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Published in:
Ernest Nagel

DOI:
10.1007/978-3-030-81010-8_7

Publication date:
2022

Chapter 7
Putting the Cart Before the Horse: Ernest Nagel and the Uncertainty Principle

David Atkinson and Jeanne Peijnenburg

Abstract In *The Structure of Science*, Ernest Nagel finds fault with Werner Heisenberg's explication of the uncertainty principle. Nagel's complaint is that this principle does not follow from the impossibility of measuring with precision both the position and the momentum of a particle, as Heisenberg intimates, rather it is the other way around. Recent developments in theoretical physics have shown that Nagel's argument is more substantial than he could have envisaged. In particular it has become clear that there are in fact two uncertainty principles; as a result, there are four pairs of quantities to examine, whereas Heisenberg considers only one. These findings throw new light on Nagel's criticism. They enable us to see that his intuition was surprisingly apposite, but also make clear where his argument misses the mark.

Keywords Ernest Nagel · Quantum mechanics · Uncertainty principle

7.1 Introduction

Generally the act of observing macroscopic quantities does not alter those quantities appreciably. One can observe for example the position of a ballistic missile at two successive times, t_1 and t_2, and infer what the momentum of the missile was in the interval between t_1 and t_2. No appreciable alteration of the trajectory of the weapon is effected simply by looking at it.

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M. Neuber and A. T. Tuboly (eds.), *Ernest Nagel: Philosophy of Science and the Fight for Clarity*, Logic, Epistemology, and the Unity of Science 53,
https://doi.org/10.1007/978-3-030-81010-8_7

As is well known, the matter is different in the submicroscopic domain of the fundamental particles. The position of an electron, for example, can be observed by letting a particle of light, a photon, collide with the electron. The photon will be absorbed and a new one will be emitted, the detection of which enables us to determine the electron's position with fair precision. However, our knowledge of its momentum will be poor, for the interaction of the photons with the electron causes an alteration in the electron's momentum which cannot be made negligible, because of the discrete nature of light. Thus it seems impossible to determine both position and momentum at a particular moment of time with great precision.

The above comprises the celebrated uncertainty relation; since Werner Heisenberg published it in 1927, it has become the centerpiece of the orthodox (or Copenhagen) interpretation of quantum mechanics. It is therefore interesting that at a relatively early stage philosophers of science have expressed criticism of the uncertainty principle. In this paper we concentrate on the criticism voiced by Ernest Nagel, who in Chap. 10 of *The Structure of Science* launches a lengthy critique of Heisenberg's discovery. Taking recent developments in physics as our point of departure, we argue that Nagel's criticism is more substantial than he could have envisaged. However, as we shall explain, the new results also allow us to see where exactly Nagel's argument falters.

We start in Sect. 7.2 by summarizing Nagel's criticism of the way in which Heisenberg presents his uncertainty principle. In Sects. 7.3, 7.4 we explain recent findings in theoretical physics. We first make clear that there are in fact two different uncertainty principles, both dating from 1927, which have incorrectly been put on a par (Sect. 7.3). Then we show that the original uncertainty principle of Heisenberg has questionable validity, and we describe the introduction in 2003 of a corrected version, which has a stronger claim to be valid (Sect. 7.4). In Sect. 7.5 we explain the relevance of these results for Nagel's criticism: they reveal the extent to which his criticism is correct or misses the mark.

7.2 Nagel's Criticism of Heisenberg

In Chap. 10 of *The Structure of Science*, Ernest Nagel discusses the question of the precise sense in which classical physics is deterministic while quantum mechanics is not. A theory is deterministic, Nagel writes, if "analysis of its internal structure shows that the theoretical state of a system at one instant logically determines a unique state of that system for any other instant" (1961, p. 285). The word "theoretical" is important here, for Nagel makes it clear that he is not addressing errors in empirical measurements or the consequences of these errors in actual practice.

Newtonian mechanics is clearly deterministic in this sense.[1] Moreover, Nagel notes that under this explication even quantum mechanics can be said to have a deterministic structure. In quantum mechanics the theoretical state of a system is described by a wave function; if the wave function is given at one time, then the Schrödinger equation of motion specifies the wave function at another time, much as Newton's equations of motion in classical physics tell us how the positions and momenta of bodies change as time passes.

Why then is quantum mechanics so often put forward as a standard example of a theory that is indeterministic? According to Nagel, this is because of Heisenberg's uncertainty principle:

> [...] the usual ground for regarding quantum mechanics as an "indeterministic" theory is the set of formulas logically derivable from the assumptions of the theory and known as the "Heisenberg uncertainty relations" (Nagel, 1961, p. 294).

Nagel himself disagrees with this view:

> Quantum mechanics cannot be validly characterized as indeterministic merely on the grounds that the uncertainty relations exclude the possibility of precise values for the simultaneous "positions" and "momenta" [...] (Nagel, 1961, p. 305).

Nagel is of course right to reject the idea that the uncertainty principle is the source of indeterminism in quantum mechanics, for it is mathematically impossible to infer the latter from the former.

He recalls that the core of the uncertainty principle is the impossibility to obtain simultaneously precise values for position and momentum; and the reason for this impossibility is that the product of these uncertainties can never be smaller than a bound given by Planck's constant:

> One of the [Heisenberg uncertainty relations] is expressed by the formula $\Delta_p \Delta_q \geq h/4\pi$. In this formula the variables 'p' and 'q' are commonly read as the instantaneous coordinates of the "momentum" and "position", respectively, of an electron or other subatomic element, and 'h' as the universal Planck's constant. On the other hand, 'Δ_p' is interpreted as the coefficient of dispersion (or deviation, sometimes also called the 'uncertainty') from the mean value of the momentum at a given instant; and similarly for 'Δ_q'. The formula therefore asserts that at any given instant the product of the dispersions of momentum and position, respectively, of a subatomic "particle" is never less than $h/4\pi$ (Nagel, 1961, p. 294).

Nagel then notes that the uncertainty principle has been understood in the following way:

> A widely held [...] interpretation of the uncertainty relations is that they formulate the relatively large but inherently unpredictable variations in certain features of subatomic particles and processes, produced by the interaction of the latter with the instruments used in measuring those features (Nagel, 1961, p. 295).

[1] Of course the domain has to be specified, e.g. certain singular situations have to be excluded, but this is to be understood.

He stresses that even Heisenberg interpreted his principle in that manner:

> Heisenberg declared that when measuring large-scale objects, the effects generated in those objects by the processes of measuring them can be neglected, since the magnitudes of the disturbances thus produced are relatively small. In subatomic physics, on the other hand,
>
>> "the interaction between observer and object causes uncontrollable and large changes in the system being observed [...] every experiment performed to determine some numerical quantity renders the knowledge of others illusory, since the uncontrollable perturbations of the observed system alters the values of previously determined quantities" (Nagel, 1961, p. 295; Heisenberg, 1930, p. 3).

So according to this interpretation, supported by Heisenberg himself, the interaction between the observer and the observed leads to unpredictable and uncontrollable variations in subatomic particles and processes, which in turn imply the uncertainty principle:

$$\text{uncontrollability} \quad \rightarrow \quad \text{uncertainty relations.}$$

Despite its defense by a "high authority", Nagel (1961, p. 297) finds this interpretation fundamentally mistaken. For it places the cart before the horse:

> [It is] as if the uncertainty relations were the conclusions of a purely factual examination [...] for testing quantum theory. In point of fact [...] such a contention puts the cart before the horse. For the 'uncontrollable' [...] alterations that the electrons are said to suffer [...] do not constitute the *evidence* for the uncertainty relations, but are part of the *consequences* drawn from the relations (Nagel, 1961, p. 297, original emphases).[2]

Nagel's point is that one does not simply observe uncontrollable alterations to the coordinates of the electrons and then infer from this that we are apparently dealing with uncertainty relations. Rather it is the other way around, the uncertainty relations are the reasons for the uncontrollability:

> In consequence, it is hardly cogent to argue that the simultaneous positions and momenta of electrons cannot be ascertained with unlimited precision, on the ground that perturbations are produced in electrons when they are measured. In short, the impossibility of such unlimited precision follows from the uncertainty relations and not, as is sometimes maintained, simply from the familiar experimental facts (Nagel, 1961, p. 298).

That is,

$$\text{uncertainty relations} \quad \rightarrow \quad \text{uncontrollability.}$$

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[2] It is important not to confuse Nagel's criticism with that of Niels Bohr. In 1927, both at the Volta conference in Como, Italy, and at the Solvay conference in Brussels, Bohr objected to Heisenberg's claim that the uncertainty relations follow from the gamma ray thought experiment; according to Bohr, that thought experiment is defective (see Sect. 7.3). Bohr was right, but the objection is not the same as Nagel's, which is that the uncertainty relations do not follow from uncontrollability.

The fact that Nagel criticizes Heisenberg's explication is somewhat remarkable. True, Nagel had a Master degree in mathematics and he wrote his Ph.D. thesis on the concept of measurement; but it is nevertheless impressive that he, being first and foremost a philosopher, mastered quantum mechanics sufficiently to be able to make a trenchant criticism of Heisenberg's achievements in physics. One might argue that Nagel was not reprimanding Heisenberg on a strictly physical matter, but was merely making a philosophical point about the theory ladenness of observation. A recurring theme in Nagel's work is that reports of what are commonly regarded as simple experimental observations often are couched in the language of some theory. This goes for quantum mechanics, but also for classical physics. For example, Nagel would have said that the statement with which we started this paper, namely that observing macroscopic quantities does not alter them, is actually a theoretical assumption, not the report of a straightforward empirical fact. Indeed, as Nagel writes:

> [...] we remind ourselves that perturbations produced by measuring instruments in the objects measured were fully recognized in classical physics. In classical physics, however, the extent of such perturbations can in principle be precisely evaluated with the help of established physical laws, so the mere fact of such perturbations does not lead to the uncertainty relations. (Nagel, 1961, p. 297)

Although that may well be the case, there is more to be said about Nagel's criticism of Heisenberg. For recent developments in theoretical physics have shown that this criticism is actually more substantial than Nagel himself could have envisaged. As we shall indicate in Sect. 7.3, it has been found that there are in fact two uncertainty principles; and, as we explain in Sect. 7.4, there are four pairs of quantities to consider, whereas Heisenberg considers only one. In Sect. 7.5 we discuss the relevance of these new findings for Nagel's argument. We shall see that his basic intuition was apposite, but also that he unknowingly went back and forth between what were later identified as two different versions of the uncertainty principle.

7.3 Two Uncertainty Principles

As has been explained carefully by Jan Hilgevoord and Jos Uffink (2001/2016; 1990 and Uffink & Hilgevoord, 1985), there are actually two versions of what is called the 'uncertainty principle' or the 'uncertainty relation'. The first was introduced by W. Heisenberg in his paper of March 1927 in the *Zeitschrift für Physik*, the second was described by E.H. Kennard four months later in the same journal (Heisenberg, 1927; Kennard, 1927). The Heisenberg version basically concerns a single pair of measurements, one of the momentum and the other of the position of a particular particle. The Kennard version, on the other hand, refers to an ensemble of particles, in which the momenta and positions display a spread about their respective average values—it does not concern measurements at all. Heisenberg's formula can be expressed as

$$\delta_p \delta_q \sim h,$$

where δ indicates some measure of accuracy ('Genauigkeit') or error ('Fehler') which however remains completely unspecified; the formula as a whole says that the product of δ_p and δ_q is roughly equal to Planck's constant.[3] Kennard's uncertainty principle is

$$\Delta_p \Delta_q \geq h/4\pi$$

where Δ stands for the usual standard deviation in statistics, and where the entire formula says that Δ_p times Δ_q cannot be less than Planck's constant, h, divided by 4π.

Before Hilgevoord and Uffink published their finding, no clear distinction between the two versions was made, and when people talked about the 'Heisenberg uncertainty principle' they were usually referring to the Kennard version. Nagel is no exception in this respect; however, as we will make clear in Sect. 7.5, his case is a bit more complicated, since he appears to oscillate between the two versions.

Heisenberg's principle is often called 'the observer effect', but we shall follow Hilgevoord and Uffink (2001/2016, p. 42) in adopting a more perspicacious name, 'the measurement uncertainty principle'. Heisenberg explains it on the basis of a thought experiment. After having recalled that one is able to obtain information about the positions and momenta of a single electron by detecting a photon scattered from the electron in question, he notes that a photon of visible light would be too crude to do the job very well, because the wavelength of visible light is much larger than an electron. To localize an electron accurately in this manner one needs much smaller wavelengths, of the order of the size of the electron itself. That is why Heisenberg described a thought experiment in which a 'gamma ray microscope' was used. A gamma ray is a very hard X-ray, and it would in principle allow one to pin down the position of the electron with only a very small uncertainty.

However, the difficulty is that the shorter the wavelength the higher the energy of the photon: that is part and parcel of the quantum nature of light. So one can determine the position of an electron with high accuracy at the expense of giving it a massive recoil from the energetic photon that is being used to observe it. This changes the momentum of the electron, making it uncertain what that momentum was before the collision. Heisenberg argued that there is a trade-off between the accuracy of a position measurement, and the ineluctable disturbance that this measurement brings about. This leads to a lower bound on the product of the uncertainties in the position and the momentum of the electron. This lower bound is of the order of Planck's constant of action and gives us $\delta_p \delta_q \sim h$.

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[3] The actual formula in Heisenberg's seminal paper is $p_1 q_1 \sim h$, where p_1 is "die Genauigkeit, mit der der Wert p bestimmbar ist" and q_1 is "etwa der mittlere Fehler von q" (Heisenberg, 1927, p. 175).

At two conferences in 1927, Niels Bohr famously explained that the trade-off which Heisenberg assumes does not exist (see footnote 2). He questioned the soundness of the gamma ray thought experiment on the grounds that one needs a short wavelength gamma ray to determine the position precisely, whereas to determine the momentum precisely one must use a large wavelength gamma ray. The change in the momentum, as a result of the position measurement, can therefore never be precisely determined *in the same experiment*.[4]

Earle Hesse Kennard, a theoretical physicist from Cornell University, discovered what we have called the second version of the uncertainty principle. Kennard spent a sabbatical in 1926–1927 at the University of Göttingen, from which Heisenberg had just departed.[5] Hilgevoord and Uffink (2001/2016, p. 41) refer to his discovery as the 'preparation uncertainty principle'. Instead of a single electron, Kennard considered an ensemble of electrons, prepared in some way that assured that they all had very approximately the same position and momentum. He showed that the statistical spreading of the position, multiplied by that of the momentum, must be greater than or equal to Planck's constant, h, divided by 4π:

$$\Delta q \Delta p \geq \frac{h}{4\pi}.$$

As noted before, the measure of statistical spreading, Δ, is the familiar standard deviation of statistics, to be distinguished from the unspecified and rather vague δ of Heisenberg. Planck's constant arises because of the following fundamental relation of quantum mechanics that requires the commutator of two conjugate quantities like q and p to be determined by this constant:

$$[q, p] = qp - pq = \frac{ih}{2\pi}.$$

The details of Kennard's calculation are given in Appendix A. All this is quite different from the measurement uncertainty principle which Heisenberg considered in his 1927 paper.

Yet Kennard takes his own version of the relation to be only a generalization of Heisenberg's uncertainty principle. Referring to the inequality concerning the product of the standard deviations, i.e. his own preparation uncertainty principle, he writes:

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[4] Camilleri (2009, p. 93) has argued that for Heisenberg the matter is actually not epistemic but semantic: an accurate measurement of the position of an electron renders the concept of its momentum meaningless, rather than meaningful but unknowable. This distinction is interesting, but for our argument not relevant.

[5] See Kennard (1927). Heisenberg was formally *Privatdozent* in Göttingen from 1924 to 1927; but from May 1926 to the end of 1927 he was actually employed in Copenhagen as lecturer and assistant to Niels Bohr.

> This is the somewhat generalized uncertainty relation of Heisenberg. It places a lower bound on the product of the uncertainty measures for every pair of canonical variables (Kennard, 1927, p. 339).[6]

Interestingly, Kennard's assumption that the measurement uncertainty principle is a special case of the preparation uncertainty principle is taken over lock, stock and barrel by Heisenberg three years later. Heisenberg, too, thought of the two principles as essentially being the same. This is for example clear from the following passage, taken from a book which Heisenberg based on lectures he gave in 1930 at the University of Chicago:

> It must be emphasized again that this proof does not differ at all in mathematical content from that given at the beginning of the section [...] (Heisenberg, 1930, p. 19 of the English translation).

Here 'this proof' is Kennard's demonstration, which Heisenberg reproduces just before the above quote; and 'that given at the beginning of the section' refers to the gamma ray thought experiment that Heisenberg had used to explain what we have called the measurement uncertainty principle.

Given the positions of both Kennard and Heisenberg, it is hardly surprising that textbooks in quantum mechanics do not make a distinction between the two versions of the uncertainty principle. For example, in Leonard Schiff's justly famous book of instruction, the Kennard proof is given (with reference only to Heisenberg's 1930 book, not to Kennard's 1927 paper), with the concluding remark: "This is the precise expression of the Heisenberg uncertainty relation (3.1), when the uncertainties Δ_x and Δ_p are defined as in Eq. (12.1)" (Schiff, 1955, p. 55).

Schiff's Eq. (12.1) is simply the definition of the uncertainties as standard deviations, and he uses Δ_x instead of Δ_q to mean the dispersion of the position. However, his inequality (3.1) is the original Heisenberg version, and Schiff bolsters its interpretation by a discussion of Heisenberg's gamma ray microscope. He claims that

> [t]he relation (3.1) means that a component of the momentum of a particle cannot be precisely specified without our loss of all knowledge of the corresponding component of its position at that time (Schiff, 1955, p. 7).[7]

Kennard, Heisenberg, Schiff and others are however mistaken when they assume that the two versions are the same. Heisenberg's claim that Kennard's inequality

[6] Our translation from the German: "Dieses ist das etwas verallgemeinerte Unbestimmtheitsgesetz von Heisenberg. Es setzt eine untere Grenze für das Produkt der Unbestimmtheitsmaße für jedes Paar kanonischer Variabeln fest." The inequality that Kennard is talking about is inequality (27) in his text.

[7] Another example is the textbook by L.E. Ballentine (1990, p. 166), who first derives in a very elegant way what is actually the preparation uncertainty principle and then ascribes it to Heisenberg. On a personal note, one of us remembers from his time as student a feeling of unease when confronted first with one version and then with another as if they were equivalent. At the time he did not worry too much about the matter but now finds it rather satisfying to learn that his unease was justified after all.

'does not differ at all in mathematical content' from his own uncertainty principle, is misleading. At a formal level the two mathematical formulae are the same, but their interpretations are very different: Heisenberg's formula is about one single particle, whereas Kennard's applies to an ensemble. As Hilgevoord and Uffink (2001/2016, p. 21) note: "both in status and intended role there is a difference between Kennard's inequality and Heisenberg's previous formulation".

One might even go further. For as we will explain in the next section, Kennard's principle is always true, whereas there exist situations in which Heisenberg's is false.

7.4 Universally Valid Uncertainty Relation

For a long time, the widespread practice of using the term 'the Heisenberg uncertainty principle' to refer to the preparation uncertainty principle appeared perfectly legitimate. At worst, it seemed merely a matter of employing a somewhat confusing label. But appearances can be deceptive. About the validity of the preparation principle there is no disagreement: it is a consequence of the mathematics of quantum mechanics. In a paper of 2003, Masanao Ozawa has argued that the same can however not be said of Heisenberg's measurement uncertainty principle. It is doubtful and may well be false.

In Heisenberg's original gamma ray thought experiment, the position is measured before the momentum. This order is however arbitrary and could be reversed: a position measurement typically disturbs the momentum and a momentum measurement typically disturbs the position. We shall nevertheless follow the convention and take the position as being what is measured first and the momentum as what is measured second.

When one tries to estimate the position of an electron, the best one can do is to measure the sum of two quantities: the position plus some perturbation or deviation.[8] And when one tries to measure the momentum just afterwards, one is confronted with the sum of the momentum plus the perturbation that is caused by that position measurement. Ozawa calls the perturbation in what one measures first (here: the position) *noise* and the perturbation in what one measures afterwards (here: the momentum) *disturbance*. He then rephrases the uncertainty relation in Heisenberg (1927) as the claim that the noise times the disturbance cannot drop below $h/4\pi$:

> "By the γ-ray thought experiment, Heisenberg […] argued that the product of the noise in a position measurement and the momentum disturbance caused by that measurement should be no less than $h/4\pi$." (Ozawa, 2003, p. 1; Ozawa has $\hbar/2$, which is equivalent.)

[8] This goes not only for an electron, but for all objects and processes in both the micro- and the macroworld.

Ozawa argues that this claim of Heisenberg is *not universally valid*. It would be universally valid if the noise and the disturbance were independent of the position and the momentum. However generally there is such a dependence, and then Heisenberg's claim is false. This can be seen once one realizes that the existence of noise and disturbance gives rise to four pairs of quantities which are conceptually different:

(1) The position and the momentum of the electron.
(2) The noise and the disturbance.
(3) The noise and the momentum of the electron.
(4) The disturbance and the position of the electron.

The pair (1) involves the variables that quantum mechanics deals with. These variables are operators, the combination of which leads to the appearance of Planck's constant, which reflects the finite granularity of energy and matter. The noise and disturbance in (2) constitutes the pair that Heisenberg considered in his gamma ray thought experiment (although the terms 'noise' and 'disturbance' are Ozawa's).

Ozawa's major contribution lies in his analysis of (3) and (4). Normally the noise and the momentum in (3) are correlated, and the same goes for the disturbance and the position in (4). As Ozawa realized, these correlations must be included in the calculation in the form of two new terms which have to be added to the Heisenberg term (2). That is, whereas Heisenberg considered only (2), Ozawa insists that he should have added (3) and (4) to (2) before confronting the result with (1), the diktat of quantum mechanics.

Taking the sum of (2), (3) and (4) leads to a new, expanded version of the uncertainty principle, which Ozawa calls 'the universally valid uncertainty relation'. Ozawa's relation applies to one particle, just like Heisenberg's principle, but if differs from the latter in implying that (2) + (3) + (4) is bounded by the Planck constant. Mathematically Ozawa's relation reads

$$\Delta_n \Delta_d + \Delta_q \Delta_d + \Delta_n \Delta_p \geq \frac{h}{4\pi}.$$

A derivation of this inequality is given in Appendix B. This new uncertainty principle gives rise to a remarkable possibility. While $\Delta_n \Delta_d + \Delta_q \Delta_d + \Delta_n \Delta_p$ may not be smaller than $\frac{h}{4\pi}$, the Heisenberg term, $\Delta_n \Delta_d$, could be zero. What is needed for this possibility is that $\Delta_q \Delta_d + \Delta_n \Delta_p$ be no less than $\frac{h}{4\pi}$. This means that Heisenberg's (1927) uncertainty relation, aka the measurement uncertainty principle, would be false. There is no positive lower bound for the product of the noise and the disturbance! As Ozawa has it:

> The universally valid uncertainty relation shows that for measurements of dependent intervention, the lower bound of the noise-disturbance product depends on the premeasurement [...] standard deviations [of the position and momentum]. In order to obtain the trade-off among the noise, the disturbance, and the premeasurement standard deviations uncertainties, we apply the [Kennard] uncertainty relation (Ozawa, 2003, p. 3).[9]

Note that Ozawa's work does not undermine the preparation uncertainty principle in Kennard's form. The latter only involves the statistical spreading of position and momentum as a result of the preparation of the state. Since it does not refer to any measurement, noise and disturbance do not play a role in that calculation. Thus Ozawa's principle is in a sense an amalgam of Kennard's preparation and Heisenberg's measurement principles, for it uses the former and corrects the latter.

While Ozawa's (2003) paper contained only theoretical results, his findings have subsequently been tested empirically. Two experiments deserve mention. Firstly, an investigation headed by Yuji Hasegawa has verified Ozawa's relation, although not for measurements of the position and momentum of an electron, as in Heisenberg's thought experiment, but rather for those of the spin of a neutron in two perpendicular directions (Erhart et al., 2012). These spin components are, like position and momentum, also subject to the measurement uncertainty principle; and Ozawa's two new terms have been measured in the experiment: the Heisenberg inequality is seen to be violated, while that of Ozawa is respected. Secondly, an entangled state of two photons was examined by a Chinese team led by Yang Liu and Zhihao Ma (see Liu et al., 2019). Again, the two measured quantities are not position and momentum, but rather amplitude and phase, which are also subject to uncertainty relations. Here, too, Heisenberg's relation was shown to be violated, indeed in some cases the product of noise and disturbance was zero.

Although it is generally agreed that Ozawa's work is mathematically correct, it has been objected by Busch et al. (2013) that Ozawa's standard deviation estimation of noise and disturbance is not the only measure possible. They propose an alternative measure that involves a different estimate of the noise and the disturbance, and they claim that with this measure the original uncertainty principle of Heisenberg would be correct. If Busch et al. are right, then for a different choice of measures Ozawa's claim no longer holds.

The final word in this dispute has clearly not been voiced, and it is not our intention to defend a definitive claim. Having said this, we are inclined to think that the balance at the moment would appear to be on the side of Ozawa. We have two reasons for this. The first is related to the experimental results mentioned above. Since they explicitly falsify the Heisenberg formulation, everything that vindicates this formulation seems *prima facie* suspect. Second, the alternative measure of Busch et al. has itself been criticized. Three kinds of criticism are worthwhile mentioning.

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[9] Because of the structure of quantum mechanics, it is possible in special cases for the noise to be zero (a so-called noiseless measurement). According to Heisenberg's 1927 version of the measurement uncertainty principle, this would mean that the disturbance in the momentum is infinite; but Ozawa's correction gives merely a finite, and not necessarily large lower bound for this disturbance.

The first came from Hilgevoord and Uffink, who argue that the alternative measure provides an estimate that is unnecessarily large; as a result, we might find ourselves not close to the inequality but rather a long way above it.[10] In itself, this criticism is not a fatal objection. It merely states that the measure by Busch et al. is a long way from being optimal, but the same can be said of Ozawa's method, so this objection does not favor one approach above the other.

The second criticism, also from Hilgevoord and Uffink (2001/2016), is graver. It is that the measure used by Busch et al. does not do the job it claims to do. It fails to estimate Heisenberg's measurement uncertainty of the momentum *in the same state* as that in which the uncertainty of the position is evaluated.

The third kind of criticism was put forward by Ozawa (2019). He argued rather convincingly that the measure of Busch et al. suffers from more serious deficiencies, while his own measure does not. According to Ozawa, it does not satisfy *correspondence*, i.e. agreement with classical measures where these should be applicable. Nor does it meet *completeness*, i.e. the requirement that the error measure never vanishes for inaccurate measurements. If Ozawa is right, then this lends great plausibility to his contention that Heisenberg's version of the measurement uncertainty principle is erroneous.

7.5 Nagel's Criticism Revisited

When *The Structure of Science* appeared in 1961, nobody realized that Kennard's uncertainty principle differed from Heisenberg's original version, let alone that the latter stands in need of correction. Small wonder, then, that Nagel's work is also innocent of the distinction. Yet it is interesting to ask ourselves what exactly Nagel had in mind when he was criticizing Heisenberg, and to what extent his criticism is sustainable in the light of later discoveries.

As we have seen in Sect. 7.2, Nagel accuses Heisenberg of "putting the cart before the horse". It is not the case that the uncertainty relation follows from the uncontrollable alterations:

$$\textit{not}: \text{uncontrollability} \quad \rightarrow \quad \text{uncertainty relation.}$$

Rather it is the other way around:

[10] Hilgevoord and Uffink (2001/2016), and (1990). In a somewhat similar vein, Uffink and Hilgevoord (1985) earlier criticized the Kennard measure, explaining that for some exceptional probability distributions it gives too ample an estimate of the real spreading of position or momentum. One is more interested in where, say, 95% of the distribution is located than in what the least-squares measure would give. For normal, or near normal distributions with Gaussian tails, the criticism would be of little consequence; but one can think of other cases in which the Kennard measure would give a gross overestimate of the real uncertainty. This is not to say that the measure is wrong; rather for some abnormal distributions it is too crude.

$$\text{uncontrollability} \quad \leftarrow \quad \text{uncertainty relation.}$$

Our previous considerations have shown that these claims are in need of disambiguation. We have distinguished no less than three uncertainty relations:

H: Heisenberg's measurement uncertainty principle

K: Kennard's preparation uncertainty principle

O: Ozawa's universally valid uncertainty relation.

In addition, the notion of 'uncontrollability' is unclear. It could mean

K-uncontrollability: The inverse relation between the dispersions of position and momentum in an ensemble of particles, such as electrons.

H-uncontrollability: The impossibility of determining the position and momentum of a particle, e.g. an electron, with arbitrary accuracy.

What exactly does Nagel mean by 'uncertainty relation'? Obviously not *O*, for Ozawa's discovery had not yet seen the light of the day. That leaves *H* and *K*. Earlier we noted that for decades scholars have been talking about Heisenberg's measurement principle while actually referring to Kennard's preparation principle. The same goes for Nagel. When he comments on Heisenberg's *H*, he gives the formula that corresponds to Kennard's *K*:

> One of the [Heisenberg uncertainty relations] is expressed by the formula $\Delta_p \Delta_q \geq h/4\pi$. In this formula [...] 'Δ_p' is interpreted as the coefficient of dispersion (or deviation, sometimes also called the 'uncertainty') from the mean value of the momentum at a given instant; and similarly for 'Δ_q' (Nagel, 1961, p. 294).

However, when talking about 'uncontrollability', Nagel is referring to Heisenberg's idea of the notion, and thus to *H*-uncontrollability rather than *K*-uncontrollability. This is for example clear from what he writes in Chap. 10:

> For the 'uncontrollable' [...] alterations that the electrons are said to suffer when they interact with instruments of measurement do not constitute the *evidence* for the uncertainty relations, but are part of the *consequences* drawn from the relations. This will be clear if we ask ourselves what ground we have for claiming that the alterations are uncontrollable and unpredictable, and if we remind ourselves that perturbations produced by measuring instruments in the objects measured were fully recognized in classical physics. [...] According to the Heisenberg uncertainty relations, however, the alterations produced in electrons by measurements on them cannot be calculated even in principle, because in this case electrons undergo "uncontrollable changes" (Nagel, 1961, p. 297, original emphases).

From this it follows that what Nagel actually means when he criticizes Heisenberg is expressed by the following two claims:

$$not: H\text{-uncontrollability} \quad \rightarrow \quad K,$$

but rather the other way around:

$$H\text{-uncontrollability} \quad \leftarrow \quad K.$$

Are these claims correct? The first one evidently is. For irrespective of whether one has in mind K or H, and irrespective of whether one is thinking of H-uncontrollability or K-uncontrollability, it is never the case that the uncertainty relations follow from the uncontrollable alterations. So in this sense Nagel's criticism is perfectly appropriate.

This can however not be said of the second claim. Since K refers to the properties of an ensemble rather than those of one particle, the second claim is a *non sequitur*. It differs in important ways from the correct:

$$K\text{-uncontrollability} \quad \leftarrow \quad K\,.$$

In this connection, it is interesting to note that

$$H\text{-uncontrollability} \quad \leftarrow \quad O\,.$$

is also valid. That is, Ozawa's universally valid uncertainty relation entails the impossibility of determining the position and momentum of a particle with arbitrary accuracy. For such a determination would be tantamount to making both the noise and the consequent disturbance inherent in two linked measurements arbitrarily small. That Ozawa's relation does not permit this is clear from Ozawa's inequality $\Delta_n \Delta_d + \Delta_q \Delta_d + \Delta_n \Delta_p \geq \frac{h}{4\pi}$, since $\Delta_n = 0$ and $\Delta_d = 0$ would make all three terms on the left of this inequality zero, which would lead to inconsistency. Although Nagel knew nothing of Ozawa's discovery, we might perhaps grant that he was seeing the truth through a glass darkly. For although he erroneously thought that Kennard had validated H, nevertheless the corrected version of H, namely O, does indeed provide an explication of the claim that uncontrollability follows from the uncertainty relation.

In addition, there is another unclarity in Nagel's treatment of the uncertainty relations. In Sect. 7.2 we noted that, according to Nagel, many people see the existence of the uncertainty relations as the main reason for calling quantum mechanics indeterministic. We have also seen that Nagel rejects this view, and rightly so. This, however, raises the question what according to Nagel is the real reason for the indeterminism of quantum mechanics. Here is his answer:

> [D]espite the fact that quantum mechanics is deterministic with respect to the state description defined by the Psi-function, [...] outstanding physicists maintain that quantum theory is "indeterministic" in the important sense that its state description is associated with a statistical interpretation and that its predictions are based on statistical assumptions. This characterization is unquestionably appropriate (Nagel, 1961, p. 308).

Nagel thus accepts the view that quantum mechanics does not apply to individual particles, only to averages of properties in an ensemble. This is the 'statistical interpretation' of quantum mechanics, which Einstein and others espoused. It still has its advocates (see, e.g., Ballentine, 1990), despite the fact that there are experiments which indicate that quantum mechanics really does work at the level of individual particles. Oddly enough Nagel does not mention the 'standard' view that quantum mechanics is indeterministic because the wave function undergoes a collapse after an observation, the so-called projection postulate of John von Neumann (1955). There are many versions of how precisely the collapse takes place, but Nagel, who in 1961 must have been aware of at least some of them, apparently chose to ignore them all. On his reasons for doing so we can only speculate.

7.6 Conclusion

In this paper we have endorsed Nagel's criticism of Heisenberg. Nagel arraigns Heisenberg on the grounds that the latter has ineptly put the cart before the horse. The uncertainty principle is not a consequence of the uncontrollability of certain measurements in the submicroscopic domain. On the contrary, it is part and parcel of a theoretical edifice, viz. quantum mechanics; and it is this theory that entails the uncontrollability in question.

However, when Nagel goes further and forays into the sequence of implications

$$\text{quantum mechanics} \rightarrow \text{uncertainty principle} \rightarrow \text{uncontrollability}$$

he plays on a sticky wicket. For he confounds Heisenberg's and Kennard's uncertainty principles, and he fails to discriminate between uncontrollability at the level of an individual electron and the ineluctable dispersion in an ensemble. Nevertheless Nagel succeeds in his negative claim, i.e. his criticism of Heisenberg; and if he falters in his positive assertion, it is a shortcoming shared by many.

Acknowledgements We would like to thank Matthias Neuber and Adam Tamas Tuboly for organizing the conference "Ernest Nagel and the Making of Philosophy of Science a Profession", and the participants for posing interesting questions. We are also grateful to Jan Hilgevoord, John Norton, Jos Uffink and an anonymous referee for having commented on earlier versions of this paper. Hanoch Ben-Yami we thank for showing us the heights and depths of Budapest's old and recent history.

Appendices

A Kennard's Uncertainty Relation

The position and momentum of the electron are represented by the self-adjoint operators q and p respectively. These operators on Hilbert space are conjugate, and they satisfy the commutation relation $[p,q] = -i\hbar = -\frac{ih}{2\pi}$. Let $\langle q\rangle$ and $\langle p\rangle$ be the expectation values of q and p with respect to some quantum state; and define the shifted operators $\tilde{q} = q - \langle q\rangle$ and $\tilde{p} = p - \langle p\rangle$. Clearly $[\tilde{p},\tilde{q}] = -i\hbar$. The variances are

$$\begin{aligned} \Delta_q^2 &= \left\langle \left(q - \langle q\rangle\right)^2 \right\rangle = \langle\, \tilde{q}^2 \,\rangle \\ \Delta_p^2 &= \left\langle \left(p - \langle p\rangle\right)^2 \right\rangle = \langle\, \tilde{p}^2 \,\rangle \,. \end{aligned}$$

Define the operator $t = \tilde{p} + i\omega\tilde{q}$, where ω is a real number, so

$$\begin{aligned} 0 \le \langle t^\dagger t\rangle &= \langle(\tilde{p} - i\omega\tilde{q})(\tilde{p} + i\omega\tilde{q})\rangle \\ &= \langle\tilde{p}^2\rangle + i\omega\langle[\tilde{p},\tilde{q}]\rangle + \omega^2\langle\tilde{q}^2\rangle \\ &= \Delta_p^2 + \omega\hbar + \omega^2\Delta_q^2 \\ &= \left(\Delta_p + \frac{\omega\hbar}{2\Delta_p}\right)^2 + \frac{\omega^2}{\Delta_p^2}\left(\Delta_q^2\Delta_p^2 - \frac{\hbar^2}{4}\right) \end{aligned}$$

Minimize this by setting

$$\omega = -\frac{2\Delta_p^2}{\hbar}$$

so

$$\Delta_q^2\Delta_p^2 - \frac{\hbar^2}{4} \ge 0$$

from which Kennard's result follows:

$$\Delta_q\Delta_p \ \ge\ \frac{\hbar}{2} \ =\ \frac{h}{4\pi}\,.$$

B Ozawa's Uncertainty Relation

Consider the operators

$$q^{out} = q + n \quad \text{and} \quad p^{out} = p + d\,.$$

q^{out} and p^{out} are measured in different experimental setups, so they commute:

$$[q + n, p + d] = 0$$

i.e.

$$[q, p] + [n, d] + [q, d] + [n, p] = 0\,.$$

So successively

$$[n, d] + [q, d] + [n, p] = -[q, p]$$

$$\langle [n, d] + [q, d] + [n, p] \rangle = -\langle [q, p] \rangle$$

$$|\langle [n, d] \rangle| + |\langle [q, d] \rangle| + |\langle [n, p] \rangle| \geq |\langle [n, d] + [q, d] + [n, p] \rangle| = |\langle [q, p] \rangle| = \tfrac{h}{2\pi}\,.$$

By Kennard's method,

$$\Delta_n \Delta_d \geq \tfrac{1}{2}|\langle [n, d] \rangle|, \qquad \Delta_q \Delta_d \geq \tfrac{1}{2}|\langle [q, d] \rangle|, \qquad \Delta_n \Delta_p \geq \tfrac{1}{2}|\langle [n, p] \rangle|,$$

and therefore

$$\Delta_n \Delta_d + \Delta_q \Delta_d + \Delta_n \Delta_p \geq \tfrac{h}{4\pi}\,.$$

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