THE NATURAL ONTOLOGICAL ATTITUDE FROM A PHYSICIST'S PERSPECTIVE: TOWARDS QUANTUM REALISM

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Abstract

The debate between Arthur Fine and Alan Musgrave is well known amongst those involved in the scientific realism debate and centres upon two papers, namely Fine's "The Natural Ontological Attitude" (NOA) and Musgrave's reply "NOA's Ark – Fine for Realism". The debate in fact is so prominent that these two papers are quite often found together in philosophy of science anthologies. Reading them like this gives the very strong impression that Musgrave is the victor which is the commonly held view. In this thesis, I wish to overturn this view by placing Fine's paper in context, namely as part of a larger work on the history and philosophy of quantum physics. Fine's book, *The Shaky Game: Einstein, Realism and Quantum Theory*, gives us good reason to believe that quantum physics significantly undermines the whole scientific realism debate, and as such, has strongly influenced Fine's development of the Natural Ontological Attitude, which is as Fine believes a middle ground between realism and anti-realism. The present thesis evaluates the Natural Ontological Attitude from a physicist's perspective and defends Fine against Musgrave's reply to the extent that it demonstrates that Musgrave would do well to read Fine's paper in context.

That said, just as Fine in his youth hoped that a quantum realist position will one day be found, so I also possess this aspiration; and so, despite my concluding that Fine is justified in holding to NOA, I argue furthermore that NOA is but a precursor to a potential quantum *structural* realist position. After showing that structural realism is worthy of consideration by using it to counter Fine's objections to scientific realism, I analyse the results of quantum physics in an attempt to understand what it can tell us about reality in the quantum realm. Eliminativist Ontic Structural Realism holds great promise as a quantum realism contender, and as such, it inspired the questions regarding individuality and reality that are discussed in the final main chapter. I thus resuscitate hope that the cause of the quantum realist is not yet lost.

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I wish to thank several people without whom this thesis would not be possible. First, I wish to thank my supervisors, Dr. Philip Catton and Assoc. Prof. Mike Reid, for their insightful discussion, and Dr. Catton in particular for pulling me from the ranks of an impatient physicist to a careful philosopher. Secondly, I wish to thank the Philosophy Department for their hospitality and welcome distractions. Lastly, I wish to thank my family for their support throughout this process.

"Of course it is happening inside your head, Harry, but why on earth should that mean that it is not real?" ~Albus Dumbledore (J.K. Rowling), *Harry Potter and the Deathly Hallows*

Dom Cobb: I know this bridge. This place is real isn't it?
Ariadne: Yeah. I cross it every day to get to the college.
Dom Cobb: Never recreate places from your memory. Always imagine new places.
Ariadne: You gotta draw from stuff you know, right?
Dom Cobb: Only use details. A...a...a street lamp or a phone booth. Never entire areas.
Ariadne: Why not?
Dom Cobb: Because building a dream from your memory is the easiest way to lose your grasp on what's real and what isn't real.
Ariadne: Is that what happened to you?

~Inception

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Part A: The Natural Ontological Attitude from a Physicist's Perspective

Chapter 1: Introduction

***Morpheus**: What is "real"? How do you define "real"?*

§1.1: Motivation

Arthur Fine is a philosopher of science specialising in the philosophy of physics while also dealing with more general topics in the natural and social sciences.[1] In preparation for his book *The Shaky Game: Einstein, Realism and the Quantum Theory*[2], he spent many hours looking through original articles and correspondence of physicists such as Schrödinger, Born, Bohr, and of course Einstein. In Fine's own words:

> The background to these essays is my own attempt to come to terms with issues in the interpretation of the quantum theory, and in particular with the issue of realism there.[3]

This is an attitude that I have a great deal of sympathy with, and I would speculate many other physicists share the same concerns. To put it mildly, quantum mechanics is weird. It teaches that electrons are both particles and waves, that they cause interference patterns with themselves, and form fuzzy probability clouds as atomic orbitals. It also teaches us that we cannot take the notions of locality and determinateness for granted, i.e. quantum mechanics makes it seem reasonable that cheering your favourite sports team on at home affects them in the stadium, and that the moon actually is not there when no one looks at it. Fine continues:

> While working out the technical defense of both determinate and indeterminate forms of quantum realism … I gradually began to realize that in some cases Einstein had already pioneered similar ideas and had been severely criticized for so doing.[4]

A certain sense of dissatisfaction has been felt over quantum mechanics for a long period of time. Even Einstein, who was one of the fathers of the 'old' quantum theory, himself

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[1] University of Washington Philosophy Department, www.phil.washington.edu/people_fine.htm
[2] **Fine, Arthur**, *The Shaky Game: Einstein, Realism and the Quantum Theory*, University of Chicago Press (1986)
[3] *The Shaky Game*, pg 3
[4] *The Shaky Game*, pg 3

proposed forceful objections to the 'new' quantum theory that was being developed by the likes of Schrödinger and Bohr. Since Fine is well aware of the weirdness of quantum mechanics, it is no surprise that the physicist in him would 'bristle' at the thought of it (as Einstein would say). This drive led him to investigate further to gain a deeper understanding of what Einstein's views were on quantum realism and into the rationality of holding a realist attitude towards quantum mechanics. In the end, he finds that Einstein's view is really only a 'motivational realism'[5]; Einstein desperately wants the notions of determinateness and locality to remain in the quantum realm,[6] but Fine sees no philosophical justification for such a position. Fine's Natural Ontological Attitude (NOA) is what follows. At the end of his NOA paper, we find this:

> Just as the typical theological moralist of the eighteenth century would feel bereft to read, [in the present day,] say, the pages of *Ethics*, so I think the realist must feel similarly when NOA removes the "correspondence to the external world" for which he so longs. I too have regret for that lost paradise, and too often slip into the realist fantasy. I use my understanding of twentieth-century physics to help me firm up my convictions about NOA...[7]

It seems that Fine's knowledge pains him at times and still gives him reason to want to slip back into a 'realist fantasy', but it is also this knowledge that allows him to maintain assurance in NOA.[8]

On the other hand, Alan Musgrave is an epistemologist and philosopher of science[9], but lacks the same training in physical science that Fine possesses. His paper opposing NOA is taken from an entirely classical, or common sense, point of view, and as such does not acknowledge the effect that quantum weirdness has on someone's realist perspective. This becomes clear in statements such as:

> I use humdrum commonsensical examples [such as looking at a full moon], rather than esoteric scientific ones, because if there is a problem it will be a quite general one, which afflicts the common-sense realist metaphysic as much as the scientific one.[10]

[5] *The Shaky Game*, pp109-111
[6] This desperation was seen by many of Einstein's contemporaries as the result of Einstein getting old. However, it was Einstein who wanted to develop quantum mechanics anew using non-classical concepts while it was Einstein's greatest opponent, Bohr, who was unwilling to let go of classical concepts, and hence the complementarity principle arose.
[7] *The Shaky Game*, pg 134
[8] *Ibid.*
[9] University of Otago Philosophy Department, www.otago.ac.nz/philosophy/Staff/alan_musgrave.html
[10] "NOA's Ark – Fine for Realism", pg 392

However, there is a great difference between discussing that which is clearly observable, such as a full moon, and that which is (at least currently) unobservable, such as quarks. Making a measurement on a quantum system vastly changes the system into a new state that is for all practical purposes unpredictable, whereas making a measurement on the fullness of the moon does not affect the moon at all since the light coming from the moon has already bounced off of it, independent of whether you wish to measure its fullness or not. At the end of his paper, Musgrave does mention quantum mechanics and Fine's book *The Shaky Game*, but I get the sense that Musgrave wants to sweep this under the carpet and take the natural ontological attitude chapter as a standalone paper (as it is indeed very often found in philosophy of science anthologies). This does Fine an injustice and sets up NOA as a sitting duck ready to be shot down. A reading of the rest of *The Shaky Game*, along with knowledge of quantum mechanics, is definitely needed before one can fully appreciate Fine's position.

Like Fine, Einstein and many other physicists and philosophers of physics before me, I too would like to see quantum physics interpreted in terms of a realist description. The present thesis not only looks in-depth at the admirable efforts of one of these philosophers of physics, and to an extent defends these efforts, but also explores whether new light that has been shed subsequently in the form of structural realism is better able, with respect to this holy grail (of quantum physics), to illuminate the path towards realism, if indeed the path exists at all.

§1.2: An Overview of the Scientific Realism Debate

Since there are probably as many scientific realism positions as there are philosophers of science, I will give an overview of the central issues discussed in the debate by looking at some prominent positions that have played an important role leading up to the publication of *The Shaky Game*. Some of the central questions that are considered in the scientific realism debate include:

- What does it mean to say that electrons exist?
- How do we know our scientific theories are true, if at all?
- What is (are) the aim(s) of science?
- Does science describe an objective reality?

In describing the nature of the scientific realism debate, I shall look at the two extremes of the scientific realism debate, along with three important, and more moderate, positions (the other two, in dashed arrows, I only mention in passing since they are discussed in greater detail later). I recognise that there are many issues discussed within the scientific realism debate and that placing all of these positions along a single continuum is highly simplistic, but to give an idea of which positions are "more anti-realist" or "more realist", the continuum is given as figure 1 below.

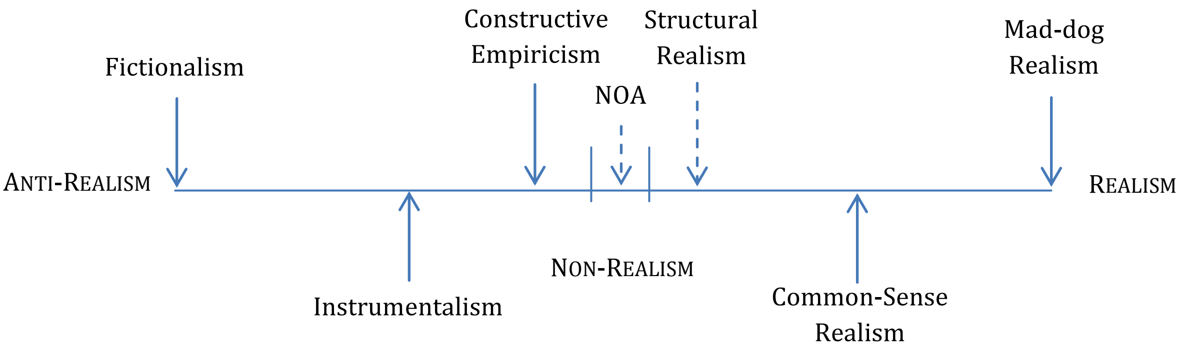

Figure 1: Scientific Realism Continuum

Beginning with the two extreme positions, mad-dog realism can be described as saying that:

> there really are such entities as current science claims there to be and ... what current science tells us about such entities is true.[11]

Such a charge has been levelled at Musgrave himself, and in good humour, he accepted a plaque saying "BEWARE OF THE mad-DOG realist". However, Musgrave is quick to point out that even he believes that mad-dog realism is incorrect. He essentially says that the mad-dog realists have gone without fear where metaphysics should fear to tread; "scientists tend to be pretty cagey about entities and theories at the frontier – realist philosophers of science would do well to follow them in this".[12] Even Musgrave acknowledges that our degree of belief should be proportional to the evidence we have supporting that belief, and hence the mad-dog realist, in being besotted by whatever conceptual forms issue from current science however new or cutting-edge, has simply gone too far.

[11] **Musgrave, Alan**, "Realism, Truth and Objectivity", found in *Realism and Anti-Realism in the Philosophy of Science: Beijing International Conference 1992*, pg 20

[12] "Realism, Truth and Objectivity", pg 22

On the other end of this spectrum we have fictionalism which can be characterised by saying that the:

> various entities presupposed by scientific and common sense discourse [are] merely "useful fictions", or that we cannot, at any rate, possibly know that they are *more* than "useful fictions" (and so we may as well say that that is what they are).[13]

Michael Neumann concludes that fictionalism "seems like an overly extreme attempt to correct the excesses of an overly credulous realism".[14] He goes on to say that although fictionalism correctly recognises that a realist would be unwise to say that all scientific theories are true, as we have seen above, fictionalism's reaction to this is excessively sceptical to the point where experimental corroboration of a theory cannot secure any degree of truth in that theory such that the theory and any entities it proposes can only be considered to be useful tools and nothing more. Even though the aims of science are a topic for debate within the scientific realism debate, to be so sceptical of scientific practice such that it is impossible to even corroborate theories seems to be very defeatist towards all but the most extreme of views regarding the aims of science. This, along with the insulting implication that the work of our scientific predecessors if worth very little, if anything, leads me to also conclude that fictionalism is not worthy of consideration, and it merely creates more problems than it solves.

Moving onto the more reasonable positions, we continue with Musgrave in looking at common-sense realism. Chapter three deals with Musgrave's position in great detail, but in response to the four questions above characterising the scientific realism debate, Boyd defines scientific realism as:

> (i) "Theoretical terms" in scientific theories (i.e. non-observational terms) should be thought of as putatively referring expressions; scientific theories should be interpreted "realistically".
>
> (ii) Scientific theories, interpreted realistically, are confirmable *and in fact often confirmed* as approximately true by ordinary scientific evidence interpreted in accordance with ordinary methodological standards.
>
> (iii) The historical progress of mature sciences is largely a matter of successively more accurate approximations to the truth about both observable and unobservable phenomena. Later theories typically build upon the (observational and theoretical) knowledge embodies in previous theories.

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[13] **Putnam, Hilary**, *Philosophy of Logic*, Harper and Row (1971), pg 63

[14] **Neumann, Michael**, "Fictionalism and Realism", Canadian Journal of Philosophy, Vol. 8, No. 3 (Sep 1978), pg 541

(iv) The reality which scientific theories describe is largely independent of our thoughts or theoretical commitments.[15]

Notice the language used here including the terms 'putatively', 'approximately true', 'typically' and 'largely'. This highlights the difference between common-sense realism, generally referred to as simply 'realism', and mad-dog realism in that common-sense realism recognises the difficulties posed by anti-realist arguments such as the pessimistic induction from the history of science (past theories have been shown to be false and current theories are likely to come to the same fate) and theory underdetermination (finite amounts of data cannot distinguish between the logically infinite number of theories) and has adjusted accordingly. Structural realism, which was around when *The Shaky Game* was written but was not a major position, is even more cautious and takes great pains to describe scientific practice realistically with these concerns in mind. Chapter four contains a detailed discussion of structural realism so I shall not elaborate on it here, but I mention it here simply to make note of its presence and that it has become important in recent times.

On the anti-realist side of the debate, there are positions such as instrumentalism and constructive empiricism, among others. Instrumentalism and constructive empiricism are very similar in that they both have a large focus on empirical adequacy; the idea that theoretical prediction and experimental results are sufficiently close, at least for the range of the theory in question. It is this empirical adequacy that separates them from fictionalism in that it provides the corroboration required to justify the use of a particular theory rather than being completely sceptical. However, this empirical adequacy is as far as these two anti-realist positions are willing to go in terms of a theory's description of reality; realism also prizes empirical adequacy, but it does so because it feels that theory has actually latched onto some aspect of reality whereas the instrumentalist and the constructive empiricist are unwilling to take this step. There are differences between instrumentalism and constructive empiricism in that instrumentalism states that:

> theories are construed as calculating devices or instruments for conveniently moving from a given set of observations to a predicted set of observations. As such the theoretical

[15] **Boyd, Richard N.**, "On The Current Status of the Issue of Scientific Realism", Erkenntnis, Vol. 19 (1983), pg 45

> statements are not candidates for truth or reference, and the theories have no ontological import.[16]

Given that to be a banner-carrier for a particular position in the scientific realism debate about all of scientific practice seems unreasonable because of the vast range of phenomena that scientific practice encompasses, instrumentalism makes itself heard the loudest with regard to unobservable entities, i.e. to be an instrumentalist about chairs is to border on a fictionalist's level of scepticism but to be sceptical about the existence of quarks is more reasonable. In answering the question of whether or not electrons exist, an instrumentalist would say that the electron concept is a useful tool towards gaining an empirically adequate understanding of nature, but they are nothing more than this. A constructive empiricist, however, will say that electrons may exist, but that the proper attitude towards electrons is agnosticism.[17] One stark difference between the instrumentalist and the constructive empiricist is with regard to truth. An instrumentalist will say that theories, especially with regard to unobservables are outright false since they are nothing more than useful tools but for the constructive empiricist, the question of truth is not even an issue since for them:

> science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate.[18]

The last region on the scientific realism continuum above is that of non-realism. This is where Fine's Natural Ontological Attitude falls since (1) it is a rejection of both the realist and anti-realist sides of the debate due to the inadequacy of the descriptions of either side so it cannot fall into either of those categories, and (2) it is said to be the core attitude to which either of these two sides in the debate is able to hold to, hence it falls in the middle. Chapter two is dedicated to giving a detailed look at NOA and so I also only mention NOA in passing here. Suffice it to say, as far as I am aware, *The Shaky Game* is the first time non-realism becomes a significant contender in the scientific realism debate.

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[16] **Delaney, C.F.**, "Instrumentalism" entry in *The Cambridge Dictionary of Philosophy*, Cambridge University Press (1995), pg 379

[17] **McMichael, Alan**, "Van Fraassen's Instrumentalism", The British Journal for the Philosophy of Science, Vol. 36 No. 3 (Sep 1985), pg 258

[18] **van Fraassen, B.**, *The Scientific Image*, Clarenden Press Oxford (1980), pg 12

§1.3: Synopsis

So far we have seen that chapter one has highlighted the motivation behind the thesis, as well as giving a broad introduction to the topic of scientific realism. For the rest of the thesis, we will deal more specifically with the issues found in the debate. This will range from an analysis of Fine's own position, NOA, to Musgrave's objections, and finally onto whether scientific realism can really provide a suitable philosophical description of quantum physics.

We continue by looking at NOA from a physicist's perspective in chapter two which considers Fine's take on the scientific realism debate as seen in *The Shaky Game*. It first deals with the objections Fine has to realism, or more specifically, classical or common-sense realism. This is done in two parts, namely from historical and philosophical perspectives. Historically, Fine believes that if the physicists of the early 20th century held a common-sense realist view about reality that modern physics as a whole, not just quantum mechanics, would have been brought to a standstill. Philosophically, Fine poses three ways in which scientific realism is inadequate, i.e. unable to provide an overarching description of the practice of science. Firstly, Fine criticises the methodology of the realist by showing that the realist must either use fuzzy philosophical notions such as approximate truth, or that the realist must beg the question. Secondly, Fine criticises the realist's approach to theory conjunction in which he feels that realists are too confident about the level of approximate truth held within scientific theories. Thirdly, Fine spends a great deal of time on the small-handful problem (in which he tries to show that scientific realism is inadequate in explaining why there are only a small handful of contender theories on offer for a particular phenomenon at any given time), and contends that even instrumentalism, the archenemy of realism, is able to account for this phenomenon more convincingly.

From there, we move onto Fine's objections of scientific anti-realism. Fine breaks down the anti-realist theories into two groups: those that take part in truth-mongering (compare: warmongering), and those that do not. With regard to those that do, Fine states that they distort the concept of truth to the point where it may satisfy their needs in terms of a philosophical position, but in doing so, their position becomes so difficult to decipher that it is difficult to understand what they are actually claiming about what scientific discourse is trying to achieve. In terms of those anti-realist positions that do not use a truth-mongering tactic, Fine uses the example of constructive empiricism where Fine objects to its attempt to draw sharp dividing lines in science where none exist, which in this case is the line of observability.

Now that Fine has got his objections to a philosophical description of the scientific endeavour off his chest, he moves on to describing his proposal for a replacement, the Natural Ontological Attitude. The concept of NOA as an attitude rather than a philosophical position is introduced along with a summary of how this attitude plays out in various areas of scientific discourse. Fine's defence of NOA is then considered in which he shows how ontology and truth commitments play out for a NOAer (someone who holds to NOA) and how, in comparison, realists are merely chasing a phantom. I then make a note of Fine's brief mention of how NOA would deal with the problem of conjunctions but in doing so, elaborate how having a NOA-like attitude towards quantum complementarity can be beneficial.

Now that Fine has made his case, chapter three looks at Musgrave's reply, and most importantly, the extent to which reading Fine out of context makes a difference. I first look at two related issues that are raised by Fine which are reciprocity and contamination. The first issue, reciprocity, states that measured objects are inherently interacted with, and hence not objective. Musgrave, with only a classical, common-sense worldview takes this to be rather pedantic. However, by considering one of the most important series of papers ever published in physics, namely those related to the EPR experiment, we see how reciprocity becomes important in the quantum realm. After describing the EPR paper itself and how even Einstein found it difficult to reconcile with his realist leanings, I analyse Bell's Theorem which is found in the second paper of the trilogy. Bell's Theorem plays a large role in influencing Fine's view about realism, and by having a look at specific examples of Bell's Theorem, backed up by the Aspect experiment (the final instalment in the trilogy), we see that the weirdness found in quantum physics goes a long way to justifying Fine's proposition of NOA.

The second issue, known as contamination, is dealt with next. Contamination is the idea that measurements tell us information about interacted-with things, not about the things-in-themselves. Musgrave reacts to this by using his common-sense realism and in doing so touches on a very important point which is the idea of concepts in science. However, Musgrave's insistence on using common-sense realism means he continues unhindered down a slippery slope into conceptual relativism, whereas considering Kant's more sophisticated position allows us to stop the slide and show us the intricacies of Fine's reasoning. By applying Kantian reasoning to the weirdness of quantum mechanics, we can see that to think of things-in-themselves at the quantum level poses problems and that Fine is once again justified. Furthermore, we consider the work of another important thinker, Bohr, and see how his principle of complementarity is intimately linked to contamination and that even the most carefully constructed quantum experiment is unable to avoid it.

Musgrave does deserve some credit though. Near the end of his reply to Fine, Musgrave does consider quantum mechanics as a significant driving force behind Fine's development of NOA. This, however, turns out to be brief and really brings to light Musgrave's misunderstanding of the influence that quantum physics had on Fine's reasoning.

In anticipation of a discussion regarding a potential form of quantum realism that also makes use of Tarskian truth claims, chapter three ends with a comparative analysis of the truth claims made by both NOA and common-sense realism. Given that Musgrave is an epistemologist, his experience means he is better qualified to comment on NOA in respect to its stance on truth than to comment upon NOA's taking inspiration from quantum physics. However, Musgrave misapprehends what NOA's commitments to truth claims are and this leads him to believe that NOA is nothing but common-sense realism. This misunderstanding first makes itself known when we consider the truth claims of NOA in comparison with those of the anti-realist positions. Musgrave wishes to claim that NOA cannot possibly be thought of as a core position in the scientific realism debate since NOA appears to be incompatible with the truth claims made by anti-realist positions, but Musgrave has incorrectly identified the Tarskian truth claims of NOA with those of correspondence truth. Continuing this error into a discussion on scientific concepts, we find that Musgrave has oversimplified the idea of scientific concepts such that he claims we see reality as it really is rather than through concept-tinted glasses or as an interfered with reality. Thus ends the objection to Musgrave and also "Part A: The Natural Ontological Attitude from a Physicist's Perspective".

"Part B: Towards Quantum Realism", which contemplates the possibility of a scientific realist view of quantum mechanics (henceforth: quantum realism) begins with chapter four. Chapter four begins by recognising that although Fine has been unjustly lambasted by Musgrave, Fine's reaction against realism indeed may have been misplaced. It is the purpose of part B to consider a middle ground between Fine and Musgrave, namely a scientific realist position known as structural realism. This begins by explaining what scientific realism is with the help of Worrall's "Structural Realism: Best of Both Worlds" paper which kicked off a structural realism renaissance. After also detouring into the philosophical nature of truthlikeness, the rationality behind moving from NOA to structural realism is highlighted, first by describing their similarities, such as their attitudes towards ontology, and secondly by using general philosophical arguments as well as structural realism specific properties to answer the objections Fine has to common-sense realism, namely the realist's methodology, the problem of conjunctions and the small-handful problem.

Chapter five specifically deals with the issues regarding the plausibility of quantum realism. This is done by posing two important questions, the first of which is inspired by a structural realist position known as eliminativist ontic structural realism whose central tenet claims that it is incorrect to speak of the individuality of quantum particles. After providing a description of the Principle of the Identity of Indiscernibles, including the worth of the various forms it can take, the important differences between the three types of statistics used in physics are elaborated upon. The discussion converges on the Indistinguishability Postulate where we find what it means physically to think of particles as indistinguishable and it is here that we learn what the results of quantum physics tell us about a NOAer's reaction to quantum individuality, as well as the reaction of one who insists on developing more of a philosophical description.

The second of the two questions posed deals with what quantum physics tells us about what we can consider to be real and that Bell's Theorem may not be as powerful as was once thought in answering this question. In the last chapter of *The Shaky Game*, Fine considers two forms of realism, namely minimal and reductive realism so, to set the scene, these are analysed in light of what we know from quantum physics and earlier discussion. With this in mind, we finally come to trying to gain an insight into what we can consider to be real in a quantum system and what form this representation of reality takes.

Lastly, the future of quantum realism is considered. This involves taking a brief look at where we are currently, whether it is worthwhile considering developing a form of quantum realism given the weirdness of quantum physics, and some potential avenues for continued research.

Chapter 2: Arthur Fine and the Scientific Realism Debate

***Morpheus**: Have you ever had a dream, Neo, that you were so sure was real? What if you were unable to wake from that dream? How would you know the difference between the dream world and the real world?*

Over the course of two chapters in *The Shaky Game*, "The Natural Ontological Attitude" and "And Not Anti-Realism Either", Fine provides reasons why he does not hold to the realist and anti-realist positions respectively. In "The Natural Ontological Attitude", he begins by giving philosophical reasons why realism is flawed such as the realist's methodology lacks in stringency and that it fails to deal with what Fine terms the small-handful and conjunction problems. He also discusses realism from an historical perspective stating that if a realist mind-set was held to in early 20th century physics, monumental theories such as relativity and quantum theory would not have come about. In "And Not Anti-Realism Either", he deals with anti-realist positions, first by dealing with truth-mongering philosophies that try to import their own definition of what it means for a theory to be true, and secondly with constructive empiricism and the issue of observability in particular.

§2.1: Fine's Historical Objections to Scientific Realism

As mentioned earlier, Fine began his research for *The Shaky Game* with the realist intent of vindicating Einstein, but ended it by claiming that Einstein's realism is nothing more than motivational realism and even going so far as to begin the NOA chapter with "Realism is dead." So, what happened? From a historical perspective[19], in the development of the two giants of 20th century physics, relativity and quantum mechanics, Fine sees prime examples of theories that would have struggled to develop if approached with a realist mindset. Einstein himself, in his general relativity paper of 1916, says that the requirement of general covariance "takes away from space and time

[19] *The Shaky Game* , pp 122-126

the last remnants of physical objectivity"[20]. It was Einstein's positivistic youth and reverence for Mach that gave him the leeway required to develop relativity that would not have been possible under the stringent conditions of realism. Later in life Einstein fought vehemently for realism, in relativity and especially in quantum mechanics. He wanted to associate genuine reality to the four-dimensional space-time manifold and the related tensor fields. However, such notions confuse even the best of scientists since this would mean that motion as we know it would cease to be real. Therefore Fine believes that for the sake of progress, general relativity needs to be thought of as a powerful scientific tool and nothing more.

With regards to quantum theory, Heisenberg and Schrödinger, the fathers of the 'new' quantum theory, developed their theories with an anti-realistic mindset. Heisenberg only spoke in terms of observables in his theory while Schrödinger, after seeing the inherent problems of assigning a reality to the wavefunction, reluctantly abandoned the realist idea. Ultimately it was Bohr who fought the war against realism believing that realism would lead the next generation of students into scientific dead ends. Landé goes so far as to say that the realist intent of "explaining" the quantum is a hopeless task, and that scientists should "accept it as fundamental and help work out its consequences"[21], i.e. they are to "shut up and calculate"[22].

It is interesting to note that Fine has defended against our being propelled along a descending spiral into an anti-realist whirlpool. He believes "that we may still be able to keep realism afloat in this whirlpool"[23] even if we consider the "charybdis of realism"[24], the Bell Inequality. It is this desire to keep realism afloat, along with his appreciation of the historical development of 20th century physics, that leads him to the development of a viable form of nonrealism, namely NOA.

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[20] **Kox, A.J. et al (eds)**, *The Collected Papers of Albert Einstein Volume 6, The Berlin Years: Writings, 1914-1917*, Princeton University Press (1997), pg153. Originally found in **Einstein, A.**, *The Foundation of the General Theory of Relativity*, Annalen der Physik, Vol. 49.

[21] **Landé, A.**, "Albert Einstein and the Quantum Riddle", American Journal of Physics, Vol. 42 (Jun 1974), pg 460

[22] **Mermin, N. David**, "What's Wrong With This Pillow?", Physics Today (April 1989), pg 9

[23] **Fine, A.**, "Antinomies of Entanglement: The Puzzling Case of The Tangled Statistics", Journal of Philosophy, Vol. 79 (1982), pp733-747

[24] **van Fraasen, B.**, "The Charybdis of Realism: Epistemological Implications of Bell's Inequality", Synthese, Vol. 52 (1982)

§2.2: Fine's Philosophical Objections to Scientific Realism

§2.2.1: The Scientific Realist Begs the Question

From a philosophical perspective, the first section of Fine's paper on NOA deals with arguments that have been put forward in support of scientific realism. Overall, he finds the methodology of the realist to be unsatisfactory:

> Indeed, this schema of knots in the realist argument applies across the board and vitiates every single argument at the methodological level. Thus my conclusion here is harsh, indeed. The methodological arguments for realism fail, even though, were they successful, they would still not support the case. For the general strategy they are supposed to implement is just not stringent enough to provide rational support for realism.[25]

On his way to coming to this conclusion, Fine recalls the work of Hilbert and Gödel,[26] two famous mathematicians. Hilbert attempted to devise a system by which the consistency of a mathematical theory could be shown using the most stringent methods possible. Unfortunately for Hilbert, Gödel showed that such an attempt was doomed to fail. Nevertheless, Fine sees a great deal of merit in Hilbert's intentions. He believes that:

> to argue for realism one must employ methods more stringent than those in ordinary scientific practice. In particular, one must not beg the question as to the significance of explanatory hypotheses by assuming that they carry truth as well as explanatory efficacy.[27]

What I believe Fine is saying here is twofold. Firstly, realism is a philosophical position by which we can analyse whether or not the theories we have in science form an accurate representation of what *really* takes place in the world 'out there', independent of us. The realist must try to show that the truth obtained by scientific practice is at least close to the blueprints of the universe (not to mention that they must also show that there actually is a world 'out there', that there actually are blueprints).[28] When the realist turns his attention to scientific practice itself, the realist must produce a theory of scientific practice that is at least approximately true. However, even if this theory provides a good explanation for the observations, it in no way ensures its truth.

[25] *The Shaky Game*, pg 122
[26] *The Shaky Game*, pg 114-115
[27] *The Shaky Game*, pg 115
[28] It has come to my attention that the use of the word 'blueprint' strongly suggests that the design plans for the world came before the world itself, contrary to the realist view of science which says that nature was here first and that science is a description of such an objective reality. Therefore, as a point of clarification, when I (and presumably Fine) mean when we use the word 'blueprint' is that it is 'a complete and perfectly accurate description of the workings of an objective reality'.

What Fine is saying is that the realist has smuggled truth into all good explanations and hence the realist's explanation of science must be true, which amounts to question begging. In other words, a realist is always going to say that a good explanation is at least approximately true, because that is what realists do, and hence their good explanation of scientific practice must be true. This question begging methodology is in no way mirrored by science itself. One would not allow a scientist to use a piece of data explained by a theory to be used in the derivation of that same theory. For example, Einstein knew that his general theory of relativity would have to explain the 43" per century precession of Mercury that exists over and above the known Newtonian precession effects due to Newtonian gravitational perturbations by the other planets. However, it is because Einstein did not use this fact in the derivation of the theory that we are able to use Mercury's precession as a test for the theory. This would not be possible if this extra element of Mercury's precession was used in the 'recipe' of general relativity, the theory would predict this extra 43" per century precession of Mercury whether the theory was correct or not since the theory is already assumed to explain this fact.[29]

It is in this area of the explanation of explanations that Fine can see problems with both the realist and anti-realist positions. The realist conditions seem to be too strong for it to handle, and vice versa, the anti-realist is too weak for its own good. By this I mean that the realist is not only trying to achieve a theory that is just empirically adequate, but a theory that is actually true, one that corresponds directly to the universe's blueprints. This is a very lofty task, and while the intentions are commendable, it is realism's own question begging when attempting to describe scientific practice that means it falls short of its own high standards. On the other hand, antirealists modify the concept of truth to their own liking, assuming they use it at all ("they fix the concept of truth, pinning it down to acceptance"[30]) and as such, even if their explanation of scientific practice were to be successful, it would mean that their description could not be true of scientific practice, but merely (say) empirically adequate. Fine's NOA takes the middle ground between these two positions in saying that science does what it does, nothing more, nothing less.

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[29] **Snyder, Laura J.**, "Is Evidence Historical?", found in **Curd, Martin & Cover, J.A.**, *Philosophy of Science: The Central Issues*, W. W. Norton and Company (1998), pg 465

[30] *The Shaky Game*, pg 140

Second, and closely related to the first, Fine thinks that the methodology of the realist, and the anti-realist for that matter, is too ‘hand wavy’, i.e. it is not stringent enough. When compared to the stringent check, double check, triple check methodology that is employed in scientific practice, the performance of the scientific realist looks dismal. The scientific realist must bring in notions such as approximate truth, which are fuzzy at best. A scientist cannot possibly know if a particular theory has reached the status of approximately true since this requires a comparison with the actual ‘blueprints of the universe’, something no scientist would ever know they had. I think what Fine would want to see, in line with Hilbert’s idea, is either the development of a logically sound scientific realism position built up from first principles, or to abandon the idea of an overarching description of science in this manner. It is this second option Fine has chosen which takes the form of the natural ontological attitude which is discussed in depth in section four of this chapter.

§2.2.2: Scientific Realism and the Problem of Conjunctions

To compound the problems caused by using a term such as ‘approximate truth’ is the problem of conjunctions which deals with how we can know if the conjunction of two theories is reliable:

> If T and T’ are independently well-confirmed, explanatory theories, and if no shared term is ambiguous between the two, then we expect the conjunction of T and T’ to be a reliable predictive instrument (provided, of course, that the theories are not mutually inconsistent).[31]

A realist would respond by saying that if T and T’ are approximately true, and that there is nothing inconsistent between the two theories, then their conjunction must also be approximately true, and hence produce reliable predictions.[32] This means that a realist would be happy to combine, say Newtonian mechanics (T) and electromagnetism (T’) to find the behaviour of an electron in a synchrotron (up until the relativistic limit of course). Scientists in general are happy to do this now, but this is only because the conjunction has actually been empirically verified. Without such a verification, the realist must make the question begging move of assuming that their theories are already approximately true, and from there the argument becomes a mere formality. There is

[31] *The Shaky Game*, pg 120
[32] *Ibid.*

nothing within the logic of well confirmed theories that leads to this conclusion deductively. We can look at an historical example where T is Maxwell's electromagnetism and T' is Galilean relativity, both of which were well confirmed in their day. However, applying Galilean relativity to electromagnetism should mean that the speed of light changes depending on your own speed, i.e. $c' = c \pm v$ where c' is the observed speed of light, c is the speed of light for a stationary observer (i.e. stationary with the ether) and ±v is the speed through the ether. The Michelson-Morley experiment showed that such a conjunction is false. We now take the conjunction between electromagnetism and special relativity to be successful, but only because the conjunction has been empirically verified and not just because both theories are highly corroborated in their own right. In this case, the problem arose because Galilean relativity was being applied to a situation outside its range. Let us say the two theories T and T' are being applied within their appropriate range, does this help? Not always, the more complicated a situation, and hence the more theories that need to be conjoined, the less accurate the conjunction is likely to be. One simply needs to compare the difference in difficulty found in predicting weather systems over that of a simple pendulum experiment. A small change in a variable in one theory used in weather prediction has a flow-on effect through the others and the error becomes multiplied.

Fine concludes that even in the best case scenario, if both approximately true theories T and T' were incorrect by an amount ε for a certain parameter, the conjunction of T and T' would lead to a theory that was within 2ε of the correct value for the parameter in question. Furthermore, in response, the realist is unable to confirm that the conjunction has not left us worse off, logically because there is nothing within the logic of realism that says this can be done, and empirically because the realist does not have access to the Truth to be able to compare their theory conjunction with[33] (plus theory conjunction would seem pointless if you already had the Truth).

§2.2.3: Scientific Realism and the Small-handful Problem

Fine also believes that realism is inadequate when facing the small handful problem. This small handful phenomenon is as follows: at any given time, there are only a small handful of competitor theories competing to describe the same phenomena, theories

[33] *The Shaky Game*, pg 121

which in general bear a strong family resemblance, and maintaining well confirmed aspects and only deviating in the less confirmed aspects of the theories. This phenomenon raises three questions, which are as follows:

> (1) Why [is there] only a small handful [of theories ever under consideration] out of the (theoretically) infinite number of possibilities? (2) why [is there] the conservative family resemblance between members of the handful? and (3) why does the strategy of narrowing the choices in this way work so well?[34]

The realist is said to respond in the following manner: If current theories are at least approximately true, then modifying these theories by keeping all that the theory entails in its well confirmed components and adjusting the less confirmed components slightly, the successor theories will be an even better 'approximately true', edging ever closer to actual truth. However, Fine believes:

> [t]he realist has no answer for the first of these [questions], begs the question as to the truth of explanatory hypotheses on the second, and has no resources for addressing the third.[35]

Let us see why Fine thinks this way.[36] With regard to the first question, he believes the realist does not deal with the issue at all. The realist recognises there is a handful, but makes no comment on its size, or why. The question begging nature of the response for the second question lies in the phrase "if current theories are at least approximately true". By doing so, the realist has already confirmed his beliefs before going on to explain why his beliefs are any good. Here Fine has drawn from Larry Laudan's paper called "A Refutation of Convergent Realism"[37], section six of which deals with the retention of previous successful theory components in later theories. Laudan argues that there is no necessary reason why later theories must use the ontology of earlier theories and that earlier theories do not need to be the limiting cases of newer theories while using historical examples to back up his case. This makes perfect sense since if one branch of this theory family tree happens to be moving in the wrong direction, it is more profitable to find another branch that provides a better explanation rather than continue along a defunct path. For the third question, even though realists appear to put a lot of weight into the answer to this question, but it is really only resting on the pillow of approximate truth. A realist wants to say that this method is successful because

[34] *The Shaky Game*, pg 117
[35] *The Shaky Game*, pg 119
[36] *The Shaky Game*, pg 117-119
[37] **Laudan, Larry**, "A Confutation of Convergent Realism", Philosophy of Science, Vol. 48 (1981), pp 19-49

scientists carry over the successful parts of old theories into new theories while the less successful parts are modified to improve the knowledge over the well-confirmed ground. However, Laudan has shown this is far from a foolproof strategy. Fine believes the realist cannot adequately account for the success of novel predictions, especially when the typical 'guess and check' method usually fails. The problem for the realist is how to explain the occasional success of a strategy that usually fails.

To add insult to injury, Fine believes that the instrumentalist, the archenemy of the realist, is able to deal with two of these three questions.[38] The instrumentalist, with their focus on empirical adequacy, recognise that it can be very difficult to come up with an alternative theory that accounts for the data at least as well as current theories, and hence the handful will always be small. Also, the instrumentalist attitude of "if it has worked before, try it again" means that this small handful is going to come from what they know best, which is previously confirmed theories, and therefore there will be a strong family resemblance. With regard to the third question, the instrumentalist must just say that science works and leave it at that since they cannot draw on the continuance of truth between theories, the emphasis is on the numbers, and only the numbers. Therefore, Fine says that the instrumentalist is able to give an account of two of the three small handful questions while the realist completely strikes out.

§2.3: Fine's Philosophical Objections to Anti-realism

§2.3.1: Truth-mongering

If Fine is not a realist, he must be an anti-realist, right? No, things are not as black-and-white in a debate as complicated as this one. In his paper "And Not Anti-realism Either"[39], the chapter that follows "The Natural Ontological Attitude", Fine wants to assure his readers that he is not an anti-realist either and proceeds by discussing the anti-realist's concept of truth. As alluded to earlier, he claims that they replace the notion of truth to such that it suits their needs and then take this new criterion into their anti-realistic theory:

> Such, indeed, is the way of these antirealisms: they fix the concept of truth, pinning it down to acceptance. One certainly has no more warrant for imposing this constraint on

[38] *The Shaky Game*, 119-120
[39] *The Shaky Game*, Chapter 8

> the basic concept of truth, however, than the operationalist has for imposing his constraints on more derivative concepts (like length or mass).[40]

Or to put it more bluntly:

> It seems to me that the acceptance idea never *can* get off the ground, and that we cannot actually understand the picture of truth that it purports to offer.[41]

He believes that an antirealist position such as behaviourism leads us into an infinite regress since it requires us to extrapolate from how acceptance behaviour currently is to how it would be in an ideal situation, with the problem being that trying to find out how the acceptance behaviour is currently would require extrapolation from yet another acceptance situation. Then there is also the problem of behaviourism's attempt at drawing limitations where none need to exist, 'making out everything it touches to be less than it is'.[42]

§2.3.2: Constructive Empiricism and the Line of Observability

This is the problem Fine sees with the truth mongering antirealist positions but not all anti-realist positions are truth mongering. One example of this is van Fraassen's constructive empiricism[43], which says that:

> Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate.[44]

Fine takes issue at constructive empiricism's notion of what is observable and where the line of observability is to be drawn. van Fraassen believes that we must let science decide what is observable; we must look to the theory itself. According to Fine, if one lets science decide what is to be considered observable, then one must be able to scientifically measure a physical property called 'observable'. But one must observe an object to observe thus its observability, so for science to decide what is to be considered observable would lead us into a regress or a circle. This is not to say that this line of thinking is wrong *per se* (as long as the property 'observable' is defined in terms of its other properties that fall within certain observable ranges), it is just not very helpful since saying something is observable because it is observable does not tell us much. The

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[40] *The Shaky Game*, pg 140
[41] *The Shaky Game*, pg 141
[42] *The Shaky Game*, pg 140
[43] **van Fraassen, B.**, *The Scientific Image*, Clarenden Press Oxford (1980)
[44] *The Scientific Image*, pg 12

line of observability itself is fuzzy, just like that of approximate truth earlier. It shifts with new and potential changes in technological capability, and the concept of measurement is yet to be pinned down, the fact the quantum measurement problem still exists is a testament to this. Fine goes further and wonders why we need an observability line at all[45], after all, it is entirely plausible that an entity can exist and be detectable, but not observable, where 'observable' refers to entities directly accessed by our senses and 'detectable' refers to those entities that can be accessed using a machine. There is no reason why we have to restrict ourselves to the belief in only that which is observable; belief could reasonably extend to those entities which are detectable also.[46]

Fine's overall discontentment with the results of the scientific realism debate is captured in the following passage:

> I can well understand how the sight of realism unveiled might bring on disturbing, metaphysical shudders. And it's understandable, I think, that we should seek the seeming-security provided by sheltering for a while in a nest of inter-personal relations. But it would be a mistake to think that we will find truth there. For the antirealism expressed in the idea of truth-as-acceptance is just as metaphysical and idle as the realism expressed by a correspondence theory.[47]

Realism tries to reach out to a truth ideal that cannot possibly be reached while anti-realism tries to replace truth, removing the need to venture outside their comfort zone.

Since he is neither a realist nor an anti-realist Fine considers himself a non-realist. He believes that science in itself is in no need of an overarching philosophical interpretation, and that asking about the aim of science is akin to asking about the meaning of life,[48] and that we are to take science on its own terms without trying to read anything into it.[49] It is to this non-realist attitude that we now turn with a discussion of the natural ontological attitude itself.

§2.4: The Natural Ontological Attitude

We now come to what Fine considers to be the answer to the scientific realism debate. NOA appears very simplistic at first glance, but this is its beauty and is also quite

[45] *The Shaky Game*, pg 145
[46] *The Shaky Game*, pg 146
[47] *The Shaky Game*, pg 139-140
[48] *The Shaky Game*, pg 148
[49] *The Shaky Game*, pg 149

profound. Fine is well versed in philosophy of science, and also in modern physics, which leads him to being fully aware of the fact that trying to package science into a neat, self-contained unit is bordering on mission impossible (if not there already). In a nutshell, NOA resonates with Einstein's quote: "the whole of science is nothing more than a refinement of everyday thinking".[50] Just as our worldviews are free to change and become refined with the inclusion of more information, so should those of science. NOA says that we should not be pinned to the theories of the past or remain within a set practice framework, but that science should be free to adapt and change as needed.

§2.4.1: What is the Natural Ontological Attitude?

The natural ontological attitude is Fine's homely, non-realist solution to the scientific realism debate. It says that we are to "accept the evidence of one's senses and, *in the same way*, to accept confirmed scientific theories"[51]. What does this actually mean? Fine continues:

> It is to take them [scientific theories] into one's life as true, with all that implies concerning adjusting one's behavior, practical and theoretical, to accommodate these truths. … When the homely line asks us, then, to accept the scientific results "in the same way" in which we accept the evidence of our senses, I take it that we are to accept them both as true. I take it that we are being asked not to distinguish between kinds of truth or modes of existence or the like, but only among truths themselves, in terms of centrality, degrees of belief, or such.[52]

This is intended to be a core position that both the realist and the antirealist can subscribe to. Both the realist and the anti-realist are to recognise that the knowledge gained by scientific means is really no different from that gained through everyday experience. This means that they are still able to recognise the difference between well confirmed knowledge and that which is less confirmed, but they should recognise that there is nothing special about scientific knowledge, it is still knowledge. The difference between the realist and antirealist, however, is what they add to this core position. The antirealist wants to import their own brand of truth, while the realist wants to add foot-stamping and convincing shouts of "really!" as in "electrons really exist, really!"[53] Fine

[50] *The Shaky Game*, pg 173
[51] *The Shaky Game*, pg 127
[52] *Ibid.*
[53] *The Shaky Game*, pg 128-129

sees neither of these options as suitable and hence proposes that this natural ontological attitude is considered as a California pure, no additives, core philosophical position all by itself.[54]

In the "Afterword" of *The Shaky Game*, Fine describes eight hallmarks of NOA's approach[55]. They can be summarised as follows:

1. *Attitude*: NOA is an attitude towards scientific discourse, or philosophical inquiry in general, rather than philosophical position intent on getting hung up describing it.
2. *Trust*: A NOAer both trusts that scientists are people who are making a sincere effort to try to understand the world and believes that science is the best method for developing this understanding. This is not a naïve trust though, as a NOAer is also open to critical discussion regarding the effectiveness of scientific practice. A NOAer also trusts that science is open to criticism not only from scientists, but also from those involved in other areas of inquiry such as the social and ethical aspects of scientific development.
3. *Anti-essentialism*: NOA places a strong emphasis on the openness of science which is applied in two ways. Firstly, it means that none of the scientific disciplines have a special essence, i.e. physics having a physics-ness, presumably making any attempt at reducing all sciences to physics, or any other discipline, a useless enterprise. Secondly, NOA sees concepts regarding the aims of science such as explanation, rationality, truth, etc. as being applicable to local, specific contexts rather than being able to apply a general truth essence, say, to scientific theories.
4. *Philosophy's Agenda*: Since NOA is anti-essentialist, i.e. focuses on local and context specific inquiry, it requires that philosophers branch out from their comfort zone and engage meaningfully with those from other disciplines. This is especially true in philosophy of science where philosophical inquiry should not only be guided by actual scientific progress, but also by those in sociology, political science, ethics, etc. No doubt, it is difficult to branch out and learn new disciplines, but such an effort is required if philosophy is to be seen to be

[54] *The Shaky Game*, pg 130
[55] *The Shaky Game*, pp 173-180

productive and useful in the real world, rather than simply the snobby domain of the ivory tower.

5. *Philosophical Reason*: NOA was inspired partly by the failure of the more philosophically rigid forms of inquiry found in philosophy. What NOA proposes is that we use resources such as induction and intuition more heavily in philosophical inquiry. This is not to say that it is wrong to use more restrictive types of reasoning, like deductive logic, but that if science is a refinement of everyday thinking, then the everyday modes of thinking such as induction and intuition should play a larger role in the philosopher's discourse.
6. *NOA's Natural*: In saying that NOA is natural, Fine is not trying to convey that science has its own science-like nature. In fact, Fine is trying to say the complete opposite; there are no science-specific secrets and that because science is a refinement of everyday thinking, i.e. science is a refinement of our *natural* ways of thinking, that there is nothing special about scientific knowledge that no other type of knowledge possesses. To the NOAer, there is no science 'essence' and to dissect science in the hope of finding it is not only hopeless, but detrimental.
7. *Normativity*: Continuing with the anti-essentialist theme of NOA, a NOAer will claim that there is any dualism between facts and values is unjustifiable. Usually it is thought that one is unable to get norms out if norms are not put in to begin with. NOA, with its emphasis on simply wanting to get things done, says that the dualism between facts and values is mostly a creature of the imagination and that what we should be doing is simply looking at what science produces and developing the required values from that. Again, in the spirit of NOA, this is to focus on the local and context specific issues at hand rather than requiring general norms to be developed before anything of practical benefit can emerge.
8. *The global and the local*: Contrary to appearances, NOA is not trying to establish a dualism between global and local ways of thinking; NOA is simply trying to establish that they are different scales from which to tackle a problem. Rather than attacking all problems from the top down, NOA suggests that dealing with each problem locally is the way to go, and if a generality surfaces across similar problems, then so be it. In fact, NOA even suggests that we do look for these generalities, for that will bring coherence to philosophy. However, this is to be

> achieved by looking for answers (and new questions) in the local nooks and crannies first, and then bringing coherence to it all later.

Now that we have an understanding of what Fine meant when explicating NOA, we now turn to how Fine defends NOA's adequacy as an attitude towards science in particular.

§2.4.2: Fine Defends NOA's Adequacy

To show why NOA is an adequate position on its own, Fine goes on to say what NOA actually means in terms of ontology. He says:

> NOA sanctions ordinary referential semantics and commits, via truth, to the existence of the individuals, properties, relations, processes, and so forth referred to by the scientific statements that we accept as true[56]

where using ordinary referential semantics means that we are to:

> treat truth in the usual referential way, so that a sentence (or statement) is true just in case the entities referred to stand in the referred-to relations.[57]

Let me expand on what is meant by 'the usual referential way' by having a look at the sentence 'Snow is white'. Under Tarski's Convention T an adequate theory of truth for a language L must imply, for each sentence ϕ of L, $\lceil\phi\rceil$ is true if and only if ϕ. In other words, our sentence 'Snow is white' is true if and only if snow is white.[58] This seems very simple but this is part of the beauty of NOA. To accept NOA means that we are able to avoid getting bogged down with theories of truth; scientific statements simply say what they say. In our sentence, 'snow' refers to what we call snow, while 'white' refers to a property we call white. However, 'snow' is a very classical, macroworld notion, and 'white' is a very observable property, if it was not observable, we would not see it as white. Does it make a difference when we move into the microworld? Let us look at the sentence 'The electron has charge' which is very similar to the 'Snow is white' sentence in that it is a simple thing-property relation. (I have used 'thing' instead of 'object' since 'object' implies the electron actually exists whereas NOA does not want to tie itself down to the existence or non-existence of particular particles). Under NOA, one would recognise that scientists have classified a concept which has been labelled as 'electron'. This concept may or may not refer to a particle that actually exists, but scientists are nonetheless trying to understand how this 'electron' behaves in terms of other concepts

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[56] *Ibid.*

[57] *Ibid.*

[58] **Glanzberg, Michael**, *Truth*, Stanford Encyclopedia of Philosophy (2006), Section 2

used in science. One of the ways they do this is by using another concept known as 'charge'. Again, 'charge' may or may not exist, but it is useful for describing the electron's behaviour in electric and magnetic fields. In other words, since attributing a 'charge' to a supposed entity 'electron' provides us with a good explanation of the behaviour of the electron, we are therefore committed to the link between electrons and charge. However, this does not tie us to the link between electrons or charge and the universal blueprint. To be committed to such a link between electrons and the universal blueprint is known as a correspondence theory of truth which Musgrave equates with the Davidsonian-Tarskian theory and hence sees NOA as nothing but realism (discussed in detail in chapter three). Musgrave appears not to have realised that a connection between electrons and charge, or the moon and its fullness in his case, in no way ensures the connection between these entities and the universal blueprint. Fine believes that when a realist claims this connection between scientific concepts and the blueprint, the realist is chasing nothing but a fantasy:

> If I am right, then the realist is chasing a phantom, and we cannot actually do more, with regard to existence claims, than follow scientific practice, just as NOA suggests.[59]

This reference scheme, the one in which we are to treat truth in the usual referential way, allows one who holds to NOA (a NOAer) to remain uncommitted to scientific progressivism which is inherent to realism.[60] When theories and paradigms change, a NOAer sees no cause for alarm. The NOAer sees no problem in replacing a strand or two of the web of knowledge because, unlike the realist, he does not think of this web as having a sacred aura since a NOAer does not commit themselves to the connection between the web and the universal blueprints. It could be the case that a strengthening of some strands in the web, i.e. an improved explanation in one area of science, could lead to an overall weakening of the web since the new concepts introduced could clash with those already in place in other areas. This would make a realist uneasy; all change must be progressive in the eyes of the realist. However, a NOAer recognises that science is a living and ever-changing entity; it is capable of repairing itself. Science can only do the best it can with the resources it currently has; as knowledge grows, revision takes place, but until then science will use what it currently has recognising that it does not have to be perfect. It is its simplicity, its 'science says

[59] *The Shaky Game*, pg 132
[60] *The Shaky Game*, pg 130

what it says' sort of attitude that shows us how minimal an adequate philosophy of science really is.[61] When NOA says that everyday truths are on par with scientific ones, it is also saying that "no other additions are legitimate and none are required"[62]. So, what does it mean to say that something is true under NOA? NOA's stubborn refusal to amplify truth says that we are to look at historical circumstances that allow us the check the logical relations between sentence elements, and only this, because this is all we can say.[63]

§2.4.3: NOA and the Conjunction Problem

We then come full circle, returning to the small-handful and conjunction problems. With regards to the small-handful problem,[64] Fine recognises that even though guessing based on the confirmed parts of current theories is better than a blind guess, success comes relatively rarely and hence we should not put the same emphasis on the positive progression of science that the realist does. For the conjunction problem,[65] NOA says that yes, it is reasonable to add two consistent, well-confirmed theories together since new truths may be found, but this new conjunction must be tested before its truthfulness can possibly be declared and one must recognise that success is certainly not guaranteed. Fine does not say much on these issues - merely a paragraph combined, for not only the small-handful and conjunction problems together but also their relationship with NOA. If I may try to speak on Fine's behalf here, I wish to look at the conjunction problem, more specifically, what happens if T="light is a wave" and T'="light is a particle". To conjoin these two theories appears to provide a contradiction, yet each statement has merit empirically such that we would scarcely seem able to let either go. The double-slit experiment gives the expected interference pattern one would expect if light was a wave, while the photoelectric effect tells us light delivers energy in little packets, i.e. photons, and the solar radiation affecting comet tails shows us that light has momentum, just like a particle. This conjunction also satisfies the condition that no terms be ambiguous between the two theories, all words mean the same thing in their

[61] *The Shaky Game*, pg 133
[62] *Ibid.*
[63] *The Shaky Game*, pg 134
[64] *The Shaky Game*, pg 132
[65] *The Shaky Game*, pg 132-133

respective theories. Since a realist is aiming for a correspondence form of truth, a realist would look at this conjunction and be displeased. An actual photon cannot actually be both a wave and not a wave; it makes no sense under classical logic. On the other hand, NOA is able to recognise that while yes, this is confusing and probably not quite right, we are nonetheless able to make very successful predictions with the quantum framework we have set up and so we should continue to develop it until the tools become available that can fix it. Notice that there is no insistence that this conjunction must be true, or even approximately true, but since it works so well, we are at least committed to it. Together T and T' highlight the strangeness that abounds in quantum mechanics; it is this strangeness that strengthens Fine's resolve that NOA is the way forward as an 'interpretation' of science.

Now that we have an idea of what NOA means and what it represents, we now move onto objections of NOA that have been put forward, most significantly, those of Alan Musgrave.

Chapter 3: Musgrave's Objections to NOA

***Morpheus**: Welcome to the desert of the real.*

In his paper "NOA's Ark – Fine for Realism", Musgrave puts forward what appears to be a very powerful argument against NOA from a common sense, classical physics point of view. However, as mentioned earlier, Musgrave is not a physicist and appears to have taken "The Natural Ontological Attitude" out of context, i.e. he does not seem to relate it to the rest of *The Shaky Game* which is essential for a proper understanding of Fine's position. Throughout this section, we will see what Musgrave has to say, and attempting to speak for Fine, I will use examples from quantum mechanics where possible to justify Fine's position. In the second half of his paper, Musgrave deals with issues that can be illuminated with knowledge of quantum mechanics which touch on aspects of measurement, for example. I will leave Musgrave's most extensive objection which takes up the first half of his paper, the one regarding truth, until last since I believe the issues raised will allow us to move onto a form of quantum realism that uses NOA as a springboard.

§3.1: Reciprocity, Objective Reality and Bell's Theorem

Sections 3.1 and 3.2 deal with the very closely related issues of reciprocity and contamination which are concerned with whether or not interacted-with objects can be considered to exist independently from us. We begin with the problem of reciprocity and I quote Fine on this in full since the two issues are so closely related. Fine says:

> The difficulty is that whatever we observe, or, more generously, whatever we causally interact with, is certainly not independent of us. This is the problem of *reciprocity*. Moreover, whatever information we retrieve from such interaction is, prima facie, information about interacted-with things. This is the problem of *contamination*. How then, faced with reciprocity and contamination, can one get entities both independent and objective? Clearly the realist has no direct access to his *World*[-in-itself].[66] (Fine's italics).

Musgrave, being the non-physicist that he is, feels that it is safe to extrapolate from humdrum examples, such as looking at the moon, to any other area of science, as

[66] **Fine, Arthur**, "Unnatural Attitudes: Realist and Instrumentalist Attachments to Science", Mind, New Series, Vol. 95 No. 376 (Apr 1986), pg151

esoteric as those areas might be. With regards to reciprocity, Musgrave reasons that by this definition, the only truly independent entities are platonic entities which is too extreme for the average realist. He sums up the average realist's position on this issue as follows:

> When a scientific realist says that the moon is (largely) independent of us, he obviously means that it is non-mental, it exists outside of us, we did not create it, it existed long before we did, it *continues to exist when we are not looking at it*, and so forth.[67] (My emphasis).

This may be fine for an obviously classical object such as the moon since even if I could not see the moon due to cloud cover, I could still test its presence by monitoring the tides, or the wobble of the Earth's axis. Granted, the time taken to make these measurements does not allow me to say the moon exists *now*, but the regularity of the tides and the stability of Earth's axis would suggest the moon's presence remains constant. Because of this, I can safely say that the moon has been around for much longer than I have (previous eye witnesses confirm this, though the problem of contamination is related to this issue) and will continue to be around long after I am gone (assuming no catastrophic collision in the near future). Quantum physics on the other hand depicts a very different story and a brief detour into Bell's theorem will help to illuminate why.

§3.1.1: The Famous EPR Paper

In 1935, Einstein, Podolsky and Rosen developed the EPR argument concerning the completeness of quantum mechanics in a paper called "Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?".[68] In this paper, the completeness and reality conditions are given as follows:

> Completeness: "every element of the physical reality must have a [one-to-one] counterpart in the physical theory."
>
> Reality: "If, without in any way disturbing a system, we can predict with certainty (i.e., with probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity."[69]

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[67] "NOA's Ark – Fine for Realism", pg 393

[68] **Einstein, A. et al**, "Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?", Physical Review, Vol. 47 (May 1935), pp 777-780

[69] "Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?", pg 777

According to Musgrave and Einstein's realist, each entity spoken about in scientific theories exists all the time, always carries all of its properties with it and the scientist should be able to predict these properties with certainty. The current understanding of quantum mechanics, Bell's Theorem in particular, stops such a realist in his tracks. The following discussion shows Einstein's struggle with the weirdness of quantum mechanics and what physics itself has to say on the matter.

As it turns out, Podolsky was the author of the EPR paper and Einstein was unhappy with the way it turned out as stated in a letter to Schrödinger ("the main point was, so to speak, buried by the erudition")[70]. In the same letter, Einstein goes on to clarify what he means by completeness by getting us to imagine a ball that is placed in either one of two boxes. We can make an observation simply by lifting the lid of either box and we are asked if 'the probability the ball is in the first box is ½' is a complete description or not. The Born interpretation says no, we must state which box the ball is in; the Schrödinger interpretation says yes, the ball's wavefunction is 'smeared' throughout the two boxes and the act of measurement puts the ball in a particular box. The third alternative, the Talmudist (referring to Bohr) is not concerned with reality since it is regarded as the 'hobgoblin of the naïve'. Einstein then invokes what he calls the 'separation principle' which says that what happens in the second box does not affect what happens in the first box and that each box is in its own separate state. By doing this, Einstein removes the Talmudist point of view (there are actual boxes and an actual ball) and also Schrödinger's view since opening the first box cannot remove half a potential ball from the second box. Eliminating Schrödinger's view also allows Einstein to keep completeness since having one wavefunction describe two balls as in the Schrödinger case, when at most one of them actually exists, ruins the one-to-one relationship required by the completeness condition. It is this invoking of the separation principle that gets to the heart of Einstein's problem with quantum mechanics which is, as Don Howard puts it:

> in Einstein's estimation the truly puzzling feature of quantum mechanics, its most objectionable departure from the classical world view, is not its way of treating conjugate parameters, but its way of treating spatially separated, previously interacting systems.[71]

[70] **Einstein** to Schrödinger, 19 June 1935 (trans. Don Howard)
[71] **Howard, Don**, "Einstein on Locality and Separability", Stud. Hist. Phil. Sci., Vol. 16 No. 3 (1985), pp 181

So, the heart of Einstein's problem with quantum theory was how quantum mechanics treated spatially separated systems and how it conflicted with the separation principle. In that same letter to Schrödinger, Einstein lays out his proof for the incompleteness of quantum mechanics. Take an experimental set-up, not unlike figure 2 below, where two initially interacting particles, α and β, become spatially separated and one is detected by detector A and the other by detector B. Let it be the case that detectors A and B can measure any observable we please, whether it is spin, position, momentum, etc. If A were to measure momentum, say, quantum mechanics says we are able to assign a wavefunction for the measurement at B that concerned momentum *only*, labelled ψ_B. By changing the observable being measured at A, we obtain a different wavefunction at B, namely $\psi_{\underline{B}}$. However, all of these wavefunctions, one for each observable, describes one and the same particle, namely β. In Einstein's words:

> Now what is essential is exclusively that ψ_B and $\psi_{\underline{B}}$ are in general different from one another. I assert that this difference is incompatible with the hypothesis that the ψ description is correlated one-to-one with the physical reality (the real state). After the collision, the real state of ([αβ]) consists precisely of the real state of α and the real state of [β], which two states have nothing to do with one another. *The real state of* [β] *thus cannot depend upon the kind of measurement I carry out on* [α]. ('Separation hypothesis' from above.) But then for the same state of [β] there are two (in general arbitrarily many) equally justified ψ_B, which contradicts the hypothesis of a one-to-one or complete description of the real states.[72]

Note that the separation principle invoked here is so important to Einstein that he has managed to reduce the EPR argument from the original four pages to what it was meant to say using a single paragraph. From a classical point of view, Einstein's emphasised sentence:

> *the real state of* β *thus cannot depend upon the kind of measurement I carry out on* α

agrees perfectly with our everyday experience. For example, after hitting a golf ball, we cannot perform a certain set of actions with the golf club to get the ball to jump sideways and avoid a water hazard. The behaviour of the golf club and ball are independent of each other after their initial contact; to deny this is to believe in action at a distance which is frowned upon in Newtonian mechanics. Also, the next sentence in Einstein's letter:

[72] **Einstein** to Schrödinger, 19 June 1935

> But then for the same state of [β] there are two (in general arbitrarily many) equally justified ψ_B, which contradicts the hypothesis of a one-to-one or complete description of the real states,

is also very intuitive from a classical point of view. To assign a different wavefunction according to the observable measured, such as ψ_B or $\psi_{\underline{B}}$, is akin to saying the golf ball changes depending on its lie (the fairway, a bunker, out of bounds etc.), or even stranger, where the golf club is, when classical notions say the ball is still the same ball, independent of where it lands or where the golf club is.

§3.1.2: Modified EPR Thought Experiment

We now turn to the quantum version of what initially seems like the ball in the box problem above, but when examined in greater detail, it highlights the place of locality in quantum mechanics. It consists of a simplified version of the EPR thought experiment which was devised by Bohm[73], the optical version of which was carried out by the Aspect group.[74] The version using electrons is given below (simply replace 'spin' with 'polarization' to get the optical version).

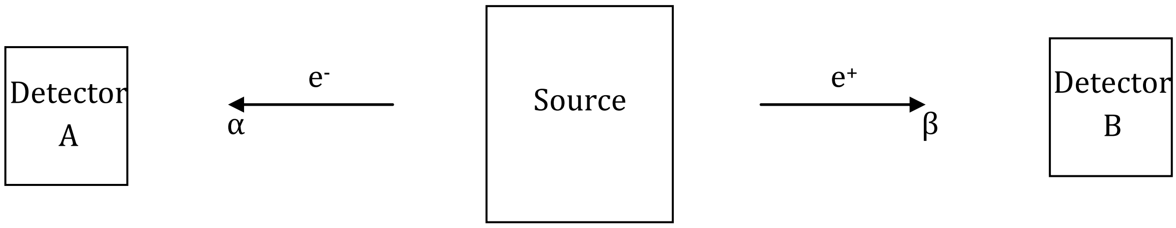

Figure 2: EPR Experiment

Imagine a source that holds neutral pi mesons decaying to an electron (α) and a positron (β) such that the following reaction takes place: $\pi^0 \rightarrow e^- + e^+$. This source is placed in between two detectors, A and B, which cannot communicate with each other and each consist of a Stern-Gerlach magnet and a screen so they are capable of measuring intrinsic spin. By conservation of momentum and angular momentum, α and β have the opposite momentum and, crucially for this experiment, intrinsic spin values, i.e., if α is spinning up, β will always be measured as spinning down along the same axis. If we set detectors A and B to measure spin along the same axis, this becomes

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[73] **Bohm, D.**, *Quantum Theory*, Prentice-Hall (1951), pp. 211-223

[74] **Aspect, Alain et al**, "Experimental Test of Bell's Inequalities Using Time-Varying Analyzers", The American Physical Society (1982), pp 1804-1807

equivalent to the ball in a box problem described above. Knowing the detectors are aligned along the same axis, we could measure α's spin value and by conservation of angular momentum say that β has the opposite spin. This is akin to saying that if the ball is not in box 1 it must be in box 2. The realist explanation that these particles always had spin values, or the ball was always in box 2, is perfectly consistent with this outcome, hence Musgrave would still be happy with holding his position. However, we are able to measure the spin of these particles along any axis we care to choose, let us label these axes $\boldsymbol{a}, \boldsymbol{b}, \boldsymbol{c}$. Fine becomes vindicated when we consider the probabilities of outcomes when dealing with multiple axes, as we shall soon see. What makes intrinsic spin a truly quantum effect is that no matter which axis we decide to measure along, we will always obtain the same value for angular momentum, $\pm\hbar/2$, whereas classically we would expect a maximum value when the axis was parallel to the angular momentum vector, and a continuous change as the angle between the axis and angular momentum vector changed.

We now turn to the derivation of Bell's inequality which gives us an expression for what we should expect the outcome of this thought experiment to be from a somewhat classical point of view.

Bell's Theorem[75]: Let us label the outcome of the measurements at detectors A and B as $A(\boldsymbol{a}, \lambda)$ and $B(\boldsymbol{b}, \lambda)$ respectively. Since there are only two possible outcomes at each detector, namely spin up and spin down, $A(\boldsymbol{a}, \lambda)$ and $B(\boldsymbol{b}, \lambda)$ take on the values ±1, +1 if spin up is measured and -1 if spin down is measured. The use of λ is inspired by the many attempts at formulating a 'hidden variables' formulation of quantum mechanics, which in turn was inspired by the original EPR paper.[76] In Bell's paper, λ is used to represent any set of variables we like. It could simply be the collection of concepts we normally use, such as position, momentum, time, etc. λ could also refer to the set of these 'normal' concepts plus a selection of 'hidden variables' that would be used to

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[75] The mathematical components for this derivation is a combination of those found in **Griffiths, David J.**, *Introduction to Quantum Mechanics (2nd ed.)*, Pearson Prentice Hall (2005), pp 424-425 and in Bell's original paper **Bell, John S.**, "On the Einstein Podolsky Rosen Paradox", Physics, Vol. 1 (1964), pp 195-200.

[76] The original EPR paper does not actually mention hidden variables; however it is strongly suggested in the last paragraph which states: "While we have thus shown that the wave function does not provide a complete description of the physical reality, we left open the question of whether or not such a description exists. We believe, however, that such a theory is possible".

complete quantum mechanics, or it could refer to the complete overhaul that Einstein longed for, where λ refers to a collection of currently unknown concepts. It is this generality that is allowed for in using λ that makes Bell's theorem so powerful.

We can then define $P(\boldsymbol{a}, \boldsymbol{b})$, which is the average of the product of the spins such that if $A(\boldsymbol{a}, \lambda) = +1$ and $B(\boldsymbol{b}, \lambda) = -1$ then the product would be $+1 \times -1 = -1$, as:

$$P(\boldsymbol{a}, \boldsymbol{b}) = \int \rho(\lambda)\, A(\boldsymbol{a}, \lambda) B(\boldsymbol{b}, \lambda) d\lambda$$

which is the generic form for a probability distribution in quantum mechanics. ($\rho(\lambda)$ is known as the probability distribution function and must satisfy $\int \rho(\lambda) d\lambda = 1$ since the probability of getting a possible outcome is 1, i.e., it is certain). What is important to note is that by writing $A(\boldsymbol{a}, \lambda)$ and $B(\boldsymbol{b}, \lambda)$ separately, they are assumed to be independent of each other, i.e., the outcome of the measurement at A is assumed not to influence the outcome at B. This is known as the locality condition and unveils Bell's motive; he wishes to show that *no* local theory of quantum mechanics satisfies experiment.

By introducing a third axis $\boldsymbol{c}$, we can write:

$$P(\boldsymbol{a}, \boldsymbol{b}) - P(\boldsymbol{a}, \boldsymbol{c}) = \int \rho(\lambda)\, [A(\boldsymbol{a}, \lambda) B(\boldsymbol{b}, \lambda) - A(\boldsymbol{a}, \lambda) B(\boldsymbol{c}, \lambda)] d\lambda$$

Due to conservation of angular momentum, we can say that $A(\boldsymbol{b}, \lambda) = -B(\boldsymbol{b}, \lambda)$ since when $\boldsymbol{a}$ and $\boldsymbol{b}$ are aligned, the spin measurements are perfectly anti-correlated, and so we know that $P(\boldsymbol{b}, \boldsymbol{b}) = A(\boldsymbol{b}, \lambda) B(\boldsymbol{b}, \lambda) = -1$. Therefore:

$$P(\boldsymbol{a}, \boldsymbol{b}) - P(\boldsymbol{a}, \boldsymbol{c}) = \int \rho(\lambda)\, [1 + A(\boldsymbol{b}, \lambda) B(\boldsymbol{c}, \lambda)] A(\boldsymbol{a}, \lambda) B(\boldsymbol{b}, \lambda) d\lambda$$

However, $\int \rho(\lambda) d\lambda = 1$, and also the detectors can only measure ± 1, so:

$$\rho(\lambda)[1 + A(\boldsymbol{b}, \lambda) B(\boldsymbol{c}, \lambda)] \geq 0 \;\text{ and }\; -1 \leq [A(\boldsymbol{a}, \lambda) B(\boldsymbol{b}, \lambda)] \leq +1$$

respectively. Therefore:

$$|P(\boldsymbol{a}, \boldsymbol{b}) - P(\boldsymbol{a}, \boldsymbol{c})| \leq \int \rho(\lambda)\, [1 + A(\boldsymbol{b}, \lambda) B(\boldsymbol{c}, \lambda)] d\lambda$$

or more simply, after a little rearranging:

$$|P(\boldsymbol{a}, \boldsymbol{b}) - P(\boldsymbol{a}, \boldsymbol{c})| \leq 1 + P(\boldsymbol{b}, \boldsymbol{c})$$

which is one form of the famous Bell Inequality.

§3.1.3: Application of Bell's Inequality

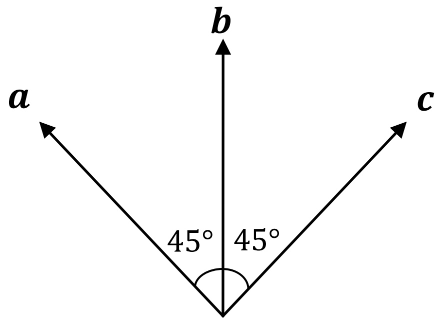

Figure 3: Stern-Gerlach Axes

By analysing a specific case of Bell's Inequality, we shall be able to see whether or not Fine is vindicated in thinking the way he does. Let our axes $\boldsymbol{a}, \boldsymbol{b}, \boldsymbol{c}$ be defined as in figure 3 to the left such that θ_{ac}, the angle between $\boldsymbol{a}$ and $\boldsymbol{c}$, is 90° and $\theta_{ab} = \theta_{bc} = 45°$. Let us also say that the axes to be measured are chosen randomly while the particles are on their way to the detectors. (In the Aspect group experiment this was done by setting up an ultrasonic wave in the water used as a travel medium which created a pseudorandom axis choice). If we are to apply the locality condition that was assumed in the derivation of Bell's theorem, we must say that both particles, α and β, each carry their own spin information with them. This is for two reasons: firstly, according to locality, the particles cannot know which axis their spin is going to be measured along until it gets to the detector, and hence must carry spin information for any possible axis orientation. Secondly, according to locality, the measurement at A cannot tell β what spin was measured and hence give the appropriate (opposite) value if the same axis happens to be measured at B. This information needs to be prearranged as the pion decays and carried with the particles along their path.

What do we expect to achieve if the information sets are carried with the particles? The possible information sets are given in table 1 below:

A	β
(+,+,+)	(-,-,-)
(+,+,-)	(-,-,+)
(+,-,+)	(-,+,-)
(+,-,-)	(-,+,+)
(-,+,+)	(+,-,-)
(-,+,-)	(+,-,+)
(-,-,+)	(+,+,-)
(-,-,-)	(+,+,+)

Table 1: Possible EPR Outcomes

where α having an instruction set of (+,-,+) would mean that if A was set to $\boldsymbol{a}$ or $\boldsymbol{c}$ then spin up would be recorded, whereas spin down would be recorded if A was set to $\boldsymbol{b}$. (β contains the opposite instruction set to α due to conservation of angular momentum). Since Bell's Inequality is given above as:

$$|P(\boldsymbol{a},\boldsymbol{b}) - P(\boldsymbol{a},\boldsymbol{c})| \leq 1 + P(\boldsymbol{b},\boldsymbol{c})$$

calculating each term in turn will be valuable. Looking at axes $\boldsymbol{a}$ and $\boldsymbol{b}$, we see that half of the sets, the product is +1 (for sets 3,4,5 & 6), and for the other half of the sets, the product is -1 (for sets 1,2,7 & 8). Therefore, if we assume the information sets are generated randomly, then $P(\boldsymbol{a},\boldsymbol{b})$, the average of the products, will be zero. Similarly for $P(\boldsymbol{a},\boldsymbol{c})$ and $P(\boldsymbol{b},\boldsymbol{c})$ which both turn out to be zero as well. Putting these into Bell's Inequality we get:

$$|0 - 0| \leq 1 + 0 \quad \Rightarrow \quad 0 \leq 1$$

which is useful since Bell's inequality assumes locality, and hence information sets. However, notice that the values calculated for $P(\boldsymbol{a},\boldsymbol{b})$, $P(\boldsymbol{a},\boldsymbol{c})$ and $P(\boldsymbol{b},\boldsymbol{c})$ are independent of the angles between the axes in question. This is important because quantum physics predicts, and experiment confirms, that $P(\boldsymbol{a},\boldsymbol{b}) = -\boldsymbol{a}\cdot\boldsymbol{b} = -\|\boldsymbol{a}\|\|\boldsymbol{b}\|\cos(\theta_{ab}) = -\cos(\theta_{ab})$ where $\|\boldsymbol{a}\| = \|\boldsymbol{b}\| = 1$ because $A(\boldsymbol{a},\lambda)$, $B(\boldsymbol{b},\lambda) = \pm 1$. One may reply that the angle dependency had been removed when it was assumed that the information sets were generated randomly but to generate information sets consistent with $P(\boldsymbol{a},\boldsymbol{b}) = -\cos(\theta_{ab})$, the pion must decay knowing what randomly generated axes will be measured along when α and β get to their respective detectors, which will be in the future and not necessarily at the same time. In other words, the pion needs to know information that does not exist at the time of decay. By using $P(\boldsymbol{a},\boldsymbol{b}) = -\cos(\theta_{ab})$ and our angles as defined above, we have:

$$|-\cos(\theta_{ab}) + \cos(\theta_{ac})| \leq 1 - \cos(\theta_{bc})$$

$$|-\cos(45°) + \cos(90°)| \leq 1 - \cos(45°)$$

$$|-0.707 + 0| \leq 1 - 0.707 \quad \Rightarrow \quad 0.707 \leq 0.293$$

which is incorrect; Bell's Inequality is predicted to be violated for certain angles and this is confirmed by the Aspect group experiment. Bell's mission has succeeded; he has successfully shown that *no* local theory of quantum mechanics can satisfy experiment.[77]

[77] This is the common interpretation of the result of Bell's theorem and hence would make up some of the quantum weirdness that Fine would be well aware of. However, in §5.3, I look at whether this is a fair

§3.1.4: Implications of Bell's Inequality for Reciprocity

Before our detour into the EPR experiment, we were discussing Musgrave in response to the problem of reciprocity proclaiming that "when a scientific realist says that the moon is (largely) independent of us, he obviously means that ... it *continues to exist when we are not looking at it*...". For a clearly classical object such as the moon, this seems to be no problem as demonstrated earlier. The EPR experiment tells a different story, however, as has just been shown above. It is important to recall the fundamental assumption made in deriving Bell's Inequality, which was the locality condition. This meant that the measurements at A and B are independent of each other, and hence, the particles must carry information sets with them concerning their spin values. Since Bell's Inequality is violated for certain angles, no local theory of quantum mechanics exists, and hence, quantum particles do not carry information sets with them. What does this mean for Musgrave? In the quantum realm, the moon actually does not exist when it is not being looked at; it is the measurement itself that gives the particles their particular spin values, they did not carry them on the way from the source to the detector. This means that reciprocity, which says that what we measure is not independent of us, is a real issue in quantum mechanics; it is our intervention that seems to bring (at least) properties into existence. As a realist, Musgrave would now be displeased. A realist believes that there is an objective world out there, a World-in-itself that is independent of the observer. If particles do not carry their properties with them, or at least the properties that are able to change such as spin, position and momentum, then they do not exist independent of us, the observers. (Frame independent quantities like rest mass may be another issue entirely). We must interact with the particles first before the particles gain their properties (at least temporarily) which destroys the notion of an independent World-in-itself. Fine, knowing what he does about Bell's Theorem and its implications (he spends chapters 3 & 4 of *The Shaky Game* on the EPR Paradox and Bell's Theorem respectively), is definitely justified in being suspicious about a realist interpretation of quantum mechanics.

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interpretation of Bell's theorem and whether or not the scientific realism debate makes too much of this result.

§3.2: Contamination, Objective Reality for Kant and Bohr

§3.2.1: Kant versus Musgrave on Contamination

The other issue that Musgrave raised that was very closely related to reciprocity was that of contamination:

> whatever information we retrieve from such interaction is, prima facie, information about interacted-with things. This is the problem of *contamination*. How then, faced with reciprocity and contamination, can one get entities both independent and objective?[78]

Musgrave takes issue with Fine because "Fine [is] suggest[ing] that, unlike the moon-in-itself, the interacted-with-moon is not objective, not out there in the World."[79] Musgrave goes on to ask: "[w]here is it then, subjective and inside our heads?"[80] Musgrave believes that Fine may hold such a view that the moon is not completely objective because we must traffic in the moon concept which is our invention; to say the moon is full is not a fact about the World-in-itself since the World-in-itself does not recognise 'moon-ness', it is merely a human invention. This is known as conceptual idealism. This slides downhill into conceptual relativism since there would be no common world-as-conceived-by-everyone. An Aristotelian has different concepts to the Newtonian, the Eskimo and the honeybee.[81] According to conceptual relativism, the 'World' changes as our concepts used to describe it change, hence it cannot be a World-in-itself. Musgrave then states that this talk of world's-as-experienced-by-X's need not be taken seriously, and since Fine is not drawn to idealism either (as discussed earlier, he is not a fan of anti-realism in general), Fine and Musgrave are able to find a point of agreement here. Put simply, Musgrave just wants to say:

> The moon-as-experienced-by-us is just the moon – and similarly for all other hyphenated entities (*including* the Kantian moon-in-itself). On this view, 'The moon-as-conceived-by-Aristotelians was perfectly spherical' is just philosopher's gobbledy-gook for 'Aristotelians thought the moon is perfectly spherical.'[82]

He continues by describing what would be the case if we actually did take this idealist notion seriously:

> Consider the moon-as-observed-by-us ($moon_o$ for short) and the moon-in-itself ($moon_i$ for short). Is $moon_o$ identical with $moon_i$? Presumably not … But if $moon_o$ is distinct from

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[78] "Unnatural Attitudes: Realist and Instrumentalist Attachments to Science", pg151
[79] "NOA's Ark – Fine for Realism", pg 393
[80] Ibid.
[81] "NOA's Ark – Fine for Realism", pg 394
[82] "NOA's Ark – Fine for Realism", pg 395

> *$moon_i$* then there is presumably some property which the one lacks and the other possesses. But to know this is to know something about *$moon_i$*, when our knowledge was supposed to be confined to *$moon_o$*![83]

By mentioning this Kantian moon-in-itself, Musgrave has touched on something important, but in doing so, I believe he has thrown the baby out with the bathwater, so to speak. Musgrave says that

> my Kantian friends … tell me that Kant was an 'empirical realist' and only a 'transcendental idealist'. I do not understand these Kantian slogans. … The Kantian metaphysic, seen as it really is, is a form of *idealism*.[84]

Since we are on the topic of the objectivity of the moon, we concern ourselves here with material idealism which declares that the existence of objects outside of us doubtful at best, impossible at worst. With regards to material idealism, Kant spends an entire section in the *Critique of Pure Reason*, the thesis of which states that:

> [t]he mere, but empirically determined, consciousness of my own existence proves the existence of objects in space outside me.[85]

Therefore, we can safely say that Kant did not have material idealist intentions; he did consider the moon-in-itself to be outside of him. But how can we have an objective moon-in-itself while still having to take contamination into consideration? Kant spoke of what I would call three levels of knowing, namely *intuition*, *understanding* and *reason*. As summarised by H.J. Paton:

> Intuition involves an immediate relation to a given individual object, and in human beings intuition is always sensuous and not intellectual, which means that it is passive and not active. Understanding is a power of thinking, by means of concepts, the objects given in intuition. Reason is a power of thinking of objects which can never be given in sensuous intuition.[86]

In other words, *intuition* is the raw sense data, e.g., blobs of green and brown in my field of vision (even though I cannot speak of it as green and brown yet, it is still just the sense data). Then I apply my *understanding*, i.e., I use known concepts such as leaf, trunk, tree, green and brown in such a way to organise this sense data so that I know I am looking at a tree. *Reason* would allow me to know the tree-in-itself. I cannot possibly say what this tree-in-itself looks like since sight requires information gained through intuition, and intuition has no access to things as they are in themselves. Also, to mention the tree-in-

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[83] *Ibid*.
[84] "NOA's Ark – Fine for Realism", pg 396
[85] **Kant, Immanuel**, *Critique of Pure Reason (1787)*, pg B275
[86] **Patton, H.J.**, *Kant's Metaphysic of Experience (Volume 1)*, Allen and Unwin Ltd (1961), pg 73

itself requires me to speak of the concept 'tree', which is our way of organising our *intuitions* of trees, not the 'tree'-in-itself.

§3.2.2: Kant and the Natural Ontological Attitude

How does this knowledge of Kant help us in our understanding of Fine's development of NOA? Applying this understanding to the $moon_i$ and $moon_o$ that Musgrave speaks of, we recognise that $moon_i$ is at the level of reason whereas $moon_o$ is only at the level of understanding. To put this in more NOA related language, $moon_i$ would be found in the universal blueprints itself whereas $moon_o$ would be found as a concept in the web of knowledge created by scientific practice. Since we see the world through concept-tinted glasses, we cannot speak of intuitions themselves providing the strength for the connections in the web of knowledge. It is only after we have filtered the intuitions through our known concepts that we can say they provide the 'strength' in the relations between our concepts.

How would Kant have reacted to what NOA proposes? I believe he would have been at least accepting of the general principle behind it. Kant believed that there were things-in-themselves out there in the world, but we cannot know of these things independent of our experience of it, and as such, Kant would declare to be incorrect the common-sense realist's belief that they had latched onto reality as it really is. In other words, Musgrave wants to say that we can read nature as it really is through experiment, whereas Kant would say that we see the world through concept-tinted glasses rather than as it really is. Therefore, Kant would have said that there is no direct link between the web of knowledge and blueprints themselves, which is exactly what NOA proposes. Kant recognised that we must organise the sense data we receive in terms of concepts we understand, and that there is every chance (read: almost guaranteed) that we have not precisely locked on to an understanding of what the object in question really is. This is mirrored exactly in NOA where Fine says that yes, we need concepts to describe what we are doing/looking at/manipulating, but these concepts can be wrong and should be free to change as our understanding of nature improves. It should come as no surprise that Kant would feel at home with NOA, after all, NOA is designed to be a core position that both the realist and anti-realist can subscribe to. What is to be emphasised here is that this is far from the "I do think that Fine's NOA should have nothing to do with these idealisms"[87] type of attitude that Musgrave spent a great deal of time emphasising in his paper.

This may all seem very picky if all we are considering are humdrum examples such as a full moon. After all, the moon is a large, bright ball surrounded by a great deal of darkness so it comes as no surprise that we have carved the concept 'moon' out of nature. The problem is, once again, brought to light when we consider quantum mechanics, or more precisely, the unobservable entities that lie within. In the quantum realm, nature does not provide us with such clean cut concepts as it does in the case of the moon. In the case of the moon, natural philosophers dealt with the following question: we know of the moon, how does it behave? In the case of quantum mechanics, the situation is reversed. We must now ask: we have certain behaviour, what concepts can we use to explain it? While it is true that we have developed concepts, extremely useful ones I might add, such as electron, photon, wavefunction, etc., this is only through our gaining intuitions of the *measuring equipment* and trying to make sense of them. We do not even have access to the direct intuitions of the quantum objects themselves, except maybe atomic spectra, so we are even further removed from the objects-in-themselves than we are in the case of the full moon. Allow me to explain this difficulty by way of an analogy. Imagine you are sitting in a windowless cabin listening to the noises outside. After a while you notice certain regularities and you postulate explanations for those regularities; these explanations may be perfectly consistent but you are well aware that these are educated guesses, you could be completely wrong. In the classical realm, you have the luxury of, say, breaking down a wall and checking your explanations using sight. Furthermore, classical logic has stood you in good stead during your investigations. In the quantum realm, however, there is no door and the walls are unbreakable; you cannot throw yourself into the quantum world, as it were, and double-check your answer. Also, classical logic appears to fail with wave-particle duality being a prime example. Nonetheless, the attempts made have produced highly successful results so the concepts used continue to be used, *even though* problems do arise in the use of these concepts. These concepts cannot, therefore, be a perfect reflection of the World-in-itself (a perfect language would be capable of explaining *one* World-in-itself with *one* consistent explanation). However, their use will be maintained unless a conceptual

[87] "NOA's Ark – Fine for Realism", pg 396

overhaul of the type Einstein envisaged occurs since the current concepts are so useful and such a complete overhaul is so difficult to achieve.

§3.2.3: Bohr and Contamination

Niels Bohr was a philosopher-scientist who spent a great deal of time and effort trying to elucidate what he called the principle of complementarity. Being more philosophically minded than the average physicist, Bohr caused a great deal of grief among the physics community, not least of which were those that attended his Como lecture where he spoke about it in public for the first time. Those that attended had expected a lecture with formulae and testable predictions, but were given a philosophy lecture instead. Needless to say, the importance of the principle of complementarity was not fully appreciated at the time.[88] One month later, at the 1927 Solvay Conference, Einstein met the principle of complementarity for the first time in spectacular fashion, engaging Bohr in the famous debates over the completeness of quantum mechanics. In Bohr's last interview before his death, he mentions that Einstein seemed to take the word 'principle' too literally and wanted a strict definition of some sort without realising that it was really a 'framework' of complementarity[89] that Bohr applied not only to physics, but to the life and social sciences as well.[90] Because the framework of complementarity is exactly that, a framework, it is impossible to summarise it succinctly. However, Folse has summarised its relationship with contamination well when he says:

> To give the classical descriptive terms the empirical reference they must have if the theory is to be confirmed by observation requires interacting with the system. But the quantum postulate entails that the interacting whole has an individuality which prohibits unambiguously defining the state of the system while it is being observed. Thus the physical conditions necessary for observation (interaction) are complementary to those necessary for defining the state of the system (isolation). Bohr regarded the uncertainty principle as directly expressing the formal consequence of this complementarity between the mode of space-time co-ordination and the mode of description of causality.[91]

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[88] **Folse, Henry J.**, *The Philosophy of Niels Bohr: The Framework of Complementarity*, North-Holland Physics Publishing (1985), pg 107

[89] *The Philosophy of Niels Bohr: The Framework of Complementarity*, pg 144-145

[90] See both of Bohr's volumes labelled *Atomic Physics and Human Knowledge* which contain several essays regarding these topics.

[91] *The Philosophy of Niels Bohr: The Framework of Complementarity*, pg 138

In other words, we must interact with the object to gain any information about it, i.e., we must 'contaminate' it. However, it is this contamination that deflects the object from whatever it was going to do had we not interacted with it. Therefore, the reality condition found in the EPR paper stating that "if, *without in any way disturbing a system*, we can predict with certainty … the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity"[92] is nothing short of a classical ideal unachievable in practice; even a classical system is still disturbed when measured, it is just that it is imperceptible and hence ignored. In the case of quantum mechanics, the measurement completely derails the system from what it was going to do before the measurement was made, and since the measurement imposes an uncontrolled change on the system, the result is that the state of the system is now unable to be predicted.

One may say: what about a prepared system? If we know what state the system is in since we are the ones that prepared it, we have no need to interact with it to find its initial conditions, and hence we are able to predict an uncontaminated outcome. Well, even if we were to ignore the fact that one must interact with the system to prepare it and only consider the system after the preparations are done, the problem still remains. Let us look at a common example, the double slit experiment with electrons.

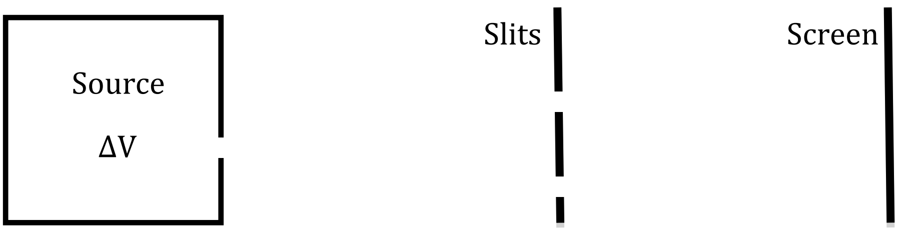

Figure 4: Double-Slit Experiment

Even if you could tune the potential difference so you knew exactly what it was (in reality, this is not possible and there are bound to be irregularities in the field as well), this would only tell you how much energy the electrons gained on their way through the electric field. You would not be able to give an exact speed for the electrons, and hence an exact wavelength, because this would require knowing how much energy that electron had while in the material. As far as I am aware, we are unable to do such a thing. When the electron leaves the source, it must go through the aperture, but we can only

[92] "Can Quantum-Mechanical Description of Physical Reality Be considered Complete?", pg 777

specify its position at that stage as ‘somewhere in the aperture’. Again, even if we make the unrealistic assumption that we know the source-slits distance, the slits-screen distance and the size of the slits exactly, we can still only say that we know the position of electron as it passes through the slits as being somewhere within one (or both) of the slits. Put simply, even if we had prepared the experiment, there is nowhere within the setup where we would know the position and velocity of the electron (along with the relevant forces) exactly to be able to make an exact classical style prediction as to where the electron will hit the screen. Sure, it may be the case that the eventual pattern on the screen looks like that predicted by theory, but the pattern generated is the result of a large number of electrons which have uncertainties in their measurements much larger than anything that could rival the uncertainty relation $\Delta x \Delta p \geq \hbar/2$.

One may also ask: if we can use quantum particles, such as light, to measure classical objects without disturbing them significantly, then maybe there are ‘ultra-quantum’ particles such that what we consider to be quantum particles behave classically to them. Assuming these ‘ultra-quantum’ particles actually did exist then this thought merely shifts the problem back a step since there will come a time when we want an accurate description of the ‘ultra-quantum’ world, which would require ‘ultra-ultra-quantum’ particles ad infinitum.

In short, the system must be interacted with, and hence contaminated, to be able to find out the information required to make a prediction, but because of the contamination, the system will no longer do what it is predicted to do. It is the complementarity between interaction and isolation that means that Einstein’s reality condition is unachievable and that contrary to Musgrave, contamination is a serious problem, at least in quantum mechanics.

§3.3: Musgrave Finally Recognises Quantum Mechanics … Sort Of

At the end of his paper, Musgrave finally mentions quantum mechanics, even if it is only briefly. He says:

> What if Quantum Mechanics, [according to the Copenhagen Interpretation], should turn out to be correct? Will not science have turned out to overthrow even common-sense

> realism and to vindicate Bishop Berkeley[, a phenomenalist]? Science and common-sense have often clashed – why should the clash not be as radical as this one?[93]

This appears promising as it seems Musgrave has caught a glimpse of the weirdness of quantum mechanics, even if it may just be a kneejerk reaction to the irreverence of quantum mechanics to Musgrave's common-sense realist position. Unfortunately for Musgrave, on speaking of the issue of ontology being determined by science he says that the Copenhagen interpretation is not the result of science, but rather of dubious philosophical theories such as the verifiability theory of meaning.[94] Given that at the time the double slit experiment was readily available for testing the probabilistic interpretation of the wavefunction, and nowadays we have the Bell-Aspect experiment that strongly suggests a measurement dependent form of ontology, it appears that science can be well utilised to inform us on the ontological aspects of quantum mechanics.

Musgrave also mentions, in that same paragraph, that a quantum physicist presupposes common-sense realism every time he sets up an experiment, which seems uncontroversial enough (to me at least), but Musgrave uses the principle of complementarity to justify this claim.[95] The principle of complementarity states that it is impossible to have arbitrarily small uncertainties for the measurements of incompatible properties, such as position and momentum, for the same experimental setup. This has nothing to do with a Hacking-style attitude towards ontology which says that "if you can spray it, it is real"[96], once again displaying that Musgrave's knowledge in the area of quantum physics is lacking and is not well suited to be criticising Fine on these matters.

Throughout this section of the discussion it appears as if Musgrave wishes to pass through the issue of quantum mechanics as quickly as possible despite the fact that it is the weirdness of quantum mechanics that motivates Fine to develop NOA. It is by using classical examples that demonstrates that Musgrave demonstrates that he has missed the underlying motive of *The Shaky Game*, a book entirely about the history and philosophy of quantum mechanics.

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[93] "NOA's Ark – Fine for Realism", pg 397

[94] *Ibid.*

[95] *Ibid*.

[96] **Hacking, Ian**, *Representing and Intervening: Introductory topics in the philosophy of natural science*, Cambridge University Press (1983), pg 23

§3.4: Truth and the Natural Ontological Attitude

So far in this chapter I have tried to show that Musgrave's lack of knowledge of quantum physics means that he is not qualified in commenting on a philosophical position that has been strongly influenced by Fine's extensive knowledge of quantum mechanics. Since any position in the scientific realism debate must recognise how the truth of scientific theories is played out in their philosophical theories, we now move on to the aspects of truth found in NOA. As mentioned earlier, a large portion of Musgrave's paper "NOA's Ark – Fine for Realism" is concerned with the issue of truth, and as an epistemologist, he has a greater entitlement to contest NOA on these grounds. However, he equates the Tarskian truth used by NOA and correspondence truth, and hence Musgrave's false assertion that NOA is nothing but realism follows from there.

§3.4.1: NOA in Relation to the Anti-realist Positions

Recall that NOA says we are to accept the results of science in the same way that we accept more homely truths, and that:

> NOA sanctions ordinary referential semantics and commits, via truth, to the existence of the individuals, properties, relations, processes, and so forth referred to by the scientific statements that we accept as true[97]

where using ordinary referential semantics means that we are to:

> treat truth in the usual referential way, so that a sentence (or statement) is true just in case the entities referred to stand in the referred-to relations.[98]

That is, we are to accept Tarski's theory of truth which is to say that we accept the statement 'Snow is white' is true if and only if 'snow' has the property of being 'white'. Even though Fine insists that NOA is a core position that anyone can accept, Musgrave disagrees saying:

> Positivists [think the assertion of] the existence of [theoretical] entities is *false*. Instrumentalists think that scientific theories are tools or rules which are *neither true nor false*. Epistemological antirealists … insist that no theory should be *accepted as true*. None of these antirealist positions, as usually understood, is consistent with Fine's core position.[99] (Musgrave's italics).

[97] *The Shaky Game*, pg 130
[98] *Ibid.*
[99] "NOA's Ark – Fine for Realism", pg 383

In other words, if NOA says that well confirmed theories are true in a homely sense, and if these anti-realist positions are not willing to ascribe truth to a theory at all, then we seem to have a problem in that NOA is not a core position able to be accepted by anti-realists. However, the problem does not lie with NOA. The problem lies with Musgrave in that he automatically associates truth with correspondence, and it is a mistake he continues to perpetuate throughout his paper. NOA, on the other hand, *does not claim correspondence truth*. I gave an example earlier to illustrate the difference between Tarskian and correspondence truth which was that NOA says we are to accept the (Tarskian) link between electrons and charge because it provides a good explanation of the behaviour of electrons but we are not committed to the (correspondence) link between electrons and the universal blueprint; we simply do not have access to the universal blueprint and hence we cannot be certain of the electron's place on that blueprint. NOA does not say that our theories regarding electrons work well, therefore electrons *must* exist, but instead says that for the sake of not getting tangled up in ontology, we will assume they exist and conduct our experiments accordingly. Positivists want to say that we cannot see electrons, therefore they do not exist. While a NOAer does not wish to take such a hard line, both a NOAer and a positivist can agree that electrons have charge. Positivists think that 'electrons exist' is false in the correspondence sense, not in the Tarskian sense. (Note that objects do not have to exist for us to ascribe properties to them, e.g., 'Harry Potter is a wizard' is a true statement even though neither Harry Potter, nor wizards, exist; the Harry Potter series would be quite uneventful if we could not ascribe the property 'wizard' to Harry.)

Instrumentalists do not consider theories to be true or false in the correspondence sense because scientific terms are really just shorthand ways of saying everything associated with that concept. It is in this shorthand way that the instrumentalist can speak of 'electrons have charge' as being true because 'charge' makes up part of the 'electron' concept bundle, i.e., there is a Tarskian link between 'electron' and 'charge', but no link between 'electron' and the blueprints which is exactly what NOA proposes.

Epistemological anti-realisms, such as van Fraassen's constructive empiricism, hold that 'electrons exist' is false in the correspondence sense for the same reason the NOAer does; that we do not have access to the universal blueprints and hence cannot read off the list of actual particles. Instead, such an anti-realist will say that belief in a theory is to believe that it is empirically adequate, i.e., the numbers we ascribe to particular Tarskian links between concepts match up to those found in experiment; saying 'electrons have charge' allows us predict the interference pattern of an electron double slit experiment after the electron passes through a potential difference (although only *adequately*, not *exactly*, as seen earlier).

So, it has been demonstrated that NOA really is in line with (at least) those examples that are given above. However, Musgrave goes further, saying:

> Homely statements are accepted as true in the homely sense of the term 'true' – bits of science are accepted as true in some esoteric sense of the term 'true'.[100]

In other words, Musgrave is saying that if different positions in the scientific realism debate take on different meanings for the word 'true', then there really is no one core position, but many different 'core' positions, each with a different meaning of the word 'true'. Unfortunately, Musgrave contradicts himself on the next page when he actually mentions that NOA subscribes to the Tarskian theory of truth; NOA has built into it a core theory of truth. Musgrave appears to be unaware of the distinction between the correspondence and Tarskian theories of truth which is exactly what leads him to think that NOA is nothing but realism. We can also see that Musgrave has missed the anti-essentialist teachings of NOA. The above quote seems to imply that Musgrave thinks that scientific knowledge has a different essence to it compared to that of everyday knowledge which is the very thing NOA is trying to oppose by portraying itself as a California-pure, core position in the scientific realism debate.

§3.4.2: Tarskian versus Correspondence Theories of Truth

The Tarskian theory of truth says that "a sentence (or statement) is true just in case the entities referred to stand in the referred-to relations"[101], while a correspondence theory focuses on the fact that there is a direct link between the statement and reality. The Tarskian theory of truth emphasises the connections within the web of knowledge while the correspondence theory emphasises the connections between the web and the universal blueprint. Musgrave, however, sees the two theories as being one and the same thing:

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[100] "NOA's Ark – Fine for Realism", pg 384
[101] *The Shaky Game*, pg 130

I have always thought (with Tarski himself) that the semantic conception of truth is a version of the common-sense correspondence theory of truth.[102]

In the next paragraph Musgrave asks:

Most philosophers (including Arthur Fine) think that the correspondence theory of truth is one thing and Tarski's theory another. What does the correspondence theorist provide (or seek to provide) which Tarski does not?

Musgrave says that Fine thinks that "realists must add to Tarskian truth a hankering after truth's essence."[103] I am not sure that this represents Fine's position entirely accurately. Fine says that "[t]he full-blown version of this [realism] involves the conception of truth as correspondence with the world, and the surrogate use of approximate truth as near-correspondence."[104] The realist knows what it would be for a scientific theory to be true, that it would correspond directly with reality. So the problem here is not that realists are hankering after truth's essence, they already know what it is. Fine sees the problem as being the fact that realists believe they have actually reached this ideal, or are at least very close, when no one is entitled to make such a claim and that the realists claims that they have done so are weak at best. This is because scientific investigations try to find out more information regarding the concepts we already possess and/or inspire the introduction of new concepts in an attempt to explain the empirical evidence. In other words, all we can know is the web of knowledge and the Tarskian relations between the concepts within this web. I am sure Fine, as much as anyone, would love to see successful scientific theories, but to make a solid claim that these theories provide a direct link to the universal blueprint is going further than we are justified in going. Fine says:

The problem is one of access. The correspondence relation would map true statements (let us say) to states of affairs (let us say). But if we want to compare a statement with its corresponding state of affairs, how do we proceed? How do we get at a state of affairs when that is to be understood, realist-style, as a feature of the *World*?[105]

As mentioned earlier in the discussion dealing with Kant, we organise the world in terms of concepts, we cannot see the *World-in-itself*. The problem is not that there is not a world out there to describe, but that for good or ill, we must use language to describe it. Even our most fundamental concepts, such as space and time, are not immune to being

[102] "NOA's Ark – Fine for Realism", pg 386
[103] "NOA's Ark – Fine for Realism", pg 387
[104] *The Shaky Game*, pg 129
[105] "Unnatural Attitudes", pg 151

an inadequate way of describing scientific processes as shown by Bohr's principle of complementarity. Concepts come and go, such as phlogiston, ether and miasma, and this should not surprise us. To say that we have everything correct and now we are doing nothing but stamp collecting, even in our most mature sciences, is nothing short of arrogance (and admittedly, quite boring).

What does this say about the differences in metaphysics between NOA and the correspondence realist positions? After discussions with Arthur Fine himself in 1987, Musgrave believes the following position to be a correct representation of the metaphysics of NOA:

> The NOA reads 'true' Tarski-style, with all this brings in the way of commitment to the entities referred to in accepted statements which are taken at face value. *But* the NOA leaves it open what those commitments actually are, because he leaves it open which statements are to be taken at face value (that is, realistically) and which are not. … It attributes to the NOA (pronounced 'knower' remember) *a complete philosophical know-nothing-ism*. The NOA is not committed to electrons, the moon, tables and chairs, physical objects, other people, his self, *anything at all*.[106]

If I may put it into my own terms, what I believe Musgrave is essentially asking is: if everything is on this so-called web of knowledge, including you, and the web is not connected to reality itself, what does that say for your existence? If you cannot say something is *really* there, then you cannot claim you are *really* there either. Now, granted, I was not privy to the aforementioned conversation, nor do I have access to a transcript, but I think that this is taking it too far. There are two related problems here that cause Musgrave to eschew Fine's position. The first is one that we have covered in depth, that of having to use concepts. After Musgrave quotes Fine's statement saying that "the problem is one of access", Musgrave replies by saying:

> What exactly is the problem here? Somebody says 'There is a full moon tonight' and I look up into the night sky and ascertain that the statement is true.[107]

As mentioned earlier, the fact we have carved out the moon concept is unsurprising, but we have carved it out nonetheless. The statement 'There is a full moon tonight' may well be true in a Tarskian sense, moon and fullness stand in the referred-to relations, but this does not mean that reality itself recognises 'moon-ness'. Musgrave is missing the

[106] "NOA's Ark – Fine for Realism", pg 390-391
[107] "NOA's Ark – Fine for Realism", pg 391-392

essential layer where we, as observers, organise the information we receive from reality into our known concepts. Returning to Musgrave's question of your existence, firstly what is scientifically important is not your you-ness, but the relevant properties you have. For example, if someone was trying to prescribe medicine for you, a relevant property may be the condition of your heart, not the fact that you are you. Secondly, to emphasise the point, I will look at evolutionary theory. From an evolutionary perspective, you would fall under the category of human but in terms of an evolutionary timescale, human is somewhat of a fluid term. On such a timescale it may be difficult to assign a particular fossil finding into the category of 'human'. This however does not mean that there is nothing at all out there in reality, such as yourself, that could be put into the category of human, it is just that the concept is not perfect and that through the imperfections of language, a perfect scientific concept will never be struck upon.

It is here that Musgrave's first problem becomes related to the second. The second problem with Musgrave's thinking here are the multiple senses in which he uses commitment. In one sense he uses commitment to mean a Tarskian commitment and in the second sense, he means a correspondence commitment. A NOAer is to hold a Tarskian commitment, i.e., he is to hold on to well confirmed links between concepts such as: electrons having charge provides a good explanation for their behaviour in electromagnetic fields, therefore a NOAer is committed to the link between electrons and charge. This however does not mean that a NOAer should be committed to electrons in and of themselves. Since electrons are unobservable (we can shift to quarks if you think such a statement is contentious), it is difficult to carve out this concept in such a way that it gives an accurate description of reality. A NOAer recognises that 'electron' (or 'quark') is a successful concept, but will not be surprised if these concepts need to be replaced at a later stage due to new findings. It is Musgrave's use of (mainly) classical terms that does make it seem as if NOA's commitments are a bit strange, but on closer inspection, it is really his misunderstanding of the history, philosophy and practice of quantum physics shining through.

Part B: Towards Quantum Realism

Chapter 4: NOA's Relationship to Structural Realism

***Morpheus**: Welcome to the real world.*

§4.1: The Transition from NOA to Structural Realism

So far I have been developing the view that given Fine's knowledge of the history and philosophy of modern physics, it is completely understandable that he would arrive at a position such as NOA, particularly regarding the issues surrounding quantum realism. This is because quantum physics teaches many things that run contrary to common sense, such as non-locality as discussed in the sections regarding Bell's Theorem. I have also been developing the view that given Musgrave's lack of understanding of quantum mechanics, he is unable to see how the intricacies of quantum physics affect Fine's reasoning and resorts to the use of common sense to say that NOA is nothing more than common sense, or classical, realism. While it will be clear that I believe Musgrave is incorrect in his thinking, I believe that Fine is also off the mark, although nowhere near as much. Fine is reacting to classical realism, which I also agree is not capable of making good an interpretation of quantum physics. However, I also believe that Fine has reacted too hastily in his pronouncement that 'realism is dead', even if modern physics is taken into consideration. I hold to the idea that a realist position known as structural realism holds a great deal of potential for coming to terms with the issues of quantum realism, and that NOA is actually very similar to structural realism, and John Worrall agrees stating that:

> It is not in fact clear to me that Fine's NOA (the natural ontological attitude) is substantially different from structural realism. Structural realism perhaps supplies a banner under which *both* those who regard themselves as realist *and* those who regard themselves as antirealists of various sorts can unite.[108]

(Recall that one of the key features of NOA is that it is intended to be the core position that both the realist and antirealist is able to accept. However, unlike NOA which is meant to describe the common ground both sides hold, structural realism is able to appeal to both the realist and antirealist alike because it draws in 'the best of both

[108] **Worrall, John**, "Structural Realism: Best of Both Worlds", Dialectica, Vol. 43 No. 1-2 (1989), pg 123

worlds'.) It must be noted, however, that to criticise Fine for this oversight could be considered anachronistic since the paper the above quote was taken from, "Structural Realism: Best of Both Worlds" (discussed in more detail in the next section), was written in 1989, three years after *The Shaky Game* was published. While it is true that structural realism had been discussed since its beginnings around 1905 with the likes of Poincaré and Duhem, it did not become a dominant realist response to the problems quantum mechanics posed to a realist interpretation of science until after Worrall's paper was published. Then again, Fine does mention the term 'structural realism' in chapter nine of *The Shaky Game*[109], but this takes place after discussing the structure of quantum logic, so it could be the case that Fine has hit on the name of an actual realist position without being aware of it. Either way, I shall give Fine the benefit of the doubt.

It should come as no surprise, then, that given there is a great similarity between NOA and structural realism that the next step to take towards quantum realism is in the direction of structural realism. To see how similar the two positions really are, we now turn to Worrall's paper which provides an introduction to structural realism, and in doing so, the similarities between the two positions can be highlighted.

§4.2: The Best of Both Worlds?

In 1989, Worrall revived what was then a little known realist position originally mentioned by Poincaré in his work *Science and Hypothesis* in 1905. This position, called structural realism, is an attempt to find the best of both worlds, which is to say an attempt to account for the ultimate arguments of both realism and anti-realism. First we will look at realism and its 'ultimate' argument, the 'no miracles' argument, and then move on to anti-realism with its own 'ultimate' argument, the 'pessimistic induction from the history of science' argument.

The 'no miracles' argument roughly runs as follows. Science is undoubtedly a very successful discipline and it would be a miracle on a cosmic scale if this success were to arise without scientific theories being able to 'latch' on to something real regarding the universe itself. Theories that are not only able to account for known phenomena, but also are able to predict new phenomena must have something essentially correct about them. However, since miracles are not a very solid form of explanation, then we should

[109] *The Shaky Game*, pg 157,159, 161

look for an alternative. The alternative arises when we realise that if a theory is 'approximately true', then it should come as no surprise that it has latched onto an element of reality. Therefore, the success of science can be accounted for by recognising that our current theories are at least approximately true and hence anti-realism is unsuitable to account for scientific practice because it does not recognise the (at least) approximate truth of theories.[110]

While it is reasonable to believe that if scientific theories were at least approximately true then they would show empirical success (although exceptions are plausible, such as one component of the theory being so incorrect that the accuracy of the rest of the theory becomes irrelevant), the reverse is not necessarily true, i.e. empirical success does not imply approximate truth. The 'pessimistic induction from the history of science' argument brings this out clearly and it runs roughly as follows. In the past, there have been some empirically successful theories, such as those involving the ether or phlogiston, which today we now consider to be false. Since it is likely that our current theories are heading towards the same fate as those that were once considered to be correct, we should not consider our current theories to be correct either. Common-Sense Realism is unable to account for drastic theory change since if central tenets of previously successful and approximately true theories, such as the ether, are no longer thought to exist, then the theory cannot possibly have been true.[111]

The realist replies that theory change is not a problem because the new theories are ever-improving approximations to the truth; they are simply refinements on previous theories. Take, for example, Newtonian mechanics which is the lower speed limit[112] of Einstein's newer special theory of relativity such that Newtonian mechanics has now been corrected to cover a larger speed range. So, the realist will recognise that in light of the special theory of relativity, Newtonian mechanics is only approximately true, but for those applications in which it is used, it is highly successful and hence can be held to be a

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[110] "Structural Realism: The Best of Both Worlds?", pg 101

[111] "Structural Realism: The Best of Both Worlds?", pg 109

[112] Newtonian mechanics is the lower speed limit of special relativity in a mathematical sense, i.e. the special relativity equations at the low speed limit take the same form of equations of Newtonian mechanics. This, however, does not eliminate the fact that Newton's assumptions/axioms about space-time are different from those of Einstein's, and that Newton had no notion of electromagnetic inertia, so they are still to be considered as separate theories, even though they do have overlapping domains of application.

very close approximation to the blueprints and the special theory of relativity now brings us even closer to the universal blueprints.

However, as has been mentioned earlier in the discussion concerning Fine's objections to realism, approximate truth is an unsatisfactory concept, at least in the purely philosophical sense. There are at least two reasons for this. The first one, very simply and as mentioned earlier: we simply do not have the universal blueprints to which to compare our theories; we are unable to say that a theory is almost correct if we do not know what the correct theory actually is. Secondly, if theory T_1 approximates T_2, which in turn approximates T_3 and so on, there will eventually become a point where theory T_n will seem to show very little resemblance to T_1 at all, unless scientific revolutions go round in circles. In other words, if T_1 is not approximately true in light of T_n, and T_n is only approximately true in light of the blueprints, T_1 was never approximately true to begin with.[113] The realist may respond by talking of mature science; we cannot blame theories in immature sciences for being wrong since the practitioners were still trying to find their feet.[114] For this argument to be currently successful, it would require an interesting definition of the word 'mature'. Inquiry into the workings of nature have been going on for thousands of years, what is it about current science that makes it mature? There are many ways one may want to classify a mature science such as: is there a culture of experimental verification of theories? Has there been the appearance of a grand overarching theory, such as evolution in biology or conservation of energy in physics? Is there a general consensus among the practitioners that the discipline is on the right track? While these may be interesting questions in and of themselves, the notion of a 'mature' science is nothing more than a smokescreen that the common-sense realist must use to amend his position. The structural realist has no need for such notions as they hold that there are elements that are passed down from theory to theory making all science effective to some degree without having to define a set of criteria a discipline must meet to qualify as mature.

All this talk of approximate truth does raise an interesting and probably the most important question regarding approximate truth which is: what does approximate truth mean practically? It may come as a great surprise to be saying this as a philosopher, but

[113] "Structural Realism: Best of Both Worlds?", pg 106
[114] *Ibid*.

as a physicist, do I really need a clear cut definition of approximate truth, assuming it even makes sense to look for a single topic-neutral notion of approximate truth? Is it not enough to have an intuitive idea of what it means? From a practical perspective, the aim of a scientist is to improve our understanding of nature; whether they succeed or not has become the storm in a teacup found amongst philosophical circles.[115] Imagine what would happen if the storm left the teacup, i.e. if scientists could be convinced that the work they do does not constitute an improvement over current scientific knowledge, then scientific work would be severely limited. In reality, when a philosopher makes the claim that scientific knowledge is not progressing, or that we cannot decide whether or not science is progressing, what they really mean is that we cannot create a clear cut and objective method which allows us to measure scientific progress. However, even those that are not involved in scientific disciplines can intuitively recognise that having effective medical treatments, clean water supplies, digital devices, increasing knowledge of the very small and very large, etc. is scientific progress. (Technically, the first three are technological advances, but they are founded upon scientific advances). The practising scientist realises that even if they do develop a ground-breaking theory, it will not be the perfect theory; it will not be *the truth* that the philosopher is hankering after. However, this does not mean their efforts are pointless. The scientist aims for increasing verisimilitude, in the intuitive sense of the word; scientists have a job to do and simply do not have the resources to achieve the perfection the philosophers would like them to.

The above criticism assumed that it even made sense to look for a single, topic-neutral notion of approximate truth. In the past, many attempts have been made in trying to develop a topic-neutral philosopher's definition of truthlikeness. In the process, a myriad of problems have surfaced, such as the language dependence of theories[116], and even if such problems have been overcome, more problems arise when philosophers try and move away from the artificial, thought experiment theories into actual scientific theories. This is no surprise since in the philosopher's quest for the epitome of a topic-

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[115] It has come to my attention (being a physicist first, philosopher second) that many of the arguments found in the philosophy of science amount to yelling in a soundproof room; the philosophers inside the room are actively engaged in the discussion, while the scientists outside the room can function perfectly well without being dragged down by the details. This is not to say that the philosophy of science is useless with regard to actual scientific practice, but it does mean that a greater level of synergy between the two disciplines is required.

[116] **Oddie, Graham**, *Truthlikeness*, Stanford Encyclopedia of Philosophy (2007), http://plato.stanford.edu/entries/truthlikeness/

neutral theory of truthlikeness they have turned to logic. However, to treat science as nothing more than a set of sentences to be analysed logically is to downplay three very important, and highly related, aspects of science. Firstly, science is a creative process. It is not uncommon to hear scientists speaking of particles 'wanting' to do things, or to use analogies in their explanations for certain phenomena. Explanations framed in such terms are perfectly capable of addressing the phenomena at hand, yet to reduce these explanations to logical tools is to not only render these story-like explanations false, but it also misrepresents the scientific process (and takes away some of the magic of discovery as well). Secondly, this is to remove the gut instinct of the expert. When formulating a new theory, the expert will get a feel for what parts of a theory are more correct than others and as such will strive to keep the more correct components in the new theory. There is no stringent logical manner in which a scientist can back up this instinct because, thirdly, a scientific statement is not tested in isolation. In testing a theory, we are also testing auxiliary statements regarding the theories governing the measuring equipment along with the theories used in the derivation of the specific theory in question. However, to analyse this logically requires us to analyse the sentence $T_1 \wedge T_2 \wedge \ldots \wedge T_n$ (where T_n is our theory in question while the others are the auxiliary theories) which is almost certainly false since it is extremely unlikely that all of the theories being tested will be true, yet this does not tell us anything about the truthfulness of our particular theory in question. It is only the irrational instinct of the expert that is able to discern the relatively certain from the suspicious within a theory.

This seems to show that reducing science to a mere plaything of the logicians is to go against the spirit of science. However, this is not the only concern raised by attempting to find a topic-neutral theory of truthlikeness. For example, let theory A successfully[117] describe the thermodynamics of black holes and also let theory B successfully describe the behavioural patterns of adopted children. How is truthlikeness to play out for these two theories? Considering the cosmologist first, he would use the competing theories available and calculate values for, say, the entropy of a black hole and what measurement the equipment should display if this were to be tested for. Intuitively, it would be reasonable to believe that if theory and measurement matched up to within experimental uncertainties, the theory used has a significant degree of truthlikeness. The question is: can the child psychologist use this same measure, i.e. can

[117] 'Successfully' does not necessarily mean 'contains the Truth', but it is a very good theory nonetheless.

she adequately quantify, say, a child's anxiety and make testable numerical predictions regarding anxiety based on other factors which would also need to be quantified? No doubt those in the field of child psychology could intuitively compare theoretical predictions with observations, but to quantify the parameters of interest is much more difficult in the social sciences than it is in the physical sciences. Therefore, it would be unreasonable to expect the psychologist to use the same measure as the cosmologist for determining truthlikeness; to expect as much would be impractical, bordering on impossible. While it may be the case that similar sciences (such as physics and chemistry, or psychology and sociology) use similar truthlikeness measures, overall truthlikeness in any practical sense of the word must be topic-dependent since we can only compare the truthlikeness of theories relative to a given phenomenon in an attempt at finding the best theory describing that phenomenon. To remove the theory from its context, which is what a topic-neutral theory of truthlikeness intends to do, is to render the theory useless.

All this discussion on the nature of truthlikeness falls very nicely within the scheme of NOA which places a great emphasis on working on problems at a local level and only focussing on the global once patterns become clear. It also shows that to search for a science-like essence to scientific knowledge different from other forms of knowledge really is futile. Once again, NOA's practicality wins out and science is shown to be fully capable of standing on its own two feet without the interference of an overarching philosophical theory.

Returning to the discussion regarding structural realism, it has been established that the 'no miracles' argument cannot be used to defend common-sense realism as Worrall himself says:

> The 'no miracles' argument cannot *establish* scientific realism; the claim is only that, other things being equal, a theory's predictive success supplies a *prima facie* plausibility argument in favour of its somehow or other having latched onto the truth.[118]

Can we then consider the 'pessimistic induction from the history of science' argument to be any better and hence claim anti-realism as the victor? There are many cases in history where successor theories can be seen to be contradictory to the theory it replaced. The example that Worrall emphasises, is the apparent contrast between the

[118] "Structural Realism: Best of Both Worlds?", pg 102

ether and the electromagnetic field in describing Fresnel's central bright spot caused by the diffraction of light around an opaque disc. While Fresnel considered light to be a mechanical wave propagating in the ether, Maxwell eventually realised that light is oscillations in a disembodied electric field. As Worrall puts it:

> It would be difficult to argue that the theory that light is a wave in a mechanical medium is an "extension" or even an "extension with slight modifications" of the idea that light consists of material particles: waves in a mechanical medium and particles travelling through empty space seem more like chalk and cheese than do chalk and cheese themselves.[119]

How, the anti-realist contends, can this possibly be a case of continuity in theory change? One theory says light is mechanical while the other says that it is electromagnetic. This is a contradiction if ever there was one. Worrall mentions two previously proposed positions that go some way to dealing with this objection, and consequently we can learn valuable lessons from these positions, but ultimately, they do not answer the anti-realist's complaint entirely.

Firstly, the pragmatist, which is an anti-realist position, recognises that there are useful components in scientific theories that are carried over while there are other components, usually the theoretical ones, which can be left behind in theory change. However, the pragmatist, being an anti-realist[120], will not claim that theoretical terms refer to anything in reality but if these terms can be carried over such that they are useful in improving empirical adequacy, the pragmatist will do so. Therefore, the pragmatist cannot appeal to the 'no miracles' argument since they do not believe that anything within the theory has latched on to reality; but any successful components are maintained and carried over into the new theory, only because they are useful, not because they are actually true.[121]

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[119] "Structural Realism: Best of Both Worlds?", pg 107/8

[120] As mentioned earlier, the scientific realism debate has many components to it so to reduce it down to a single realist/anti-realist spectrum is to oversimplify matters. However, given that pragmatism agrees with only two of the four realism conditions mentioned earlier, namely that theories are confirmable and that the history of science is progressive, I would agree with Worrall's assertion that pragmatism is anti-realist. (Pragmatism says that properties only exist once measured, and since such an emphasis on the observable reduces its commitment to theoretical entities, I would place pragmatism near constructive empiricism on the realist/anti-realist spectrum).

[121] "Structural Realism: Best of Both Worlds?", pg 110

Secondly, the conjectural realist, which is Popperian realism without verisimilitude[122], is willing to claim that theories are genuine attempts at explaining reality itself but we have no way of knowing if each scientific advance is closer to the truth than the last (even if it is more empirically successful and the best theory we have in terms of withstanding empirical testing) and we certainly will never know if we have actually latched on to the universal blueprints.[123] The conjectural realist also cannot claim that science is not miraculous since he realises that, for example, the assumptions of Newtonian mechanics are wrong (or at least special relativity claims them to be so) and yet the theory is highly successful; this is something the conjectural realist must just accept, he cannot provide an explanation.[124] So, in terms of theory change, the only thing that remains continuous is an ever-increasing empirical adequacy, but the conjectural realist is unable to give an explanation as to why this increase in empirical adequacy is taking place.

From the above two positions, the structural realist takes the 'continuity of components' element of pragmatism, along with the 'attempted description of reality' and 'denial of correspondence truth' from conjectural realism. Returning to the Fresnel/Maxwell example, the structural realist recognises that while in the theory change we have lost the ether concept because it was incorrect, there are other components of Fresnel's theory that remain:

> Although Fresnel was quite wrong about *what* oscillates, he was, from this [formal] point of view, right, not just about the optical phenomena, but right also that these phenomena depend on the oscillations of something or other at right angles to the light.[125]

So it is the *structure* of Fresnel's theory that remains, or in terms of the web of knowledge mentioned earlier, it is the links between concepts that remains. From a mathematical point of view, the mathematics does not recognise *what* is actually oscillating, it only recognises that *something* is oscillating, so in this respect, there is perfect continuity between Fresnel's theory and Maxwell's. It is this that allows structural realism to deal with both the 'no miracles' argument and the 'pessimistic

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[122] Verisimilitude says that each new theoretical advance is a successively closer approximation to the truth such that scientists are continually getting closer and closer to the universal blueprints, i.e. verisimilitude is an increase in truthlikeness.

[123] *Ibid*.

[124] "Structural Realism: Best of Both Worlds?", pg 111

[125] "Structural Realism: Best of Both Words?", pg 118

induction from the history of science' argument. Structural realism denies that the success of science is miraculous because scientific theories latch onto the structure of reality, i.e., the relations between entities such as laws while being able to interchange the entities that behave according to the structure during theory change. Structural realism also avoids the pessimistic induction argument because it is the components believed to be correct in the structure of the theory that get carried over (sometimes to be extended to cover a broader range of application, such as in the Newtonian mechanics to special relativity case) while those components believed to be incorrect drop out:

> There was a continuity or accumulation in the shift, but the continuity is one of *form* or *structure*, not of content.[126]

Structural realism does not consider a scientific theory as a whole when considering theory change such that it can avoid claiming that if one part of the theory is wrong, the whole theory is wrong. Some parts of a theory have accurately identified certain relations between phenomena,[127] which is why we find empirical adequacy, and these are the components of the theory we wish to retain during theory change. In short, the structural realist finds what nature *does* to be most important, not what nature *is made up of* like the common-sense realist wants to include.

§4.3: Similarities between NOA and Structural Realism

Many of the similarities between NOA and structural realism stem from the fact that both of them reject a correspondence theory of truth. Furthermore, both of them can be said to subscribe to a 'web of knowledge' type approach to truth. Recall that in §2.4.2 I described how a NOAer would understand a scientific statement such as 'electrons have charge'. This was done using the Tarskian conception of truth which stated that 'electrons have charge' is true if and only if electrons have charge. The point to emphasise here is that under NOA, it is the relationship between electrons and charge that is important, not whether or not the concepts 'electron' or 'charge' have latched onto some part of the universal blueprints. Structural realism functions in the same way with regard to the relationships between concepts and varies only in what we are to think of concepts such as 'electron'. It is the *structure*, or relationships in the web of

[126] "Structural Realism: Best of Both Worlds?", pg 117
[127] "Structural Realism: Best of Both Worlds?", pg 119

knowledge, that survives theory change and hence it is prized by the structural realist for being able to provide an answer to the pessimistic induction argument. In simple terms, the structure is what provides the link between the web of knowledge and the universal blueprints, though this way of thinking can be thought of as a little too simplistic since the structural realist's emphasis on the continuation of the formal component of theories, namely the equations, means that the link is more abstract than one would associate with correspondence truth. The link is only really there up to a partial isomorphism.

As mentioned in the last paragraph, the variations between NOA and structural realism lie in the difference in what is thought of when concepts such as 'electron' are mentioned. A NOAer will trust the results of science and say that if scientists think there are electrons then there really are electrons, i.e. a NOAer scientist will hold the assumption that they are manipulating *actual* electrons until they have good scientific reasons to think otherwise. The two overarching forms of structural realism, epistemic and ontic structural realism, take a slightly different approach. Epistemic structural realism, which is more closely aligned with NOA, will say that there are particles out there that account for the behaviour we attribute to particles like electrons, but we cannot know the true nature of these particles. For now, however, the word 'electron' refers to a set of properties belonging to an entity that we think best describes the observed behaviour but we are free to define a new set of properties associated with a new entity if it is going to provide a more effective description. The ontic structural realist goes further. While the epistemic structural realist thinks that all we can know is the structure (but that nature consists of more), the ontic structural realist believes that in terms of scientific theories, we can only say meaningful things about the structure. Therefore, 'electron' can be thought of as a convenient way of describing electron-related ideas in some contexts, a sort of placeholder if you will, and in other contexts such as in quantum field theory, it can also provide the mistaken impression that electrons are entities in and of themselves when in reality they are part of the field's structure.

Even in these variations, NOA and structural realism still has this in common with regard to entities: they are not the be all and end all. A key feature of NOA is that it allows scientists the freedom to do their work without being constrained to any particular scientific outlook and if entities are not thought of as holy grails linking us to the blueprints to the universe, this helps no end. The structural realists are slightly more constrained when it comes to the structure itself since there must be a continuation of structure through theory change, but it is a small price to pay to be able to satisfy the major arguments of both the realists and antirealists.

<u>§4.4: Structural Realism's Answers to Fine's Objections</u>

At the beginning of this chapter I claimed that Fine has reacted too hastily in his statement that 'realism is dead'. In the last section I showed that NOA is actually not that far removed from a realist position known as structural realism which holds the more positive elements of NOA. In this section I want to show that structural realism is capable of dealing with the objections Fine has of common sense realism which he laid out in his chapter on NOA, and thereby completing our journey from NOA to structural realism. Recall, from §2.2, that Fine puts forward three philosophical objections to scientific realism in his chapter on the natural ontological attitude; namely that the methodology of the realist is unsatisfactory, and that the realist is unable to deal with the problem of conjunctions, as well as the small-handful problem. By looking at each of these objections in turn, I intend to show that Fine's pronouncement on realism, classical realism to be precise, is unwarranted.

§4.4.1: The Methodology of the Realist

The first of Fine's philosophical objections, that the methodology of the realist is unsatisfactory, comes from his appreciation of the work of those such as Hilbert and Gödel. Earlier, we found that he believes that:

> to argue for realism one must employ methods more stringent than those in ordinary scientific practice. In particular, one must not beg the question as to the significance of explanatory hypotheses by assuming that they carry truth as well as explanatory efficacy.[128]

What Fine is saying here is twofold. Firstly, that for a realist to assume that good explanations also carry truthfulness and to apply this thinking to either scientific practice or the explanation thereof (namely the philosophical description that is scientific realism) is simply question begging; and secondly, that the realist is being too

[128] *The Shaky Game*, pg 115

‘hand-wavy’, the realist must be more stringent than scientific practice itself in his description of scientific practice.

I will deal with the issue of question begging first. On this issue we find this more pointed quote from Fine:

> … at the ground level and at the level of methodology, no support accrues to realism by showing that realism is a good hypothesis for explaining scientific practice [since] such a demonstration (even if successful) merely begs the question that we have left open (“need we take good explanatory hypotheses as true?”).[129]

Realists (common-sense realists at least), being realists, want to say that a good scientific explanation carries with it a great deal of truthfulness, and since science abounds with good scientific explanations, we are entitled to believe that scientific theories are at least approximately true. Also, since realists feel this provides a good explanation of scientific practice, we are also entitled to believe that scientific realism is true with regard to scientific practice. Fine’s complaint here is that the realist is yet to justify why a good explanation actually carries any truth content, and hence he feels that the realist’s conclusion that realism is a good, truthful position is question begging. In other words, Fine is unwilling to claim that *any* good explanation carries any truthfulness. One must then ask: what makes a good explanation? It seems circular to try to explain what a good explanation is, the best you can hope for is consistency, i.e. that the explanation you are giving matches that explanation’s description of what a good explanation is. Furthermore, according to Fine, one cannot even say that this explanation is truthful of good explanations; it appears Fine is asking the realist to do the impossible! While Fine is correct in thinking that if an explanation is a good one it does not *guarantee* its truthfulness, it would be wrong to say that good explanations are never true.

But how can a realist show that scientific theories hold a significant measure of truth? Must they run into walls explaining explanation as Fine’s attitude seems to suggest? Fortunately, no. One method the realist can use is to look at theory change, or more specifically, what remains after theory change. This is where structural realism comes in. Initially it might seem that one would be reasonable to believe that a theory has a measure of truthfulness if it has been successful. Yet further reflection reveals that one would still in that case not be able to show to what extent the theory is truthful. If,

[129] *Ibid.*

however, some element of a previous theory survived a theory change and/or was expanded upon, one's conviction that a theory held a measure of truthfulness is solidified with the surviving/expanded component of the theory making up some of this measure. Structural realism focuses on components of theories, rather than theories as a whole, and when one does so, there is historical evidence that can be used to attribute truthfulness to theories.

This newfound truthfulness of theories is not stringent enough for Fine, however. Fine believes that 'to argue for realism one must employ methods more stringent than those in ordinary scientific practice'. Appealing to the history of science is an inductive method, so at best this practice can only be as stringent as ordinary scientific practice. Here, again, I think Fine's demands are unrealistic; Hilbert's maxim, i.e. showing the consistency of a theory using the most stringent methods possible, need not apply to scientific realism, or even to the realism debate at all. This is because science is a very rigorous discipline whereas most of philosophy lacks the same rigour. Science involves peer reviewing with a check, double check, triple check mentality; it is generally pretty difficult for your work to become accepted science. Scientific theories stand or fall on experimental data,[130] this is the way it is. Scientific realism on the other hand is a philosophical position, and as such lacks the ability to stand or fall on experimental data (though this does not mean that it should not be influenced by it). Sure, philosophy of science can draw on the history of science for examples, and academic papers can be checked by fellow philosophers, but this cannot possibly have the same rigour as experimental verification; words can convey multiple intentions while numbers generally do not lie. As far as I am aware, the only way a realist philosophy could be more rigorous than inductive experimentation is to build up arguments deductively. However, as mentioned earlier, the only way we can test our realist hypotheses is by using the history of science, which would be an inductive method. This means that either Fine is correct that no description of the practice of science is possible (realist or not), or that we must recognise that science is a discipline that describes nature whereas philosophy of science is a discipline that describes science; they are two different

[130] In saying this, I am not advocating some form of anti-realism, or realism for that matter; any position in the scientific realism debate worth considering should at least put some emphasis on empirical adequacy. What I am saying is that science has the luxury of being able to check with nature whereas much of philosophy is unable to rely on empirical testing.

entities with two independent methods of description. So which option do we take? Given science's impact on our improved living standards, increased life expectancy and greater understanding of natural phenomena over the millennia, it would be extremely difficult to argue that science is not successful. There must be a reason why science is so successful, and to find that reason is a major driving force in the scientific realism debate. In other words, contrary to NOA, there must be a possible description of the practice of science and why it is so successful. However, the fact that nature always sticks to the rulebook, while science, being a creative discipline, has a less rigid set of rules means that it should not surprise us that it is more difficult to describe science as a discipline than the rule-abiding nature. Hilbert's maxim is more understandable for a discipline like mathematics, which was its intended benefactor anyway, since mathematics has a very rigid logical structure. Philosophy of science, however, is not so rigid, and hence I claim that Hilbert's maxim need not apply. While this conclusion does not provide a vote of confidence for any position in particular in the scientific realism debate, it does mean that the blockade put in place by Fine's 'realism is dead' claim has been removed so that realism still remains a suitable candidate.

§4.4.2: The Problem of Conjunctions

The next problem raised by Fine in objection to scientific realism is that of the problem of conjunctions. To recap, the problem of conjunctions is to explain why:

> If T and T' are independently well-confirmed, explanatory theories, and if no shared term is ambiguous between the two, then we expect the conjunction of T and T' to be a reliable predictive instrument (provided, of course, that the theories are not mutually inconsistent).[131]

In general, a structural realist would have the same line of thought that any other position in the scientific realism debate would, or at least should, hold which is that we cannot know of the adequacy of the conjunction of two theories unless we experimentally verify it. In this regard, structural realism holds no advantage over NOA (it holds no disadvantage either).

However, back in §2.4.3 I gave an example of how NOA would deal with the problem of conjunctions, namely with regard to using T="light is a wave" and T'="light is a particle". With regard to this apparent contradiction, all the NOAer could do is simply

[131] *The Shaky Game*, pg 120

say that thinking of light as a wave or a particle works for each of their respective experimental setups and we should allow science the freedom to maintain thinking of light in such a way until the resources are developed that allow for a greater comprehension of the nature of light. A structural realist, an epistemic structural realist in particular, goes further. Since structural realists do not treat entities as holy grails attached to the universal blueprint, they are able to focus on the properties of the alleged particle rather than the particle itself. Therefore, they are able to look upon statements such as "electron as wave" and "electron as particle" simply in terms of the wave and momentum properties respectively; the fact that both statement include "electron as" is of little consequence since 'electron' merely refers to the collection of properties we believe electrons have which can be replaced by a new set of properties, and hence a new entity, at any time. Like NOA, this not only gives scientists the freedom of not having to be weighed down by thinking that the particles we deal with are actually links to the universal blueprints, but it also allows scientists a more comprehensive understanding of theory change and scientific progress than is to be found in NOA's 'science does what it does' type attitude.

§4.4.3: The Small-handful Problem

The last of the problems highlighted by Fine is known as the small-handful problem. Recall that the small-handful problem asks us to explain why, at any given time, there are only a small handful of competitor theories competing to describe the same phenomena, theories which in general bear a strong family resemblance, maintaining well confirmed aspects and only deviating in the less confirmed aspects of the theories. Recall that from §2.2.3, I dealt with Fine's issues regarding the realist responses to these three questions. Fine believes that the realist does not answer the first question at all and that the realist assumes too much about the truth of current theories for questions two and three, i.e. the realist wants to say that if current theories are approximately true, then small modifications from these are the way to go, whereas Fine, and more specifically Laudan, has called into question the approximate truth of current theories. What I wish to do here is show that structural realism is capable of dealing with the small-handful problem. So, what can the structural realist do?

The first question asks: why is there only a small handful out of the (theoretically) infinite number of possibilities? All Fine says in response to the realist is that:

> The realist response does not seem to address the first issue at all, for even if we restrict ourselves just to successor theories resembling their progenitors, as suggested, there would still, theoretically, always be more than a small handful of these.[132]

What I believe Fine is doing here is placing too much emphasis on the hypothetico-deductive form of reasoning (he speaks of guessing later on in his response to question three). He is taking issue here with the fact that the realist seems unable to explain why science works so well if scientists only have a 'guess first, check later' type of attitude. This is made even worse if the guesses are being chosen from a theoretically infinitely large pool of possible theories. There are a few comments that need to be made with regard to this.

First of all, even if scientists do use hypothetico-deductive reasoning a portion of the time, to refer to this process as nothing more than a guess is to demean it unnecessarily. Granted, this method uses guesses of sorts, but educated ones at that, and to understand why it is educated makes all the difference. To simply say 'guess' implies that scientists are picking at random from the infinite pool of theory options when in reality scientists do not have an infinite pool before them. Firstly, this is because scientists are going to put forward a reasonable adjustment such that testing this adjustment is likely to make a measurable impact on our understanding of the phenomena at hand, i.e. adjusting Newton's inverse-square law of gravitation by adding 'and Fred will have toast for breakfast tomorrow' may be measurable, but it goes no way towards helping to explain the phenomenon of gravitation. Secondly, this reasonable adjustment will most likely come about with the use of conjunctions. If one wishes to form a hypothesis regarding how a system's behaviour will change when subjected to a previously untested range of parameters for that system, it makes sense to look at how other systems have fared under those new conditions and applying the explanations formed for those systems, in a suitably adjusted form if necessary, to the system in question. This practice is nothing less than theory conjunction where T and T' could be said to be T="explanation under normal circumstances" and T'="explanation of similar system under extreme circumstances". As mentioned earlier, the use of conjunctions is by no means flawless, but it does account for the fact that there are a small handful of

[132] *Ibid.*

theories rather than the theoretically infinite: there are only a small number of relevant conjunctions that can be made since there are only a finite number of current theories to choose from and only a small proportion of these will actually be relevant to the phenomenon at hand.

Alternatively, and this is the option Fine avoids entirely, scientists will not be using hypothetico-deductive reasoning all the time and will simply do the experiment without any well-formed hypothesis in mind. (Admittedly, if you were taking part in a project as expensive as the Large Hadron Collider this method is not ideal, but for your average lab experiment, it works fine.) Isaac Asimov once said "The most exciting phrase to hear in science, the one that heralds the most discoveries, is not "Eureka!" (I found it!) but "That's funny..."" which is an entirely accurate representation of scientific practice. It is the strange and unexpected results of science that force scientists to think outside the box and find explanations for these new results. It is here that a great deal of progress in science is made and it is Fine's bypassing of this method, intentional or not, that leads him to believe that realists are unable to answer this first small-handful question. We have already seen that if a scientist is using the hypothetico-deductive method only a small handful will arise since there is only a small relevant pool of theories to choose from. Now, in the case of this 'strange result' method, the handful is small because it is difficult to come up with even one legitimate explanation for the new data, this is why scientific revolutions are rare. Either way, the handful is small. However, we have been working with the suggestion that successor theories resemble their progenitors and have assumed we are justified in doing so. The answer to question two allows us to see why we are indeed justified in following this method.

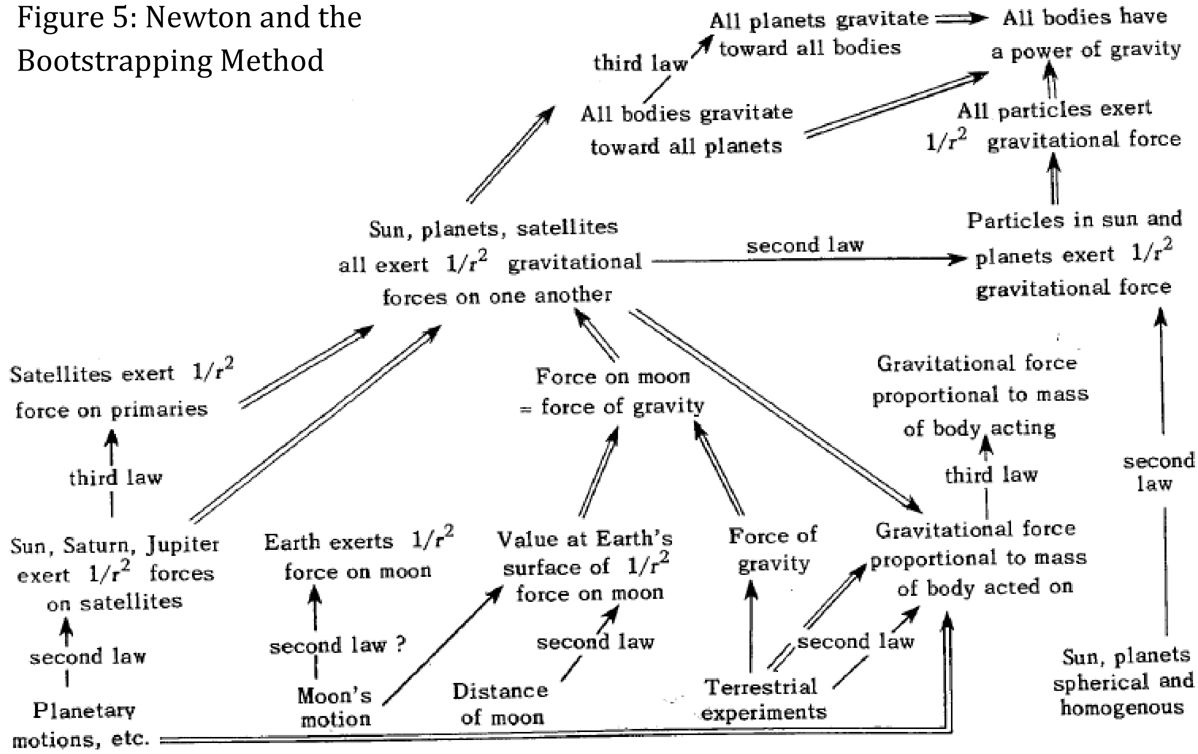
We can go further in our explanation of the size of the handful of theories when we consider the work of Clark Glymour. In his book, *Theory and Evidence*[133], Glymour emphasises the bootstrap methodology of theory testing. The bootstrap methodology involves taking a set of experimental data, E_1, along with a set of background hypotheses, H_1, and applying a set of computations, C_1, to them and arriving at a value for quantity P.[134] The beauty of the method is that theory confirmation relies on being able to take a different set of experimental data, E_2, (i.e. the measuring of a different set

[133] **Glymour, Clark**, *Theory and Evidence*, Princeton University Press (1980)
[134] *Theory and Evidence*, pg 130

of properties, not simply someone else's measurements of the same properties), another set of background hypotheses, H_2, and another set of computations, C_2, and arriving at the same result for P.

One of the historical examples Glymour gives is that of Newton's conclusion of universal gravitation (Glymour provides a summary in diagrammatic form which has been replicated as figure 5 below).[135]

Figure 5: Newton and the Bootstrapping Method



By beginning with what is essentially three different empirical data sets (namely E_1 = motion of the planets around the Sun, E_2 = motion of moons around their host planets, and E_3 = motion during terrestrial experiments such as pendulum experiments), and using hypotheses such as Newton's and Kepler's laws, Newton reached the same conclusion P = "All bodies have a power of gravity" by various routes (computations) as seen in figure 5. Scientists can have confidence in such a bootstrap method because for the claim P to be satisfied, all the claims leading up to it must also be satisfied. This sounds very difficult to achieve, and it is, which is why there is only a small handful of theories being considered in a field at any given time. Any theories that do not satisfy the stringency of the bootstrap method are thrown out[136], whereas those same theories may

[135] *Theory and Evidence*, pg 223

[136] To say 'thrown out' is to oversimplify things. It can actually be useful to keep in mind those theories, or even those pathways that do not satisfy the bootstrap condition because it allows us to test parts of

have remained under the less stringent hypothetico-deductive methodology since it is much easier to achieve empirical adequacy through one path than it is through multiple paths.

Before I answer question two, let me begin by introducing some formalism. To be general, let us say that the modification of a theory can be expressed as $T' = mT + X$ where the current theory T is made up of claims $c_1, c_2, \ldots, c_n$, the modifier m replaces claim c_i with a new claim m_i (or removes c_i entirely) and $X = x_1, x_2, \ldots$ is a set of additional claims. Let us go further and order the claims of T such that c_1 is the most confirmed claim while c_n is the least confirmed. Since a theory cannot be tested in isolation, this means that the claims of a theory not only include the main statement that could be said to be *the* theory, but also the auxiliary theories and assumptions that went into testing the theory. Therefore, the most confirmed claims, those nearest c_1 will be those that are tested implicitly in every science experiment; those nearest the middle might apply to a particular field of science, say physics, while those nearest the end, closest to c_n, may apply only to non-linear optics, say.

Now onto question two, which asks: why is there the conservative family resemblance between members of the handful? Since the earlier claims in a theory have been tested by many experiments, at least implicitly, and have as yet not been found wanting, it would be a more productive use of a scientist's time to test the least confirmed claims, i.e. c_n would be tested first, followed by c_{n-1} etc.[137] If it is more efficient for the scientific community to be attending to the claims nearest c_n, then a large bulk of the claims will remain virtually untouched by specific scrutiny (though they are being tested implicitly with every experiment). It is this large bulk of actively untested claims that forms that strong family resemblance; however, it is because they are implicitly tested with each experiment and yet to be found wanting that we can have confidence that there is a measure of truth to be held within them, and hence the conservative family resemblance remains. From the standpoint of particular positions in the scientific

theories rather than the holism that hypothetico-deductivism suffers from; that is to say, if some pathway to P goes wrong, we can narrow down the potential problems to the differences between the correct and incorrect paths.

[137] I am not going to attempt to quantify degrees of confirmation since the argument is capable of running with an intuitive notion of degrees of confirmation, and we have already seen that searching for a clear cut definition of truthlikeness is futile.

realism debate, the structural realist is capable of providing the best explanation regarding the success of this strong family resemblance. The best the anti-realist can do in explaining the success is recognise that it is rational to hold to theories that have been tested and yet to be disconfirmed, but as far as the anti-realist is concerned, this does not impart any measure of truth onto the theory. The common-sense realist, as Fine rightly points out, must use the flawed notion of approximate truth. The problem with using approximate truth is that it states that *on the whole* a theory is approximately true; it does not attempt to identify which parts of the theory hold the truth (or at least significant claims to it). This is where structural realism comes in. It recognises that theories are broken up into claims, or structural elements, c_1 to c_n and knowing this allows the structural realist a greater measure of confidence in the success of the family resemblance. This is because they are able to not only count on the fact that some claims are yet to be falsified, but that some claims actually have a great deal of confirmation because they have successfully been passed on through theory change. Furthermore, these claims that have been passed on will be related to *the theory itself*, not just the auxiliary claims, and hence this allows us to place great confidence in the claims nearer c_n which were those that would normally appear less solid under any other position in the scientific realism debate.

Thirdly, why does the strategy of narrowing the choices in this way work so well? Fine sees the main problem as being the fact that the realist is unable to account for science's ability to make novel predictions. Fine asks:

> And what can the realist possibly say in this area except that the theorist, in proposing a new theory, has happened to make a good guess?[138]

Fine believes that nothing in the approximate truth of a theory makes it likely that modifying the less confirmed components of a theory will make a progressive shift; this sort of idea is nothing less than a chimera. The realist needs to be able to explain the occasional success of a strategy that usually fails; the realist's usual fallback onto approximate truth is nothing more than a gentle pillow, according to Fine.[139]

Let us review how this narrowing took place. The narrowing occurs either by creating a limited number of relevant conjunctions as part of the hypothetico-deductive

[138] *The Shaky Game*, pg 118-9
[139] *The Shaky Game,* pg 119

method, or by realising that trying to explain strange new data is no mean feat and only a few explanations at best will be devised. The answer to question two allows us to see that under structural realism, putting significant stock in the strong family resemblance has great potential for success. According to Fine, however, potential for success is not good enough; he wants an explanation for the occasional success of a strategy that usually fails. In other words, why is it that the same method that leaves the ruins of scientific theories, hypotheses, conjectures etc. in its wake is also capable of producing the occasional success?[140] This sounds like it carries a strong hint of the pessimistic induction argument which structural realism can quite capably deal with, as we will soon see. Fine's question is analogous to asking: why must animals die for evolution to provide better adapted species? Just as a lack of death in ecological systems would not permit nature to establish who the fittest species were, not being able to 'kill off' unsuitable theories in the presence of better theories would not allow us to develop an advanced catalogue of theories and hence progress would not occur.

But what are the evolutionary pressures, as it were, on scientific theories? When a new scientific idea is born, its originator is trying to solve problems caused by anomalies found in previous theories. It would be unlikely for this new idea to answer to all of them, but if a significant number are dealt with, the scientific community takes notice. However, with new ideas come new problems. Those that still hold to the previous theory attempt to find flaws in the new one while advances in technology, especially measuring equipment, bring to light anomalies in the new theory that would not have been in the scope of possible testing at the time the theory was born. These pressures mean that although the theory may have been well suited for its time but as measurement techniques improve, the limitations of the theory become apparent, both in terms of the parameter range it can account for and its ability to be coherent with other newly developed theories. It is at this point that the need for an even newer theory becomes apparent. Does this mean that the original theory is a failure? Not at all. For a once accepted theory to have been discarded, it must once have enough merit to be seriously considered. Furthermore, by focussing on the structural components of a theory, we are able to understand why a theory 'fails' in light of its successor by finding which structural components are passed on, and why those that are left behind are

[140] **Curd, Martin & Cover, J.A.**, *Philosophy of Science: The Central Issues*, W. W. Norton and Company (1998), pg 1206

either inadequate or unnecessary. So, although the history of science looks as if it is littered with failures, at the time those theories were dominant, they were successful. After all, we consider previous theories to be wrong because they are in disagreement with those we have now which we consider to be successful. Today's theories will be on tomorrow's scientific trash heap, and yet we consider them successful today. Therefore, Fine's complaint is unwarranted. Not only does the small-handful method provide successful theories, but continues to provide theories that are iteratively *more* successful.

Chapter 5: The Plausibility of Quantum Realism

***Spoon boy**: Do not try and bend the spoon. That's impossible. Instead... only try to realize the truth.*
***Neo**: What truth?*
***Spoon boy**: There is no spoon.*
***Neo**: There is no spoon?*
***Spoon boy**: Then you'll see, that it is not the spoon that bends, it is only yourself.*

§5.1: Learning from Quantum Physics Itself

The purpose of chapter four was to show that structural realism was the next logical step on our way to developing a realistic interpretation of quantum physics. This was done firstly by describing structural realism and the advantages it brings including how it accounts for both the 'no miracles' and the 'pessimistic induction' arguments. Secondly, the similarities between NOA and structural realism were elaborated upon, while thirdly, the strengths of structural realism were further demonstrated by using it to deal with the problems raised against realism by Fine in his chapter proposing NOA. However, as it stands, all we have done is shown that structural realism seems like a reasonable candidate, we are yet to establish what form of structural realism we are to use in developing a realist interpretation of quantum physics. In trying to establish this, I will look at what quantum physics itself has to teach us. By looking at certain results in physics, such as shot noise in laser physics and the different forms of quantum statistics, among others, I will be focussing on two main and highly related questions:

- Does it mean anything to speak of the individuality of a quantum particle?
- What does quantum physics tell us about what is real?

The first question is important because the difference between the two main forms of structural realism, epistemic and ontological, hinge on the question of particle individuality where ontic structural realism holds that it is incorrect to speak of the individuality of quantum particles while the second question is important because under the Copenhagen interpretation, reality does not exist until it is measured.

§5.2: Quantum Physics and Individuality

I shall deal with the issue of quantum individuality first. As I have just very briefly alluded to, the central tenet of ontological structural realism is that it is inaccurate to speak of quantum particles as individuals. More specifically this is the view held by those who espouse eliminativist ontic structural realism, the most prominent of these are Steven French, James Ladyman and Michael Redhead. Therefore, I shall focus on the writing of these three, as well as considering the relevant physics, in an attempt to establish the reasoning behind and the merit for adopting such a view about quantum particles. However, to even define what is meant by saying a particle is an individual is no easy feat, so first we shall have a look at Leibniz's Principle of the Indentity of Indiscernibles (PII) and the range of potential interpretations to see which one, if any, is a suitable description of the findings of quantum physics.

§5.2.1: Principle of the Identity of Indiscernibles

The original statement of PII is found in Leibniz's *Discourse on Metaphysics* where it states:

> … it is not true that two substances should resemble each other entirely and differ *solo numero* [(in number only)]…[141]

As I read it, this can be interpreted in two different ways. If one is to focus on the *solo numero*, it could be read as saying that if two objects were to resemble each other entirely, then these two objects are in fact one and the same object. However, if one where to focus on the 'two substances', the above definition could also be read as saying that two substances cannot resemble each other entirely; there will always be a haeccity, a this-ness, for each substance that separates it from all others. This latter form is known as *transcendental individuality*, and since talk of haeccities is not well regarded in philosophy, mainly because it is immeasurable by definition and amounts to nothing more than speculation, the first interpretation of Leibniz's PII has gained the greater share of attention.

With regards to the first interpretation, one can write PII in a logical form, which is as follows: [142]

[141] **Leibniz, G.W.** (trans. Lucas, P.G. & Grint, L.), *Discourse on Metaphysics* (§IX), Manchester University Press (1953), pg 14

$$\forall F\big(F(a) \leftrightarrow F(b)\big) \rightarrow a = b$$

This means that if and only if property F held by object a has the same value as that of b for the same property F for every property within the range of F, then we can say that objects a and b are one and the same object. This seems pretty straight forward but there is no consensus over what the range of F actually is; different articles within the literature even differ over what the possible options for the range of F are. To attempt to alleviate this problem, I have gathered seven potential options. However, I do not make any claims as to the completeness of this list either. My seven potential options for interpreting PII in light of the above formal sentence are that according to the sentence objects a and b have:

1. all properties in common.
2. all physical (i.e. non-logical) properties in common including spatio-temporal properties. This is known as the weak form of PII.[143]
3. all physical (i.e. non-logical) properties in common excluding spatio-temporal properties. This is known as the strong form of PII.[144]
4. all empirical (i.e. currently measurable) properties in common.[145]
5. all monadic, non-relational properties in common.[146]
6. all qualitative properties in common.[147]
7. all non-relational qualitative properties in common.[148]

The first option (PII1 for short) was understandably found in all the literature I consulted on the matter. It is considered to be trivial since a holds the property of 'being identical to a' and if b also held this property, b would trivially be a. It must also be noted that 'being identical to a' is more of a summary property rather than a property in itself, i.e. we cannot know that b is identical to a until all of the other properties have checked out, otherwise we would have just as much reason to assign the property 'b is

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[142] **French, Steven & Redhead, Michael**, "Quantum Physics and the Identity of Indiscernibles", Brit. J. Phil. Sci., Vol. 39 (1988), pg 234

[143] "Quantum Physics and the Identity of Indiscernibles", pg 234

[144] *Ibid.*

[145] **Morganti, Matteo**, "On the Preferability of Epistemic Structural Realism", Synthese, Vol. 142 (2004), pg 84

[146] **French, Steven**, "Why the Principle of the Identity of Indiscernibles Is Not Contingently True Either", Synthese, Vol. 78 No. 2 (Feb 1989), pg 144

[147] **Casullo, Albert**, "Identity of Indiscernibles" entry in *The Cambridge Dictionary of Philosophy*, Cambridge University Press (1995), pg 359

[148] *Ibid.*

not identical to a' as we do in assigning 'b is identical to a' from the beginning, bypassing the entire process. It is for these reasons, triviality and its summary nature, that I believe a form of PII in which the non-logical, i.e. physical, properties are to be focussed upon.

However, to say that we should be focussing on physical properties in a discussion concerning quantum realism immediately poses the question: what physical properties, if any? If the Copenhagen interpretation of quantum physics is indeed correct, a system does not have reasonably sharp values until measured, in which case it would be meaningless to speak of any form of indistinguishability before measurement. This measurable type of indistinguishability is essentially the type of indistinguishability that PII4 is trying to establish but Morganti (correctly, I feel) takes this to be too strong a condition since the only reason we may be unable to distinguish two entities is due to technological limitations,[149] or that trying to empirically determine a from b completely changes the system rendering the investigation moot and no closer to establishing whether or not they are actually indistinguishable. We could look at this situation from a NOA-like perspective and simply say that if we cannot practically discern them, then they are not discernible, but this may change as science improves. I think from a physicist's point of view, this approach has a great deal of merit, but also speaking as a philosopher, I feel that we can do better. It will be the purpose of §5.3 to show what the merits for such a view are and whether it is worthwhile searching for ways to satisfy PII2/PII3 or even if they have already been satisfied. With regards to the other three forms of PII listed above, PII5 will be discussed in relation to what is known as the indistinguishability postulate, while PII6 & PII7 are inapplicable in the quantum realm because qualitative properties do not have much use, i.e. we cannot speak of a quantum particle as being smooth, or red, for example.

§5.2.2 Quantum Statistics

To illustrate the three types of statistics relevant to this discussion, imagine two fair six-sided dice.[150] Classically, we expect there to be 6x6=36 different outcomes when we throw the two dice. This is because we are able to track each individual die through time by either physically or mentally labelling them in some way, such as by colour. Under

[149] "On the Preferability of Epistemic Structural Realism", pg 84/5

[150] **van Fraassen, B.**, *Quantum Mechanics: An Empiricist View*, Oxford Scholarship Online (Nov 2003), pg 380

this classical form of statistics, known as Maxwell-Boltzmann statistics, a red die displaying a six and a blue die displaying a three is considered to be a different combination to that of a blue die displaying a six and a red die displaying a three. However, under a form of quantum statistics known as Bose-Einstein statistics, these two combinations are considered to be one and the same and, as a result, we cannot label particles in the quantum realm like we can in the classical realm. Under Bose-Einstein statistics, there are 21 different, *equiprobable* outcomes when these two dice are thrown, namely the six doubles and the 15 'pairs', where each option in the pair is considered to be the same outcome. Admittedly, using two classical objects to highlight a quantum principle is confusing, but it highlights the weirdness of quantum mechanics. Classically, obtaining a (6,6) combination is half as likely as obtaining a (1,2) combination in either guise, i.e.:

$$P[(6,6)] = {}^{1}\!/_{36} \quad \text{whereas} \quad P[(1,2)\ or\ (2,1)] = {}^{1}\!/_{18}$$

so to speak of the two as being equiprobable is counterintuitive. However, quantum experiment verifies this counterintuitive idea; it really does seem as if particles lose their individuality in the quantum realm. The final type of statistics to be considered is called Fermi-Dirac statistics, which considers antisymmetric states only, hence it does not consider the doubles to be a possible outcome and so there are only 15 equiprobable outcomes to be considered under Fermi-Dirac statistics.

To see what is meant by the term antisymmetric, I shall explain this in more quantum mechanical language. Imagine a situation where we are considering the distribution of two particles that have the same intrinsic properties such as mass, spin and charge where the two possible quantum states for each particle are $|\alpha\rangle$ and $|\beta\rangle$. In classical statistical mechanics, the four possible arrangements would be as follows:

$$(1)\ \ |\alpha\rangle \otimes |\alpha\rangle$$
$$(2)\ \ |\beta\rangle \otimes |\beta\rangle$$
$$(3)\ \ |\alpha\rangle \otimes |\beta\rangle$$
$$(4)\ \ |\beta\rangle \otimes |\alpha\rangle$$

where the state before the tensor product sign, $\otimes$, is that of particle 1, and the state after is that of particle 2. Under classical statistical mechanics, (3) and (4) are considered distinct but in quantum statistics, bosonic or fermionic, they are considered to be the same arrangement for the purposes of assigning weights (and we are empirically justified in doing so). This, as the argument goes, is meant to show that these two arrangements are not only indistinguishable but identical. However, the two cases cannot be identical if the particles are individuals. Therefore, the particles are not individuals.[151]

French and Redhead disagree with this argument by noting that the relevant arrangements for quantum statistics are:

$$(5)\ \ |\alpha\rangle \otimes |\alpha\rangle$$

$$(6)\ \ |\beta\rangle \otimes |\beta\rangle$$

$$(7)\ \ \frac{1}{\sqrt{2}}(|\alpha\rangle \otimes |\beta\rangle + |\beta\rangle \otimes |\alpha\rangle)$$

$$(8)\ \ \frac{1}{\sqrt{2}}(|\alpha\rangle \otimes |\beta\rangle - |\beta\rangle \otimes |\alpha\rangle)$$

For the purposes of boson statistics, there are three relevant configurations, namely (5), (6) and (7). When the arrangements are written like this (8) is not included as part of the weightings, *not* because it is considered to be the same as (7), but because it is not symmetric. Bosons only have access to symmetric states, and similarly, fermions only have access to antisymmetric states and so (8) is the only one available to fermions. (System states (5), (6) and (7) are symmetric because you can swap the possible states the particles are 'in' and still arrive at the same system state. (8) is antisymmetric because doing this puts the system state in the negative of the previous system state). In other words, states are eliminated from consideration because they are inaccessible, not because they are considered to be the same as another state. From this, French and Redhead come to the conclusion, not that the particles must be individuals, but that it is possible for them to be individuals[152], i.e. this is yet to rule out the individuality of quantum particles.

§5.2.3: The Indistinguishability Postulate

In quantum mechanics, the way one would establish the indistinguishability of quantum particles is by using the indistinguishability postulate (IP) which is written as follows:

$$\langle P\phi|Q|P\phi\rangle = \langle \phi|Q|\phi\rangle \qquad \forall Q, \forall \phi$$

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[151] "Quantum Physics and the Identity of Indiscernibles", pg 236
[152] "Quantum Physics and the Identity of Indiscernibles", pg 237

This states that particles in an arbitrary N-particle state $|\phi\rangle$, with all particles having the same intrinsic properties, are indistinguishable if there is no difference in the expectation value of observable Q for any "arbitrary permutation of the particle labels" $|P\phi\rangle$.[153] This is simply what I was discussing earlier regarding the inability to label quantum particles like we could with the red and blue dice; IP simply says that if you switch around two indistinguishable particles, the physics is still the same.

From the point of view of NOA, this is entirely practical and I will use the example of shot noise from laser physics to illustrate this. In laser physics, shot noise refers to the noise in the signal generated by our inability to accurately measure the number of photons passing through a given point in a given time.[154] This is because photons do not form a continuous stream but are in fact a series of discrete particles emitted at random times by the laser and can be described using Poisson statistics. Under Poisson statistics, the uncertainty in a measurement is given as $\sqrt{N}$ where N is the number of measured events, so if the number of measured photons is 100, the uncertainty in the measurement will be 10 photons, or 10%, which is significant. Now, imagine there may be some sort of 'Laser Demon', analogous to Maxwell's Demon, who decides he wants to play with our laser experiment. Given that we can only know the number of photons emitted by the laser in a given time to within $\pm\sqrt{N}$, the laser demon could quite happily swap around photons or even add in or take away photons, assuming he was being careful about it (i.e. he did not put in photons of the wrong wavelength, adjust the number of photons too much, or send the new photons in the wrong direction), and we would not be able to detect it. From the point of view of NOA, if a laser demon can do this without impunity, and yet the physics still works, then we should let science be free to get on with the business of doing science without trying to drag it down with an unjustified and unnecessary metaphysics. I, for one, can see a great deal of merit in this way of thinking since it allows scientists to get on with the job at hand. However, in the pursuit of knowledge, we sometimes need to dig a little deeper than the practicalities to gain a comprehensive understanding of nature. So, while NOA works well for science in a practical sense, it is an insufficient attitude for attempting to provide a complete

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[153] "Quantum Physics and the Identity of Indiscernibles", pg 238

[154] **de Jong, Marc**, "Shot noise emerges as metal wires go to shorter lengths", Physics World (Aug 1996), pg 22

description of nature and further investigation, as difficult as it may be, has the potential to be very fruitful indeed.

To see why, we will take a closer look at the indistinguishability postulate itself. The N-particle state $|\phi\rangle$ contains particles that have all of their intrinsic properties in common, such as having the same charge, mass, spin, etc. As far as quantum physics goes, this is hardly controversial, but it does mean that any observable, Q, that is going to be of any interest here must be a relational observable since all of the monadic properties are, by definition, constant across particles and hence unchanged by a particle swap. Therefore, if PII is going to be of any interest in the quantum realm, then F as found in the logical form:

$$\forall F\big(F(a) \leftrightarrow F(b)\big) \rightarrow a = b$$

above must also include relational properties. In other words, PII5 would be trivially true according to the indistinguishability postulate. This also means that all relational properties involving the intrinsic properties, such as "being heavier than" or "having less charge than", will be the same across all particles since all particles having, say, the same mass, will all be heavier than a given particle if one of them is. So, what properties are we left with? Of the two options left for the properties we include in set F, PII2 and PII3, which are the weak and strong forms of PII respectively, the difference lies in whether or not we include spatio-temporal properties.

To decide whether we should include spatiotemporal properties or not, I feel we need to pay close attention to the phrase "arbitrary permutation of the particle labels" as found in the description of the indistinguishability postulate above. At first glance, it does seem as if such a statement is trivial, i.e. particle labels need not be physical properties, and even if they were, they would not affect the experimental outcome (a die's redness does not influence the outcome of the roll), so why is there so much emphasis placed on the effect of physical properties when interchanging a non-physical tag? The issue centres on the fact that we cannot consider those quantum particles as independent entities like we can in the classical case. In the example of the two dice, the result of the red die is independent of that of the blue die, and vice versa. We could throw the two dice together, or a week apart and in any order, the statistics will always be the same. However, in the quantum case, particles of the same kind in a system seem to collude with each other; their actions are dependent upon the actions of the others in the system. I do not mean this in the way that the actions of the Earth depend on the Sun, it is a much deeper connection than that. At least as far as the wavefunction representation is concerned, the quantum particles lose their individuality as they become absorbed into the system's wavefunction. After describing a situation in which there is a box containing two indistinguishable[155] particles with an impenetrable partition between them, Ginsberg explains it this way:

> Once the partition is removed, however, the fact that we are really dealing with a single unified system, viz., the underlying quantum field, can no longer be ignored. The propensity waves now intermesh producing one unified system of propensities. We can no longer regard the system as being composed of two independently existing subsystems each having an equal propensity to particulate on either side of the box, each evolving independently in time. We have one object, the quantum field in a certain two-quanta state, disposed to particulate twice should a precipitating interaction occur.[156]

So, this system has no continuants that persist through time; if we had a video camera that was capable of monitoring at the quantum scale we would not be able to track two particles with independent spatio-temporal tracks. It is only when we interfere with the system that it collapses down into its component particles. Therefore, it makes no sense to speak of tagging a quantum particle as an individual, but rather as a means of describing its potential states and how this configuration of potential states relates to the rest of the system. It is at this point that we can now see a great deal of merit in a statement of French and Redhead where they say that "PII would not be either true or false but simply inapplicable".[157] Therefore, it would seem unnecessary to try to distinguish between PII2 and PII3. However, all of this discussion has focussed on a *system* of quantum particles. Is it possible to conceive of a quantum particle in and of itself? It seems to me that this question has no meaning without performing a measurement on 'the particle' since we cannot know if the particle is part of an entanglement until interacted with, at which point information has only been gained regarding the properties of the particle post-collapse, not necessarily pre-collapse (i.e. information regarding the system pre-measurement is inaccessible and so to make any

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[155] Ginsberg actually says 'identical' but I presume he means this is the physicist's sense since according to the philosopher's sense of the word 'identical', it would be impossible to have two identical particles; they would be one and the same particle.
[156] **Ginsberg, Allen**, "On a Paradox in Quantum Mechanics", Synthese, Vol. 61 No. 3 (Dec 1984), pg 344
[157] "Quantum Physics and the Identity of Indiscernibles", pg 235

definite claims that the particles are individuals before measurement would seem to amount to speculation, under the Copenhagen interpretation at least).

In an attempt to try to convey a classical analogue, I shall use the example of sound waves. Even if one is in an extremely remote location, they will be within earshot of several sources of sound. The sound waves (analogous to wavefunctions) coming from those sources superimpose with each other in the medium they are travelling through such that the wave that arrives at the ears is a single wave, admittedly complicated in form, but it is a single superimposed wave nonetheless. Sound waves are characterised by the motion of the particles in the medium, and yet each particle in the medium cannot be labelled with the individual sound it is carrying, simply because there is no distinct set of particles carrying a particular sound, say footsteps, while another distinct set carries another sound, say someone speaking. It is the combination of the particle motions that determine the sound that is heard and it is only through experience and measurement that we are able to pick out the individual sounds (particulation), such as footsteps and speech.

Now that we know that quantum particles are at best sneakily maintaining their individuality and at worst absorbed into the system's wavefunction, the discussion will now turn to what this means about what we can speak of as being real in the quantum realm.

§5.3: Quantum Physics and Reality

I shall now deal with the second question posed for this chapter, namely 'What does quantum physics tell us about what is real?' Recall earlier, in §3, a great deal of attention was paid to Bell's theorem and the implications thereof. The common interpretation of Bell's theorem shows us that no local theory of quantum mechanics is capable of satisfying experiment which implies that the particles do not carry information regarding their properties with them. This means that each particle in each wing of the Bell-Aspect experiment does not have a set value for a particular observable until it is measured (a concept emphasised by the Copenhagen interpretation in general) and so reality does not appear to be as objective as realists would like it to be. However, we must ask ourselves, are we making too much of this result with regard to its implications towards quantum realism?

In chapter nine of *The Shaky Game*, Fine asks the question "Is scientific realism compatible with quantum physics?" In this chapter, Fine attempts to analyse the schizophrenia over realism and "urge[s] the forgotten moral of the new positivism; namely, that realism is a metaphysical doctrine that finds neither support nor refutation in scientific theories or investigations"[158] which, if nothing else, is the key point to be taken from *The Shaky Game* as a whole. Furthermore, Fine believes that it is unlikely that a legitimate realist interpretation of quantum physics will be forthcoming, but he also believes that those arguing against quantum realism using Bell's work are not compelling either.[159] Throughout this chapter of *The Shaky Game*, Fine explains the relationship between Bell's Theorem and a possible plausible form of quantum realism, and it is this explanation that shall form the centrepiece for this section.

§5.3.1: Minimal and Reductive Realism

Minimal realism, as the name suggests, approximates to the bare necessities of a realist standpoint. The first claim of minimal realism is what I was describing in the previous section, that the object or system in question must have the definite properties that the wavefunction says are there at all times, independent of whether the system or object is being measured or not; or as Fine puts it:

> … a system S has a state ψ (or "is in" the state ψ) just in case the real object corresponding the S has the array of particular forms of the generic features that ψ attributes to that object.[160]

In terms of the probabilistic side of quantum mechanics, Fine has this to say:

> If system S is in state ψ and A is an observable of S, then ψ attributes to the real object corresponding to S a particular form of the generic feature that corresponds to A. If and only if that particular form is associated with the probability distribution that quantum mechanics assigns for finding values of A, will we say that these various probabilistic assertions are true for system S in state ψ.[161]

In other words, since any position in the scientific realism debate must put some emphasis on empirical adequacy, the common-sense realist in particular must insist on the reality of the properties being measured. Furthermore, because quantum mechanics

[158] *The Shaky Game*, pg 151
[159] *The Shaky Game*, pg 155
[160] *The Shaky Game*, pg 160
[161] *The Shaky Game*, pg 160/161

is a very successful theory empirically, any realist interpretation must take this into account and any measurements must at least reduce to the statistical interpretation that has served quantum physicists so well thus far.

Reductive realism, on the other hand, is defined by Fine such that:

> The Bell inequalities hold if, and only if, it is possible to reduce the probabilities (for the observables mentioned in the inequalities) to averages over a single ensemble.[162]

This can be described alternatively such that:

> In the context of quantum theory, reductive realism amounts to requiring, first, that the measures of minimal realism all be dispersion free, and then to changing the correspondence rule for the states to treat them the way statistical mechanics treats macrostates.[163]

So, if one were to be a reductive realist about quantum mechanics, one would have to commit themselves to factorisability and locality. This is in stark contrast with what was discussed earlier in §5.2 where quantum particles behave as if they have lost their identity within the system. To treat states in the way statistical mechanics treats macrostates is to use Maxwell-Boltzmann statistics which is the classical form of particle statistics. To use this form of statistics is to say that the particles behave in an independent manner, i.e. they are factorisable in that the probabilities of specific measurement outcomes is independent of those of the other particles, and that any effects on the particle are local, i.e. there are no action-at-a-distance type causes.

Fine emphasises that reductive realism is an inadequate description of the quantum world by, not surprisingly, focussing on Bell's Theorem, or more specifically, joint probabilities. If each particle were to act independently, then the joint probability of getting a pair of outcomes would be a simple case of combining the two independent probability spectra. However, Bell's Theorem, discussed earlier, shows that we cannot provide an ensemble representation as far as incompatible observables are concerned, such a position and momentum, or x-spin and z-spin.[164]

In the interests of space, and because it has been shown, albeit briefly, that we have strong reasons to think that reductive realism is inadequate as a form of quantum

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[162] *The Shaky Game*, pg 164
[163] *Ibid.*
[164] *The Shaky Game*, pg 166

realism, I will now pay closer attention to Fine's minimal realism because I believe he hits on a very interesting application of complementarity.

§5.3.2: What is Real in a Quantum System?

Notice that in the two quotes used to describe minimal realism above, we can see two complementary aspects of quantum theory in action. In the first description, the focus is on the properties of the particles themselves, such as position, momentum, etc. that would compose the wavefunction description, ψ. However, the second description focuses on how one would measure the accuracy of a quantum theory, namely in terms of the probability distribution $|\psi|^2$. If nature is as objective as the realists wants to claim it is, then it might be reasonable to expect that at most one of these, either ψ or $|\psi|^2$, can correctly represent reality as it truly is, but which one? To answer this, it may be interesting to note that Fine only talks of the wavefunction of the system in the above descriptions of minimal realism and not of the wavefunctions of those particles making up that system. In doing so, it seems Fine may be giving the realist a little more leeway than he intended, but it also means that under minimal realism, experiment will show the correct probability distributions just in case there is an actual system that actually has those properties in question, i.e. if experimental methods verify that the momentum probability distribution is correct, the system actually has a momentum. Therefore, as far as the Bell experiment goes, since quantities such as the angular momentum and spin product of the quantum *system* satisfy the theoretical predictions, under minimal realism we are entitled to say that the system (such as the pion and then the ensuing entangled electron-positron pair) are real. This seems highly uncontroversial given that it would be hard to imagine that nothing at all was causing these experimental results to come to pass unless one was extremely sceptical. Given that the system is the largest relevant entity of interest we are trying to measure, I feel we can at least say that without a great deal of controversy the system exists. This is in line with Harvey Brown's review of *The Shaky Game* where, when speaking of Fine's earlier work which formed Fine's opinion of the implausibility of reductive realism, Brown has this to say:

> If this view is correct, it goes a long way towards nullifying the unusual importance that large numbers of philosophers and physicists alike have attributed to the Bell Theorem,

> and the (roughly) dozen experimental tests of the Bell-[Clauser-Horne] inequality performed to date.[165]

It would seem that physicists and philosophers alike have tended to bring classical thinking into a quantum situation, namely that if we cannot speak of the particles existing in and of themselves in the realist sense, then this anti-realist stance is automatically transferred to the system those particles are composed of. This type of correspondence between the particle and the system seems to be misplaced.

Now, to answer the question at hand, namely: What is real in a quantum system? We are at the stage where it seems incorrect to speak of the particle as real, at least in the sense of an independent entity. We are also at the stage where it seems uncontroversial to speak of the quantum system existing for if it did not exist, this would amount to saying there was nothing there to measure, and yet we get a measurement! But what is the representation of the system? One could be forgiven for thinking that the wavefunction, ψ, is the representation of reality; after all, it contains the "information" regarding, say, potential momentum states, energy states, spin directions, etc. Furthermore, it is the wavefunction that the quantum operators act upon and it is the wavefunctions of each particle in the system that is superposed to give the state of the system. However, it could also be argued that the probability density function, $|\psi|^2$, gives a representation of reality since this is what we are measuring and seeking to verify in experiments. The probability density function is also what is used to explain the shape and behaviour of molecules in chemistry. Maybe this dichotomy is comparable to that of force and acceleration. It could be the case that forces, what we superpose, are not actually a part of reality but are nothing more than a useful mathematical tool. It is only when we consider the acceleration and make measurements thereof that we are actually dealing with reality. Furthermore, it is only the net acceleration that we are able to measure, and hence directly infer the net force, but we would be unable to say what the component forces were from this acceleration measurement alone just as in the case of the system's wavefunction where we are working with a net probability distribution (hence net wavefunction) but not with the individual wavefunctions of the particles making up the system.

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[165] **Brown, Harvey R.**, Review of *The Shaky Game: Einstein, Realism and the Quantum Theory*, American Journal of Physics, Vol. 56 No. 6 (June 1988), pg 572

It may also be the case that we only need $|\psi|^2$ because of our inability to provide a complete deterministic representation of the quantum state such that our wavefunctions must be given in terms of possible states rather than actual states. If only we were able to provide a form of quantum mechanics like that envisioned by Einstein $|\psi|^2$ would be unnecessary and a complete description of quantum mechanics could be found in ψ alone. My point is that neither ψ nor $|\psi|^2$ readily lends itself to being the sole carrier of quantum reality and that much work needs to be done to develop an adequate quantum realist position assuming this is at all possible. This is not to take away from minimal realism since it considers both to be important but it does lend credence to Fine's insistence that reductive realism is inadequate since reductive realism wishes to reduce quantum mechanics to either ψ or $|\psi|^2$, but not both.

§5.4: The Future for Quantum Realism

The intention for chapters four and five was neither to develop, nor to critique, a specific position in the scientific (quantum) realism debate but to see what the physics told us about reality and where it led us in terms of a scientific realism position. Admittedly, in the interests of efficiency, it has leant heavily towards a position known as ontic structural realism (OSR). This is because OSR shows a great deal of promise in not only being able to deal with the major arguments of both realism and anti-realism since it is a form of structural realism, but also because it draws heavily on what science has to tell us, mainly in terms of particle individuality, or the lack thereof, in the case of eliminativist OSR. Even if OSR is a complex philosophical position, I believe a NOAer can at least be sympathetic with OSR's intent to draw from the results of recent science, namely that of quantum field theory.

This is not to say that the holy grail of quantum realism has been found in earnest for there are many questions left to be resolved. One of the major results in physics that seems to contradict the non-individuality of quantum particles is the Pauli Exclusion Principle which says that no two fermions within a system have all of their quantum values in common. If, say, the electrons in an atom all have different sets of quantum numbers then they must be distinguishable to some extent, at least in the philosopher's sense of the word. However, any electron is the same as any other in that it does not matter how the electrons are 'arranged' in the orbitals, the behaviour of the atom is still the same, i.e. the indistinguishability principle applies. It therefore appears we have a contradiction, namely we have distinguishable indistinguishable electrons. This means that either Pauli's Exclusion Principle is really smuggling in unjustified classical notions of particle, or it means that the Indistinguishability Principle's insistence on using expectation values, which are not necessarily a physical property of any of the particles in particular, is already assuming away quantum individuality, hence begging the question.

Furthermore, if quantum particles are not really particles, what are they? Quantum field theory says that what we call quantum particles are really just excitations in the quantum field and as such, a deep understanding of quantum field theory is necessary to provide a complete understanding of the nature of the quantum realm. However, quantum field theory is no speciality of mine, so I shall defer such musings to those more knowledgeable than myself for the time being since speculating wildly in a field unfamiliar to me after chastising Musgrave for doing the same would be hypocritical. Needless to say, these are interesting and useful puzzles to solve.

The discussion of quantum realism can also be placed in the larger context of the quantum measurement problem. In Antony Valentini's article, "Is Quantum Mechanics Tried, True, Wildly Successful, and Wrong?", he argues that quantum mechanics has been wandering in a philosophical wilderness all because of the anti-realist turn quantum mechanics took,[166] contrary to Fine's belief that an anti-realist approach to quantum mechanics was the key to its success. Valentini also laments that very few scholars are actually dedicating a large portion of their time to developing alternative interpretations of quantum mechanics and finishes his article by saying:

> "Maybe in 200 years people will look back and say the time wasn't right to reexamine the foundations of quantum mechanics," he says. "Or it might be that they'll say, 'My God, it opened up a whole new world.' We can't tell. One thing is certain: We won't find out if we don't try."[167]

It is only by continuing our search for what nature itself has to tell us about what is to be considered real and how it is to be interpreted that we are able to gain a proper understanding rather than resorting to a 'shut up and calculate' style attitude.

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[166] **Valentini, Antony**, "Is Quantum Mechanics Tried, True, Wildly Successful, and Wrong?", Science, Vol. 324 (Jun 2009), pg 1512

[167] *Ibid.*, pg 1513

Although Valentini's fellow philosophy of physics colleagues are reluctant to tackle what is undoubtedly a difficult issue, Emilio Santos has argued[168] that given that local realism serves us so well in the classical world, and that we are yet to obtain conclusive evidence that it does not hold in the quantum world, that we should not be so keen to give up on quantum realism so easily. Both Valentini and Santos urge further work needs to be done from theoretical and experimental standpoints respectively before the quantum realist even considers conceding defeat.

From an experimental perspective, we may not need to go down in scale, but up slightly to the emerging field of nanoscience. Nanoscience deals with the behaviour of materials at scales that border the quantum and the classical so one interesting question to ask may be: What happens at the boundary between quantum and classical statistics? As the size of groups of matter grows, it would be expected that there should be a transition between quantum and classical behaviour, yet there is a sharp distinction between classical and quantum statistics, so it may be the case that a form of correspondence statistics that acts between these two extremes can be found.

In short, I see no reason why quantum realism should concede defeat in the face of the more dominant anti-realist interpretation; only time will tell which interpretation is correct. This is the beauty of allowing science to act as its own living entity without being dragged down by metaphysical constraints.

[168] **Santos, Emilio**, "Bell's theorem and the experiments: Increasing empirical support for local realism?", Studies in History and Philosophy of Modern Physics, Vol. 36 (2005), pp 544-565

Chapter 6: Conclusion

***Morpheus**: This is your last chance. After this, there is no turning back. You take the blue pill - the story ends, you wake up in your bed and believe whatever you want to believe. You take the red pill - you stay in Wonderland and I show you how deep the rabbit-hole goes.*

§6.1: Arthur Fine and the Scientific Realism Debate Revisited

Part A of this thesis examined the exchange between Arthur Fine and Alan Musgrave over the issues surrounding the Natural Ontological Attitude. This is done from a physicist's perspective since quantum physics plays a crucial role in motivating Fine towards the development of NOA. It is from this perspective that we can understand, and defend, Fine's thinking and see the flaws in Musgrave's criticisms.

Like many physicists and philosophers of physics, Arthur Fine searched long and hard for the holy grail of the philosophy of quantum mechanics: a realist interpretation. The end result is *The Shaky Game*. In it, Fine proposes what he considers to be a worthy successor in the scientific realism debate which he backs up with the previous chapters of the book regarding the history and philosophy of quantum mechanics. It is this knowledge that gives him the conviction, and the authority, to make such a claim. In chapter two, we saw that Fine not only objects to both the realist and anti-realist claims regarding scientific practice, but also towards the scientific realism debate itself.

We first see Fine's objections to scientific realism in both historical and philosophical guises. Historically, Fine believes that if those working in modern physics during its infancy had held to realist interpretations, modern physics would not have been able to progress as well as it did, if at all. This is because abstract notions such as tensor fields and probability distributions are difficult for even the best of minds to get their heads around these concepts, and yet a 'shut up and calculate' attitude towards these ideas gave fruitful results.

Moving on to the largest of Fine's objections, that of the philosophical objections to scientific realism, Fine sees three problems which are the realist's methodology, the problem of conjunctions, and the small-handful problem. With regard to the realist's methodology, Fine believes that the realist must smuggle truth into their explanations such that if a scientific theory provides a good explanation for a phenomena, then it must contain significant truth content. As a corollary, given that scientific realism is a good explanation of scientific practice, to the realist at least, then scientific realism must also contain a significant amount of truth content which appears to Fine as if it begs the question. Fine is also not convinced by the notion of approximate truth the realist must use to account for the pessimistic induction argument from the anti-realists. It is a much too fuzzy notion for Fine and holds no philosophical rigour. Fine would rather see the realists undertake work similar to that of Hilbert and develop a system to describe science that is more stringent than scientific practice itself.

The inadequacy of approximate truth also plays a role in Fine's objection to the realist's attitude towards the problem of conjunctions. A realist wants to say that if theories T and T' are both approximately true, then the conjunction of the two should also be approximately true whereas Fine thinks that combining two approximately true theories would leave us worse off with the realist unable to defend herself against such a claim. It is only by testing the theory conjunction, just as we would treat any other theory, that we can have confidence that the conjunction is able to provide adequate results.

The last of Fine's philosophical objections to scientific realism dealt with the small-handful problem. Fine asks: why are there only a small handful of contender theories for a given phenomenon at a time, why is there a strong family resemblance between them, and why does this strategy works so well? The realist wants to say that if previous theories are approximately true, then it would be reasonable that new theories retain the truth content of the old theories with small modifications which explains the size of the handful and the family resemblance, while the theory's inherent approximate truth explains the success of the method. Fine, however, does not think that the realist has really answered the first question since the realist provides no method for narrowing down the technically infinite number of possible theories. In answering questions two and three, Fine believes the realist must rely too heavily on approximate truth which is a fuzzy notion at best; the realist must assume that theories are already approximately true which begs the question and enhances Fine's claim that the methodology of the realist lacks philosophical rigour. Furthermore, even if such an assumption were correct, there is enough incorrect wiggle-room within the theory to allow its descendants to mutate into a wildly incorrect theory. To add insult to injury, Fine believes the instrumentalist, the archenemy of realism, is able to answer the small-handful problem better than the realist.

Fine is a scientific non-realist, which means he is neither a realist, as we have seen, nor is he an anti-realist which becomes clear when we look at his objections to anti-realism. Fine focuses on two issues in this regard, namely truth-mongering and drawing dividing lines in science where none exist. Truth-mongering refers to the dealings the anti-realist does in the commodity of truth by manipulating the concept such that it meets their needs, but that it no longer becomes clear what they mean by truth, and that their position become noticeably weakened as a result. The example given for the practice of drawing lines where they do not belong is to be found in constructive empiricism's use of the term 'observability' which, when analysed in any depth, cannot be found to hold a clear-cut meaning.

Now that Fine has objected to both sides of the scientific realism debate, he is now in a place to put forward his own response. This takes the form of the Natural Ontological Attitude which, as the name suggests, takes a common-sense approach towards science without having to tie either the scientist or the philosopher down with more metaphysics than is necessary. There are eight key, interrelated components to NOA which are as follows:

- NOA is an attitude, not a philosophy.
- NOA trusts both the intent of scientists and their willingness to be criticised.
- Science has no special essence as a form of knowledge, and that focus should lie in context specific scientific practice.
- Scientists should be willing to cross 'discipline boundaries' as the application at hand requires it.
- Given that science is a refinement of everyday thinking, induction and intuition should not be seen as an inferior form of knowledge generation.
- To go looking for a special nature of science not only makes science artificial, but is actually detrimental and ultimately futile.
- There is no dualism between facts and values and that any information gained about values from scientific practice should be built from context specific knowledge.

- The local and the global are simply two different ways of thinking; the local should be dealt with first, and then, if possible, find key themes to form global knowledge.

Fine then goes on to defend NOA by looking at the issues of ontology and truth claims. NOA uses the Tarskian form of truth which emphasises the connection between a thing and its properties rather than between the thing and the universal blueprints. This gives science the freedom to change concepts as required without having to feel as if it has ripped holes in the sacred tapestry of reality which the realist feels they are dealing with.

This nonchalance towards the universal blueprints is made clear when the conjunction problem is considered in light of NOA where it is perfectly acceptable under NOA to think of light as both a particle and a wave. If we test the conjunction and it works, then use it, if not, try again; NOA is very pragmatic in this regard.

§6.2: Alan Musgrave's Objections to NOA Revisited

In chapter three we analysed Musgrave's objections to NOA where it was shown that he had taken the NOA chapter in *The Shaky Game* out of context and had misunderstood not only quantum mechanics and its importance for NOA, but also the distinction between Tarskian and correspondence truth. From the quantum perspective, reciprocity was the first issue to be discussed which is the idea that to measure something, we must interact with it, hence it cannot be independent of us. With Musgrave's common-sense, classical knowledge of physics, he says this is pedantic when it comes to classical objects such as the moon, and rightly so, but when considering the famous EPR paper and the ensuing experiments, we see that Fine has a strong case. Even Einstein struggled with the implications of quantum theory and how it dealt with spatially separated systems and one could be forgiven for siding with Musgrave at this point. However, unlike Einstein, we now have access to Bell's Theorem and the Aspect group experiment that confirms Bell's Theorem. Bell's Theorem provides a method for statistically testing local theories of quantum mechanics, and by analysing specific examples of the Aspect experiment we see that for certain angles, Bell's Theorem is violated which means there is no local theory of quantum mechanics compatible with experiment. What this means for Musgrave is that it is actually the interaction with one wing of the Aspect experiment that influences the result in the other wing, i.e. this is a clear example where reciprocity appears to create what is real.

We then move onto the strongly related concept of contamination which says that the results of measurement give us information about interacted with things, not things in themselves, hence the information is contaminated. Again, Musgrave's classical understanding of nature leads him to think it is absurd not to think of the moon as an entity independent of us. However, it is Musgrave's misunderstanding of Kantian ideas, which he derides, that leads him down this slippery slope. The difference between Musgrave and Kant, and by implication Fine, is that Musgrave feels that we are able to pull information off of reality at will whereas Kant feels that nature gives us information and we are to organise as best we can. It is NOA's use of Tarskian truth rather than correspondence truth that best emphasises this. Quantum physics again shines through for Fine since we cannot interpret quantum phenomena directly through our senses but instead through the results of our measuring equipment which adds an extra layer to Kant's intuitions and further denounces Musgrave's view.

Bohr, who is probably the most famous philosopher of physics, exemplified contamination with his work on the complementarity principle. The complementarity principle states that it is impossible to measure the values of incompatible quantities to an arbitrary accuracy in the same experiment, no matter how well the experiment has been set up. It is our interaction with the system that disturbs it which in turns gives results that are contaminated. In short, contamination cannot be avoided.

Although I had been fairly critical of Musgrave at this point, there was a slight glimmer of hope in that he recognised that quantum mechanics may have been an important factor in Fine's formation of NOA. Unfortunately, we see that Musgrave does not realise that physicists are experimentally justified in believing as they do rather than being influenced by bad philosophy. He also misunderstands, and hence misapplies, the principle of complementarity. Both of these serve to highlight the claim that Musgrave is unqualified for making claims on a position strongly influenced by quantum physics.

Moving onto the truth component of NOA, and given that Musgrave is an epistemologist, it would seem reasonable that he would correctly identify the truth claims made by NOA. However, Musgrave appears to misunderstand the difference between the

Tarskian truth that NOA espouses and the correspondence truth that common-sense realism propounds; he simply says there is no difference.

Fine emphasises the fact that NOA can be used as a California-pure, core position in the scientific realism debate. Musgrave claims that this cannot be the case since he believes NOA deals with correspondence truth claims while anti-realist positions do not, hence NOA cannot be thought of as a core position. Musgrave is correct in saying that anti-realists do not hold to correspondence truth, but this is irrelevant. Even an anti-realist is able to be committed to the link between, say, electrons and charge without making definite claims regarding the existence of either electrons or charge. That is to say, anti-realists hold to Tarskian truth and NOA is still valid as a core position.

When examining the difference between Tarskian and correspondence truth closer, we see that the difference between the two forms of truth is one of metaphysical access, i.e. the realist feels he is justified in making claims about the universal blueprints while the NOAer feels this is akin to chasing a phantom. By looking at Musgrave's chosen phrase 'There is a full moon tonight' we see that such a statement is ambiguous when it comes to commitment. A NOAer would read that statement as saying "what we call the moon holds the property of being full" whereas the realist would go further by adding "… and the moon exists". Nature does not recognise 'moon-ness', but properties, and only those properties seen in the universal blueprints no less. Therefore, to go further than the properties we think we know of in our commitment claims is nothing more than chasing a phantom.

§6.3: NOA's Relationship to Structural Realism Revisited

Part B of this thesis was designed to show that even though Fine is justified in proposing NOA given the weirdness of quantum mechanics, realism is not as dead as he boldly proclaims. This becomes clear when we realise that NOA is very similar to structural realism, and it is in this direction we moved to find a viable form of scientific realism.

To first understand how NOA and structural realism compare, structural realism is described. It is a position designed to account for the major argument from both the realist and anti-realist sides of the argument, specifically the 'no miracles' argument from the realists and the 'pessimistic induction from the history of science' argument from the anti-realists. Structural realism is able to do this because it emphasises the structural components of scientific theories rather than theories as a whole. It is able to recognise that equations, say, are able to transfer across a theory change which allows it to avoid the pessimistic induction argument. Also, because of this focus on continued components, we have no need to consider the success of science a miracle since theory change acts like evolution in that effective components are maintained while the less useful ones are discarded. We also took a detour into the nature of truthlikeness to show that NOA's insistence on acting locally, pragmatically, and with no regard for special essences shows it to be a worthy position.

Now that we have an understanding of the key points of structural realism, we can see that NOA and structural realism are actually quite similar. The structural realist, unlike other forms of realism, use Tarskian truth, i.e. structural realism keeps its commitments to within the web of knowledge without extending this commitment down to the level of the universal blueprints. It is because of this detachment from the universal blueprints that both NOA and structural realism give science the freedom to change entities and concepts as required without worrying that they are defiling any sort of Holy Grail.

There would be no point in adopting this form of realism over NOA if it was unable to answer Fine's objections to realism. To accomplish this goal, we first revisited Fine's objections to the methodology of the realist. It was found that Fine was really asking the realist to do the impossible. First of all, Fine asks the realist to essentially explain explanation and then show this is a good explanation, which is impossible. Fortunately for the structural realist, they are able to look at what theoretical components have been carried over during theory change to get an idea of the truth content, or the adequacy of explanation, of current theories. Secondly, Fine wants the realist to provide a stringent, deductive method for arriving at their position; it needs to be more stringent than scientific practice itself. Given that the 'laboratory' of philosophy of science is the history of science, which would require inductive methods, and that science is a social discipline with 'laws' less constant than that of nature, it would seem that Fine is again asking the realist to do the impossible. All one needs to realise is that nature and the scientific endeavour are two different things requiring two different types of explanation; this explanation is not structural realist specific, but is certainly compatible with it.

Next is the problem of conjunctions. Here, structural realism agrees with NOA in that all theory conjunctions should be tested, just like any other theory, before we make claims about their adequacy. However, when it comes to wave-particle duality, the structural realist is able to provide a more comprehensive description since it considers particles as a convenient way of describing property bundles as part of a coherent web of knowledge *as well as* allowing science to get on with the business of doing science and changing as necessary. Structural realism is like a NOA 2.0 in this regard.

Lastly, we dealt with the small-handful problem. The first of the questions making up the small-handful problem was why the handful is so small given the infinite number of possible theories. Fine only thinks there are an infinite number of potential theories because he regards hypothetico-deductivism far too highly. Fine appears to ignore two other methods for generating scientific knowledge, namely the 'strange result' and 'bootstrapping' methods. The strange result method involves recognising experimental results contrary to commonly accepted science and attempting to devise an explanation as to why this result has occurred while the bootstrapping method involves taking results from seemingly disparate phenomena and using them to come to a consensus which gives a single explanation for all of these phenomena. In both cases the handfuls must be small since it is difficult coming up with even one explanation suitable to account for a strange result, while for the bootstrapping method, as the number of phenomena to be explained grows, the number of potential explanations to account for them all shrinks. Again, this is not structural realist specific, but certainly compatible with it.

The second of the questions in the small-handful problems asked why there was a conservative family resemblance between theories. From the standpoint of the structural realist, it is the continuation of theory components that give the family resemblance. Unlike the common-sense realist, which must rely of the approximate truth of the theory as a whole, the structural realist is able to recognise the importance of these components and the ones that remain through theory change are the ones that work and contain a significant measure of truth content.

The third, and final, question in the small-handful issue is why this method of narrowing theories down works so well. To see why, we look at the evolutionary model of theory change allowed by structural realism. As the 'ecology' of the scientific endeavour changes with the development of new theories in related disciplines and new measuring equipment, theories in the current landscape are attacked and defended, and these theories are adapted or replaced. It is in light of these newer theories that we can recognise the successful components of the older theories and why they worked well and by using this method we are able to not only produce successful theories, but *more* successful theories.

§6.4: The Plausibility of Quantum Realism Revisited

Knowing that structural realism has been shown to be a suitable successor to NOA, we adopt the policy of letting science influence our philosophy of science and see what quantum physics has to tell us about reality. This was done by analysing two questions: Does it means anything to speak of the individuality of a quantum particle, and what does quantum physics tell us about what is real?

One form of structural realism, known as eliminativist ontic structural realism holds that quantum physics shows us that the individuality of the quantum particle is a misnomer and it is for this reason that we evaluated what quantum physics had to say on the matter. We began by looking at Leibniz's Principle of the Identity of Indiscernibles and the different forms it can take to gain an idea of the most reasonable way we can distinguish between particles. At this stage we established that either spatio-temporal properties should play a key role, or that we should just look at the monadic, non-relational properties of particles to determine their distinguishability.

After a discussion on quantum statistics which emphasised the difference between the classical Maxwell-Boltzmann statistics and the two quantum forms, Bose-Einstein statistics for bosons and Fermi-Dirac statistics for fermions, we moved onto the Indistinguishability Postulate which says that if we can arbitrarily change the particle labels and none of the expectation values for the system change, then the particles in that system are indistinguishable. An example of this, which NOA would look on approvingly, is that of shot noise where, due to the Poisson distribution of photons, we are unable to say exactly how many photons are emitted from a laser. NOA would say that if the physics still works even though we do not know the exact number of photons, then there is no need to get bogged down in unnecessary ontology claims.

However, digging deeper philosophically can bring some surprising results. By analysing the Indistinguishability Principle closer, we see that to only consider monadic, non-relational properties as a means for determining distinguishability would make it trivially true. So, we are left with whether or not to include spatio-temporal properties. To come to a conclusion on this matter, we looked at what it meant for physical properties when exchanging non-physical tags. The reason why quantum statistics is so different from classical statistics is because quantum particles do no behave independently like classical particles do; each particle interacts in a melded state that is strongly entangled with the other particles in the system. So, just like it would make no sense to speak of tagging an air molecule with a specific sound, it makes no sense to tag particles as individuals until measured. Rather, the tags show the potential states and how that particle mingles with the rest of the entangled system. It is now that we can see why French and Redhead make the claim that the Principle of the Indiscernibility of Individuals is irrelevant to quantum individuality.

Now that we have reason to believe that quantum particles do not behave as individuals we moved on to what, if anything, that we can consider being real in the quantum realm. In the last chapter of *The Shaky Game*, Fine attempts to answer this question with particular reference to Bell's Theorem. By defining two types of realism, minimal realism which covers the bare essentials one must assent to and still be considered a realist such as the carrying of properties, and reductive realism which emphasises factorisability and locality. Unfortunately for reductive realism, this means that it must treat quantum particles as individuals which we have already given strong evidence to the contrary for this being an accurate representation of reality. That leaves minimal realism, which thankfully for the realist, considers the reality of the system rather than of the particle, and once this is done, Bell's Theorem loses the sting it once had with regard to quantum realism. However, the real question lies not in whether the system is real, but what it is about the system that is real. We have reason to believe that it is both the wavefunction and the probability density function that give us an insight into the reality of quantum systems.

Lastly, we considered what all of this means for the plausibility of quantum realism and what the prospects are for future research in this area. It is recognised that ontic structural realism has made great headway into developing a legitimate quantum realist position, but even so, there is still much work to be done. For example, one must consider the objections raised by Pauli's Exclusion Principle which gives us reason to believe that fermions actually are distinguishable. One must also realise that to say that to say that quantum particles are not individuals opens up more question including: if quantum particles are not individuals, what are they?

I am not alone in my urging other physicists and philosophers to consider these fundamental questions more deeply. Valentini claims that quantum physics has been wandering in the philosophical darkness for too long and that we should not be content with a 'shut up and calculate' type attitude to quantum physics. Santos also holds out hope for quantum realism by realising that no definitive experimental evidence has surfaced that once and for all denies the viability of local realism on the quantum scale.

Finally, the emerging field of nanoscience is proposed to help answer questions regarding what happens to quantum behaviour as it borders the classical realm, and in particular, how the transition from quantum to classical statistics plays out.

To conclude, it is Fine's knowledge of the history, philosophy and science behind quantum physics that gives him the required justification to propose NOA. Musgrave, in objecting to NOA, is seen to have taken NOA out of context and hence is unable to appreciate the more subtle points raised by Fine. That said, structural realism is capable of providing a suitable successor to NOA and that by continuing research in this area, we may be able to develop a comprehensive quantum realist position and maybe even see Einstein's ideal of a re-envisioned quantum mechanics as a result.

Neo*: I know you're out there. I can feel you now. I know that you're afraid... you're afraid of us. You're afraid of change. I don't know the future. I didn't come here to tell you how this is going to end. I came here to tell you how it's going to begin. I'm going to hang up this phone, and then I'm going to show these people what you don't want them to see. I'm going to show them a world without you. A world without rules and controls, without borders or boundaries. A world where anything is possible. Where we go from there is a choice I leave to you.*

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