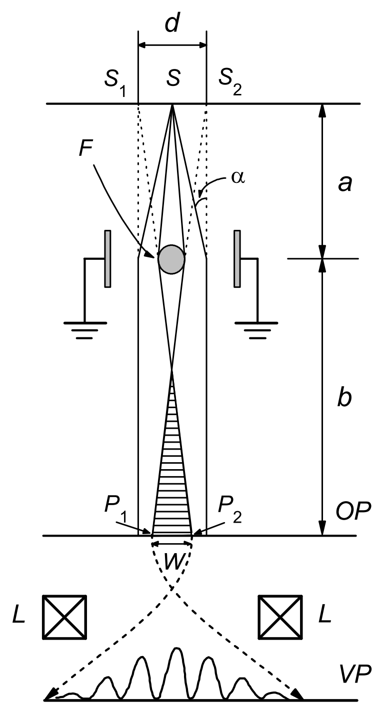

Fig. 1. Diagram (not in scale) of an electron biprism illuminated by a monochromatic electron point source S; S_1 and S_2 are virtual sources at distance d; α is the deflection angle when a positive voltage is applied to the wire F; a is the distance between the wire F and the source S; b is the distance between the wire F and the observation plane OP; W is the width of the interference field delimited by the boundaries P_1 and P_2; the hatched region is the overlapping region; L is the system of lenses used to magnify an image of the plane OP onto the viewing plane VP.

calculation can be made [Komrska 1971] of the angle of deflection α through which an electron of mass m and speed v_0 is diverted when it passes at a distance of x from wire F. This angle of deflection is given by:

$$\alpha = \frac{2eV}{mv_0^2 \ln(r/R)} \tan^{-1} \frac{(R^2 - x^2)^{1/2}}{x} \tag{1}$$

where e is the electron's charge. If $V > 0$ (converging biprism), the electron will be deflected towards the wire; on the other hand, if $V < 0$ (diverging birprism) then the electron will be deflected in the opposite direction. In the experiment, α has a value of several 10^{-5} rad, and in this model is independent of x.

In the hatched region in Fig. 1, termed the *overlapping region*, a 'non-localized interference pattern will be produced' [Missiroli et al. 1981, p. 654]. The authors use this expression to indicate the fact that interference occurs in the entire overlapping region, but in order to 'see' it, the overlapping region needs to be bisected by a plane OP (the *observation plane*) situated at a distance b beneath the biprism. If we place a fluorescent screen on this plane, then we shall 'see' the electrons forming interference fringes in an area, called the *interference field*, of width W, given by:

$$W = 2\left|\frac{a+b}{a}\right|\left(\alpha\frac{ab}{a+b} - r\right) \tag{2}$$

With $\alpha = 5 \times 10^{-5}$, and as in Merli et al. [1976b], $r = 200$ nm, $a = 10$ cm, $b = 24$ cm, then it follows that W = 23 μm. In order that the fringes be *actually* visible, a system of lenses is required, designed to provide a sufficiently enlarged image of the fringes on a further plane, VP (the *viewing plane*), so that they may be viewed with the naked eye and/or recorded directly onto a photographic plate. In Merli et al. [1976b], the plane OP was enlarged 240 times on plane VP. As we mentioned at the beginning of the present article, experiments based on the above diagram and designed to obtain interference fringes using an electron microscope, had already been conducted during the mid-1950s [Möllenstedt and Düker 1955, 1956]; however, in order to see the arrival of one electron at a time at viewing plane VP, we had to wait until MMP's experiment in which they employed an image intensifier connected to a cathode tube, which replaced the photographic plate. This device works on the basis of the consecutive conversion of an 'electronic image' into an 'optical image' that is projected, by means of optical fibres, onto the image intensifier's photocathode. At the end of this initial process, a new optical image is obtained which is 200 times brighter than the image that would have been seen on a microscope's fluorescent screen. The resulting image is, in turn, transmitted via optic fibres onto the photocathode of the SEC tube, connected to the monitor by means of a video amplifier and control unit. The SEC target can hold the electrostatic charges for a relatively long period even after the electron beam has been switched off. This enables scientists to work with extremely low intensities (one electron at a time) and to wait as long as it takes for the formation of the image. In MMP's experiment, the lowest storage time achievable with the TV image intensifier was 0.04 s.[4] It was possible to operate with such a low electron current density that, within the interval of 0.04 s, only one electron (or very few electrons) were seen on the final screen. The arrival of an individual electron on the monitor was perceived as a tiny white dot. By increasing storage time to the order of minutes, one could see the electrons striking certain areas of the viewing plane, one at a time, so that after the arrival of thousands of such electrons, fringes began to be discernible.

As we have said, eq. (1) gives the angle of deflection of the electron caused by the biprism's wire. If we know the angle of emission from source S, we can thus use eq. (1) to compute the point of arrival of the electrons on the OP. However, the distribution of such points calculated by means of eq. (1) is clearly not the same as the distribution we observe experimentally; in other words, it does not reproduce the interference fringes in the interference field W. In fact, in order to account for the experimental results, we somehow need to introduce the wave aspect of the electron's behaviour. The standard de-

[4]The storage time of the image intensifier plays a similar role to that of the exposure time of the photographic plate.

scription of the experiment involves a move from elementary optics to de Broglie's waves. The system illustrated in Fig. 1 can be seen as the electronic equivalent of Fresnel's optical biprism. In a similar optical model, it is as if the electrons came from two virtual punctiform sources S_1 e S_2 positioned symmetrically in relation to the real source S and on the same plane as the latter. The distance between the two virtual sources, d, is equal to $2|\alpha a|$. Continuing the analogy with the optical biprism, and thus introducing the de Broglie wavelength λ, fringes of interference in the interference field are forecast with a periodicity l given by:

$$l = \lambda|(a + b)/d| \tag{3}$$

The above-mentioned optical model is very useful when working out the operative parameters of the experiment, which for the purposes of the present discussion are not, however, of essential importance. It should be said that various other models have also been proposed, some more complex than others, in which QM equations are used directly to explain the observed phenomena (see, for example, Missiroli et al. [1981]).

2.2 Comments

The first comment I would like to make concerns an event which, albeit of historical importance, has never received the recognition it really deserves. In referring to the technical specifications and working of the image intensifier, Merli, Missiroli, and Pozzi [1976a, b] mention a work by K.-H. Hermann and coworkers [Hermann, Krahl, Kübler, Müller, and Rindfleisch 1971]. This article was part of the *Proceedings of the International School of Electron Microscopy*, held in Erice (Sicily) in April 1970, which Merli and Pozzi also attended. On that occasion, Hermann illustrated a number of experiments conducted using the image intensifier created by Siemens a few years beforehand. These experiments are of considerable historical importance in that they show the formation of Fresnel fringes resulting from the arrival of individual electrons passing through a tiny hole in a carbon film. By setting the density of flow at $10^{-15}\mathrm{A/cm}^2$ (see Fig. 21 in Hermann et al. [1971]), and a storage time of 0.04 s, 'only the signals of individual electrons are visible' (*Ibid.*, p. 265). By increasing the storage time (up to 120 s), we may directly observe how the Fresnel fringes take shape (*Ibid.*, Fig. 21 p. 267). We know that the history of physics is much more convoluted than certain textbooks would have us believe (one example being the difficulty of ascribing authorship to the single electron interference experiment mentioned at the beginning of the present article); in fact, the experiments conducted by Hermann *et al.* were mainly designed to illustrate the potential of the image intensifier sold by Siemens, and as such were only of interest to electron microscope experts and were limited to certain techniques. The wider scientific community failed to grasp the fundamental importance of the results obtained by Hermann *et al.* which, nevertheless, were to substantially influence the subsequent single electron experiment carried out by

MMP, as they themselves mentioned in their article Merli, Missiroli, and Pozzi [2003b], describing the history of their experiment.

The second comment directly concerns the experimental set-up. The use of the electron biprism here differs in certain important ways from its use in the 'traditional' thought (or non-thought) double-slit experiment. The term 'traditional' is used here to refer to the thought, or real, experiment in which electrons are directed, for example, onto a tungsten plate or a carbon film with two holes or two (or more) slits. The first important aspect is that with the biprism there are no real slits; the second is that *in the very same experiment* it is possible observe both the wavelike and the particle nature of the electrons. We are now going to examine this second point, while the first shall be discussed at greater length a little later. In the electron biprism experiment, the statement 'the electron passed through slit #1 (#2)' should be replaced by 'the electron passed to the left (right) of the wire', or, in terms of the optical model, 'the electron was emitted by virtual source S_1 (S_2)'. The interference fringes only form in the overlapping region containing electrons coming both from left and right of the wire. As we have said, eq. (1) does not envisage the formation of fringes at the observation plane OP inside the interference field W; however, it calculates perfectly the point of arrival at the OP outside of region W. The situation may be described more precisely as follows: plane OP contains a region A within which those electrons deflected by the biprism's wire arrive; within this region A there is another region, W, in which the interference pattern forms; a certain number of electrons arrive outside W all the same, and the trajectory of these electrons can be calculated using eq. (1). As we have said, region A can be enlarged onto plane VP, and using the image intensifier it can be observed on the TV screen. In fact, the film of MMP's experiment clearly shows a number of spots, caused by those electrons that have not gone into the overlapping region, next to those electrons forming the fringes. It is now routine practice in interference fringe experiments to experimentally set the width of the region in which interference takes place, and thus to leave a region in which one can think in terms of classical trajectories and utilise eq. (1). In the single electron experiment, if the electron arrives at point $x = P_1 - \epsilon$ (where ϵ is the experimental resolution limit), we may say that it passed to the left of the wire, that is, that its trajectory can be perfectly specified; if, on the other hand, the electron arrives at point $x = P_1 + \epsilon$, then the trajectory cannot be specified (if indeed it makes any sense to use the term 'trajectory' in this particular case). This technical detail is of importance in that it highlights the fact that in the same experiment, the transition from a description of events in classical terms to one in quantum terms takes place, as it were, in a continuous fashion.

A final observation I would like to make, suggested by the technical specifications of the experimental apparatus, concerns the option of observing the electron either within or outside region W *after* it has interacted with the biprism. Having established the potential of the wire, the width of region W depends on distance b, which we can choose after the electron has passed through the biprism. This means that we can choose the width of the region in which the electron reveals its wave-like nature after interaction with the biprism. This experimental variation, although it has yet to be tested, is reminiscent of the delayed choice Wheeler proposed during the 1970s in the case of a completely thought experiment [Wheeler 1978].

3 Experiment and probability

3.1 Initial considerations

The theoretical double-slit experiment has been utilised to support the non-validity of the axioms (or at least one thereof) of the probability theory in QM. Feymann's arguments are widely known, and tend to show that the axiom:

$$P\left(=\bigcup_{i=1}^{\infty} A_i\right) = \sum_{i=1}^{\infty} P(A_i) \tag{4}$$

is not valid in QM, in that it is disproved by the double-slit experiment. The meanings of the symbols are the ones normally adopted: Ω is the space of the elementary events; $\mathcal{A}$ is an σ-algebra of the sub-sets of Ω; P is the measure of the probability of $\mathcal{A}$ and $\{A_i, i = 1, 2, \ldots\}$ being a countable set of mutually exclusive events. If there are only 2 events (A e B), then eq. (4) is as follows:

$$A \cap B = \emptyset \Rightarrow P(A \cup B) = P(A) + P(B)$$

and if the events are not incompatible, this shows that:

$$A \cap B \neq \emptyset \Rightarrow P(A \cup B) = P(A) + P(B) - P(A \cap B) \tag{5}$$

We shall now follow Fine [1973], where the author provides a full account of the argumentation complete with all the implications thereof. If A is the event $\{$*the electron passed through slit #1*$\}$, while B is the event $\{$*the electron passed through slit #2*$\}$, then A and B are exhaustive and mutually exclusive. The probability $P(X)$ of event X $\{$*the electron arrives at point x on the screen*$\}$ is given by the probability that the electron passes either through slit #1 or through slit #2 and arrives at point x. This can be expressed as follows:

$$P(X) = P[(A \cup B) \cap X] \quad \text{by the distributive property} \; = P[(A \cap X) \cup (B \cap X)]$$

For eq. (5):

$$P(X) = P(A \cap X) + P(B \cap X) - P(A \cap X \cap B \cap X)$$

The last addend equals 0, given that $P(A \cap B) = 0$. By the law of compound probability:

$$P(A \cap X) = P(X|A)P(A) \quad (6)$$

and likewise for $P(B \cap X)$. It is not restrictive to presume that $P(A) = P(B)$, and thus we get the formula:

$$P(X) \propto P(X|A) + P(X|B) \quad (7)$$

which means that the probability of arrival at point x on the screen is the sum of the probabilities of the events, the one incompatible with the other, that the electron reaches x having passed through slit #1 or slit #2. What we observe experimentally are the points of arrival of the electrons on the screen, and how they are distributed. In truth, the sum of the distributions with only one of the slits open is different from the distribution with both slits open. This result contradicts conclusion eq. (7), which in turn derives from axiom (4). Therefore, the aforesaid axiom is contradicted by empirical evidence.

An initial criticism of this line of reasoning focused on the utilisation of conditioned probability. This argument was advanced by Koopman [1955], analysed in Fine [1973] and returned to by, among others, Ballentine [1986]. Basically, it points out that $P(X|A)$ with just one slit open is not the very same conditioned probability $P(X|A)$ with the other slit also open. Conditioning is performed in two different experimental situations, and thus there is no *a priori* reason why the probabilities in the two situations should be the same. Placing the emphasis on the experimental set-up leads towards the propensity interpretation of probability, to be discussed in the next section, strictly associated with MMP's experiment. Other rather tricky points are those concerning the law of total probability and the application of the distributional property. I am not going to go into these right now (for a more detailed analysis see Fine [1973]), but merely wish to focus on the meaning in the said experiment of assigning a probability to the event X $\{$*the electron arrives at point x on the screen*$\}$.

What the single electron experiment reveals, *prima facie*, is a paradigmatic example of the frequentist interpretation of probability. In fact, claiming that the probability of an electron reaching a given point x on the screen, from an *operative* point of view means counting the electrons within a radius of say $\mathrm{d}x$ around x, and relating this number to the total number of electrons that have reached the screen. This count is performed, for example, using a microdensometer to measure the blackening of a photographic film employed as a screen, in a direction perpendicular to the interference fringes, thus obtaining a curve like the one shown in Fig. 10 in Merli et al. [1976b]. MMP have the following to say about this curve (all the translations from Italian are mine):

> This curve, which is familiar to us from the study of the intensity resulting from the interference of two wave-like perturbations, in this case indicates the number n of electrons that have hit the various regions of the photographic plate. Thus, if N is the

> number of incident electrons, the curve enables us to gather what fraction of them is distributed in the various different positions. If this curve refers to a single electron, then it will show the probability the electron has of arriving at one point rather than at another.

This quote clearly shows how the authors support a frequentist interpretation of probability which is very similar (if not identical) to the one advanced by Karl Popper prior to his propensity interpretation. The probability that a single electron reaches a given point x on the screen is that which it shares with the probability distribution described by the 'collective' of electrons as a whole. I shall show later that MMP give a more extensive interpretation of their experiment.

The fact that the single electron experiments demonstrate that the interference pattern is the result of the accumulation of single events, which is no different from what happens in other contexts in the case of other distributions such as Gaussian or Poissonian distributions, would seem to lend support to what Popper wrote in his famous work [1967, p. 19 (author's italics)]: 'Now what I call the great quantum muddle consists in taking a distribution function, i.e. a statistical measure function characterizing some *sample space* (or perhaps some 'population' of events), and treating it as *a physical property of the elements of the population*. It *is* a muddle: the sample space has hardly anything to do with the elements'. The muddle thus consists in mistaking the physical properties of the elements in a statistical ensemble for the distributive properties of the same statistical ensemble. In the double-slit experiment, the muddle in question is the hypothesis according to which the observed distribution is the same as that of light intensity in light interference phenomena, in that it reflects the very nature of the entities that produce the distribution. This, in turn, means admitting the existence of a real wave (or a wave packet) of a known physical nature, that is, an electromagnetic wave, which is in some way linked to the electron. In this case, the fringe-formation mechanism may be explained if we hypothesise that the electron reveals: *a*) its corpuscular nature during emission; *b*) its wave-like nature at the interferential device; *c*) its corpuscular nature once again when it reaches the screen. However, this hypothesis cannot be applied to the experiment in that, as the authors say [Merli et al. 1976b, p. 96]:

> The fringes of interference (and of diffraction) are therefore not due to the fact that the electron is spatially distributed in a continuous manner, becoming a wave (in fact, if this had been the case we would have had fringes of decreasing intensity as the current decreased).

Indeed, this did not happen: what did occur was that as the intensity of the electron beam's current was reduced, the number of electrons reaching the screen during a given interval of time also fell.

It should be said that in MMP's experiment, the events are *independent from one another* given that only one electron at a time passes through the interferometer. In Merli et al. [1976b, p. 101], the authors specify that the electrons were spaced out 10 metres from each other on average. This also means that a given electron hit the screen when the previous electron had already been absorbed. This aspect of the experiment regarding the independence of electrons – which I should emphasise was performed for the first time by MMP – is of crucial importance for the following reasons. First and foremost, it excludes the hypothesis that the fringes can be generated as a result of the interaction of the electrons in some way inside the interferometer. It also excludes any interaction at the level of the photographic film (or other detector), as posited by the theory advanced in Suppes and Acacio de Barros [1994a, b] regarding photons.

At this point, I would like to digress a little and look briefly at the aforesaid theory, in that MMP's experiment empirically questions seriously, or better disproves (albeit, strictly speaking, not definitively), the proposal advanced in Suppes and Acacio de Barros [1994b]. These authors in this outstanding article explain photon diffraction and interference without assuming any wave-like properties of photons, but on the basis of the following hypotheses (*Ibid.*, p. 501): '(i) Photons are emitted by harmonically oscillating sources. (ii) They have definite trajectories. (iii) They have a probability of being scattered at a slit. (iv) Detectors, like sources, are periodic. (v) Photons have positive and negative states which interfere locally, i.e. they annihilate each other, when being absorbed'. MMP's experiment shows that there cannot be any kind of destructive interference with the detector, even if we admit the existence of 'virtual' states (or something of that kind) for electrons, as presumed by Suppes and Acacio de Barros, in that the electrons never meet either during their journey or upon their arrival. In particular, the above-mentioned authors write (*Ibid.*, p. 511): '[. . .] we assume that the absorber, or photodetector, itself behaves periodically with a frequency ω'. But in MMP's experiment, the 'absorber' is a well-defined macroscopic device, which may be a photographic plate or an image intensifier, totally devoid of particular periodic oscillations.

The 'source' from which the electrons depart, as we said in section 2.1, is an *image* of very small diameter produced by a lens system that gathers the electrons emitted thermionically by an incandescent pointed filament. This mechanism does not provide for any periodicity in electron emission from the filament. In any case, the fact that the probability of the simultaneous presence of two or more electrons between the source and the detector is negligible would exclude any significant form of interaction during the entire course of the experiment.

Of course, Suppes and Acacio de Barros' proposal concerns photons rather than electrons. Indeed, in the context of the double-slit experiment it was pointed out [Simonsohn and Weihreter 1979] that in interference experiments with photon beams, the similarity between photons and electrons, albeit frequently mentioned, is valid only in a restricted sense.[5] Nevertheless, the fact is that the hypotheses advanced by Suppes and Acacio de Barros cannot be applied to electrons as they have been empirically disproved by MMP's experiment. All the technical details of this experiment were performed and described in such a way as to leave no room for any ambiguity or any *ad hoc* hypotheses that cannot be experimentally tested. In this sense, MMP's experiment, being a real experiment, is more 'transparent' than certain ideal experiments. This means that similar real experiments should have been, or should be, borne in mind when new hypotheses were/are advanced on the basis of thought experiments regarding electrons or photons (in the case of the

[5] 'Photons are the result are of field quantization in quantum electrodynamics and have particle properties only in a restricted sense' [Simonsohn and Weihreter 1979, p. 203].

latter, within the limits of the analogy between photons and electrons).

Since only one electron at a time arrives, it would be fair to ask the question: when (that is, after the passage of how many electrons) do 'interference fringes' begin to appear on the screen? As we have said, the fact that the points of arrival on the screen appear periodically is only recognised after the arrival of thousands of electrons, whereas after the arrival of just a few electrons all that can be seen is a series of apparently random spots. The answer to this question is the one furnished by statistical tests, as is true of any other form of probability distribution. If the experiment is carried out with only one slit open and with 'classical' particles, such as real bullets fired at a plate with a hole, the number of bullets gathered along a horizontal strip passing through the slit can be represented by a Gaussian distribution. A goodness-of-fit test, such as the Kolmogorov-Smirnov test, could answer whether the distribution of the bullets is conformed, at a given arbitrary level of confidence, to the hypothesised distribution. The same is true of the double-slit electron experiment in which the comparison should be made between the experimental intensity of the electrons on the screen and the corresponding distribution of probability used to describe interference in optics.[6]

The problem of statistical distribution leads us to analyze the question of the *statistical stability* of the distribution. In fact, to a certain degree the MMP's experiment can be seen as a *microscopic* 'Landé's blade' (see Popper [1982, section 29] for a discussion of this imaginary device). The electrons are the billiard balls, while the biprism's wire is the blade. In the device put forward by Landé, billiard balls roll down a chute onto the edge of a blade so that about half of them are deflected to one side, while the other half fall on the other side.[7] What Landé and Popper want to show is that there can be no deterministic explanation of statistical probability. I do not wish to go into Popper's reasoning here, nor that of his supporters [Watkins 1974, 1985] or indeed his critics [Mackie 1978; Miller 1995]. The single electron experiment is clearly not designed to disprove determinism in the macroscopic world, but rather to re-propose the basic question of how it is that stable frequencies can result from antecedent, totally nondeterministic conditions. Before asking why an *interference pattern* is in fact formed, which is clearly the fundamental problem at the very heart of QM, it would also make sense to ask why the same probability distribution is seen on the screen when experimental conditions (biprism wire diameter, potential applied, energy of the electrons, etc.) remain the same. There may well be small imperfections in the system of lenses, minor acceleration voltage fluctuations, or other similar features, but these do not affect the end results. In fact, the electrons are emitted one at a time in a completely nondeterministic manner; their trajectory within the overlapping region is clearly nondeterministic; and they reach the screen in an unpredictable, yet statistically stable, manner. The context in question is a microscopic one, this is important since it excludes any appeal to epistemic probability and/or to underlaying deterministic chaos. The latter case is pointed out by Miller [1995], according to whom

[6] See, e.g.,Vermillion [1994] for a *simulated* two-slit electron interference experiment, in which the build-up of a pattern of fringes is obtained by sampling from a given probability distribution.

[7] As Miller [1995, p. 140] points out, however, the important thing is not that the balls are equally distributed on each side, but that that the device does not cause all the balls to fall to one side only.

the billiard balls may be ‘piloted’ by the logistic map in the chaotic regime. For this author, to consider the differences between the macroscopic and the microscopic Landé’s blade appears to be relevant [Miller 1995, p. 142]:

> To be sure, in the case of macroscopic chute and blade, as in the tossing of a die or a coin on to a soft surface, I accept, as everyone else does, that there are usually small asymmetries present in the apparatus (and, indeed, that these can with care be eliminated). But I do not think that this is obviously so in the case, say, of incidence of photons at a half-silvered mirror.

However, there is a difference between the above-mentioned experiment with a photon beam and the experiment with single electrons. In the former, what is seen is a sequence of events taken as a whole which is then sub-divided into two further sequences. Thus there is an initial sequence of events followed by two final sub-sequences obtained by means of a given rule of selection. Probability in this case is defined on the basis of the observed frequencies, that is, of intensity. Thus 50% of the photons are said to be reflected, and 50% transmitted, if the measured intensities of the two beams are the same. Probability therefore is relative to the sequences. In MMP’s experiment, the observed system is the single electron, and the sequences are the products of single events. Thus probability has to be assigned to a single event. The single electron experiment therefore leads, in a natural manner as it were, to the search for an explanation of statistical stability in terms of propensity. It is known that in Popper, the double-slit experiment constitutes the basis of his propensity theory, an argument which we shall be looking at in the following section; the above is simply designed to draw the reader’s attention towards the fact that philosophical analyses of questions such as the problem of determinism/nondeterminism and statistical stability may benefit from the empirical results of a real experiment actually conducted more than thirty years ago.

3.2 From statistical to propensity interpretation

The propensity interpretation of QM originates from the ‘statistical interpretation’, but the two are characterised by substantial differences. I am now going to briefly sketch the question, focusing as I do on those points we shall be returning to in the light of the MMP’s experiment.

It is generally acknowledged that since the beginning of QM, the ‘statistical interpretation’ (or ‘ensemble interpretation’) has contended with what is generally known as the orthodox or Copenhagen interpretation, in an attempt to provide an interpretation in real physical meaning of the formalism of the theory [see, e.g., Jammer 1974, pp. 115–120]. The statistical interpretation was founded on the tenet that QM deals essentially with statistical ensembles, i.e., the state vector represents an ensemble of similarly prepared systems, each of which has precise values for all dynamical observables; the orthodox interpretation, on the other hand, states that the state vector provides a complete description of an individual system. The two interpretations gave rise to conflicting ways of looking at other concepts underlying all quantum theory: wave-particle duality, the ‘collapse’ of the wave function; uncertainty relations, etc. Historically speaking, these contrasting ways of viewing QM left their mark on the most interesting 20th-century debate in physics, i.e. the famous dispute between Bohr and Einstein regarding the real physical meaning of theory. The article by L. E. Ballantine [1970] can be considered the *manifesto* of the statistical interpretation: in this article, the author brings together and summarises the main hypotheses underlying the said interpretation, in which the paradoxes, including the EPR argument and Bell's extension thereof, cease to exist as such. The uncertainty relations are relations of statistical dispersion in that they express the lower limit of statistical variability in the experimental preparation of a collection of statistically reproducible systems (*state preparation*). Thus the 'reduction of the state vector' is not a physical process, but simple derives from the theory of probability, in the sense that one moves from the conditioned probability $P(A|B)$ to the probability $P(A|A)$, of course representing the given event equal to 1. This is like saying that if I toss a coin, when the coin is still airborne the states of 'heads' and 'tails' are superimposed, whereas observing the result, the coin 'collapses' into one of the two states.

The most fundamental aspect distinguishing the two interpretations is the ontological state of the objects dealt with by QM. In the statistical interpretation, the wave function is not a physical entity, but simply a mathematical device for calculating probability. The wave-like pictures are epiphenomena, produced by the individual impact of *particles*. The single electron experiments – not only MMP's experiment but also the one performed by Hermann et al. [1971] and all the other experiments conducted by various physicists thereafter (see, for example, Matteucci [1990]) – would seem to support this theory, at least at a first glance. In fact, what is observed – an image that gradually appears on the screen – is produced by the individual collisions of single electrons, and when there are a sufficient number of them, their probability distribution is the same as that used to depict the trend in light intensity in the corresponding wave phenomena. However, it was the double-slit single electron experiment that called for the assignment of probability to the 'single case'. Moreover, a physical explanation remained to be given for the particle scattering mechanism that gave rise to those images observed experimentally. The problem of the single case was investigated by Popper at various times from 1957 [Popper 1957] up to his last works [Popper 1990]. Popper's answer to the said problem is known as the propensity interpretation of probability. Popper put forward his interpretation of probability in terms of propensity to the Ninth Symposium of the *Colston Research Society* [1957]; since then, it has been the subject of much philosophical debate, and even very recently interest in this question among philosophers of science appeared not to have abated. For example, numbers 1 and 2 of the journal *Synthése*, 2002, were dedicated to 'Propensities and Probabilities'.

The idea of propensity derived from the double-slit experiment, as explicitly admitted by Popper himself [1959, p. 28]:

> [The interpretation of the two-slits experiment] convinced me that probabilities must be 'physically real' – that they must be physical propensities, abstract relational properties of the physical situation, like Newtonian forces, and 'real' not only in the sense that they could influence the experimental results, but also in the sense that

they could, under certain circumstances (coherence) interfere, i.e., interact, with one another

Popper's scheme of things contains diverse ideas, some of them conflicting, that have given rise to various different lines of philosophical research. Certain problems appear repeatedly in the literature and are sometimes the subject of diametrically opposed views: see, among others, Gillies [2000] and chapter 5 of the book by Maria Carla Galavotti [2005]. I am only going to investigate a few aspects of Popper's proposal here; those aspects which are of importance to the question of the MMP's experiment in the light of the propensity interpretation of probability. The proposal constituting the cornerstone of Popper's entire programme is not simply that of formulating a reinterpretation of the calculation of probability, but is something deeply rooted in the lengthy investigation into quantum theories carried out by the philosopher, that had already coloured several pages of his *Logik der Forschung* [1934]. In Popper's work, the *interpretation* of probability in terms of propensity is, first and foremost, the *theory* of propensity, based on certain specific empirical hypotheses, subject to experimental falsification. Popper does not start from a reworking of probabilities, designed to remove the flaw present in his version of the frequentist interpretation of probability developed in the *Logik der Forschung* [1934]. He clearly states that he decided to reconsider the problem of the individual case after having formulated the idea of propensities as physical properties [1959, p. 27]: 'It was only after I had developed, and tried out, the idea that probabilities are physical propensities, comparable to Newtonian forces, that I discovered the flaw in my treatment of the probability of singular events'. Exactly what it is that possesses these physical properties has been the subject of debate and the focal point of various schools of thought. In Popper's view, propensity is a dispositional property of the entire *experimental arrangement*, that is, of the combination of the physical system as such (the dice, the electron) with those experimental conditions that determine the behaviour of the system (the launching device, the double-slit apparatus), including all important related conditions with which the system interacts. In the views of others, such as Mellor [1971], propensities can only be attributed to the physical system as such, that is, to the dice or the electron. This perfectly mirrors those dispositional properties, which we may call 'deterministic', such as fragility, solubility, etc. Solubility, for example, is a property of a sugar lump, and not of the sugar lump and the basin of water into which it is plunged. However, propensity cannot be observed from one single result, as would be true in the case of an iron nut that dissolves when immersed in sulphuric acid. Propensity manifests itself in the statistical distribution of all possible results, in the distribution of the face of the dice bearing the number 6, if the dice is thrown properly [Mellor 1971, p. 70]. In fact, if we look at all the various proposals that have been submitted regarding the question of propensity, the relationship between the *meaning* of the term 'propensity' and the *testability* of statements including this term, remains highly debateable. Basically speaking, this problem comes down to the need to resolve the connection between the meaning that may be attributed to the probability of a single event, and the relative frequencies with which the value of the probability of that event is controlled. On the one hand, propensities are seen as real physical properties that are experimentally measurable, while on the other hand the only measurements that can be made remain those of the counting of relative frequencies.

Despite the absence of any explicit reference to the above-mentioned philosophical problems, the article by Merli et al. [1976b] clearly reveals the tension between the need to assign to an individual electron the probability it has of reaching a given point on the photographic plate, and the inevitable need to acknowledge the interference fringes as a statistical distribution of relative frequencies. Moreover, in the same article the authors underline how the effect of interference is to be perceived as the product of the interaction of the single electron within the experimental arrangement, that is, of the 'generating conditions' underlying the intensity distribution. In fact, MMP state that [1976b, p. 94]: 'the electron is a particle that reaches a clearly identifiable point on the screen, exposing a single grain of the photographic emulsion, and *the interference pattern is the statistical result of a large number of electrons*'. Further down the same page, one reads: 'Thus we may conclude that *the phenomenon of interference is exclusively the consequence of the interaction of the individual electron with the experimental apparatus*' (the italics are mine). How does the electron interact with the experimental apparatus however? Before looking at the answer to this question, I would like to take a closer look at Popper's analysis of the double-slit experiment contained in his theory of propensity.

Popper's idea consists in correlating quantum theory's mathematical entity 'wave function' to the physical property 'propensity'. The wave function, in fact, is claimed to determine the propensity of the states of the individual micro-object. Popper himself writes that [1957, p. 68]: 'In our interpretation the Schrödinger *psi*-function determines the propensities of the state of the electron. We therefore have no 'dualism' of particles and waves. The electron is a particle, but its wave theory is a propensity theory which attributes weights to the electron's possible states'. The two-slit experiment – writes Popper [1959, p. 38] – '[it] may be said to be something like a crucial experiment between the purely statistical [i.e. frequency] and the propensity interpretation of probability, and to decide the issue against the purely statistical interpretation'. Can MMP's experiment be considered a crucial experiment in favour of the theory of propensity? It is certainly true that by varying the potential applied to the biprism's wire, the number of fringes produced per unit of length varies; however, it is obvious, with or without propensity, that a change in the experimental conditions will produce a change in the probability that certain results are given. Popper proposes an analogy with a ball on a pin board. Once again, in fact, we may see this in terms of the propensity of a single ball to reach a certain point of the lower edge, the value of which changes of course if we modify the construction of the device (if the plane is inclined, if the position and/or number of pins is changed, and so forth). Likewise in the double-slit experiment [Popper 1957, p. 69]: 'If we shut one slit, we interfere with the possibilities, and therefore get a different *psi*-function, and a different probability distribution of the possible results'. However, Popper is perfectly aware that the analogy with the mechanical pin-board model fails to constitute a complete interpretation of a QM experiment in that the 'wave-like' effects are missing. In fact, in the case of the pin board [Popper 1967, p. 34], '[t]here will no interference of amplitudes: if we have two slits Δq_1 and Δq_2, the two probabilities themselves (rather than their amplitudes) are to be added and normalised; *we cannot imitate the two slit experiment.*' (the italics are mine). In order to understand the effects of interference, we need to resolve the physical problem of the interaction between the electron and the experimental arrangement. Popper in [1967] returns to Landé's explanation, based in turn on the mechanical theory of particle diffraction developed by Duane [1923] in the early 1920s. This author attempted to explain X-ray diffraction in crystals by introducing a 'third selection rule' for linear momentum (the other two rules are those for energy $\Delta E = h\nu$ and for the angular momentum $\delta p_\phi = h\phi$ with $\phi = 2\pi$, following Planck and Sommerfeld-Wilson, respectively), according to which a body periodic in space along a certain direction (e.g., a crystal) is entitled to change its momentum component in this direction in amount $\Delta p = h/l$, where l is the length of the period (e.g., the interplanar distances). By taking into account the law of momentum conservation, Landé was able to use Duane's rule to derive the same values of Bragg's formula for wave interference [1965, pp. 124–125]. 'The incident particles – comments Landé [1965, p. 124] – do not have to spread like waves [...]; they stay particles all the time. It is the crystal with its periodic lattice planes which is already spread out in space and as such reacts under the third quantum rule'.

The same kind of reasoning is extended by Landé to an ideal double-slit apparatus, resulting in the following conclusion: the slit screen reacts as a mechanical unit, a 'whole solid body', in such a manner that it transfers a quantized momentum to incident electrons. The result of this collective action of the screen consists in bringing about interference-pattern behaviour for each single electron [1965, p. 125]. According to Landé's model, therefore, the spread mode of action in interference experiments is due to a spread quality inherent not in electrons, but in the 'quantum-mechanical activity of the diffractor', which may be a crystal as well a screen with two holes [1966, p. 1160].

I have dwelt on this Duane-Landé theory because it was adopted both by Popper, within the framework of his theory of propensity, and by Ballentine [1970] in that necessary for the statistical interpretation of QM. The Duane-Landé theory, however, is not capable of explaining the results of the MMP's experiment. In this experiment, the interference image is obtained with no mechanical transfer of momentum to or from the double-slit apparatus. In fact, the description of the experimental apparatus reveals that the 'slits' are merely virtual, and there is nothing mechanical about the formation of the interference fringes. In a previous article to the one written by Merli et al. [1976a], written by the Bologna group regarding an interference experiment using an electron biprism (but this time not with single electrons), the authors [Donati, Missiroli, and Pozzi 1973, p. 639] write as follows (the italics are mine):

> In interference experiments it is not necessary to introduce the concepts of interaction between electrons and atoms, regular distribution of atoms in crystalline lattice, their dimensions, etc., as for diffraction experiments, but *the splitting and superposition of the electron beam is achieved by macroscopic fields without any interaction of the electron with the material*.

With regard to the aforesaid arguments, another historically important aspect emerges from the MMP's experiment. This experiment demonstrates, in fact, that certain hypotheses of crucial importance for the statistical interpretation of QM could be empirically tested. At that time in history, it was commonly believed, e.g., Jammer [1974, p. 44], that it was impossible to perceive any difference at the experimental level between the Copenaghen interpretation and the statistical interpretation; thus the choice between the two became merely a matter of personal taste, for example, in the name of Occam's razor [Ballentine 1970, p. 378]. The result of the MMP's experiment seem at first sight to support the statistical interpretation; however, the description of the real details of the experimental apparatus proved that the opposite was in fact true, that is, that the said interpretation fails to explain wave-particle duality, since it invariably has to resort to a model based on a mechanical momentum transfer. MMP's experiment, as we have said, failed to engender any rethinking on the part of supporters of the statistical interpretation, and perhaps to the problem remains unresolved this very day. Some authors [Gibbins 1987, p. 79] agree with me that the diffraction or interference phenomena produced by lasers or electron microscopes cannot be explained on the basis of both the statistical and the propensity interpretation, as far as both interpretations lean on the Landé's theory. In particular, Krips [1984] points out the inadequacy of the Landé's theory to account for interference effects in the Popper's view of QM.[8]

On the contrary, in the excellent book by Ballentine [1999], which makes explicit reference to the single electron experiment (that conducted by the Hitachi Group), the author advances two analyses in order to explain electrons' 'wave-like' phenomena: that within wave-particle duality, and that based once again on the 'quantized momentum transfer to and from a periodic object' [1999, p. 136]. As in 1970, Ballantine continues to believe that the latter would be preferable in the name of Occam's razor, since it makes no appeal to any hypotheses concerning the wave-like nature of the electron.

4 Conclusions

There have been numerous criticisms made of Popper's propensity theory, starting with the initial criticism submitted by Feyerabend at the very same *Colston Research Society* Symposium [Feyerabend 1957, note 8, p. 122]. In particular, Milne [1985] believed that the results of the (thought) double-slit experiment were incompatible with Popper's claim that propensities are real relational properties of whole experimental arrangements which are capable of interfering with one another.[9] The propensity theory, however, is not necessarily bound to the interpretation of QM given by Popper, and in any case it has been the subject of conceptual elaborations that are somewhat removed from the spirit with which Popper originally proposed the said theory. One path taken by some, such as Mellor [1971], consists in attributing propensity to a single physical system. Hence in Maxwell [1988], propensity is perceived as a dispositional property belonging exclusively to the

[8] '[. . .] the problem for Popper is that Landé's theory has by no means proved successful in other areas of QT [quantum theory]. For example it has not successfully explained interference effects between differing spin states or polarization states. Landé's theory is as yet too incomplete to constitute a serious alternative to its non-realistic orthodox competitor' [Krips 1984, p. 269].

[9] At the end of the article, Milne acknowledges that his argument is substantially the same as that offered by Popper in the third volume of his 'Postscript to The Logic of Scientific Discovery', that is, in the reprint of the article Popper [1967] quoted above.

microscopic world; or in Shimony [1999] where propensity – or more precisely 'quantum propensity' as opposed to 'classical propensity', attributed to quantum systems (as opposed to classical systems) – is related to Heisenberg's notion of *potentia*; or more recently in Suárez [2004], where propensity is seen as 'quantum disposition', and as such provides the basis for a solution to the measurement problem. For this author, propensities are properties of the micro-objects, properties revealed by means of interactions with suitable experimental arrangements. 'Each dispositional property – writes Mauricio Suárez (*Ibid.*, p. 244, author's italics) – is displayed under the right test conditions as a chance distribution [. . .]. Hence, these properties are *propensities* in the sense of Mellor [1971]'.[10] Very recently the same author [Suárez 2007], in an article with a very eloquent title, reviews different attempts to interpret QM by employing explicitly the notion of propensity.

MMP's experiment may also be seen from similar perspectives. In trying to follow the experiment as closely as possible, I have not extended probabilistic considerations to the macroscopic context (e.g., coin-tossing or dice-rolling), but have simply tried to illustrate and analyse what the experiment itself demonstrates. The question I put above 'Can MMP's experiment be considered a crucial experiment in favour of the theory of propensity?' has not a clear-cut answer. Clearly the experiment did not show that the interference pattern in the double-slit experiment is the result of the interference of the two propensities each associated with one single-slit experiment.[11] But we have seen there were other features which deserved attention. The crucially important aspect of the MMP's experiment basically consists in its having shown the empirical meaning of the probability of a single event within the experimental context of QM. In physics experiments conducted in the microscopic world, statistical distribution as such is the subject matter of study when, for example, we check to see whether it conforms or otherwise to theoretical expectations. So that the frequencies by themselves are seen as the sole constituent of the probability. In the single electron experiment, the situation is, as it were, turned on its head. The focus is now on the individual particle in the sense that there are empirical grounds for enquiring as to what probability there is that a single electron will reach a certain point on the screen even after the arrival of just one such particle and after having switched off the microscope. The result thereof leads us to regard probability as a physical property that is revealed in the single electron case. The experiment finally excludes that the interference fringes may be due to a real (electromagnetic) wave (or wave packet) associated in some way to the electron, or to the interaction between one electron and another, or to any specific characteristics of the electron source, or even to a reciprocal mechanical transfer of an impulse between the electron and the slit screen. The remaining explanation is that the very nature of the electron determines the frequency of

[10]This point of view about the 'quantum-mechanical propensities' is shared also by Redhead [1995, p. 172].

[11]The single-slit experiment was actually performed by Matteucci and Pozzi [1978] with the same interference electron microscope used for the double-slit experiment. In the single-slit experiment, all the electrons passing in one side of the biprism wire were intercepted by means of an aperture below the biprism, so that they were prevented from reaching the observation plane.

the impact points on the screen, as shown in the interference pattern. This is tantamount to admitting that the electron displays a physical probability (propensity) as a consequence of its interaction with the experimental apparatus.

Gillies [2000] shows his scepticism about the role (if any) played by single-case propensities in the natural sciences with the words:

> If, as Fetzer [i.e., Fetzer [1982]] suggests, we ascribe propensities to a complete set of (nomically and/or causally) relevant conditions, then in order to test a conjectured propensity value we must make a conjecture about the complete list of the conditions that are relevant. This necessary conjecture might often be difficult to formulate and hard to test, thereby rendering the corresponding propensities methaphysical rather than scientific. Once again, then, I have a doubt as to whether single-case propensities give an appropriate analysis of the objective probabilities that appear in the natural sciences.

It is clear from the above quotation that in Fetzer's opinion, put forward in Fetzer [1974], propensities are dispositions possessed by the experimental apparatus, rather than properties of individual systems. In any case, the worries expressed by Gillies are perfectly justifiable as far as propensities are used in scientific context. However, such concerns are certainly pertinent regarding to games of chance, in which actual relevant conditions may elude the experimenter, for instance an underlying deterministic chaotic mechanism, while in the MMP's experiment we have the complete knowledge of the whole electro-optical apparatus, specified in all its details, so that all the factors which are relevant for the end results are well identifiable and under tight control.

I am fully aware that in nearly all the articles that have been written regarding single-case probability, reference is made to the single atom emitted by an electron, or to the single photon that passes or otherwise through a semi-reflective screen, and so on. Therefore, mere reference to a single electron reaching an image intensifier does not appear to add anything new to the numerous pages already written with regard to this matter. However, the difference lies in the fact that the latter case has been the subject of a real experiment which, in virtue of the complete identification and control of all experimental details, has led to the exclusion of various alternative hypotheses and has rendered the corresponding propensities scientific rather than metaphysical. In this sense, I believe that MMP's experiment deserves recognition not only as 'the most beautiful experiment in physics', but also, thanks to its empirical relevance, in that it may be particularly significant in the philosophical debate on propensity.

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