RECONSIDERING SCIENTIFIC REALISM: A CRITICAL ANALYSIS OF THE NO MIRACLES ARGUMENT

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ABSTRACT

RECONSIDERING SCIENTIFIC REALISM: A CRITICAL ANALYSIS OF THE NO MIRACLES ARGUMENT

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In this study I'm examining the recent history of realism debate in the contemporary philosophy of science. I am focusing on the central argument to support realism which is first coined together by Putnam in 1970s and has been the object of countless criticisms from anti-realist philosophers ever since, namely the No Miracles Argument(NMA). I have surveyed through some canonical anti-realist objections to NMA like the criticisms from base rate fallacy, the claim of committing rule circularity, the role of theory change and problem of approximate truth. My main goal is to assess to which extent aforementioned main criticisms are defeat-able from a traditional realist framework and in which cases we should fall back to more cautious and possibly narrowed positions regarding realism. Synthesizing what we can gather from these objections to revise our undertaking of realism, I propose a cognitive turn towards a more feasible, localised and flexible approach to realism in light of the evidence I've gathered from my survey.

Keywords: Scientific realism, No Miracles Argument, Theory Change, Verisimilitude

ÖZ

BİLİMSEL GERÇEKÇİLİĞİ YENİDEN DÜŞÜNMEK: MUCİZE YOK ARGÜMANININ ELEŞTİREL BİR İNCELEMESİ

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Bu çalışmada bilim felsefesindeki gerçekçilik tartışmasının yakın tarihini inceliyorum. Bilimsel gerçekçiliği desteklemekte merkezi bir rolü olan ve ilk olarak Putnam tarafından ortaya atılan Mucize Yoktur Argümanı'na odaklanıyorum. MYA 1970'lerde ilk kez dile getirildiğinden beri pek çok anti-realist eleştirinin odak noktası oldu. Bu çalışmada kanonikleşmiş itirazlardan taban değeri yanılgısı, döngüsellik iddiası, teori değişiminin rolü ve gerçekliğe yakınsama problemi gibi itirazları ele aldım. Asıl amacım bahsi geçen ana eleştirilerin geleneksel realist çerçeve içerisinden ne ölçüde yanıtlanabildiğini ve hangi noktalarda realizm anlayışımızı kısıtlamak veya daha mütevazi bir realist pozisyon almak gibi çözümlere gidilmesi gerektiğini incelemekti. Bu eleştirilerden çıkarabildiğimiz sonuçları sentezleyerek, klasik realizmi tamamen benimsemek yerine realizm anlayışımızda bilişsel bir dönüş alarak daha lokal,daha uygulanabilir ve esnek bir realizm anlayışını benimsemeyi öneriyorum.

Anahtar Kelimeler: Gerçekçilik, Mucize Yoktur Argümanı,Teori Değişimi, Döngüsellik

To my pomegranate tree, my beloved mother

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LIST OF ABBREVIATIONS

NMA	No Miracles Argument
PMI	Pessimistic Meta Induction
SPR	Selective Pragmatic Realism
SR	Standard Realism
QFT	Quantum Field Theory
QM	Quantum Mechanics
GR	General Relativity
SM	Standard Model of Particle Physics

CHAPTER 1

INTRODUCTION

Have you ever wondered why our scientific theories are so successful in their predictions? Are they pointing to an underlying reality? Are scientists making genuine discoveries about some real phenomena, or do all they do is explain patterns in our sensory experience to ourselves in an organized way? This awe has a particular philosophical importance as it's a step in the direction of determining our metaphysical, epistemological and semantic stance regarding our ways of examining this universe we're in. Therefore in this study, I will focus on the Scientific Realism and Anti-Realism debate in the philosophy of science.

Although the realism debate can be traced back to earlier debates in epistemology regarding empiricism, and the first anti-realists can be considered as Hume and Berkeley; the modern debate around scientific realism has its roots in the late 19^{th} century. There have been disputes among some physicists regarding the methodology they should follow. The main focus of the conflict was whether we should take scientific theories as true descriptions of the universe or not. The other alternative was regarding them as tools for organizing our perceptions, or more scientifically, the observations. Philosopher and mathematician Poincare opted and argued for the latter, and suggested Conventionalism, according to this view theories are defined as conventions to organize empirical data(1905).

Later on, in the latter half of the last century, post-WWII atmosphere also shows its effects on the philosophy of science. With Thomas Kuhn's revolutionary book about the structure of scientific revolutions, the realism debate also evolved significantly. Kuhn pointed out the discontinuities or quantum jumps in the history of science and talked about paradigm shifts to explain these, where the underlying assumptions in a scientific domain are undergoing massive changes. According to his descriptions, scientific inquiry is like solving a puzzle, an internal dialogue within a specific paradigm rather than a description of an outside reality(1962, pp. 52-65). Furthermore, Larry Laudan raised the biggest challenge for standard realism, which is known as pessimistic meta induction; similar to Kuhn's criticism from theory change; he argued that the discarded successful theories of the past suggest a pessimism regarding the truth of the current ones(1981, pp. 33-34).

To tell the other part of the story before we begin our discussion, we should note that scientific realism has also undergone significant evolution in response to these challenges. Structural realism emerged as a compromise position. This view, initially developed by John Worrall, maintains that while specific scientific theories may not describe the world as it is, the mathematical or structural relations they describe can be approximately true(1989, pp. 117-118). Structural realism thus focuses on the continuity of the mathematical structures between different theories, arguing that these aspects of theories are likely to survive theory change. Selective realism, another modern development, argues that not all aspects of scientific theories are likely to be true but that certain parts, particularly those that are responsible for successful predictions, may correspond to reality. For example, while the broader context of a scientific theory may be revised, key mechanisms or structures within it may remain valid, and true to the external world.

Note that there's a significant jump from the 19^{th} century debate among physicists to the 20^{th} centuries' more philosophical considerations; earlier the debate was about determining the methodology that will be of importance to theoretical physics and also centred around notions of space and time. We will restrict ourselves to the contemporary part of this age-long debate, which is more philosophically oriented, hence we're starting around the 1970's, with Putnam and Boyd's strong version of realism. Particularly I will examine the debate surrounding the No Miracles Argument, as it has a compelling claim about our motivating question. My main task is to re-examine major criticisms of this argument to detect the solvable and unsolvable issues on the contemporary scene of the debate and determine whether we could keep a considerably realistic stance regarding this issue. Let me re-introduce the idea of scientific realism in its full depth, introduce some sub-types of realism and recite the aforementioned argument first; then I will be able to introduce the aim of this study more properly.

1.1 Scientific Realism

In the introduction to the first of his collection of philosophical papers, Mathematics, Matter and Method, early Putnam tells us:

> The statements of science are in my view either true or false (although it is often the case that we don't know which) and their truth or falsity does not consist in their being highly derived ways of describing regularities in human experience. Reality is not a part of the human mind; rather the human mind is a part - and a small part- of reality (1975, pp. 73-74).

This quote could considered to be a summary of traditional scientific realism with its full force, it suggests a commitment in metaphysical, semantic and epistemic dimensions of realism, which will be made clear later on. But since the early days of Putnam a lot has been changed, therefore what we will be discussing throughout this study should incorporate more recent version(s) of the view. Let us have a look at Chakkravartty's definition in SAP then: "Scientific realism is an umbrella term pointing to many sub-schools in the Philosophy of Science that share a common positive epistemic attitude towards scientific inquiry and its findings"(2017). But this is also a very broad way to define what scientific realism encompasses. To get a more detailed understanding of the term, let us first start with realism as a philosophical concept in general and have a look at its vertical and horizontal dimensions.

1.1.1 Realism

In general, the term "realism" is used to describe a positive attitude towards the existence of the relevant objects of inquiry about which we are concerned. For instance, if you believe in the existence of lines, numbers and operations you are called a mathematical realist or Platonist. Whereas if you accept the existence of the objects that are independent of mind, then you are a realist, ontologically. Because of the broadness of the scope, let us partite it into some vertical dimensions first.

1.1.1.1 Vertical Dimensions of Realism

Following Kukla, we can divide the realm of objects into four sub-groups, and a belief in each of them indicates adherence to different degrees of realism.

1. Sense-data
2. Common everyday objects
3. Unobservable entities posited by scientific theories
   - a) Detectables
   - b) Undetectables
4. Abstract entities like numbers, sets and so on (1998, p. 3).

Note that the first category only consists of our perceptions, such as ‘appearing to be red’, whereas the second one includes trees, birds, and silver particles. The line between the second and third categories is drawn at the border wherever our five senses permit. Things detectable with the help of scientific equipment are still considered "unobservables". At the same time, there is a category under the third group for things that are not detectable with scientific equipment but posited to exist by theories, called undetectables (Chakkravartty, 2007, p. 15). The difference between the category of undetectables and the fourth category is timelessness; the objects falling under category four are timeless, whereas objects in category 3b have a lifetime.

Let us clear our terminology before we move on. Kukla lists the philosophical stances corresponding to each, as follows: Phenomenalism is a belief in the existence of the first set while denying all the others; a belief in the existence of the second category suggests commonsense realism; the third one corresponds to scientific realism, and the fourth one is known as Platonism or mathematical realism (1998, pp 3-5). If we consider our vertical axis to start from the first one and rising up towards the fourth, every new step already includes the previous ones. For instance, a scientific realist accepts objects on categories 1, 2 and 3 as real.

Keeping this picture in mind, the views of scientific realism and anti-realism are not contradictory but contrary. The arguments against one do not necessarily support the other. For instance, an argument against scientific realism might favour the other three hierarchical categories here. For instance, an argument that shows a weakness of scientific realism and is motivated by a scepticism towards our perception also applies to everyday observable objects. That type of argument, therefore, would support Phenomenalism rather than scientific anti-realism, which also accepts the first and the second categories together and only problematizes the third category. What we can gather from this dimensional analysis is that there are common grounds between realism and anti-realism although their naming is unfortunately implies a complete opposition. For instance both the realist and the anti-realist accept the success of science, especially the obvious predictive success. The point that they are diverging would be in whether this success needs an explanation or not, and if so which explanation we should commit to. But before diving into that discussion let me complete my promise of introducing the dimensions, by following with the horizontal ones.

1.1.2 Horizontal Dimensions of Realism

Our general definition of scientific realism as a positive epistemic attitude towards science, including unobservables, hides some details about the exact scope of that attitude. Earlier we've mentioned a quote from Putnam, which points out to three pillars that classical realism had. There is no consensus on what these pillars are. Niiniluoto, for instance, gives five sub-categories within realism in the realms of semantics, ontology, epistemology, methodology and theory construction(1999, pp. 13-14). But I will consider only three of as the distinguishable tenets, following Putnam(1975), Psillos(1999), Kukla(1998) and Chakravartty(2017).

1.1.2.1 Semantic Realism

Realism in the semantic dimension asserts that truth is a semantical relation between language and reality. Following the same line of thought, the semantic component in scientific realism refers to taking scientific propositions literally. For instance, Snell's Law is true if and only if the light diffracts with the specified angle when transpassing from a medium with a different refractive index than the previous one.

Hence, scientific propositions have truth values determined by real-world events. The linguistic turn in philosophy in the 20^{th} century, resulted in some philosophers seeing no reality outside of language, denying semantic realism in general. Instrumentalism, which objects to the semantic element in scientific realism, is considered as the most potent form of anti-realism since the commitment to this semantic component is the weakest form of realism.

1.1.2.2 Metaphysical Realism

As an ontological thesis, realism is the doctrine that there exists a mind-independent reality. We've discussed the subtleties of ontological realism when discussing horizontal divisions in realism. Hence, a repeating will be avoided here. The commitment to scientific realism regarding ontology is the acceptance of unobservable objects, whether detectable or undetectable. Although accepting unobservables when supporting the reality of scientific theories seems counter-intuitive, these entities actually constitute a considerable part of modern science. For instance, electrons are non-extensive objects; their radii are zero. As such, they are non-observable (furthermore non-detectable) according to our division in part 1.1.2. However, they are indirectly said to be detected by their effect on other particles and objects. Thompson discovered them when experimenting with cathode rays, due to evidence of something having a mass of one-thousandth of the mass of the proton and acting like it has a negative charge.

The counter-thesis of the ontological commitment of scientific realism suggests that everything in the universe is observable or can be reduced to the observables, this thesis is known as Reductionism. Instrumentalism, which denies semantic commitment, also necessarily objects to the ontological part of realism(Kukla, 1998, p. 37). Both Instrumentalism and Reductionism are forms of positivism, which can be defined as accepting that the universe equals a sum of observable objects and that there is nothing beyond our perception, theoretically. Here, I will give the benefit of the doubt to the anti-realist and take the 'observable' as equivalent to 'detectable' in this discussion since the scientific equipment can be considered an aid to our five senses and enlarges our sensory-perceptive power. Considering that, at some point in history, all scientific measurement equipment was first built by observations made through our senses or older devices built by inferences from our sense-data, this chain, if it could be traced back, will end in our five senses in any case. Hence, what should be a matter of serious dispute in the metaphysical dimension is the status of the "undetectables" -like electrons- which are posited by theories and can only be confirmed indirectly by relying on theoretical properties associated with them.

1.1.2.3 Epistemic Realism

Epistemically, realism posits that obtaining knowledge of the mind-independent reality is possible. Mostly, this attitude amounts to a rejection of the Kantian notion of *noumena* or things in themselves. Our knowledge might be uncertain or incomplete, but it is about reality (Niiniluotto 1999, pp. 84-88; Chakravartty 2017, Section 1.2; Kukla 1998, pp. 10-13).

A scepticism towards this dimension of scientific realism while maintaining the other two, results in the constructive empiricism of Van Fraessen. He accepts a semantically realist attitude, but according to his view, the existence of theoretically posited entities cannot be known(1980, pp. 12-13). Although the Instrumentalism we mentioned earlier also entails anti-realism in the epistemic dimension, constructive empiricism doesn't entail a rejection of the semantic dimension of the realist commitment and does not necessitate Instrumentalism.

Now that we've covered different dimensions, let us also examine what sub-schools are active in the present day because a full commitment to the original form suggested by early Putnam is a rare occurrence in contemporary debate. Even Putnam himself left that position behind after his early writings. To give you some spoilers ahead, these type of selective commitments will be helpful in responding some anti-realist claims and in the end they will become handy for our suggestive method to leave this debate behind and move on.

1.2 Main Varieties of Realism

1.2.1 Entity Realism

According to this version of realism, posited by Hacking(1983) and Cartwright(1983), we should believe in the entities postulated by scientific theories but be sceptical of the theories themselves. Here, what an entity means is not limited to physical objects; it is a more encompassing term, including forces, fields, processes, waves and the like.

Hacking's argument for this position is motivated by practical rationality, resembling a philosopher just walking to disprove the impossibility of motion implied by Zeno's paradox. The central argument of this position is known as the manipulation argument, which states that we are able to manipulate some entities and use them as tools or instruments. We observe their indirect effects when we use them in our experiments and get data accordingly. The famous one-liner of Hacking, "If you can spray them, they are real"(1983, p. 23), refers to a solution of electrons. It elegantly shows the core tenet of this position, which is that the belief in the microscopic or unobservable entities should not be based on:

1. Whether underlying theories imply their existence or not.
2. Whether we can see them with the right equipment.

Our belief in the entities mentioned above should be solely based on the fact that we can use them instead.

1.2.2 Structural Realism

As opposed to entity realism, structural realism suggests being sceptical of the entities while recommending belief in the underlying mathematical structure. For instance, Snell's Law, mentioned before, survived the test of time, although it was first formulated when natural scientists thought there was an aether in which light waves travelled. The law turned out to be true independent of the aether's existence.

To understand structural realism in more depth, we need to define what structure is first. Let us have a look at the classical definition given by Russell(1927, pp. 254-255).

Definition: if a set α has a relation **R** in it and another set let's call β has another relation denoted as **S** and $\forall a$ in set α there exist a corresponding b in set β, furthermore if for a_1 and a_2 in α which has relation **R** within themselves, the corresponding b_1 and b_2 in set β has the relation **S**; then the tuples $(\alpha, \mathbf{R})$ and $(\beta, \mathbf{S})$ are similar in structure.

However there's a problem with this definition, any trivially arranged set can be ordered and show structural similarity to another one. Therefore, to get a meaningful structure, we must require the relation in consideration to be an objective feature of the world, not only a mathematical trick. The problem is that judging which relation is essential and related to the objective features of the world already requires a knowledge of things beyond structure.

A second attempt to give a definition of structure might be in terms of relations themselves. However, even if we redefine the concept that way, then the structure is also lost in the theory change, which is the core problem we will examine when we dive into PMI debate in the following sections. To briefly mention, entities entail structure, so the elimination of entities results in the elimination of structure too. Therefore committing persistently to one of these two, across the theories, is not an easy dedication to take.

Sub-types of Structural Realism are:

1.2.2.1 Epistemic Structural Realism

According to this view, we can only learn about structure or abstract relations. Whether there are underlying entities responsible for the observed structure is beyond our knowledge.

1.2.2.2 Ontic Structural Realism

According to this view, there is nothing beyond structure, so it is not a problem of knowledge; there is nothing else to know. Ladyman thinks a type of under-determination observed in quantum mechanics supports this type of structural realism (1998, pp. 419-420). This under-determination can be explained by an experiment where there are two particles, let us call them A and B, and two boxes Box_1 and Box_2. Classically, there are four combinations:

- A and B in Box_1,
- A and B in Box_2,
- A in Box_1 and B in Box_2
- A in Box_2 and B in Box_1

Each of these configurations can occur with a 25% possibility classically. When we look at the situation in quantum mechanics, there are only three cases:

- A and B in Box_1,
- A and B in Box_2,
- one of them in Box_1 one of them in Box_2.

To sum up, we cannot distinguish whether A is in Box_1 or B is in Box_1 in QM. Therefore, there is a 33.3% chance for each configuration. The identity of indiscernibles suggests that A and B must be identical then, since they are indiscernible. However, earlier we assumed there are two entities A and B, so they should have been distinguishable.

Ladyman suggests the solution to this puzzle as follows: If the ontology of QM consists of objects, then the problem of the individuality of these particles is unsolvable by physics. Therefore, the ontology of QM should be one of relations only (ibid, 422). When we accept OSR, there is no such paradox related to underdetermination. The problem with this view is that OSR fails to capture the difference between mathematical and physical structures. A general problem with structural realism also appears: it is biased towards Physics. This structurality is generally very suitable for Physics, especially the theoretical part of Physics, whereas other sciences like Biology, Geography and Geology are not so precisely abstractable.

1.3 No Miracles Argument

Many arguments favour scientific realism, but we will examine the most famous one, known as the "No Miracles Argument". First uttered by Putnam as:

> The positive argument for realism is that it is the only philosophy that does not make the success of science a miracle. That terms in mature science typically refer [...], that the theories accepted in mature science are typically approximately true, that the same terms can refer to the same things even when it occurs in different theories - these statements are viewed by the scientific realist not as necessary truths but as part of the only scientific explanation of the success of science and hence as part of any adequate scientific description of science and its relation to its object (1975, p. 73).

Notice that this is a case of reasoning that is an example of *modus tollens*:

- If scientific realism were not true, then the success of science would be a miracle.
- There are no miracles.
- Therefore, scientific realism -the thesis that successful scientific theories are true- is true.

Although the original version mentions a truth, later on, that claim is converted to 'approximately true', which encaptures the scientific process more closely since a certain truth would be a significantly strong demand from any hypothesis. Although this is the most known form of the argument, let us put its content in a more formal and slightly different form. This form expands the 'miraculousness'. The hidden implication under this term that other explanations fall short of the explanatory adequacy realism provides. Let us call this form NMA*:

P1) Science is extremely successful.

P2) There must be a reason why science is successful.

P3) Scientific realism -accepting that successful scientific theories are approximately true- provides a better explanation for the success of science than any other theory.

P4) We should accept the philosophy of science that best explains the facts about science.

C) We should accept scientific realism.

In this form, the argument has four premises: the first one is observing a fact that is been accepted by both the realists and non-realists, the second and third premises are the direct result of Putnam's original *modus tollens* on scientific success, the fourth one is actually a prescription commonly known as inference to the best explanation and was hidden in the original argument. The conclusion is similarly a prescription in this case.

The first premise of NMA* is generally accepted by people of the realist and anti-realist camps alike; as we've stated, anti-realism and realism are not contradictory but contrary. In the horizontal degrees of realism, the step from item 2 to item 3 was the point of disagreement between the two camps. Unlike a relativist or social constructivist, an anti-realist already accepts P1 of the NMA*. So, this premise- regarded as an established fact- is hidden in most articulations of NMA. Here, success could be interpreted from many dimensions, such as technological or medical developments that are cases of application of science or the predictive success most scientific theories achieve. Some objections to NMA are directed to P2 and P3, either denying that realism(especially the epistemic component) is the best explanation of this success or that there needs to be an explanation at all. Premise 4 is an explicit station of the method used - it is not especially stated in Putnam's formula but inherent to the deduction itself- it is known as 'Inference to the Best Explanation'; it is a prescriptive claim, and we will see that it becomes a ground for objection, too. Finally, some objections are regarding the form of the argument as a whole.

The argument seems so intuitive that some people may even wonder why it had to be stated and debated. However, when we look closer, we will understand why there is a huge debate surrounding this argument. We will put under the spotlight some of the main objections to this argument in the contemporary scene of the realism debate. We are going to consider their details and try to assess whether there are satisfying responses to those objections and figure out whether realism (or a particular variety of it) can still be supported or not.

As many known fronts of the debate surrounding No Miracles Argument have resulted in ennui and each side is just talking past the other, as detected by Magnus and Callender (2004), I will try to incorporate the findings from these specific criticisms of NMA into realism to account for anti-realists'rightful concerns whenever they're raised, within a considerably realistic framework. Some main fronts of discussion that I will address here are the criticism from the circularity, the criticism from base rate fallacy, the issues regarding the concept of approximate truth, and the theory change criticism. Some other major issues are omitted due to the limitations in scope. For instance, the problem of whether the realist explanation is the best explanation and whether anti-realists could provide a better one is left out of this discussion because comparing all the suggested anti-realist explanations in literature against the realist explanation is beyond my expertise and would require a full-length book to make each of them justice. Due to similar reasons, the underdetermination of theory by data is left out of the main discussion, although it can be read as an objection to NMA indirectly. The pragmatist objection that there's no need of an explanation for such tremendous chances, would require a proof from that camp. Since demanding an explanation and asking 'why?' are not supposed to be crimes, especially in the field of Philosophy, I will insist that the burden of proof in that case, is lying on the anti-realist if they raise such an objection indicating that looking for an underlying mechanism for such a success would be pointless.

Returning back to the major objections that I will confront here, my purpose is to show that even if we can't hold onto traditional scientific realism when we assess the issue on some major fronts, we can apply a strategy of narrowing its scope, similar to what has been done with choices of structural and entity realism in the past.

A Brief Map of What's Next

In the next chapter, we will examine the criticism from base rate fallacy(BRF), which is a common probabilistic thinking error. Some anti-realists claimed that NMA commits this fallacy by ignoring the base rates. We will examine whether NMA is really probabilistic, we will see the realist answer that the debate at the end turns out to be about selecting the prior base rate and that's a philosophical matter rather than statistical, finally, we will see the local and global facades of NMA and how that realization defeats the accusation of base rate fallacy altogether.

In the third chapter, we will consider a similar objection to NMA, this time from circularity. We will examine different types of circularities, refresh our epistemology toolkit with externalism and internalism and see that an externalistic framework will defeat the objection from rule circularity. We will also have a look at Fine's more meta-philosophical criticism stemming from a Hilbertian demand and will demonstrate how getting into the roots as he suggests, will undermine his demand's validity.

In the fourth chapter, we will consider the most famous of objections to NMA and realism, namely pessimistic meta-induction. Analyzing realist objections to this attempt of meta-induction, like eliminative inference considerations and random sample studies to show how the claim about most theories' undergoing change is a false accusation, finally we will add the exponential growth rate of science and improvement in scientific methodology in the picture to show how the non-realist portrayal of the history of modern science is really that, non-realistic.

Finally in the fifth chapter, we will have a look at the debate surrounding the concept of approximate truth, analyze two famous attempts to define the concept rigorously and recognize how it's a project that is doomed from the beginning to trying to define this concept. However, the efforts were not futile, with Niiniluoto's definition of approximate truth it appears that we can utilize philosophical concepts, attribute degrees and continuous values to some values rather than binary thinking and use them to guide us. We will synthesize this new approach with the learnings from instrumentalist criticisms in the discussion.

In the sixth chapter, after clearly examining the debate centring around NMA from different fronts and in-depth, the ennui appears more and more on the surface. As we will see later on, some fronts of the debate are just turning out to be inherently unsolvable. In some cases, the careful examination of a proposed criticism in more computable fronts- like whether there is a base rate fallacy, attrition discussion under theory change and determining the size of successful theories pool - the discussions brings us back to our starting point. When we carefully calculate probabilities in such cases, it turns out that whichever side of the debate you initially adhere to, and want to bet on, mathematically you're destined to end up inferring that one. Hence, your initial choice results in a self-fulfilling prophecy and you keep turning back to square one. Therefore, I suggest a different approach than a fully epistemic or philosophical discussion of the matter. After re-evaluating the existing issues covered in the literature, we see that directing our discussion with pragmatic and cognitive tilts can save the day and point out a potential pathway to the solution of this decades-long ennui. Opting for weaving pragmatic considerations into our discussion when epistemic and metaphysical ones left us in a dissolution, we can find a solution that satisfies demands of the both sides of the debate at the end, keeping in mind that these views are not contradictory but contrary, and utilizing shared axioms, I'm proposing to commit to a selective pragmatic realism based on the previous discussion, in this chapter. What I will be suggesting at that point will be to defend a realism that's updated to encapsulate rightful criticisms raised by the contraries like instrumentalists and constructive empiricists. But for now, let us begin with our first step, which is dissecting the BRF criticism to depict its weaknesses and how we can reject the accusation from a realist framework.

CHAPTER 2

BASE RATE FALLACY IN NMA

2.1 Introduction

At the beginning of the new millennia, the "Ultimate Argument for Scientific Realism" was being criticised from different fronts. One of the charges it was accused of was committing a common error in probabilistic thinking, namely the base rate fallacy. Philosophers like Howson (2000, p. 52-59; 2013, p.205-206), Magnus & Callender (2004, pp. 324-326) had claimed that they have depicted this erroneous thinking pattern in the argument. Although it is not as serious as the other accusation of "begging the question" by Musgrave (1988, pp. 237-238), ultimately, these two criticisms turn out to be equivalent to each other and we will see that each can be dismissed after a careful analysis. Let us introduce the criticism first and then we will elaborate on why this criticism is missing its target when it is aimed at NMA.

2.2 Colin Howson's Criticism of NMA

Howson gives a probabilistic formulation of the No-Miracles-Argument (2000, pp. 52-59; 2013, pp. 205-206) as follows:
Let S represent the statement: "A theory h is predictively successful." and T represent the statement: "The theory h is substantially true."
$\rho(A|B) :=$ the conditional probability of event A occurring given event B and $\neg A :=$ It is not the case that A.
Given this scheme, the no-miracles argument can be rewritten as the following probabilistic argument; let us call it NMA-P:

P1) $\rho(S|T)$ is quite large

P2) $\rho(S|\neg T)$ is quite small

C) Therefore $\rho(T|S)$ is large

Howson then goes on to claim there is a base rate fallacy in the argument because the base rates of true theories among predictively successful theories, namely $\rho(T)$, are not taken into account. However, to better grasp his criticism, let us refresh ourselves about the fallacy mentioned above and Bayesian probabilities.

2.2.1 Base Rate Fallacy

Consider a ghost haunting Europe; the ghost of black death rises from its grave, and another epidemic is at our door. The medical professionals are on a whim to depict who is infected and trying to prevent the disease from spreading, so they test random persons who apply for mundane everyday medical symptoms for the *Yersinia pestis* infection. Suppose your friend Boccaccio visited a doctor and has been tested for the grueling disease, unfortunately they tested positive. You know that the prevalence of the disease is now relatively common: 1 out of 1000 people in your country have the disease. The test they use has a $\%5$ false positive rate. Should you worry? Many people answer "yes!" and they think that there is a 95% chance that their friend is actually sick. This is considered a base rate fallacy in the literature. Let us look closer at the data to ease your worries for your friend:

- $\rho(D) = 0.001$
- $\rho(P|D) = 1$
- $\rho(P|\neg D) = 0.05$

Where $\rho(D)$ represents the prevalence of the disease, $\rho(P|D)$ stands for the probability of a person having the disease and testing positive, and $\rho(P|\neg D)$ stands for the false positive rate. Note that we have taken $\rho(P|D) = 1$ since the false negative rate is never mentioned, and we took it to be zero; therefore, everybody who actually has the disease is assumed to have tested positive. The probability concerning us as a worried friend is $\rho(D|P)$, which is the rate of actually having the disease among people who tested positive. The fallacy occurs because we tend just to subtract $\rho(P|\ D) = 0.05$ from 1 and get the value 0.95. According to Bayes' theorem about probabilities, we are totally mistaken in doing so, and the actual result is surprisingly low, so your friend is most likely to be healthy.

Bayes'Theorem:

$$P(B|A) = \frac{P(A|B).P(B)}{P(A)} \tag{2.1}$$

But we need to expand this a little bit to see where the erroneous thinking pattern lies, clearly:

$$P(B|A) = \frac{P(A|B).P(B)}{\sum_{i=1}^{n} P(A|B_i)P(B_i)} \tag{2.2}$$

Expanding the previous formula for $i = 2$ since our case is a 2-valued case where either our friend have the disease or they have not, we get the bi-nomial formula:

$$P(Y|X) = \frac{P(X|Y)P(Y)}{P(X|Y)P(Y) + P(X|\neg Y)P(\neg Y)} \tag{2.3}$$

When we use equation (2.3) in our case, the probability of Boccacio having the disease becomes:

$$\rho(D|P) = \frac{\rho(P|D).\rho(D)}{\rho(P|D).\rho(D) + \rho(P|\neg D).\rho(\neg D)} \tag{2.4}$$

When we make our calculations using equation (2.4), hence taking into account the prevalence of the disease in the population, we get the result $1/51 \approxeq 0.02$. Which is surprisingly lower than 0.95. The fallacy stems from neglecting the rate of the disease in the whole region. Howson claims the same negligence is occurring in the No Miracles Argument (2013, pp. 205-211). Let us turn back to NMA-P and calculate the odds of a theory being true given it is successful:

$odds(T|S) = \lambda odds(T)$ where λ is defined as probability of $(S|T)$ divided by probability of $(S|\neg T)$, and called Bayes' factor. In this case, premise 1 and premise 2 only tell us that λ is large and say nothing about the probability of the theory being true, i.e. $\rho(T)$. Howson points out the fact that even if λ is quite large; if it is not equal to infinity, we can always find a $\rho(T)$ such that $\rho(T|S)$ is quite small and then goes on to apply the same odds in the medical example we have mentioned to the NMA-P to get the value 0.02 as the probability of a theory being true given it is successful. Therefore, he concludes that we need a further premise such that:

P3) $\rho(T)$ cannot be quite small (ibid.).

For instance, $\rho(T) = 1/2$ might be assigned. However, assigning a high value would create the risk of begging the question since NMA is constructed in the first place to claim successful scientific theories are pointing out the truth. At this point, Howson suggests a subjectivist Bayesian reconstruction of the NMA to save the argument. To see why, let us first try an objectivist reconstruction. For instance, relying on the principle of indifference, we can assign that the probability of T equals to probability of negation of T and they are both 0.5. In this case, if the probability of $(S|T) = 1$ and probability of $(S|notT) = 0.05$, the resulting probability of $(T|S)$ is still greater than 0.95. But Howson claims this indifference principle itself is problematic and causes paradoxes. He gives the example of a book's colour, let us re-iterate:

ρ (book is green) $= 0.5$ and ρ (book is not green) $= 0.5$
ρ (book is red) $= 0.5$ and ρ (book is not red) $= 0.5$
ρ (book is violet) $= 0.5$ and ρ (book is not violet) $= 0.5$

In this case, the probability of the book being green, red, or violet is 1. 5, which is greater than 1 and a violation of the rules of probability calculus (ibid, pp. 205-211). However, this example is a clear example of fallacious thinking regarding probabilities. Let us say your colour palette includes ten colours: Red, Violet, Green, Yellow, Blue, Magenta, White, Pink, Orange, and Turquoise. Then, if we assume an indifferent distribution of colours among books, a random book you are picking in this universe has the chance of being green = 1/10, and the negation of the statement 'book is green' is 'it is not the case that book is green', which in this case has a probability of 9/10. So, assigning the probability of 0.5 to this statement and its negation was a mistake in a universe with more than two colours. The problem is trying to analogously apply this example to the NMA-P case where there are two options for theory h: either it is true or it is not the case that it is true. The principle of indifference would not cause such a paradox in a 2-valued event. With more than two values, we have seen that assigning probability 0.5 to an event's chance of occurring and not occurring is not a correct application of the principle of indifference. However, leaving this discussion aside, Howson also argues that there is no way to ground prior probabilities in any uncontroversially objective manner, i.e., no way to justify assigning even 0. 5 to the probability of a theory being true without being circular. Hence, Howson concludes that subjective Bayesianism is the only possible theoretical environment that can accommodate large enough prior probabilities to turn NMA into a valid argument (ibid, pp. 205-211). I will dig a bit into Bayesianism to understand the debate between Howson and the defenders of NMA more clearly.

2.2.2 Different Forms of Bayesianism

Bayesianism is an epistemological approach which can be summarised with its three basic tenets:

1. Probabilities are rational degrees of belief of the agent. In this view, probabilities are rational estimates that an agent assumes in order to make the best possible decisions.

2. Rationality is defined as assigning probabilities that confirm probability calculus. For instance, if you are rational, you accept that new evidence E confirms your hypothesis H just in case the probability of H given E is greater than the probability of H itself.

3. Bayesian conditionalisation tells us how to update our probabilities over time in response to the evidence. When you receive evidence E, you set $P(H) = P(H|E)$. According to Bayes'Theorem, posterior probabilities are calculated by the prior probabilities according to this formula:

   $P(H|E) = P(E|H).P(H)/P(E)$, where $P(H)$ is called the base rate, ignorance of which causes the aforementioned fall (Bayes, 1764).

Two sub-types of Bayesianism concern us, namely the subjective and objective Bayesianisms.

A) Objective Bayesianism

According to objective Bayesians, further rational constraints must exist on an agent's assessment of prior probabilities beyond probability axioms, such as the principle of indifference. Principle of Indifference states that if there is no reason for predicting our subject one rather than another of several alternatives, then relatively to such knowledge, the assertions of each of these alternatives have an equal probability. One problem with this principle is the question of how we know that they are equal. That is also an assumption in itself. Another example is the famous cube factory example.

B) Subjective Bayesianism

Subjectives assign no further constraints on prior probabilities beyond the probability axioms. One can assign whatever probability they like, provided they are logically consistent. There is nothing irrational about maintaining an unjustified belief, provided that there is no evidence for or against it. Subjectivists have a structural conceptualisation of reality; what matters is how one's belief system hangs together as a whole.

According to this subtype's proponents, whatever a subject assign priorly is unimportant because continued application of conditionalisation will lead to convergence of belief or washing out of the priors. As the evidence comes in, posteriors will converge to the same value. "But is this really the case?", one might object; the evidence cannot be guaranteed to convince all scientists to converge in the same direction. As we have seen many times in the history of science, rival theories exist simultaneously about the same concept. A Subjective Bayesian will object that belief will converge in the long run.

2.2.3 Psillos'Respond to BRF Criticism

We have seen previously that Howson suggests a subjective Bayesian re-take of the NMA. However, there are problems with that approach. One immediate thing is the case that $\lambda = \infty$ is not actually impossible. Psillos mentions this by defining a factor:

$$f = \frac{\rho(S|\neg T)}{\rho(S|T)}$$

f is the multiplicative inverse of λ. The constant λ going to infinity is the case when only one theory successfully explains a particular data (1999, pp. 75-80). This is not impossible, but it is just a rare occurrence. Since the underdetermination of theory by data is another big issue that philosophers in the debate on realism address. We will not dive deep into this subject to prevent a digress, but if we just grant the possibility that not every data is underdetermined and there are some cases in which some evidence is just perfectly explicable by a specific theory h, then there are cases in which $f = 0$, therefore, the first two premises of NMA * depicting λ or f provides sufficient ground for the conclusion without considering the base rates.

The second and more prominent point Psillos points out is that we can just pick $\rho(S|T) = \rho(S|\neg T) = 1/2$ by relying on the principle of indifference. As Howson himself recognised, such a selection of the prior probabilities creates a scenario where the resulting probability in the aforementioned disease testing scenario becomes greater than 0.95, regarded as a highly reliable probability by most standards. This suggests that even if we grant that priors have a vital role in our deduction, they need not be subjective. Base rates and prior probabilities are two distinct concepts, and Howson's criticism suggests that prior probabilities are important in the subjective Bayesian sense. Nevertheless, in reality, base rates are objective statistical data, and they can inform the assigning of priors. For instance, if you know that your friend is a smoker and the given disease is more prevalent among smokers compared to non-smokers, this can lead to an assignment of higher prior probability than the specific diseases' base rate in the whole population and you would be totally justified to do so. Hence, Psillos argues for incorporating objective base rates into Bayesian reasoning to enhance accuracy and avoid the pitfalls of purely subjective priors (ibid, pp. 75-80).

Another mishap of the Bayesian critique of NMA is lying in the assumption that base rates of true and false theories are knowable, representing the case as a purely statistical problem. In reality, there are no clear guidelines to select the set of empirically successful theories, hence depending on your selection, the rate of true theories might be anywhere between 0.00 to 1.00. A prominent of realism will require stricter and stricter conditions to deem a theory successful, hence narrowing the pool to an extent that remaining theories inside the pool are fated to be true. In contrast if one wants to be suspicious of realism to the point of sophism one can enlarge the pool to encompass almost all hypotheses ever mentioned in the history of science hence the rate of successful theories will be close to zero. Therefore, there is a philosophical problem rather than a statistical one, and the base rates are not clearly obtainable. We will be elaborating upon this issue in more detail while discussing Pessimistic Meta Induction in chapter 4; for now, let it suffice that the assumption of certain and absurdly low base rates is just an unjustified claim and needs to be shown rather than being assumed as a matter of fact.

The importance of correct probabilistic thinking may be overrated when it comes to obtaining the truth; Psillos invites us to think of this new scenario: Of the total number of cabs in the city, 85 per cent are green and 15 per cent are blue. There was a late-night hit-and-run car accident, and the sole eyewitness said that it was a blue cab involved. The eyewitness is very reliable: in test situations involving blue and green objects at night, he made the correct identifications in 80 per cent of the cases and he was mistaken in 20 per cent of cases. What is the probability that the culprit was a blue cab? When presented with the cab scenario, most subjects are inclined to trust the eyewitness and give a high probability of the culprit actually being a blue cab. When we make our Bayesian probability calculations, we can see clearly that this is a textbook base rate fallacy; given the ratio of green cabs in the city, there is a high chance that the culprit is one of them. Another factor is that it was dark when the eyewitness saw the event. Hence, there is an ambiguity in the situation, and the subjects should have rendered that into their thinking when faced with such an event. On the other hand, subjects also knew that green cabs are more prevalent in the city. Therefore, the eyewitness should have expected to see one of them and should be more inclined to think it is a green one when, in the darkness, they cannot actually tell the colour of the cab; despite this fact, they have reported seeing a 'blue' one, this factor actually increases the reliability. All in all, the subjects are actually right to conclude that the witness reported the truth; if they were unsure, they would have defaulted to the more prevalent colour rather than the other one. So, if we want to reach the truth, there is no certain answer to when to include base rates and when not to take them into account. In this example, we see that there are case histories relevant to specific events, which would be more important factors than base rates. When we transfer this analogy to our discussion, we see an ironical twist occur: If we accept that the prevalence of false theories is very high, then one might well be inclined to say that a theory h is false, given its success. Then, if the eyewitnesses (the scientists, in this case) say that a specific theory T is approximately true (despite that this is unlikely, given the base rates), they should be trusted – at the expense of the base rates (2009, pp. 63-68).

Now let us modify the cab example a little bit; the subjects are told that 85 per cent of car accidents are caused by blue cabs and 15 per cent by green cabs. In this case, the subjects concluded the culprit was actually a blue car, taking the base rates into account, but not only for the sake of thinking correctly; rather, it was causally relevant information that helped them in the process of obtaining truth. Applying this to scientific theories, if there is actually an explanatory power in false theories being more prevalent among successful ones, then this information should be taken into account when deciding whether a successful theory h will actually be about some observable regularities in nature, i.e. pointing out some truth. Conversely, even if we grant the base rate of true theories is low, there is an explanation as to why true theories are successful. Therefore, this causally relevant information should play a role in our thinking over the aforementioned base rate. Even if the posterior probability of h being true -given that it is successful- is low, this has nothing to do with the individual theory: it is just about low base rates. This situation can be described by a saying in Turkish, "kurunun yanında yaşı da yakmak", which can be translated as "burning an alive tree with an old forest of dry trees", which is obviously not the right action to take. Taking novel theories which are predictively successful to default to false ones would equate to the imaginary courts in the aforementioned scenario always deciding to press charges against the green cab company in the city when lawsuits about accidents are discussed just because the green cabs are more prevalent in the city.

In conclusion, we should not disregard the importance of case histories in favour of general base rates in the population. Regardless of the prevalence of true or false theories, a specific theory in its own domain will remain approximately true if it fits clearly with the underlying structure of the domain and the data it is supposed to explain. Psillos drives our attention to the double helix model of DNA or the explanation of the anomalous perihelion of Mercury for instance (2009, p. 65). The fact that "truth is hard to get and it is a rare occurrence in the history of science to get to the truth" does not affect whether DNA has a double helix or not. We should consider the model itself and how it explains or cannot explain certain phenomena occurring in the cell. This leads us to our next section, a distinction made by Magnus and Callender between retail and wholesale arguments.

2.3 Magnus and Callender's Criticism and Distinguishing Two Forms of NMA

In their paper "Realist Ennui and the Base Rate Fallacy", Magnus and Callender take neither a realist nor an anti-realist stance, but they want to examine the scene of contemporary realism debate in the philosophy of science and detect an ennui caused by opponents talking past each other (2004). The effects of this frustration have reached a degree where some authors just suggest taking an agnostic stance and leaving the issue aside. Some philosophers like Blackburn (2012) and Maddy (2001) suggest a dissolution rather than a solution to the debate and propose that there is nothing beyond the empirical success of science and it needs no explanation other than being empirically successful, that is all. This meta-philosophical stance of not doing philosophy at all about this issue is not what Magnus and Callender suggest, however. They point out the fact that "wholesale" arguments that both sides are using are the main underlying problem that causes proponents of each idea to talk past each other (2004, pp.320-321).

On the one side, there is the No Miracle Argument, which is appointed "the ultimate argument for realism", and on the other side, there is "Pessimistic Meta Induction", which is the go-to theorem for undermining the realist claim. The authors take a neutral stance towards both opinions, and they claim these two arguments are on an equal footing when it comes to probabilistic thinking, which is that they commit a base rate fallacy. I will skip their criticism of the Pessimistic Meta Induction since I dedicate another chapter to this argument itself, and the criticisms will be discussed there.

Magnus and Callender re-formulate NMA probabilistically to show where the fault lies; their formulation is parallel to what Howson did, so a re-iteration is pointless. Diving into their criticism, we see that they also observed that ignoring the base rate of true theories among successful ones is the real problem here. They state, "One can also point out that "gruesome" hypotheses shortcircuit the argument, for there is an infinity of these that will make the same predictions as our successful theories."(year,pg). This is another way to formulate the underdetermination of theory by data and how our pool of hypotheses that successfully predicts the same outcome is much larger than the realists believe. They also recognise a deficiency in the probabilistic formulation, which is that h is an element in the set of current theories in a specific mature domain in science. This adds a new premise to our NMA-P, let us call it P3:

$Pr(h \subset H) >> 0$ where H denotes current theories in mature science. And the argument- let us call it NMA-MC becomes:

P1: $Pr(S_x \& x \subset H) >> 0$

P2: $Pr(S_x | T_x \& x \subset H) >> 0$

P3: $Pr(S_x | \neg T_x \& x \subset H) << 1$

C: $Pr(T_x | S_x \& x \subset H) >> 0$ (ibid, pp. 323-325)

After we add the missing premise, it becomes a matter of deciding whether premise 1 obtains and if it obtains whether success is a reliable indicator of truth. However, it is impossible to count up all theories of a mature science and check whether the first premise is obtained. Magnus and Callender claim the realist can insist on choosing H to include only the theories actually professed by the mature sciences. In this case, premise 1 will be true and premise 2 will hold regardless of the connection between success and truth, hence trivially true, while probability in premise three will be almost zero, trivially. Therefore, realists would avoid the base rate neglect but with a price of sample selection bias (ibid, p. 325).

I disagree with that since the realist claim itself here is that the sample of "current" successful and mature theories in a given scientific domain would be approximately true; hence, there is no reason to include already discarded theories in the mature sciences. We obviously need to consider the actually ‘professed’ ones if we are talking about "current" theories of a mature science. Let us say if we are talking about general relativity’s success in explaining physical phenomena pertaining to the field if we include luminiferous aether theory in our sample of hypotheses/statements about relativity, any 4th-year physics undergrad would be amused at our ‘ignorance’ or would deem us crazy. If the part of the deal of being empirically successful and belonging to a mature domain of science already implies approximate truth and that is what diminishes premise 2 to a trivial truth and diminishes the conclusion to a truism, then this is an acceptance of the realist claim by the authors at this point. If one’s definition of success already includes such an elegant, selected sample of theories that they almost need to be approximately true, then the scientific process that eliminates them in such a way is a successful judge, and that is the point of scientific realism tries to make in the first place. We will see this reply in more detail when we talk about Henderson. If we come back to Magnus and Callendar, due to the aforementioned reasons, they see the debate of realism centring around NMA as pointless and futile; they claim it is impossible to get fruitful results since it boils down to showing either pool of past scientific theories which seemed to be successful turning out false one after another -by the anti-realists- or trying to show that current pool is narrower and more inclined to be including mostly true theories -by the realist-(ibid).

To solve this so-called "dilemma" Magnus and Callender suggest we can move forward by distinguishing between wholesale and retail arguments. No Miracles Argument, in its general form, is a wholesale argument, a generalisation about all of science according to their definition of wholesale. Therefore, the aforementioned result of chasing one’s tail in futile attempts to justify the realist position depending on NMA is helpless. What is suggested here, instead is surprisingly close to Psillos’conclusion of leaning into case histories and grounding a scientific statement in their relevant field to assess whether the claims resemble parallels to the realities they try to describe. Magnus and Callender suggest leaning into retail arguments instead of wholesale ones (2004). Retail arguments are about specific theorems at specific instances of space-time, to be evaluated at their own merit. This brings us to Henderson’s reply, which is going to solve the whole debate around the base rate fallacy.

2.3.1 Henderson's Reply to the Base Rate Fallacy Criticisms

Henderson points out two different readings of NMA; one is global, and the other is local (2015). Even though it is similar to Magnus and Callender's wholesale vs. retail distinction, Henderson concludes in the end that the target of the criticism is the local one (given by Musgrave, Worrall et al.) as opposed to the global one (Putnam, Psillos et al.). We see that the global version actually supports the local one and protects it from the alleged charges of BRF. To see how, let us look at his distinction first:

Global Version of NMA(G-NMA)

P1) If realism is true, the fact that the best confirmed successful theories in mature sciences are successful in new domains is just what we expect.

P2) If realism were not true, then success would be astounding- a miracle-.

P3) Theories are empirically successful.

C) Therefore, probably realism is true, i.e., our best confirmed mature and successful theories are probably approximately true.

Local Version of NMA(L-NMA)

For a mature theory that is well-confirmed and successful,

P1′) If h is approximately true, the fact that it is successful in new domains is unsurprising.

P2′) If h is not approximately true its success would be unexpected.

P3′) h is successful in new domains.

C′) Therefore probably h is approximately true.

Note that the adverb 'probably' is added later on to the original argument by the aforementioned authors and Henderson, whereas it was not explicitly stated in Putnam's argument; hence, actually, it is still a matter of debate whether NMA is probabilistic or not. Henderson detects that Howson's argument targets the local version of NMA, and it does not apply to the global one (2017).

Given theory h, let $T = h$ is approximately true and $S = h$ is predictively successful. **L-NMA with probabilities** is:

i) $P(S|T)$ is high

ii) $P(S|\neg T)$ is very low.

iii) $P(T|S)$ is high.

NMA was initially accused of committing BRF because it ignores $P(T)$. We need an extra argument to make this argument valid, which is that $P(T)$ is not very low. Adding this as a premise helps with its validity but messes with its soundness. She then formulates two possible replies to this criticism.

Reply 1: Minimising the importance of priors

This response is based on the convergence of priors as discussed in the Subjective Bayesianism subsection. Repeated and varied successes drive the NMA conclusion, so one prior probability of being higher or lower does not matter in the end. In the corroboration process, updating our priors each time in light of the success of the theory, the updated probability will converge to higher and higher values for $P(T|S)$ (ibid).
One caveat with this approach is that we do not know how long we will wait before the realist claim accrues. Well, in the long run, we are all dead.

Reply 2: The Baselessness of the Base

Even if we do not deny the role of base rates in NMA, it is not justified to take the whole range of theories as our population (ibid). Let me explain her answer to BRF by going back to our medieval disease case, we portrayed a pessimistic scenario where there is a pandemic, but even in that case, the doctors do not randomly draw individuals from the streets to test for the disease. In the normal flow of everyday life, people visit doctors'offices when they show symptoms, and the medical professional runs a differential diagnostic process in their mind and evaluates the patient before directing them to any test. So, the sampling process is such that individuals who are sent to take the test are already much more likely than the general population to actually have the disease. Therefore, a rational prior probability should not be the rate of the disease in the general population but some other rate, such as that of doctors'previous patients who presented with the same symptoms and then actually turned out to have the specified disease. In that case, it is not that unlikely for the tested individual to actually have the disease of concern in that scenario.

Transposing the situation to the scientific theories, if the scientific method selects a group from the general population which is not just a random sample of all theories that fit the data, then it is not appropriate to take the prior probability as the base rate of approximately true theories in the overall population of alternative theories. Therefore, it is not unlikely that the theory is approximately true, given that it is successful. The point at the centre of the issue in the realism debate is whether the scientific method produces a good sample among all possible alternatives. In the global NMA, the claim is about the scientific method itself; it is not a probabilistic argument committing a BRF. The realist hypothesis can be summarised in a statement as the following:
R: The theories the scientific method produces as best confirmed in their given field are mostly approximately true.

Now, the essential question is whether we have the means to assess the sampling process itself. Going back to the disease case, checking the track records of the doctors'office and examining how many of the patients they have suspected of the disease turned out to have the disease is a way to assess the reliability of sampling. We also know the rates of the patients who tested positive and then the disease progressed and they showed other more indicative symptoms later on. If the proportion of positive test results among the sampled group is considerably higher than the proportion of the positive test results in the general population, then we can say that the sampling was not random and had some merit. G-NMA is similar to our answer about the disease scenario. The claim is parallel: A high overall proportion of success among theories serves as evidence that the scientific sampling procedure is not random. So, the scientific method is biased toward approximately true theories. Therefore, the prior probability of the best, confirmed theory h being approximately true is not very low.

Thus, the L-NMA is supported by G-NMA and protected from the BRF, as Henderson concluded (ibid).

The previous may seem like a futile effort to save L-NMA, and it may be read as just transferring the problem a level higher to the G-NMA. However, in G-NMA, although there is still a question of what priors to assign to R and $\neg R$, which is the anti-realist antithesis of R, there is no base rate fallacy as claimed. The G-NMA is not analogous to the medical examination case; it is just an alternative hypothesis about the nature of the scientific method used in the sampling process. So, until the realism debate is solved in favour of either one of the sides, we can take prior probability $(R) \sim$ prior probability $(\neg R)$.

2.4 Conclusion

To summarize in a list, we have found out in this chapter that:

i) It is appropriate to use the base rates as priors only if your theory is randomly selected from the overall population, which is not the case with successful scientific theories.

ii) The global form of the NMA takes scientific success in general as evidence that the sampling provided by the scientific method is not random.

iii) The local version of the NMA is unleashed from BRF since it is shielded by the global version.

iv) Magnus and Callender's retail/wholesale discussion is not helpful since the BRF criticism actually targets the local (retail) version, unlike the picture they have painted in their article.

All of these provide us with rightful reasons to reject the accusation of Base Rate Fallacy in the structure of this argument.

CHAPTER 3

CIRCULARITY CHARGE AGAINST NMA

3.1 Introduction

In the second half of the last century, scientific realism was charged with a crime, the most unforgivable one a philosophical argument can commit, namely begging the question via their central argument, NMA. Critics like Laudan (1981) and Fine (1984, pp. 86-90) raised this criticism in their papers on top of the minor offences they have pointed out. Laudan calls this Petitio Principii of realism in his paper A Confutation of Convergent Realism (1981, pp. 45-48). According to him, the general strategy of the realist argument is:

> ...an abductive inference which proceeds from the success of science to the conclusion that science is approximately true... that epistemic realism can be reasonably be presumed to be true by virtue of the fact that it has true consequences (ibid, p. 45).

Although he does not explicitly give the argument's name, it is clear that he refers to the No Miracles Argument. I will first look at their criticisms and then argue that they were unjustified in their objections. I want to warn you that I won't give a strong defence, that is I'm not going to deny the occurrence of some particular form of circularity -called rule circularity- in the process of defending the truth-conveyingness of scientific theorems with an argument in the form of inference to the best explanation. Rather, I will argue that there are subtleties under the label of circularity, and sub-types of it are bound to occur. It's a phenomenon that's occurring all over our domain of inquiry, because some substantially fundamental rules are non-justifiable otherwise. When it comes to rule circularity, under certain conditions, it's expected and harmless. By making a detour into logical foundations and philosophy of mathematics, we will see that epistemic systems are destined to be either inconsistent or not all statements in them are totally derivable from within the system. Therefore some things are necessarily bound to be axioms, hence they are used without rigorous justification and derivation. Therefore there will be rule circularities, in every system that worth talking about, at the end. If this phenomena is inevitable, there's no point in demanding a philosophy of science that is pure of such occurences, as it can't exist.

Let us recall our argument, and let me recite it in a way that brings all the hidden premises to the surface:

P1) Science is extremely successful.

P2) If we cannot explain the eminent success of science, then it is a miracle.

P3) There are no miracles.

P4) There must be a reason why science is successful.

P5) Scientific realism- accepting scientific theories are approximately true- provides a better explanation than any other approach in the philosophy of science.

P6) We should believe in the philosophy of science that best explains the facts about the success of science.

C) Therefore, we must accept scientific realism.

In this version, which I will name E-NMA, where E stands for extended, premises from 1 to 4 are the original no miracles argument, and premise 4 is the conclusion of the first three. In the remaining of the argument, premises 5 and 6, and the conclusion bridges the unmentioned gap from NMA to its intended implication: scientific realism. As the careful reader would recognize, premise 6 is a prescriptive item and a re-iteration of a method of thinking, namely, Inference to the Best Explanation. This premise is the one that causes the circularity charge mentioned by opponents of realism.

Laudan presumes his aforementioned criticism by the following lines:

> But this is a monumental case of begging the question. The non-realist refuses to admit that a scientific theory can be warrantedly judged to be true simply because it has some true consequences...If non-realists are chary about first-order abductions to avowedly true conclusions, they are not likely to be impressed by second-order abductions...(ibid, p. 45).

Here; Laudan mentions non-realists' istaste of accepting a scientific theory as true just because its consequences are true, in other words, just because it is successful in one way or another. Then he goes on to indicate that since the core argument of the realists also applies a similar form of inference called IBE, which is an abduction, a non-realist would not buy that kind of an argument either, just like they did not buy the truth of scientific theories, which are a result of the same abductive reasoning.

What Laudan tries to convey with his criticism is that a non-realist, for instance an instrumentalist, who does not even support the form of inference which is a first-order abduction in scientific theories, would not consider a second-order abduction from a general pattern of predictive success in the history of scientific theories to a result of approximating truth. In that case, using abductive inference -the inference rule that he thinks a realist tries to justify with NMA, whereas the argument is only advocating for approximate truth of successful theories- is begging the question. One misunderstanding here is that the realists don't try to justify the rule of inference called IBE with the No Miracles Argument. Although a realist would defend the usage of IBE, it's not with this particular argument NMA. Wİth this, we have the purpose of justifying the legitimacy of scientific theories' truth by using IBE, IBE itself is assumed to be a legitimate method of inference from the beginning. However in the conclusion of E-NMA where we've extended all the premises clearly to see what is actually going on, we prescribed a belief in the truth of scientific theories . Since scientific methodology relies mostly on IBE, there's indeed an indirect link with what NMA concludes and the rule of inference called IBE, or abductive reasoning in general. Hence we will examine and form a response to this criticism, even though there's no direct circularity in the argument as claimed here.

Arthur Fine - The Shaky Game

Arthur Fine brings up a similar criticism to Laudan's, albeit starting from a slightly different ground. According to Fine, meta-theorems used to evaluate a theorem must be stronger than the theorem of concern (1986, pp. 113-117). He grounds this demand on Hilbert's program in mathematics. Starting with set theory, he mentions: "If one were concerned over the consistency of set theory, then clearly set-theoretic proof of consistency would be of no avail." (ibid, p. 114). The example of set theory is given to explain the more general demand of Hilbert from mathematics; he wants to construct a formal system that is finite to use in meta-mathematics. Fine bridges the gap between this mathematical pursuit of the 20th century and the philosophy of science without providing a reason to do such an expansion. Fine's argument for demanding a stronger meta-theorem for any given theorem is given as:

> Meta theoretic arguments must satisfy more stringent requirements than those placed on the arguments used by the theory in question, for otherwise the significance of reasoning about the theory is simply moot. I think this maxim applies with particular force to the discussion of realism." (ibid, p. 114).

As we see, no bridging statements are provided other than "I think this applies to...". Maybe, to Fine, an expansion of Hilbert's meta-theoretical demands regarding mathematics to the philosophy of science, a meta-thinking about whole science not particularly mathematics, is an obvious move without a need of justification. Let us grant that it is such, but keep in mind, in that case, the same loose move from meta-mathematics to meta-science should be justified when we respond to Fine's objection mentioned here. Hence, we will also start with Hilbert's program and show how it is a stricter demand than what realistically can be met in the field of meta-mathematics. Then, we will also expand from the meta-mathematics domain to the meta-science domain as this criticism itself made the same loose move.

Leaving that logico-mathematical discussion to further sections, there is also a second problem with this demand: it assumes a stringent or stronger inferential rule or means of justification is always possible to begin with. According to this view, there is no end to where we can begin and build our arguments, and there must always be a way to go one step back and prove our axioms. Hence, there is the possibility of infinitely regressing to more fundamental grounds than the said theorem in this perspective. For instance, if you are doing set theory, you can step back to logic and when you are doing logic, there is some meta-logical axiom that justifies your moves in logic, and for that axiom there must be another previous one even from a ‘purer’ domain. It is obvious that this process should stop somewhere, since we do not have an infinite collection of more reasonable reasons, this reductionist dream is non-realizable. Note that Fine also accepts that Hilbert’s strongest demands from mathematics are shown to be non-realizable by Gödel (ibid, p. 114). Nonetheless, he still insists that they should be demanded regardless. In that case, knowingly requiring a condition shown to be un-meetable by the nature of the endeavour would only result in an absolute sophism that take us nowhere, about any intellectual pursuit at hand. For now, we will not dive deeper into this because a lengthier response to this demand is provided in the following sections.

Fine completes his aforementioned criticism with a similar result to Laudan’s; he goes on to conclude:

> In that case [if anti-realists, instrumentalists are right] the usual abductive methods that lead us to good explanations(even to "the best explanation"), cannot be counted on to yield results even approximately true. But the strategy that leads to realism, as I have indicated, is just such an ordinary abductive inference. Hence, if the nonrealist[s] were correct in his[their] doubts, then such an inference to realism as the best explanation would be of no significance- exactly as in the case of a consistency proof using the methods of an inconsistent system. ... In particular, one must not beg the question of the significance of explanatory hypotheses by assuming they carry truth as well as explanatory efficacy (ibid, p. 115).

Let us now respond these two famous circularity criticisms one by one.

3.2 Responding to the Circularity Charge

First, I want to note that the defence will be: guilty as charged. Yet the crime is not as obnoxious as it is portrayed, and it is inherently inevitable; it is one of the so-called lesser evils. In general, some degree of circularity is inherent in any epistemic system. Our task is not to eliminate it but rather to ensure that it is not vicious and does not damage the system’s reliability. Within any complex system of justification, as we will see when we refresh our memory with Gödel’s theorems, some form of interdependence among methods and principles is inevitable. Let us understand what is going on with NMA and realism in more detail to assess whether the circularity involved is a game-breaker.

3.2.1 Types of Circularity

Definition: When the conclusion of an argument is identical or a re-iteration of one or more of its premises, we call it circular.
Nevertheless, not all circularities are created equal, and the end goal of the specific argument must be taken into account when determining whether the circularity is vicious or not. For instance, when you say $P \wedge Q$, therefore $Q \wedge P$; if you are just trying to show the commutativity of logical conclusion to use it as a principle in your derivations, then the circularity is not vicious. We even have a logical theorem stating "P, therefore P": the rule of repetition, which is coined together to show every sentence is a logical conclusion of itself. However, when an argument offers reasons to accept a certain statement in which one of the given reasons is the sentence itself, the circularity is vicious. We can say that whenever an argument is premise circular, it is viciously circular.

Definition: When you have premises from P_1 to P_n and apply the inference rule **R** to one or more of them to conclude **Q**, and that conclusion Q validates or strengthens the legitimacy of using the rule R, there is a rule circularity in the argument.

Rule circularity is not vicious in general; almost in every subdomain in the domain of Philosophy there are instances of applications of the same rule of inference that have been tried to be justified. For instance, Braithwaite discusses in length why rule circularity is not vicious in the case of inductive inference in length (1953, pp. 274-278). As Psillos recognizes in his book 'Scientific Realism: How Science Tracks the Truth', when we examine NMA, we see that it is only rule circular; the premises of NMA do not do much than point to the theory-ladenness of scientific methodology, therefore no assumption is made about the approximate truth of theories within the premises (1999, pp. 79-85). If we take a look at E-NMA we can clearly see that's the case, the conclusion is true based on being the best explanation of the premises. So NMA is not premise circular since the premises do not guarantee the conclusion a priorily.

In a rule circular argument, it is assumed by the anti-realists that we have to assume the reliability of the rule invoked in the argument, and this would be equivalent to accepting the conclusion priorily. But we reject this picture; there is no assumption of the reliability of the rule when an instance of that rule is used. If we accept an externalist stance in epistemology, the usage of the rule is justified before the rule itself could be justified.

To clarify my point about externalist epistemology a bit, let us look at this case: assume we have made an inference machine such that when we feed true premises to this machine, it always gives true conclusions. Then I claim that we can conclude that the machine is reliable, w/o knowing the intricacy of the machines'mechanisms. In other words, whatever rules this machine operates on are reliable. There is no need to identify the rules the machine operates on and then actively prove that they are reliable to show that the machine is reliable. Some readers might find this example worrysome, but this method is an actual way of evaluation, used in practice. Aforementioned technique is known as black box evaluation in computer science. For instance, if there is a block of code designed to do some task, but you do not actually know what is written in the code; you can test it with appropriate data sets, and it gives the desired outcome every time, you can reach the conclusion that the code-block is reliable in this process without white-boxing it, which means opening the code file and actually seeing what is written, let alone justifying it line by line. There are even data-set trained deep learning algorithms that learn concepts by themselves, give the desired outcomes such as image-recognition, and even the humans who create the said algorithm, don't exactly know how. As long as our virtual machine produces true conclusions, we do not need to question its rule choices. If the machine starts to produce false conclusions consistently, then we would have reasons to worry and we should rectify the code. A possible objection to this portrayal might be that the reliability of the rules of inference does matter for the assessment of the correctness of conclusions, well it is actually not, as we have the data and check the outcome ourselves, we can see the correctness of conclusions without knowing which rules of inference are used, let alone justifying them. In the case of science, we have other external methods to assess those conclusions as well, therefore we can check whether the machine works, I'm talking about coherency among scientific theories here as a way of double-checking whether science works, without going into a full length internalist derivation of the rule of inference called abduction, induction or any other one. Hence, on the matter of deciding whether we should be allowing a rule circularity, the discussion becomes reduced to an issue of one's epistemological stance. Externalist accounts of epistemology reject the implication that to be justified in using a reliable rule of inference; we need to know or have reasons to believe that the rule itself is reliable, therefore we can just give up on trying to justify our established inference rules, or get satisfied with some rule-circular arguments which amplifies our belief in the rule itself.

3.2.2 Externalism and Internalism in the Context of Rule Circularity

If we commit to an externalist approach in epistemology; then to justify a conclusion that is derived by the usage of a certain rule, it is enough that the rule being used is reliable. In this case, the requirement for a rule circular argument is not different from any other applications of that rule in first-order arguments. So, the requirement is that the inference rule being employed in a rule circular argument is being reliable. As we've demonstrated with the virtual machine example above. The viciousness level of a rule circular argument is not different from any first-order argument, which is an application of the same rule. The conclusion of the argument just asserts that the rule itself is reliable; it is like an ampliative argument. Justification requires no more than the reliability of the rule in this case.

The internalist undertaking of epistemology, on the other hand, requires something else beyond the fact that the rule is reliable. To know or to justifiably believe the rule of inference involved is reliable necessitates a reason that is one level above the rule itself. On this account, a separate justification of the inference rule is required. Therefore, if we take an internalist approach in our epistemology, then rule circular arguments become vicious indeed. This implies that a realist who supports their belief by relying on the NMA must hold on to a externalistic position when it comes to epistemic justification.

I suggest to accept the externalist approach and move past the rule circularity problem here. Because if we adopt an internalist version of epistemic justification, since the demand is so strict and unapplicable in most cases, it would take an infinite amount of time to be convinced of anything. And at this point, we all know what happens in the long run. Even the internalists themselves couldn't move forward in their practice, if we apply their very high demands throughout the domain of inquiry of philosophy. To demonstrate our point here, let us consider first-order logic. It seems like we cannot even study much further on that field, because one of the very first rules, modus ponens, is not deducable according to internalist criteria. Modus ponens cannot be proven without an infinite regress, as Lewis Carroll demonstrated very cleverly in his famous story What the Tortoise Said to Achilles (Carroll, 1895).

Let us attempt a formal proof:
Modus ponens:$P \Rightarrow Q$. P. $\therefore Q$

1)	Show Q	Assertion
2)	$\neg Q$	AID
3)	$P \Rightarrow Q$	Premise 1
4)	$P*$	Premise 2
5)	$\neg P*$	2,3 MT

Our proof is complete by the contradiction in lines 4 and 5.

However, we have used modus tollens in step 5, which is another rule; let us try to prove that one.
Modus tollens: $P \Rightarrow Q$. $\neg Q$. $\therefore \neg P$.

1)	Show $\neg P$	Assertion
2)	$\neg\neg P$	AID
3)	P	2, DNE
4)	$P \Rightarrow Q$	Premise 1
5)	$\neg Q*$	Premise 2
6)	$Q*$	3,4 MP

Our proof is complete by contradiction in lines 5 and 6. As the careful reader might have noticed, we used the modus ponens itself in line 6 of the second derivation. Although there might be ways to conduct the second proof without using *modus ponens*, they would require other rules, which in turn will be derived by using *modus ponens* at the end. So, trying other proofs of *modus tollens* in the second case, would just lengthen the radius of our circle. The point remains that there are some basic rules in our first-order logic that cannot be inferred without some form of rule circularity. This proves my point about the inapplicability of the internalistic justification demands in real life, or else we would end up with an infinite wait time to justify and hence be able to use any rule of inference. Some readers might object on the ground that *modus ponens* is justifiable via a truth table. It indeed is the case that truth value of the statement $((P \Rightarrow Q) \wedge P)$ is the same as truth value of statement Q, they are logically equivalent. However, that table would only be demonstrating that *modus ponens* is truth-preserving, nothing more. In that case, what we are doing is equivalent to making an inference machine that always gives true conclusions but we can only run black-box evaluation of the machine to check its reliability without knowing how the machine itself operates. That case is what externalists found enough to justify the rule being used in the machine, so the objection that modus ponens or any other rule of logic can be justified via a truth table, actually strengthens our argument rather than undermining it. However, that truth table documentation of reliability does not hold up to the high standards of internalists, so with the internalistic criteria we can't provide a sufficient proof for even *modus ponens*, then we are doomed to fail, if we were to seriously operate on with such high standards within any intellectual endeavour.

To give another example to the inevitability of rule circularity: we have also indicated that inductive reasoning is non-provable without a rule circularity. The inductive rules are not even truth-preserving, unlike *modus ponens*. Let us say you have an alien friend named E. T. who lacks inductive intuition, i.e., inductively blind. If you try to persuade E. T. that you are right to use induction, you can tell them stories and examples of learning from experience to show it is a reasonable method. However, what you are doing in this dialogue amounts to applying a case of induction. Hence, your alien friend without an inductive toolkit would not be convinced. You are relying on past successes of inductive inference to show it is reliable; someone without an understanding of the implications of past occurrences to a similar result in the present day would not buy it. Does that make induction vicious? Should we discard it completely? Carnap discusses at length in his work, Logical Foundations of Probability, that it should not be the case, and he concludes that the circularity involved in an attempt to vindicate inductive reasoning is both indispensable and harmless (1950). Then we can conclude that the same flexibility and tolerance should be given to the abduction too, no need to be discriminatory among our methods of reasoning. If we don't let rule circularity, or not-yet-justified usage of any inference rule, then we're left with nothing and even internalists themselves can't do their work; if we do let rule circularity, or at least let some rules pass by without a rigorous justification, then why should we not let the same rule circularity when it comes to abduction? Burden of proof lies here in the anti-realist camp, who claim we shouldn't be allowing the rule circularity when it comes to abductive inference in NMA, but at the same time let circularity when it comes to other inevitably fundamental rules and uses some otherwise unjustifiable rules in their practice all the time.

Overall, we can say that no inferential rule carries an absolute rational compulsion unless it rests on a framework of intuitions and dispositions that take for granted the presuppositions of this rule. Let us also remember the fact that NMA does not even particularly aim to defend IBE -it establishes the inference from the success of theories to their true value. Nevertheless, even if we put that aside and the far-fetchedness of the claim that NMA is trying to defend IBE, even if we assume that the main goal of NMA was to defend IBE, still, there is nothing wrong with that. If the rule circularity of defence is an outright vice, then we should give up any attempt to explain or defend any of our basic inferential practices. Hence, if one is inclined to allow us to reason abductively in general then one should have no special problem with using NMA in defence of the reliability of IBE. NMA is no worse than attempts to defend modus ponens and inductive reasoning. As we see, even internalist defences ultimately have to rely on rule-circular arguments. Either we have no defence to offer, or else the attempted defence will rule circular.

3.2.3 Hilbert's Program and Gödel's Response

As we've shortly introduced in the beginning of this chapter, Arthur Fine was criticizing realists'use of NMA with a referral to Hilbert's program (1986, pp. 113-118). To sum up Fine's critic once more, he is asserting that the explanationist defence of realism should give no comfort to the realists because proofs of one's meta-theorems must be more stringent than the proofs of one's theorems and he mentions the use of NMA to defend realism is not holding up to the standards of that requirement. I will request the reader's patience during this informative short side-trip to the domain of philosophy of mathematics, since the objection stems from there we will have to digress there also to be able to reply it by giving its due.

Hilbert's proof-theoretic concerns necessitate that to show consistency of math theorems, more rigid ones outside of that theorem-cloud must be used. Fine insists that this discussion applies with particular force to the discussion of realism in the philosophy of science (1986, p. 114). Although Kurt Gödel's 2nd Incompleteness theorem showed that there cannot be a stringent proof in Hilbert's sense of the consistency of Peano arithmetic - which he was using in the formalization of his incompleteness theories, and this applies to any axiomatic system of necessary strength. According to his findings (or demonstration, to put it more correctly), the consistency proof of an axiomatic formal theory is less elementary than the methods the theory formalizes. It turns out that in complex epistemic systems, some level of circularity is inevitable, and it is not necessarily problematic. Hilbert's requirements are correct in principle but too demanding to be real-izable, the pun intended.

3.2.3.1 Hilbert's Program

Hilbert proposed a formalistic approach to mathematics, which is centred around the idea of finite arithmetic being algorithmically decidable. The aim was to create a formal system to represent branches of mathematics like set theory, real analysis, functional analysis, and so on in a finitary way. This can be considered a reductionist strategy, the trend of the era. A formal axiomatic system consists of three parts: a formal language, a set of axioms, and a set of derivation rules. Hilbert intended to axiomatize all of mathematics so that we would be able to determine effectively by using finitary methods the truth value of a given statement in the language of the system; that is, they are algorithmically decidable. We should also be able to prove the consistency of the system from the axioms of the system. To sum up, more formally, specific properties that are demanded of the ideal system we described were:

1. The system must be complete, which means for any given statement that is written in the system, either the statement or its negation must be deducable from within the system.

2. The system must be consistent, i.e., no contradictions among the statements must occur.

3. Every statement of the system must be algorithmically decidable.

Furthermore, if mathematics is reducible to finite arithmetics and can be represented as a formal system, then that system itself can also be considered a finitary arithmetic. The study of such formal systems would be considered meta-mathematics, and proofs inside such a system can also be represented in finite arithmetic, hence algorithmically decidable.
According to Shapiro's "Foundations without Foundationalism", the summary of the Hilbert's project can be given as:

1. Listing all the symbols in logic and mathematics.

2. Algorithmically deciding whether a concatenation of these symbols syntactically makes sense or not.

3. Checking algorithmically whether a deduction- in other words, a set of formulas- is valid.

4. Finding an algorithm such that all true statements that can be uttered in this system can be proved from within the system (1991).

Steps 1-3 turned out to be feasible, but the fourth one was going to be problematic. Let us re-state the fourth one:

Problematic step: Devising an algorithmic procedure P such that given a true statement ψ of math, P produces the proof of ψ within the intended Hilbertian formal system.

To see the reason for this discrepancy, we need to look at Gödel's theorems.

3.2.3.2 Gödel's Theorems

First Incompleteness Theorem

Theorem: "Any consistent formal system F within which a certain amount of elementary arithmetic can be carried out is incomplete; i.e., there are statements of the language of F which can neither be proved nor disproved in F. " (Raatikainen, 2020, Section 1.1). The proof is omitted here, as it is a well-established theorem by now.
Let **A** be an axiomatic system that is strong enough; Gödel's choice was Peano arithmetics. We can form a statement S in Peano arithmetics that says: 'S is not provable from **A**'.
S is a statement such that in the formal language of **A**, if **A** is consistent, then neither S nor $\neg S$ is provable from **A**. We can represent this as it is not the case that '$(\vDash\ [S|A]) \vee (\vDash\ [\neg S|\mathbf{A}])$'. One may think of adding S to the axioms of **A** as a clever move. However, in that case, even if we have a stronger system, let us call it $\mathbf{A}'$, there will be a new statement S' in this system such that the statement '$(\vDash [S'|\mathbf{A}']) \vee (\vDash [\neg S'|\mathbf{A}'])$' will be false. Furthermore, if you go on to add S' to $\mathbf{A}'$ and get a new system $\mathbf{A}''$, there will be another statement S'', well you have guessed the rest of the story: This process is destined to go on ad infinitum.

To add insult to injury, we can also prove from **A**, a new statement:
Q1: 'If **A** is consistent, then S is unprovable from **A**'. Let us call the antecedent of this conditional, R: '**A** is consistent.' And the consequent is familiar enough; it is S itself. Therefore we can prove from **A** another statement:
Q2: 'If **A** is consistent, then S is provable from **A**.'
Here we have two statements Q1: $R \Rightarrow T$ and Q2: $R \Rightarrow \neg T$. Hence if we assume that R, the system under consideration is consistent, we get a contradiction, ironically. As a result, if **A** is consistent, then **A** cannot prove its own consistency. This gives us

the second Incompleteness theorem.

Second Incompleteness Theorem

Theorem: For any consistent system F within which a certain amount of elementary arithmetic can be carried out, the consistency of F cannot be proved from F itself (Raatikainen, 2020, Section 1.2).

The incompleteness shows us that the formal system Hilbert aimed to establish fails to have Neumann's fourth feature, which was the system being able to prove the true statements effectively. This implies that Hilbert's program to formalize mathematics was utopic, not a demand that can be met by real mathematics; mathematical activity must be more than deductive reasoning over theorems.

3.2.3.3 Philosophical Consequences of Incompleteness

There are two kinds of mathematics to consider when we discuss incompleteness:

Real mathematics: The set of true statements in mathematics we are doing.

Ideal mathematics: Hilbertian formalisation of mathematics.

What Gödel's theorems say to us is that formal mathematics cannot fully encapture mathematical truth. There are statements that are true in real mathematics that cannot be proved in the ideal formal realism. So whenever a system becomes strong enough to prove all Σ_1^0 sentences, it allows us to generate sentences like S. It states an arithmetical truth in the chosen language, but neither S nor $\neg S$ is provable from the axioms of the system. This means that statement S becomes external to the formal system. However, S is still part of the mathematical truth, the real mathematics. Which tells us that mathematics is lacking something; hence, it is incomplete. This is also equivalent to saying truth means more than provability or derivability. The set of provable things does not encompass the set of true things. Also, there is the sentence stating "The system is consistent" among these true but non-provable statements. So, if our conceptual framework is consistent, we cannot show that it is consistent by using the axioms of the system only. The good news is that neither mathematics nor any discipline that includes some amount of defining and inventing could be mechanized. Hence Gödel uttered his famous sentence to sum this conclusion:

"Either mathematics is too big, or the human mind is more than a machine."

The Missing Part of the Puzzle

What is required to capture true arithmetic then? The answer can be found in Kant's philosophy: die Anschaaung or a visionaries' intuition. Kant claimed that mathematical knowledge is acquired through representations of mathematical objects in the pure forms of intuition rather than solely by conceptual analysis. Gödel's theorems support a Kantian approach to mathematical knowledge. Kant did not take ideal mathematics as a basis when he talked about some transcendental principles that are at play in perceiving mathematical knowledge. Gödel's theorems hint that some transcendental elements like intuition and representations of objects in pure space-time forms, are needed in order to capture true mathematics.
Without jumping any loops, we can securely claim that the same thing can be said of all sciences if it can be said about Mathematics which is the most rigorous of them which is studied through direct application of rules of logic. If we accept Fine's expansion of Hilbert's demands about meta-theorems in mathematics to the field of philosophy of science; then Gödel's incompleteness response to Hilbert's demands constitutes the core of our response to his criticism. All in all, we see from Gödel's incompleteness that some irreducibility is inherent to mathematics; the activity that current mathematicians are busying themselves with cannot be mechanized or formalized to be mechanized in the future as demanded by Hilbert. If this is so, and since the same will be true of any deduction based intellectual activity, we can reach to the conclusion that there will always be some theorems, or statements or rules within any domain that can't be derived or justified from within that system. This means there is room and necessity for intuition in the end and that some things are better left to it. As we have shown in the previous section that this difficulty in justification is specifically occuring with the rules of inference - which are so fundamental in nature that most of the time, we cannot prove them without some form of circularity involved, then we can reject Fine's objection on the grounds that such a high-standard is not applicable in reality, so we don't have to adhere to it. So far, we have seen that fundamental logical inference rules and induction are among the rules that can't be internalistically justified, so I'm asking again why should we not allow abduction too?

Now that we've also covered it's mathematically impossible for the aforementioned circularity-less endeavour to exist, we see that what Fine demanded from realists, it is a perfect fantasy, rather than a realistic and reasonable demand from any inquiry. Hence, once again I want to emphasize that if the anti-realist wants to leave abduction outside of their "allowed reliable methods" list, they have to show that its status is different among other rules of inference. They are the ones who have to show why abduction should be treated differently and held up to higher standards than other methods of reasoning, like inductive inference; and why IBE should not be allowed to use rule circularly unlike other rules of logic for instance *modus ponens*. It's not the case that the realists have to desperately try to hold on to some non-realizable standards about their arguments' structure. If the critics, like Fine and Laudan, couldn't come up with such reasons against the usage of IBE, then what Carnap let for induction- that having a circular justification is unavoidable and harmless- also applies to abduction and therefore IBE should be allowed to be used inside arguments, including NMA.

3.2.4 Conclusion

In this chapter, we have looked at two criticisms of circularity to the use of NMA in defence of realism and we have seen that first, the circularity is not premise but rule circularity, and in certain cases, it is an indispensable and acceptable event to occur: for instance, building of set theory, proving modus ponens, justifying induction and so on all requires a room left for the circularity or we will have to wait until eternity to be sure of and use those rules and axioms. Then, we have covered Fine's more general criticism that meta-theorems should require stronger justifications than theorems they are intended to prove or disprove. We have seen that this perfectionist demand, although understandable in an ideal theoretical world, is not realizable within real epistemic systems, which are strong enough. That kind of basic strength, which is being able to show Σ^1_0 sentences, is already transcended when you are doing anything worthy of your efforts, for instance Philosophy. Hence, since those meta-theoretic demands are already demonstrated to be unworkable in Mathematics; it is unjust to expect it in the field of Philosophy where we are not abiding by stringent rules, and the rigorousness sometimes needs to be sacrificed to intuition and exchanged with creativity to get to some points. Otherwise, we are confined in first-order deductive logic where we all know the conclusions cannot say more than the combination of premises; hence, nothing new would ever be claimed; our statements would totally be provable without circularity then, but at what cost?

CHAPTER 4

PESSIMISTIC META INDUCTION

4.1 Introduction

In his article "A Confutation of Convergent Realism", Laudan observes that "some epistemic realists claim successful scientific theories, even if strictly false, are approximately true or close to the truth or verisimilar (1981, p. 30)". Laudan then expands this claim into two parts:

> T1) If a theory is approximately true, then it will be explanatorily successful.
> T2) If a theory is explanatorily successful, then it is probably approximately true (ibid, p. 31).

T2 corresponds to what we have claimed in NMA. Therefore, one of the favourite methods to refute NMA has been giving counterexample lists: finding successful theories in the history of science that have turned out to be false. Other than enlisting these theories, some opponents of realism also made an inference based on these lists that this is a recurring pattern in history and is destined to be repeated over and over again. Therefore, they have formed a meta-induction from most of the past theories to most of the current ones, known in the literature as the Pessimistic meta-induction.

Whereas NMA has been the primary argument for realism, Pessimistic Meta Induction has played the same fundamental role for anti-realism. People have observed that while NMA is built on the success of science, it disregards some other primary phenomena observed in the history of science: ongoing discarding of old theories. Although the history of science looks like an accumulation of knowledge on top of each other, there are instances of radical theory changes when we look at discontinuities. Laudan sums up his claim as, "What the history of science offers us is a plethora of theories which were both successful and (so far as we can judge) non-referential with respect to many of their central explanatory concepts(1981, p. 33)." Indeed, the depiction is correct, many theories or "a plethora of theories" once considered to be the bright stars of their respective constellations are now rejected, but as we will see in the end of this chapter this abundance is misleading and history of science has a lot more than that to offer. Some of the examples that anti-realists usually enlist to demonstrate the phenomenon of theory change can be listed as follows:

-Miasma theory of diseases

-Humoural theory of health and disease

-Ptolemy's astronomical model

-Origin of the solar system as an encounter

-Aether hypothesis

-Coronium element in sun

-Lamarckism

-the vital force theories of physiology

-The caloric theory of heat

-Phlogiston

-Blending inheritance

-Aristotelian mechanics

-Newtonian mechanics

-Catastrophist Geology

-Crystalline spheres of early astronomy

-Behaviorist theories of language acquisition

-Embryonal onthogeny

-Phylogenetic history

-Contracting earth hypothesis

The list can go on and on, and it is exhaustingly vast, and it may gain new members as time unfolds. Lipton summarized these occurences in another famous dramatical exaggeration: "History of science is a graveyard of failed theories" (2005, p. 1265). Pessimistic meta induction is an inductive reasoning based on this observation that can be informally put together as: Most past theories which had once been considered to be successful were eventually rejected, therefore, most current theories will probably be rejected in the future. According to the critics of NMA, this should keep us from believing that the best theories we are now holding as dear will forever stay approximately true. Eventually, the inevitable will happen, and the graveyard will host them, too. To convey the argument more formally, let us re-capitulate with Ladyman and Ross's formulation:

> P1) There have been many successful theories in the history of science that have subsequently been rejected.
>
> P2) Our best theories are no different in kind from the best-discarded theories.
>
> ---
>
> C) Many of our best theories will be rejected in the future, so we should not believe that they are approximately true (2007, p. 228).

As we can see, premise 2 is exclusive to the formal version, whereas it was a hidden assumption in the informal inference. This premise indicates that the scientific method was the same in principle when it led to the theory or origin of the solar system or Lamarckism as it is today. Therefore, according to the anti-realists, we are not magically protected from making new false theories and have no reason to be overconfident about the reliability of current scientific theories when it comes to approximating to the truth, as time goes to infinity.

4.2 How Can We Respond to Theory Change Criticism from a Realist's Glasses

4.2.1 Picking the Correct Theory of Reference

One way to deal with theory change is to think about the meaning. Do not worry; we will not dive into the meaning of life, but we will simply consider the meaning itself. Thinking about how our words refer could be the answer to dissolving the ennui in the philosophy of science. As with everything in 20th-century philosophy, the realism debate in the philosophy of science has also moved to the realm of philosophy of language at some point. Let us have a look at how a linguistic move like Philip Kitcher suggested (1993), if it were as efficient as intended, could solve our problem.

Let me pick a ferocious Queen, Daenerys from the series A Song of Ice and Fire, to accompany us in our little visit to the philosophy of language to refresh our memories about different theories of meaning. If we try to define Daenerys Stormborn, many phrases would show up in our minds instantly: The Unburnt, Queen of Meereen, Silver Lady, Queen of the Andals and the Rhoynar and the First Men, Slayer of Lies, The Dragon Queen, Daughter of Death, Bride of Fire, Khalisee of the Great Grass Sea, Breaker of Chains, Mother of Dragons and so on. One way to answer who Daenerys is, is to concatenate those descriptive phrases, piling them up and binding them with inclusive or connectives. Such as given below:
$meaning(DaenerysStormborn) := \vee$ [The Unburnt] $\vee$ [Queen of Meereen] $\vee$ [the Silver Lady] $\vee$ [The Dragon Queen] $\vee$ [Daughter of Death] $\vee$ [Bride of Fire] $\vee$ [Khalisee of the Great Grass Sea] $\vee$ [Breaker of Chains] $\vee$ [Mother of Dragons $\vee$ [Slayer of Lies]
Hence, the name Daenerys refers to whichever object satisfies these descriptions.

Another way to define the meaning of a name is given by Kripke, called the causal theory of reference (1972). Think of Daenerys the First's birth in 284 AC, Her mother Rhaella Targaryen dies and there is a big storm so they name her accordingly. Later, when they flee to the Essos, Illyrio raises her with that name and all the visitors, neighbours and friends who see the baby associate that name with her. While the years pass by, new people learn the name from each other while hearing some part of Daenerys's story in the context that they hear the name. So each of them gets some of the descriptions associated with her we have listed above, but some of the descriptions change over the years, some new ones are added and old ones can be crossed from that list. Nevertheless, they all refer to the same person. Even if, for instance, some of them talked about her before the dragon eggs are hatched, hence they do not know Daenerys will be the mother of the dragons in the future or maybe some of these people met her when she dies her hair to disguise, and don't know that she had silver hair originally.

The idea behind Kripke's approach is since at a particular instant t_0 in time, the name is fixed to refer to a certain person and the other people learn about the name borrowed the reference from the person they initially heard it from, they are justified in using the name even if some of the associations with the name have changed or will be changing. According to Kripke's theory, names refer to whichever object they are linked to by the appropriate causal chain. If you point to an object and say it is called **O**, the letter **O** now refers to the object you were pointing at. You speak to others, and the words spread around the language. Each new user borrows the reference from the person they initially heard the word from. That constitutes the causal chain. Your object may fall short of all the associations that descriptive reference theory assigns to it; they may become false associations. For instance, let us say **O** was an unbroken Greek vase at the beginning with the motives of the Goddess Athena on it. You have found it in an archaeological site and placed it in a museum. However in time, the vase got broken by accident and re-glued, also the motive of Athena is deleted by the destructive forces of time. Even in that case, people in the future will succeed in referring to it as **O**, if it is known that it is the same specific vase in the museum. So, even if they associate few if any descriptions you make about the name **O**, they will successfully refer to the object. All that is required is that their use is a part of an approximate causal chain.

If we apply the causal theory of reference to our scientific phenomena instead of picking a descriptive theory of meaning, we can explain theory change criticism that has been raised by PMI. Especially the non-referring entities of old theories that Laudan brings to our attention as the problem of theory change (1981, p. 33), can be explained this way. To demonstate how picking a different theory of meaning, can surpass the worries created by a theory change and reduce the drastic effect of the occasion; let us consider the fluid theory of electricity and compare it to modern theory of electromagnetism. Charles Du Fay posited in the 18th century, two fluids caused the attraction or repulsion of certain substances. He experimented with some materials like silk, amber, glass and so on and found out that wool creates a charging on amber, and rubbing glass with silk creates another type of charging. In the end, foils charged by these two objects attract each other. He concluded that there are two types of electric fluids: resinous and vitreous. These fluids are separated by frictional forces - normally- but they neutralize when they are combined (Du Fay, 1734). Benjamin Franklin later proposed that there is only one fluid of electricity instead of two. These two theories are called the fluid theory of electricity. Obviously, they are both discarded today.

Considering the modern explanation of electromagnetic theory and two types of charges, positive and negative, were Du Fay and Franklin really mistaken though? We call an electrons'electric charge negative and call atoms or molecules which have an excess number of electrons, negatively charged. Also we assign the name positive to a proton's electric charge today and when an atom's number of electrons is smaller than its protons, we call that atom to be a positive ion. Moreover, these charges indeed flow; only electrons flow actually, and we call it electric 'current' because of the flowing property. They flow like a fluid; there are two different object types regarding this phenomenon, and in reality, only one of them causes the flow, which also hints at why Franklin later on objected to the two fluid picture of Du Fay and proposed there is only one. Actually, if we look at this particular theory change; our modern Electromagnetic theory is perfectly parallel to the 18th century one if we accept a causal theory of meaning posited by Kripke. There seems to be a minute difference in naming, but just like Daenerys's case, even though some of the descriptions associated with the initial naming are false associations now, overall it seems like the initial fixing just points to the same object. If we think a little bit creatively, we can claim that the causal chain that originated the naming of electric charges, started with Du Fay. Fluid can refer to electric charges even if most of the descriptions associated with that name are wrong.

There are some problems with this detour for the realist, however. First of all, scientists have shown reasons to discard fluids in the discussion of electromagnetic phenomena for the last two centuries. Suppose the fluids that du Fay first proposed do indeed refer to the electric currents as we understand them today, so that fluids do indeed exist. Then how will we explain the fact that physicists for two centuries have given so much discussion about that fluid theory not being true anymore? Are we going to insist that they do not understand the words they are using, correctly? Then it would raise doubts about their expertise on the terminology they use today also, therefore we could not know whether do they really know what a quark means for instance and we can't make sure that 2 centuries later it is not also going to undergo a surmountable change to the point of being called something else by the future physicists.

Secondly, even though some theory changes, like the fluid theory of electricity or the miasma theory of diseases are easy to save with this linguistic strategy and could be directly mapped out to a more modern naming of the same concept with certain well-thought-of legends, some of the listed theory changes are not that easy to save. Therefore, we need to define a clear line for when the list of associated false descriptions differs significantly enough for different namings to be considered to refer to completely different things and when we can say they are just analogous. Consider aether, for instance; it was thought to be a background medium for electromagnetic waves to propagate for centuries. Later on, the assumption of such a physical medium contradicted the Lorentz transformations and Maxwell equations'predictions about light's behaviour in such a background medium. It is then understood that there is no such medium, and the speed of light is a universal constant. Suppose we want to follow the causal theory of reference to explain this theory change from a background aether to no such aether, here we lost a previously proposed object. In that case, we can somehow force ourselves to think of 'quantum fields' as some form of modern aether if we want to explain the theory change with Kripke's theory of meaning. With quantum fields, there are some virtual particles that are created out of this vacuum by quantum fluctuations. There are indeed reasons to consider these quantum fields as of some kind of static medium that we can measure the waves'speed relative to it. However, quantum fields are not matter-like, unlike the aether. The virtual particles are just that, virtual; so there is a significant difference between two concepts at hand. This philosophical difference is not on the level of Aragorn's hair changing its color to gray as he gets old therefore the description 'black haired man' losing its relavancy to him. With aether and quantum fields there's a significant ontological difference that one of them was matter-like, it was proposed as a literally existent object whereas the other is a virtual structure. Moreover, Quantum Field Theory is actually a synthesis of Quantum Mechanics with Special Relativity, the latter of which is the theory that is created by the epiphany of discarding of the concept of aether in the first place. So equating the concept aether to just an ancient name for quantum fields would be a borderline disrespect to the scientific progression of the last century to prove just the opposite. This would not be a realist's go-to solution to explain theory change when they are in their right minds.

In conclusion, even if we let 'fluids' refer in this context to 'electric charges', or ‘aether’ to quantum fields; these does not save the realism in the form we want to preserve it. This linguistic brushstroke just trivializes scientific realism. If the older generations of physicists were wrong about fluids; today, we can be equally wrong about quarks, gluons, neutron stars and so on, just like they were wrong about their central concepts. In this case, if there is a certain phenomenon to ignite the initial introduction of a term, we can take that term as substantially referring. However, reality might still differ significantly from the entities described in theory. This does not lead to a version of realism that any realist would like to entertain. Actually, this brings us to our next method of opposing the PMI, that is limiting realism’s span.

4.2.2 Restricting Our Realism Span

In the introduction, we have presented different forms of realism to explain the theory change across centuries. We can give up on some parts of realism and restrict ourselves to a particular subdomain of realism. My suggestion is to follow Worrall’s footsteps (1989) and root for a structural form of realism. The theoretical structure of a description of some phenomena is prone to be pertained across theory changes. Physicists even have a name for this called ‘correspondence principle’, which means that a new theory preserves the core tenets of the previous theory and can be reduced to it with certain assumptions or limitations. This is not only referring to a coincidental epiphanic moments after a new theory has been accepted and checked against the old one, but it is a working strategy for developing new theories, a method used in the progress of theoretical physics. Thus, in a structural realistic framework, we can argue that science progresses by refinements over our description of the structure of the world, even if the knowledge pertaining to the nature of the entities populating that structure radically shifts. Let us look at some examples to solidify what has been described here.

Fresnel to Maxwell

The change of theories of light in the previous century was such a frequent phenomenon that it may need observers from behavioural sciences departments to study the behaviours of the physicists who are involved, to be explained. The most significant of such changes is from Fresnel’s theory relying on waves on a background aether to Maxwell's EM waves in a vacuum. Structural realism captures this change by noticing an insistence in the wave-like behaviour of the object of study which is a continuity among these two theories even though they seem radically different, because they conceptualize waves differently.

Regarding mathematical structure, Fresnel's equations for wave propagation are similar to Maxwell's equations for electromagnetic waves. The specific equations may differ in detail, but the structural form stemming from a common wave equation remains intact across the theoretical shift. Fresnel describes the wave-like behaviour of light at a boundary between two different media and gives the reflection and transmission coefficients. Maxwell describes how the oscillating electric and magnetic fields propagating through space create electromagnetic waves, including light. Maxwell's equations on the boundary lead to the same reflection and transmission coefficients that Fresnel's equations give us. The solutions to the wave equation from Maxwell's equations are sinusoidal plane waves, which are mathematically similar to the sinusoidal waves implied in Fresnel's equations. Fresnel's results can be seen as a specific application of Maxwell's equations at the boundary, while they are a more general framework for the whole EM spectrum and describe the waves'nature. This perfectly coincides with the correspondence principle, where the old theory is encapsulated in the current one under certain conditions or limitations.

The structure preservation between the transition from Fresnel to Maxwell explains the predictive success of the former while accounting for the theory change that followed. Also, beyond the mathematical structure, the 'wave' nature of light is preserved. Even if the description changes from an ether-based framework to an electromagnetic one, the core structural insights about how light behaves remain valid. So, we can see that the transition from Fresnel to Maxwell was not a complete abandonment of the previous theory but a refinement of its structure.

4.2.3 Eliminative Inference and Attrition Considerations

> *"We have to continually be jumping off cliffs and developing our wings on the way down. "*
> -Kurt Vonnegut

Defenders of pessimism towards the history of science miss some factors while conjoining such a statistical inference from past theories. First, PMI is a statistical projection from the 'majority' of past theories to the 'majority' of current theories -and following the same line of reasoning, the future ones. The argument is about ratios when it is stated in "most -> most "form, Samuel Ruhmkorff calls the modest form (2013,pp. 409-428), but we are already focusing on this form since this is the weaker claim and refuting this would deem it obsolete to refute the stronger one. In any case, an argument concerned with such statistical ratios should hold two basic criteria to be acceptable:
i) The samples should be random.
ii) The entities included in the argument should be logically independent from each other (ibid, pp. 414-415).

In this case, the samples cannot be random because we are historically situated; we are at some point in the history of science, and this limits our independence to choose randomly. This criticism has been mentioned by many (Lipton & Worrall, 2000, p.204; Magnus & Callender 2004, p. 327; Stanford, 2006, p.10; Fahrbach 2011; Park, 2010; Mizrahi, 2012), and I will dedicate another section to discuss how the progressing in methods, increasing reliability, increasing interdisciplinariness and the like, actually makes science's curve far from static, and even linear increase and therefore a theory sample from the past could not be similar in all respects to theory sample from today, therefore these are not random samples. For now, let us focus on the second point: that entities involved in this argument -namely scientific theories- are not logically independent.

To demonstrate what a logically dependent variable set does to statistical generalizations such as PMI, let us visit Victorian Era London, for a brief adventure. In 221B Baker Street, sits Sherlock Holmes; scratching his head, over a complicated murder case. Let us say there are seven possible suspects. If he examined the first three suspects on the list and reported to the police force that these individuals were not murderers, relying on some convincing evidence, should the lieutenant of the bureau of homicide just conclude that he is an incompetent detective and leave him out of this case? That would be a pity and also a very hasty and harsh decision on their side. A clever lieutenant would instead think: "There are four suspects left, and there is a significant increase in the probability that the next one he investigates will be the actual criminal. An increase from $1/7$ to $1/4$ is a great deal, so he is a brilliant detective. Let us be patient and let him work further on the case". The crucial point here is that the subjects under investigation are not logically independent from one another; the innocence of one increases the chances of the other being the actual murderer, so a detective who is eliminating some items from a usual suspects list, is considered to be knowing what they are doing.

Transferring the line of reasoning from detective stories to our case, each theory that is once considered by scientists and then certainly falsified - or whose negation has been proven if you like- increases the chances of the remaining ones being the true description of reality. Moreover we should combine this mathematical reality with the fact that Sir Arthur Conan Doyle had written Sherlock as a truly dedicated detective. Sherlock always keeps himself a student and finds different more reliable techniques throughout the investigation, this gives us reasons to discredit any pessimistic naysayers' over-generalizations against our little fictional genius. Each time an individual that he was suspicious of, turns out to be innocent, he notes what he did wrong in the process of assuming they are the real murderer. So the next time, he would not make the same mistake again. This scenario is summarizing the role of attrition in science, as less plausible theories are being eliminated from the suspect pool; at the same time, scientific methods improve. Therefore the remaining theories, both due to sheer probability and due to this progress made in the elimination process itself, are much more likely to be true than the discarded ones. Reply of Ruhmkorff utilizes this aspect of scientific method; the factor of eliminative inference and role of attrition was not taken into account by critics of realism when calculating the odds for current theories, which is the core implication in PMI.

4.2.3.1 Caveats in Ruhmkorff's Criticism and Solutions

One caveat with the solution Ruhmkorff provided, which is eliminative inference picture of scientific progress as I've summarized above, would be the existence of infinitely many possible alternative theories. Obviously, if the number of suspected people amounts to infinity, in that case, no matter how many of them Sherlock Holmes eliminated, the probability of the next one being the actual suspect does not increase. However, this is not a serious caveat because, in reality there are no murder cases with infinitely many suspects, even in fiction, there's only so many people in the world that they are finitely many and countable. Such a scenario with infinite suspects would be just a lazy police department trying to cover up their laziness with a fantastic nihilism; i.e. what it actually amounts to is claiming that there are no suspects at all. But if there literally would be no suspects then there wouldn't be any murder, then what is Sherlock hired for, what prompted the bureau of homicide to investigate the case? Turning back to scientific theories case, this infinitely many alternatives would be tantamount to saying there's no truth to begin with, to be captured by a theory. To clarify my point, suppose a phenomenon is really explainable by infinitely many theories. In that case, every new theory has a zero chance of being true from the beginning, therefore there is no reality to explain in the first place. This is tantamount to being a sophist without confessing that you are a modern sophist, and rather than being an anti-realist in philosophy of science this position is equivalent to denying any mind-independent reality and accepting phenomenalism, and that would be a discussion for the ontologists. In any case if we entartain the scenario to satisfy the more suspicious readers. Even though logically it is possible, Ruhmkorff grants, scientifically it is impossible for there to be scientifically plausible infinitely many theories regarding some data (ibid, pp. 7-10).

Putting fictional detectives aside, let us revisit the reality of the scientific inquiry to ground ourselves a little bit. Even if we grant that there are infinitely many possible alternative and 'scientific' theories, this does not mean all of them are equally plausible. When you assess possible hypothesis to explain a phenomena, you'd find out that there is a bell curve of plausibility among your theories. According to the content a theory entartain and how many statements or hypothesis are occuring inside that theorem that does or doesn't conflict with each other; some of the theories will be falling close to the "zero probability" left end of the curve and some of them on the other edge are close to 100 per cent right edge of the graph, therefore the ones falling on the right edge of the graph, let's say above 75th percentile, will be very less in number and if you keep squeezing the limits, above 95th percentile there will be very few, sometimes none, to choose among. Keeping this standard distribution in mind, there are usually a few theories to consider starting with. Therefore our detective scenario with finitely many suspects is likely to be a true representation of scientific inquiry, rather than a hypothetical and very fantastic case of infinitely many plausible theories for one phenomena. In this sort of scenario, attrition would be a factor as some plausible theories become refuted throughout the history of science (Lipton, 1991). It is important to note here that the burden of proof lies with the defender of PMI since the argument makes a positive claim about the falsity of current theories by relying on a list from the past theories that have been discarded or changed. Now that we've shown the realists can counter that induction from past by offering an eliminative inference scenario and attrition, hence, the opponent of realism is the one who needs to show that there are too many plausible alternatives to diminish the role of attrition to reach an approximate truth. Indeed, suppose there are tens of thousands of plausible theories in a given field that could account for the same data. In that case, that particular theory in the discussion deserves serious amounts of suspicion, but proving this claim for any phenomena would be the job of the denier of a theory about that specific phenomena. Aside from local cases, proving that low odds occur for the whole scientific inquiry, which is showing that there are infinitely many possible alternative scientific explanations for any data, is an impossible task. And that burden now is on the shoulders of anti-realist since they're the camp with a positive claim about existence of such a daunting number of possibilities.

Another concern here is that the plausibility of scientific theories is changing over time, and the candidate theory pool is always unstable and hard to categorize according to plausibility. This means there is no fixed set of plausible theories to apply the attrition factor. Plausibility is dependent on the theories'consistency with background theories and data. However, this is not enough to show that there are actually almost infinitely many plausible theories, as suggested in the second point; a hypothetical logical existence of theories is not an actual threat to the existent scientific theories' chances of being the one true theory to rule them all. What needs to be shown again is that there are actually applicable scientific theories that compete with the currently favourite one. If our current data about some phenomena is relatively stable and the background beliefs are again stable, there may not be many plausible theories in the scene. Additionally, as Doppelt notes, the bar of plausibility is located higher and higher as the successive scientific theories show "empirical and theoretical virtues to a greater degree". (2007, p. 111).[1]

4.2.3.2 The Leaky Urn Model

One way the anti-realist could still hold on to the idea of PMI even though there is an eliminative inference is to show that the gains obtained by attrition are outweighed by the losses from the historical trace, leading to a pessimistic scenario. An analogy known as the leaky urn model is usually given to demonstrate this. Let us revisit it to get a better grasp of the discussion. Suppose that there is an urn with n-1 red ball and 1 green ball; the green one represents the theory, which will eventually be the true formulation of observational data, and the red ones are false theories. Suppose that the probability $p(R)$ represents the possibility of the green ball being leaked at that instant and at t_0 $p(R) = 0.1$. If we begin drawing and observing the red balls at each turn, this probability changes. In a scenario with n=100 balls, and t_i representing the instant after i many red balls have been drawn and observed, the Bayesian formula gives us the new possibilities as the following:
at t_10, $p(R) = 0.11$;
at t_50, $p(R) = 0.18$;
at t_90, $p(R) = 0.53$;
at t_99, $p(R) = 0.92$;
This is a really dark picture indeed; even if we eliminate the false theories, in the process, the chance of losing the true one increases to an enormous degree. Ruhmkorff notes in this case: "...the leaky urn model demonstrates that even in a situation with attrition, the poor track record of a process can significantly raise the probability that it has not achieved the specified result."(2013, p. 419). This makes leaky run a more challenging argument than PMI for a scientific 'optimist' because this model actually

[1] Some readers may be hasty to cleverly jump to the conclusion that plausibility is not enough for a theory to get close to truth to begin with, they are right in that claim. As we will see in the next section, realists would also agree with that; in fact there are many more adjectives to be checked for the candidate theory pool when a realist says success indicates a level of truth. Certainly plausibility by itself is a very weak criteria, it's not enough to suggest anything. But actually this depiction just strengthens realists position in our current debate on attrition, just consider this, even if this weakest adjective like being plausible can't be fulfilled by as many theories as the anti-realist entartains there exists, to defy the role of attrition, then how could all of the other adjectives enlisting even more criteria can be fullfilled by so many theories, let alone infinitely many theories? Our theories set gets smaller and smaller with each new adjective added in front, rather than getting larger.

takes into account the logical dependence of theories unlike PMI.

Nevertheless, there are some caveats in the leaky urn model, too. Although it takes the attrition factor into account, it does not model scientific inquiry itself correctly. Unlike an urn with n balls, scientific alternative theories are not ready in place from the start; they are labelled 'unconceived hypotheses' for a reason. Throughout scientific research, the previously collected data and deductions based on them collide with new unexplained phenomena at some point, and a new theory is born from this synthesis of the old with the new evidence. The previous favourite candidate is discarded in time if the new theory explains better, is predictively more successful, and is corroborated over time. Therefore, we do not have an urn filled with countably many, determinate number of balls that are leaking in time. At best, it is similar to a lake with both incoming and outgoing flows of rivers if we really have to come up with a visual analogy. In this picture, there is no way to determine prior probabilities in a Bayesian sense of the currently unconceived hypothesis; we do not even know what they look like unless we have a time machine. The plausibility of each alternative constantly changes with new information, and the birth of previously unthought-of new theories makes it madness to try to assign even somewhat reasonable probabilities to members in the current alternatives lake.

> ...the exploration of the space of possibilities constantly brings into consciousness heretofore unrecognized possibilities. The resulting shifts in our belief functions cannot be described by means of any sort of rule of conditionalization (Earman, 1992, p. 183).

Even if we let aside the impossibility of assigning priors beforehand due to the inconceivability of alternatives at any given moment, the Urn Model does not survive. The second problem with the Leaky Urn model is that it depends on the initial value of $p(R)$ we pick. There are suitable values of $p(R)$ such that even if we pick all of the red balls and only two balls left, the final probability is still really low for the green ball to be already leaked. So, how pessimistic we should be about the scientific inquiry actually depends on how pessimistic and optimistic we are about whether there is a chance that the scientists will eventually get to a true theory. Therefore, the leaky urn model does not provide sufficient reason to be pessimistically deduce anything; rather, it is a model that can show the optimist is right if we start with an optimistic approach, and it shows the pessimist is right if we start with a pessimistic prior. To put it simply, it just represents a model of how a self-fulfilling prophecy works.

4.2.4 Scientific Method Also Improves

'You cannot do an induction from the properties of swans to properties of bananas.' (Roush, 2010).

Sherrilyn Roush captures the most significant problem in pessimistic induction in this famous sentence. She draws our attention to the fact that pessimistic induction is based on the assumption that modern theories are not different in kind from older theories (ibid, p. 32). Since the scientific method improves over time, our reliability is potentially stronger than that of our predecessors. This difference undercuts an inductive inference from past theories to current ones.

For instance, 20th century polymath Ronald Fisher developed a new model called analysis of variance in statistics to be used in population genetics, which he first applied in his work 'Studies in Crop Variation I' (1921). This was a statistical method previously unavailable to scientists. Alternatively, the limits of infinite series in modern calculus solve Zeno's paradox of motion. Modern methods of data analysis via improvements in computer science were not available to scientists of the previous century and so on. Overall, we are applying a variety of sophisticated techniques, and our ancestors did not have access to these.

A pessimistic colleague would raise the objection to this observation is that our predecessors could have used the same argument since they had better methods than their predecessors. Moreover, their successful theories turned out to be false in time, so why should ours be an exception? Every person in history had reasons to believe that their present-day methodology and scientific achievement are better than the older ones, and the theories of their time are protected from the same errors their predecessors were inclined to make. However, that is actually the issue here; they were justified in doing so, too since they were more 'correct' than their predecessors. Their theories were better than past theories due to the superiority of their methods. Our predecessors had more correct theories and fewer false theories. So, they would be justified in rejecting PMI based on past theories. Similarly, our theories are better than past theories regarding the same method changes that are moving towards better results.

Furthermore, Roush's argument can actually be reformulated to encompass other improvements in science other than improvements in theoretical methods. For instance, we can say the same for our tools; we certainly have more sophisticated apparatus today; for instance, we are building quantum computers and designing algorithms based on non-classical logic rather than a two-valued logic, which is an improvement both in method and tools. Alternatively, we have a large hadron collider in Geneva that is circulating thousands of square meters instead of Rutherford's alpha particle beams (1906). We have deep learning models to study some problems in cognitive science, which were not accessible tools for almost 99. 9 per cent of the history of science.

We also have greater interaction between disciplines today, which was not possible until the 20th century. For instance, physics is united with chemistry in atomic and molecular physics and physicochemical sub-fields. Sub-atomic level interactions are studied with quantum physics, and then the chemical properties of matter are explained on that underlying level. Chemistry then unites with biology in biochemistry to explain phenomena in living matter. In this picture, we see a range of phenomena observed in nature, from sub-atomic scales to organism scales, which are totally described in a unified picture. This is also an improvement over the connectivity of explanation our predecessors could reach a century ago.

Overall, we can only infer some statements about present theories from past theories if past and present theories are similar in all the relevant respects. Roush made a brilliant observation by pointing out this gap in the induction by directing our focus on the changing methods. I suggest enlarging this difference argument by also recognizing the changes in tools, changes in the level of interdisciplinariness and hence the scope or increasing unification.

4.2.5 Determining the Theory Pool: "Most" Theories Have Not Been Replaced

One way to object to PMI is to show that its premise is false. Most of the time, defenders of the argument give a long list of once-successful theories of the past that are later rejected. However, for the statistical projection to the future to be a legitimate argument, there needs to be evidence that "most" of the past theories have been replaced, and ‘many’ doesn’t imply ‘most’.

4.2.5.1 Necessary Criteria of Being Successful Theories

Realists only recommends belief in mature theories displaying novel predictive success at their time of utterance. Hence, most of the theories listed when a pessimistic induction is to be made, fail this criterion.
Let us analyze what makes a theory empirically successful and mature; here, I will try to provide a general list that would be considered rational by both the realist and anti-realists:

i. *Predictive Accuracy*:
A theory’s ability to accurately predict future observations or experimental outcomes is a primary measure of its empirical success. Predictions should be specific and quantifiable, allowing for clear verification or falsification.

ii. *Explanatory Power*:
The ability of a theory to explain a wide range of phenomena within its domain is crucial. This includes not only the phenomena the theory was initially developed to explain but also new, unforeseen phenomena. A theory with high explanatory power provides a coherent and comprehensive understanding of the observed data.

iii. *External and Internal Consistency*:
Theories are assessed for their consistency with well-established scientific knowledge. A successful theory should not contradict established facts unless it provides a compelling reason and evidence for reconsidering those facts. Also, the logical coherence of a theory is important. It should be free from internal

contradictions and should integrate well with other accepted theories.

iv. *Empirical Adequacy*:
This refers to the extent to which a theory accurately represents and accounts for the empirical data it aims to explain. A theory is empirically adequate if its claims are supported by observable evidence.

v. *Scope and Generality*:
The broader the scope of a theory, the more phenomena it can account for, and the more general its applications, the greater its empirical success. Theories that apply to a wide range of conditions and contexts are preferred.

vi. *Fruitfulness and Pragmatic Utility*:
The ability of a theory to generate new research questions, hypotheses, and experiments is an indicator of its empirical success. A fruitful theory stimulates scientific inquiry and leads to further discoveries and advancements in the related field.

vii. *Falsifiability and Testability*:
A theory must be falsifiable, meaning it can be tested and potentially proven wrong by empirical observations. Testability is a key criterion for empirical success, as it allows for the theory to be subjected to rigorous scientific scrutiny.

viii. *Simplicity and Parsimony*:
Theories that explain phenomena with fewer assumptions and simpler explanations are often preferred. Applying Occam's Razor suggests that simpler theories are more likely to be correct, provided they have similar predictive and explanatory power.

Now, let us analyze some of the favourite theories generally used as examples in pessimistic induction; for instance, Magnus and Callender cite(year,pg):

1. Phlogiston Theory
   This theory suggests a substance called phlogiston to explain the combustion and oxidation processes, but later on, it was changed with oxygen by Lavoisier.

2. Ptolemaic Astronomy

   Ptolemy's earth-centred model has many elliptical dis-tractions from the originally suggested orbits of heavenly bodies that had been tried to be explained by complex calculations.

3. Luminiferous Aether

   The background matter called aether for light waves to propagate is assumed up until the inexplicableness of the Michelson-Morley experiment by Maxwell Equations and the aether combination created a crisis solved by general relativity.

4. Caloric Theory of Heat

   The heat was modelled as a fluid called'caloric2 that flowed from hotter to cooler bodies. This theory was successful in explaining heat phenomena for a time. However, it was eventually abandoned in favour of the kinetic theory of heat.

5. Classical Mechanics

   Newtonian mechanics was used for centuries to explain macroscopic phenomena successfully but turned out to be incomplete and needed to be replaced by general relativity and quantum mechanics depending on the scale and speed in discussion.

I have asked our new colleague ChatGPT for an evaluation of these five theories according to the criteria I have listed above, and it has reached the following conclusions about them, the items marked with an (*) when they are failed to be met by that theory:

1. Phlogiston Theory

Predictive Accuracy: Initially, it predicted the process of combustion and oxidation.
Explanatory Power: Explained a wide range of combustion-related phenomena.
Consistency with Existing Knowledge: Fit well with the chemical knowledge of the time.
Internal Consistency: Lacked some consistency, as the phlogiston concept was vague. (*)

Empirical Adequacy: Eventually, it failed to account for weight gain in metals upon oxidation. (*)
Scope and Generality: Applied broadly to all combustion and metal calcination processes.
Fruitfulness: Led to extensive experimentation and investigation in chemistry.
Pragmatic Utility: Useful in guiding practical applications in metallurgy and chemistry.
Falsifiability and Testability: This was ultimately testable and falsifiable by Lavoisier's experiments.
Simplicity and Parsimony: Initially simple, but became increasingly convoluted to explain anomalies. (*)

2. Ptolemaic Astronomy
Predictive Accuracy: Provided accurate predictions of planetary positions for centuries.
Explanatory Power: Explained the apparent retrograde motion of planets.
Consistency with Existing Knowledge: Consistent with Aristotelian physics(*)
Internal Consistency: Complex system with epicycles-not simple-, but internally consistent.
Empirical Adequacy: Adequate for predicting celestial events but not entirely accurate. (*)
Scope and Generality: Applied broadly to all known celestial bodies.
Fruitfulness: Spurred further astronomical observations and refinements.
Pragmatic Utility: Useful for navigation and calendar making.
Falsifiability and Testability: Falsifiable and ultimately falsified by heliocentric models.
Simplicity and Parsimony: Lacked simplicity compared to the later Copernican model. (*)

3. Luminiferous Ether
Predictive Accuracy: Initially provided a framework for understanding light propagation.

Explanatory Power: Explained wave-like properties of light. (then it is accurate but inadequate since particle-like properties are lacking) (*)
Consistency with Existing Knowledge: Consistent with wave theory of light. (*)
Internal Consistency: Internally consistent but faced issues with Michelson-Morley results. (*)
Empirical Adequacy: Failed empirical tests, particularly the Michelson-Morley experiment. (*)
Scope and Generality: Intended to explain all light propagation phenomena.
Fruitfulness: Led to important experiments and eventually to the theory of relativity.
Pragmatic Utility: This had limited practical utility since it was disproved relatively quickly. (*)
Falsifiability and Testability: Testable and ultimately falsified.
Simplicity and Parsimony: Initially simpler than alternatives but contradicted by evidence.

4. Caloric Theory

Predictive Accuracy: Predicted heat transfer in some contexts.
Explanatory Power: Explained phenomena like heat flow and thermal expansion.
Consistency with Existing Knowledge: Consistent with early thermodynamic observations.
Internal Consistency: Internally consistent until new evidence emerges.
Empirical Adequacy: Failed to explain heat as a form of energy.(*)
Scope and Generality: Applied broadly to heat phenomena.
Fruitfulness: Stimulated research in thermodynamics.
Pragmatic Utility: Practical applications in heat management and early engines.
Falsifiability and Testability: Testable and eventually falsified by kinetic theory.
Simplicity and Parsimony: Simple but incorrect compared to the kinetic theory of heat.(*)

5. Classical Mechanics

Predictive Accuracy: Highly accurate for macroscopic phenomena.
Explanatory Power: Explained a vast range of physical phenomena.

Consistency with Existing Knowledge: Has been consistent with observations for centuries.
Internal Consistency: Internally consistent but limited at extreme scales.
Empirical Adequacy: Empirically adequate for non-relativistic, non-quantum contexts.(*)
Scope and Generality: Broad scope, covering a wide range of physical phenomena.
Fruitfulness: Extremely fruitful, leading to numerous technological advancements.
Pragmatic Utility: Foundational for engineering and everyday physics.
Falsifiability and Testability: Testable and valid within its domain.
Simplicity and Parsimony: Simple and parsimonious for its applicable domain.

Although this is not an in-depth analysis of each one of these theories, it gives us an overall picture of the scene we are actually facing. My conclusion from this analysis is that, when we take into account the (*) signed items, the listed theories were not "empirically successful" in the first place. A common pattern is that they were later falsified by new evidence, which points out their inadequacy in explaining a broad range of phenomena, which was a criterion of success. Also, one of the examples, classical mechanics, is not exactly "false"; it just has a narrower scope and is still a valid approach. Classical mechanics is just a limiting case of QM and Relativity and not a discarded theory, as some anti-realist critics keep claiming. In conclusion, the listed examples are not enough to show the anti-realist claim S: "Empirical success does not imply the truth of a theory". My reasons for not accepting S based on the provided evidence can be summarized as:

Firstly, they are just a few selected examples among many theories that have existed throughout the history of science; a statistical analysis of all the past scientific and "successful" theories is needed to reach such a general conclusion. Drawing a general conclusion based on a sample selected specifically to show that some theories have been discarded is just a fallacy. Although the list may seem exhausting or large at first glance when we think of the number of all scientific theories that have ever been put together, the list is actually very small, and this list becomes an exemplary case of cherry-picking, rather than a serious base to warrant a proper induction, such as from "most" theories to "most" theories as intended. This approach creates a selective perception which is biased from the beginning. We will see a random sample analysis to open our eyes to the reality of how significant would be the volume of these theories among the vast ocean of history of science.

Secondly, the successfullness of the aforementioned theories are debatable, and empirical success cannot be clearly defined. They are said to be discarded and falsified, but one of them, namely classical mechanics, is not a theory, is not false and is still considered a valid domain of science within certain limits.

This discussion about selecting some list of theories properly, brings us to our next topic of discussion.

4.2.5.2 Many $\neq$ Most and Statistical Analysis of Moti Mizrahi

The evidence listed for a pessimistic induction is generally a long list of past theories that are now regarded as false in the light of modern science. However, pessimistic induction actually rests on a claim about the ratio of rejected theories to retained theories. This shows that the claim will require taking a random sample of past theories. Let me remind here that the list given earlier was not a random sample; it was specifically coined together to show the rejected ones, as we mentioned, with some form of selection bias. To clarify this point with an analogy, think of a similar example: the homophobic claims of the previous two decades were centred around the prevalence of HIV+ among queer community. Usually, the advocates of this claim start from how many gay people they know that have HIV, whereas they mentioned that they know a lesser number of heterosexual individuals with HIV. This is a textbook example of selective perception bias. Correct reasoning would start by considering the whole queer population and analyzing the prevalence of the virus among the whole community, which is almost as impossible as determining the pool of successful scientific theories of modern science. We cannot really know people's sexual orientation or gender identities accurately by looking at official demographic data, and we cannot guess the sample population correctly to begin with.

To defend PMI, the best we can do would be a random sample analysis. For that purpose, we need a long list of theories sampled randomly among past scientific theories that were in a mature field and had predictive success. Then, we need to show that the ratio of rejected theories is larger than retained ones. No large-scale study to investigate the truth value of the assumption in PMI is conducted, but Moti Mizrahi makes an attempt in that direction with a random sample analysis as we suggested, in his paper ‘Pessimistic Induction: A Bad Argument Gone Too Far’.

Mizrahi considers two cases for PMI; one is that it is actually a deduction; more specifically, reductio ad absurdum which goes as follows:

1. Assume that the success of a theory is a reliable test for its truth.
2. Most current scientific theories are successful.
3. Therefore, most current scientific theories are true.
4. Most past scientific theories differ significantly from current theories.
5. Many of these past theories were also once-successful.
6. Thus, the success of a theory is not a reliable test for its truth.
   (2013, p. 3210)

The flaw in this argument stems from the statements 3 and 4. From the differences between past and current theories, we cannot infer that they differ in truth values. For instance, from the differences between Stahl’s theory of combustion and String theory, we cannot get to the conclusion that one must be true and the other false. Stahl’s theory indicated that there is a fire principle that is expelled from metals when heated, namely phlogiston. It was proposed by one physicist at the time, and it was about one phenomenon, namely combustio. String theory, on the other hand, is studied by many physicists and mathematicians; it has different formulations, unlike Stahl’s phlogiston theory, and it is literally about ‘everything’ rather than one specific phenomenon. Stahl’s theory was backed up by experimental evidence in its days, and String’s theory is not -yet-. As we can see, they differ in many regards, but from the falsity of Stahl’s theorem (Conant, 1950), we cannot infer that String’s theory will turn out to be true just because they are so different.

One way to fix this problem would be changing the item (4) of the argument into some implication about truth values:

$4'$. Most past scientific theories differ significantly from current theories in their truth value.
$4''$. Most past scientific theories are false.

However, this rendering of the argument would be circular because we are trying to establish the falsity of most scientific theories deductively here -unlike the inductive form, which takes it as a fact-. Hence, this rewording would not save Pessimistic Induction as a deductive argument.

One last way to save the deductive reading of Pessimistic Induction would be to attempt to use inference to find the best explanation. Putting aside the fact that no anti-realist in their right mind would want to use IBE as their foundational inference method as it would be a huge irony- there is still a problem. Falsity is not always the explanation for abandoning a past scientific theorem. For instance, Newland's law of octaves was not abandoned due to its falsity, but rather because of its scope; it did not apply to the heavier elements.

After showing that Pessimistic Induction as a deductive argument is not sound, Mizrahi goes on to show the weakness of the inductive form of the argument. This is the form of PMI we have already been treating in this text, so we will omit repeating it here.

When we treat the pessimistic induction as the inductive argument it is, we have to justify the inductive step in one way or another. Godfrey-Smith mentions two ways to have a reliable inductive inference (2011, p. 33):

1) Causal Structures and Kinds: This type of inference is based on the similarity of similar objects; in nature, objects of the same kind behave in a uniform pattern. For instance, all living things die, all chloroplasts use light to create chemical bond energy, and all stars bend space-time. Pessimistic induction does not fall under this category of inferences. Since past and current theories are not uniform objects, they are not expected to behave in the same way. On the opposite, we have seen that there is even a deductive form of PMI based on this huge difference.

2) Random sampling: If each member of a population has an equal chance of falling into a sample, then statistically, it is justified to extrapolate from this sample to the whole population. For instance, if you have a dataset of 2000 students'grades and you want to find the average, if you take random 100 among them and calculate their grade average, you will get a number very close to the average grade of the whole 2000. Pessimistic induction also does not fall under this category because the list of false theories provided usually has been selected that way, and even though there are many in number, they do not form 'the majority'. Mizrahi actually runs a random sampling on theories by going through scientific dictionaries and taking every time the word 'theory' or 'law' is mentioned as an instance of a theory. Then, it uses a random number generator to pick random samples and comes up with the following picture:

Among 40 theories, 29 of them were accepted, 6 of them were abandoned, and 5 of them were still debated. Among 40 laws, 5 of them were abandoned, and 7 of them were debated. Nevertheless, even the ones that were abandoned were not actually abandoned. They were just being replaced by more useful ones with a broad range of applicability. For instance, he listed the law of octaves, Newton's law of gravitation, and Newton's laws of motion in the abandoned column. However, for most realists'they would fall under the 'approximate truth' category. (Mizrahi, pp. 3220-3224). According to these findings, 15% of sampled theories are abandoned theories. In other words, they are false, and therefore, approximately 15% of all theories are false. This is further away from the pessimist's claim that 'most' theories are false. All in all, this table looks like it could be used for an optimistic meta-induction rather than a pessimistic one, and it is a random sample unlike -for instance, Laudan's-cherry-picked lists given to support the pessimistic induction.

4.2.5.3 Exponential Scientific Growth Argument

When we give a list of theories in the so-called graveyard of the history of science, we tend to think of the dispersion of those theories as an equal distribution, considering they are from all over the history of science, including very near history. This is a valid evaluation for a process that is linear, but when we look at the statistics, the growth of science is not linear in time.

One way to assess the amount of scientific work done in a period is by looking at the number of articles published during that specific period. Another way to measure that amount is by looking at the number of scientists, for instance, new PhD receivers every year. Both of them give similar results, as noted by Fahrbach (2011, p. 148).

According to bibliometrics, the number of journal articles every 15-20 years for the last three centuries. This points to an exponential growth in the amount of work. According to De Solla Price, the number of scientists doubled every 15 years in the same period (1963, pp. 7-10). As Gerald Holton famously touches on the matter in his article 'On the Recent Past of Physics' by observing: 'Today we are privileged to sit side-by-side with the giants on whose shoulders we stand'. In the last half of the twentieth century, the amount of information creation came to a point where the foundations of the science the current scientists are working on are being provided by scientists of just one generation or two generations away as opposed to centuries-long gaps in the past. According to De Solla Price, 80% of all scientists that have ever lived are alive today.

How do all these observations save realism from pessimistic attacks? Fahrbach notes:

> Inspecting Laudan's list, we see that all entries on that list are theories that were abandoned more than 100 years ago. This means that all corresponding theory changes occurred during the time of the first 5% of all scientific work ever done by scientists. ... whereas 80% of all scientific work ever done has been done since 1950. ... the set of examples offered by anti-realists is not representative and cannot be used to support the premise of PMI (2011, p.152).

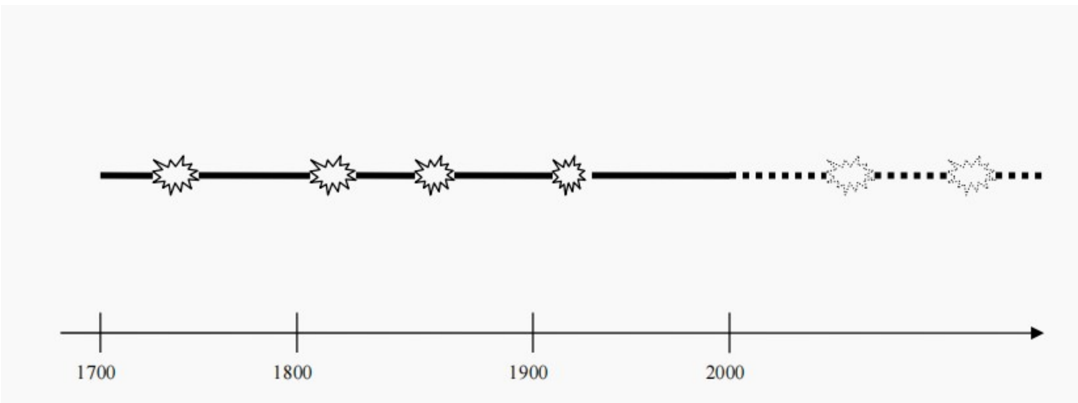

Figure **1**: History of Theories of Light (Fahrbach, 2011).

In the figure provided on the next page, we see the theory changes about light in the past century, and they seem to be equally distributed over time. The x-axis is weighted linearly in that figure. Compare it to the second figure where the x-axis is weighted exponentially.

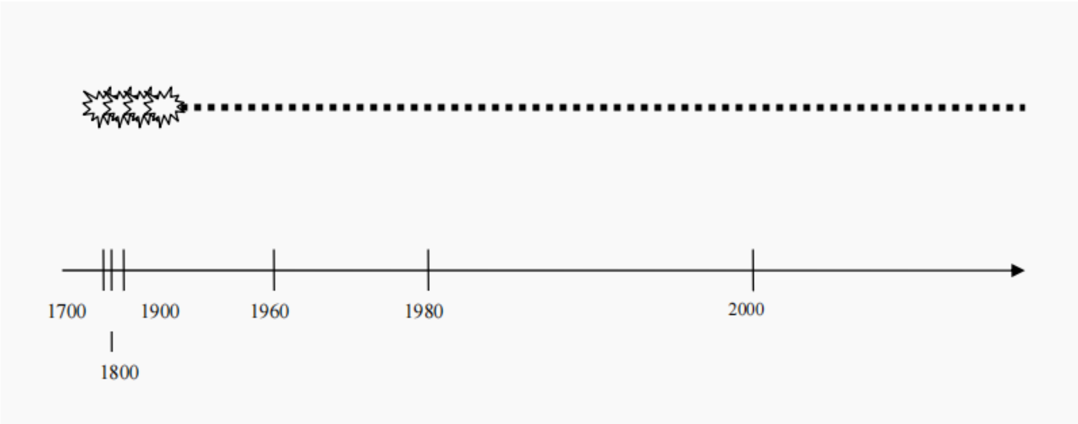

Figure **2**: History of Theories of Light - Exponential Version (Fahrbach, 2011).

The reader may be curious about the reason behind preferring the second figure over the first one concerning the history of science. Firstly, assessing the stability of a theory over a time span also requires a consideration of the amount of scientific work done in that period as much empirical evidence is gathered to falsify or confirm theories as the amount of scientific work done. Secondly, generally, more ways of testing are developed over time because the new observations and phenomena also lead to the development of new methods and tools. Passing a diverse range of qualified tests results in more success, and the more successful theories, the survivors, let us say, are the ones the realist wants to focus on in the first place.

All examples of theory change in the philosophical literature are relatively old and, therefore, not representative of the current state of the scientific endeavour in general. To come up with a more representative sample set, we should examine the last half century, where the majority of current scientific literature has actually taken place. When we look at that near history, almost all of the best theories have not been discarded. They are pretty much stable compared to the 1600s to 1900, when systematic scientific work was conducted and theories were changing more frequently. For instance, the theory of light changed four times, according to Fahrbach (ibid).[2]

[2] Some readers might entertain the idea that the explosive growth of science in the last 50 years is only normal, due to the increased population of the planet and our priviliged position in history considering the new sources are available to us compared to our predecessors. Although this is a perfectly correct observation about the matter, it's not relevant to our concerns here. It's not the topic of our discussion to explain the causes of

A list of unchanging theories of the recent past may be given as the periodic table, relativistic account of motion, physical conservation laws, brain as a neuronal network, periodic table and theory of evolution. In fact, the list points out optimism to a degree that it would be hard for the pessimist to find a successful theory that changed in the given period where the 90% of all the scientific work is done.

4.3 Conclusion

In this chapter, we have seen some major objections that can be raised against a pessimistic induction of the history of science. To sum up, we have touched upon objections such as how picking a correct theory of reference can change the way we look at the theory change, we have seen that pessimism is not a necessary implication but a matter of interpretation, as it always was in more general contexts. We have then looked at how attrition and eliminating false results to get closer to the truth might be what is actually happening at the background of scientific endeavour. Then we discussed how long lists of "many" theories do not prove anything statistically when it comes to generalizations and finally we have given reasons to exponentially weighing the time axis and finding a very different picture of history of science where all the change is actually occured in what corresponds to the crawling periods of modern science. Doing all of these gives us reasons to pertain to core of the theories and be optimistic about our best theories' retention rates, which in turn warrants a belief in optimism about the successful theories, as the realists claimed. Although the phenomenon of theory change occasionally occurs in the history of science, it is much less drastic in its scope and occurs very less frequently than the anti-realist critics

this quantitative and qualitative boost in scientific production; rather we're only interested in this fact itself and what its results would imply in the context of theory change. Because of this immense increase in science, the theories that has been proposed in the last fifty years had great chance of being tested and corroborated in equally immense amount of times, compared to their counterparts in the 18th century. In the infantile periods of modern science, the theories had the chance of being tested very occasionally and therefore survived for long periods and then suddenly drastically changed . While modern ones are tested everyday by thousands of scientists, as discussed in detail in this section, the growth is exponential. Therefore the survivor theories of today are of more strength, more empirical adequacy, tested and corroborated more, therefore they are more mature for their young age compared to their early-modern friends. Hence, such theory changes as to be called revolutionary according to Kuhnian standards, or the ones that would find themselves in the Laudan's list, are probabilistically very less likely to occur today. Our whole discussion around exponential growth is not therefore to reduce our predecessors'scientific successes'worth or to discuss our priviliged position compared to them. We're simply interested in the result of all of that increase in scientific production and how that corresponds to a stabilized and not-so-prone-to-change scene when it comes to current science.

claimed. In this case accepting that science is not perfectly capturing or mirroring truth but it still tries to reach into the underlying mechanism under the observable phenomena would be the reasonable explanation to default to. Hence, we cannot claim that successful theories always survive and never get falsified or discarded as time passes; but there's not enough evidence to undermine the realist claim either. If any, induction from random sampling of past theories just suggests otherwise. Nevertheless, to account for all the occurences of change, restricting the span of realism sounds like a reasonable idea. Suppose we restrict our realism span to the causal patterns in the description of the phenomena and mathematical models. In that case, we can trace today's theories to their meaningful origins, and we would see a trace of stability and consistency instead of abrupt changes and chaos, that would explain Mizrahi's results (2013) while also accounting for the occasional cases where changes do occur.

CHAPTER 5

ISSUES SURROUNDING "APPROXIMATE TRUTH"

5.1 Introduction

The concept of approximate truth and its relation to the success of scientific theories have been one of the objections against NMA and a central criticism of scientific realism (Laudan (1981); Fine (1986); van Fraassen (1980)). For instance, in his article "A Confutation of Convergent Realism", Laudan observes that "some epistemic realists claim successful scientific theories, even if strictly false, are nonetheless approximately true or close to the truth or verisimilar. " Laudan then expands this claim into two parts:

> T1) If a theory is approximately true, then it will be explanatorily successful.
>
> T2) If a theory is explanatorily successful, then it is probably approximately true (1981, p.19).

Actually, what NMA asserts as a conclusion corresponds to T2, and showing the statement T1 is not the purpose of the argument. The criticism of T2 is built upon theory change; Laudan goes on to give a list of historically successful theories that turned out to be false and undermines the claim in T2 by induction on changed theories as we discussed. Since this objection of theory change -or pessimistic meta induction more formally- is considered in Chapter 4 in detail; I will not repeat my arguments against Laudan's criticism regarding T2 here. Going back to T1, an opponent of realism would rightfully ask the question: "Why would we be convinced of the success of an only approximately true theory?". Let us try to understand what the critics are trying to articulate with this objection by constructing an example. Let's think of this scenario: We're a group of alien physicists visiting the Earth in a post-apocalyptic setting, we're on a mission to discover other civilizations' scientific discoveries and want to take the valuable knowledge we can gather, back home. No humans left here, but we find some remnants of their civilization. Inside an old college building, we found ourselves in front of a poster showing the standard model of particle physics(SM). This is originally a model with 17 particles: 6 quarks, 6 leptons, 4 gauge bosons and a Higgs boson. But unbeknownst to us, alien physicists, the electron is missing from the chart due to all that damage of whatever disastrous scenario this planet has been destroyed with, and what we are actually seeing is a very similar theory, SM*, with 16 entities. However, when our group gets on to work to check the Earthly physicists'theory from this picture, we start from scratch and build everything up with these 16 entities, assuming that we know the same laws of physics and have the necessary mathematical tools. In this case, we are bound to be completely unsuccessful; we will be frustrated, bumping into serious problems and setbacks, and the results we can reach would be logically inconsistent. So, we just think that Earthlings knew nothing and were completely unsuccessful when it comes to Physics therefore we'd left the planet to look for another one with more promising leftovers. The moral of the story is that approximate truth will not guarantee success.

Although this criticism does not directly target the result implied by NMA, it's inherent to realism that there's a close connection between success and truth; and T1 is assumed in the hidden premises of NMA, as we un-veiled them into the surface for instance when discussing probabilistic errors. In actuality, the realist claim that is cited by Laudan does not require success to be a necessary condition for approximate truth; at best, we can state that it is a sufficient reason. Leaving that aside and discussing the relationship between these two concepts, we should consider another point Laudan brought up while criticizing T1, namely the definition issue (ibid, pp. 20-23). Going back to our SM example, a realist would respond to the scenario I've described above by stating that it is not representationally accurate. The problem lies in the assumption that removing one entity from a model with 17 entities would provide us with a case that is "close enough" to the truth. No one ever claimed that there is a linear relation between the ratio of known items of a model over the number of complete lists of items and the theories'closeness to truth. So, a realist would follow with the lines that this example was skewed and fated to be false from the beginning, because what's been tried to pass as approximate truth here is not approximately true to begin with. This brings us back to the central point of Laudan's criticism of T1, which is the problem of defining the magic word "truthlikeness". He and many others have stated that the quick jump from "truth" to "approximate truth" to save realism essentially gave us a vague concept that can be extended as far as realists like and makes the realist claim a tautology, a claim that cannot be falsified. They asked that, if we cannot explicitly define what "approximate truth" is, then how can we utter the claim T1 and analyze its truth value? Laudan notes that no realists have managed to give a satisfying definition of the concept so far (ibid, pp. 30-31). Although I'd agree with the anti-realists that the concept is hard to define and no perfect definitions can be given, the objection I'd raise to Laudan is that his standards for a satisfying definition are too high, we can find working definitions of approximate truth to be able to demonstrate that realist claim accrues in NMA and due to the nature of this concept that's already the best we can get. Let us have a look at the history of the concept and two important trials of defining it, to convey what I mean here by working definitions and how they can save the day, more clearly.

5.2 Defining Truthlikeness

There are some inherent challenges to defining approximate truth; one of them is the very nature of scientific theories themselves. Theories are complex systems of statements; they involve models and idealizations to simplify and understand phenomena. For instance, the ideal gas law assumes that there are no intermolecular forces; hence, this description is not strictly true, but it is useful for making predictions. So, in defining the approximate truth, one must account for such idealizations and measure how they contribute to the theories'effectiveness and truthfulness. The second challenge from the nature of science is the dynamicity of science. Since scientific knowledge constantly evolves, the standard for approximate truth may also evolve with new evidence. Finally, one must take into account the domain specificity; a theory might be approximately true in some domains or under certain conditions. Newtonian mechanics is approximately true for the speeds we are used to and on everyday scales.

Nevertheless, when we increase the speed to catch the light, it no longer holds, and relativistic mechanics apply in that case. Setting these aside, there are also epistemic challenges from the concept of truth; defining what is "being close enough" to truth is internally vague and context-dependent. For instance, if one accepts a correspondence theory of truth, one must explain what it means to correspond 'approximately' to facts.

Philosophers coined together the word "verisimilitude" or "truthlikeness", which is defined as the degree to which a theory accurately describes the world. If we are epistemic realists, the picture in our minds when considering the history of a scientific phenomenon and its explanations is the concept of truthlikeness on the y-axis on a graph where the x-axis represents time, and there is a continuous, smoothly increasing function on the graph, maybe with some occasional bumps corresponding to "revolutionary" changes depicted by anti-realists. This is the convergent-ist picture Laudan criticized because what would be the metric for the verisimilitude that allows us to draw such a graph? We need to be able to compare two theories according to this metric to draw such an increasing function on our graph, but there is no absolute measure for that according to Laudan's observation. Because theories often have different scopes and explanatory focus, if we try to give such a measure, the first things we would come up with are the scope of a theory(high content) and the accuracy of that content. There is a problem that verisimilitude requires increased content and high accuracy at the same time, but there is an inverse relation between these two. Theories with high content are riskier than the ones with lower content; they make more predictions which can be falsified. This puzzle will be more clear with the formal definitions we will consider.

5.2.1 Popper's Definition of Verisimilitude

Popper's original definition of verisimilitude is a really intuitive one: if the truth content of a theory S is greater than the falsity content of the theory S, then S is approximately true. Furthermore, if we are comparing theories $S1$ and $S2$, if the truth content of $S1$ is greater than $S2$ and the falsity content of $S1$ is less than $S2$, then $S1$ is closer to the truth than $S2$. More rigorously:

$$C_{t_T}(S1) >> C_{t_T}(S2) \text{ and } C_{t_F}(S1) << C_{t_F}(S2)$$

The problem with this definition is that it does not imply that the theories'observable consequences are true. It is possible that a theory is approximately true by this definition, but all of its observable consequences are turning out to be false. Furthermore, there is a paradoxical case when comparing two false theories: increasing one's truth content also increases its content in general; hence, more predictions are related to that theory, leaving room for more falsity content. Therefore, when we compare two false theories, the right conjunct does not hold simultaneously with the left conjunct in Popper's definition, as shown by David Miller (1974, pp. 155-160).

It is not possible to always add true propositions by themselves to a theory; we are inevitably going to get some false statements alongside the domain we are trying to expand our theory into. If this domain is complex and still evolving, things are even worse since the evidence regarding some statements will be incomplete or ambiguous. Adding a true statement requires additional assumptions or related propositions that have not been verified. Expanding our framework to include new entities or mechanisms of action attracts into our set some non-verified or speculative claims. Since theorems are often interconnected sets of statements, and each hypothesis comes together with some auxiliary hypotheses that are explicitly or implicitly attached to it, our falsity content must increase while we are trying to increase our accuracy by including some true statements in our theories'domain of implication.

5.2.2 Niiniluoto's Definition of Verisimilitude

Niiniluoto modifies Karl Popper's initial ideas by introducing a probabilistic and semantic conceptual framework. First, he considers a set of possible worlds, where each world is created by a scientific theory. A theory T can be taken as a hypothesis that specifies the properties and state of the associated possible world(s). In this picture each theory implies a set of possible worlds in which it holds true (Niiniluoto, 1987, pp. 215-230). The actual world W^* is also among these possible worlds and each possible world W is a complete description of how things may pan out.

Distance measure

Niiniluoto introduces a metric $d(W, W^*)$ to account for how far a possible world W is from the actual world W^* (ibid, pp.167-169). This is the amount of discrepancy between what picture a theory paints of the world and what is actually the case. Note that his actual descriptions are a lot lengthier and deeper, and I will present a very simplified summary here. The distance metric is tantamount to the following:

$$d(W, W^*) = \sum_i \omega_i \cdot \delta_i(W, W^*) \tag{5.1}$$

where δ_i is a binary function that is equal to 0 if i^{th} proposition is the same in W and W^* and ω_i the weight assigned to i^th proposition which reflects its importance in determining truthlikeness.

Expected distance defined by using this metric gives us the truthlikeness of a theory T. Verisimilitude is then the inverse of the expected distance of the possible worlds where T holds from the actual world, weighted by their probabilities.

$$V(T) = \sum_{W \in T} P(W|T) \cdot d(W, W^*) \tag{5.2}$$

where $P(W|T)$ stands for the likelihood of world W given theory T. According to this formula, we can compare two theorems T_1 and T_2:
If $V(T_1) < V(T_2)$ then the theory T_1 is more truthlike than theory T_2 . The actual world's expected distance from the suggested world of theory T_1 is smaller in this case.

Niiniluoto's approach is based on the semantic concept of theories as sets of possible worlds. Utilizing probabilistic methods gives us a way to assess our distance from the truth within a theory's implication set. In this way, verisimilitude can be assessed by changing degrees. Truth content is considered, in addition to the likelihood. Hence, unlike Popper's system of binary evaluation, where we are counting true and false statements, Niiniluoto's definition takes into account the semantic distance between W^*- the actual world- and W -the set of possible worlds suggested by theories-.

Usually describing a convergence to truth or the concept of approximate truth is inherently a qualitative procedure, Niiniluoto's account provides a way to quantitatively assess verisimilitude as it introduces a metric that measures how much of the content of a theory matches with what is actually happening. The problem in Popper's definition occurs when we compare two false theories. Niiniluoto's account circumvents this problem by allowing partial truths. Because even in theories that have turned out to be false, there might be some propositions that are very close to the truth. Probabilistic weighing allows us to solve the paradox that emerges when we try to increase the accuracy of a theory: the simultaneous increase of falsity content with truth content. The probabilistic framework allows side-stepping this issue because we are now evaluating theories according to their overall matching with W^*, not by evaluating propositions' truth values one by one.

In this method, *truthlikeness* may change gradually with infinitesimal increments; hence, it evolves in a continuum. This is a more realistic way to assess the progress of theories. The scenery of the scientific world today matches with this description of a gradual increase in *verisimilitude* over time since new theories are built on top of old theories'partially true parts. This approach also has the potential to guide scientists with their theory choices. Comparing and choosing among rival theories, even if they contain false elements, is possible this way.

Of course, defining *verisimilitude* is a really difficult and maybe inherently non-realizable endeavour. Even though Niiniluoto's approach is an immense improvement upon Popper's version, it still has some setbacks. The main problem with any approximate truth definition is actually a problem known as "Augustine's objection" in the literature, asserting that a definition of approximate truth utilizing the distance with the truth, already assumes a knowledge of what truth is. To expand this objection a little bit, let me clarify with an analogy. Suppose that you're at a park with your friend who is a philosopher. A little girl is coming crying to you, apparently she lost her older sister. You start to look for the sister to help her and you mumble during your search 'she must resemble her' along the road. Your friend objects this inference and tells you 'The judgement that the girl resembles her sister, in turn, presupposes an acquaintance with sister, but we don't yet know the other sister.' Similarly, Augustine's objection tells us when we claim we are getting closer to the truth, it implies that we already assume we are acquainted with the truth beforehand. But if we already know the truth, why are we trying to approximate it in the first place? In Niiniluoto's case, if the actual world W^* is assumed to be known, we are able to define a distance metric from that world to compare our theory's resemblance to it. My response to this objection is, that it is a distorted picture of what is going on in Niinuluoto's or any others' effort to define approximate truth. You do not have to know the other sister to have the opinion that the girl resembles her sister; we might just know that children look like their siblings by our common sense and inductive inference, we can back that intuition up with underlying genetics, and we can suggest the girl must resemble her sister to a degree, even though we do not know the older sister at all; and even if we didn't know the girl herself and were just talking about a hypothetical girl. Also let me note that your inference helps you a little bit in the scenario by narrowing the candidate pool and directs your attention selectively, whereas your friends' theoretical concern about the validity of such an inference adds nothing to the situation and don't get you closer to finding the other sister.

To make another analogous case, suppose that you are a brave captain, sailing in the ocean in the Middle Ages, and you want to discover new places. You have a compass that is designed to show you North, as any sailor would have. You do not know what is in the northernmost point of the earth, and you may not even be aware of Iceland's existence, for instance; also your geography maps are mostly incomplete. However, if you want to sail North, all you have to do is use your compass and make sure your ship is oriented in the direction that the compass points out. Then, putting one mile in front of another -every time in the direction of North- you will be getting closer to the North Pole even though you do not know there is a North Pole, even if you do not know the earth is a sphere. What is important here is the compass; if it works to guide your next little step, you can say that it is taking you closer to the North every day. Moreover, on the road, there will be other signs to assess whether your compass is working correctly as it is designed; for instance, the moss on the side of trees that you see will be in the northern direction; the weather will get colder if you have travelled significantly far away, if it is not a foggy night you may see the pole star that is also pointing to the North; all of these will be utilities for you to check your compass and assess whether it is broken or working. You might say that the knowledge about moss on the trees or climate and plants of northern regions or the pole star all include a familiarity with the concept of North in the first place. That is a valid observation however none of that other knowledge, required knowledge about the existence of North pole or what lies in the Northernmost point of the Earth. All of these statements about what happens or what changes, when you travel North, are just cohere with each other and that is the standard: you check them against each other. Therefore if you gather a new knowledge about travelling North, you assess it according to it is coherence with the old and established facts, that is how you know your compass- a relatively recent invention compared to other statements we have mentioned about North- is working as it supposed to be.[1]

Similarly, a method like Niiniluoto's does not have to assume an established absolute truth or a knowledge of the actual world; it is like a compass we use to guide our next step, and we want to know whether we are heading in the right direction in each step rather than assessing whether going North is worth it or what we will find at the end, or whether we will find anything there at all. So perceived as a method of comparing two theories, guiding theory choice and illuminating the next most reasonable alternative among many theories, Niiniluoto's description of *verisimilitude* is really helpful, and it does not have to assume knowledge of truth to begin with, as claimed by Augustine's objection. When we put Niinuluoto's definition to work, that is when we compare two theories T_1 and T_2 according to this metric of *verisimilitude*, we will find that if T_1 is more close to truth than T_2 then it will also be the case that T_1 is more successful than T_2 in empirical regards, because the definition itself is build up to take those correspondences with empirical data, i.e. predictive success into account. Therefore the assumption we've made in NMA to conclude the realistic account of science, is working and in accordance with this definition. Hence Laudan's claim that there is no satisfying definition is not exactly correct. If your def-

[1] If some of the more suspicious among the travelers might still want to know how you know that compasses are pointing North to begin with, even if your compass work as intended, they might ask how you know that all compasses are not showing East for instance. I'd suggest relieving your guests of their suspicion by pointing out that the compass is an invention that has been in use for centuries since antiquity, first invented by the Ancient Chinese and made to point North. Hence the millions of old voyages that's been done before yours, are there to testify the fact that compasses in general, point to the North. This is tantamount to an optimistic inference on the past success of scientific theories, i.e. the global NMA itself. It's been discussed in detail in the chapter 4 that a pessimistic induction on theory change was not right, the actual data turned out to be pointing out to an optimistic one, if any. Therefore a re-utterance of those concerns about the overall inquiry of science are left out of the discussion. Therefore, in this analogy how our compass could be trusted to show North in general, is left out; as it has been already addressed and discussed in the previous chapter. To sum it up very briefly, if we were to apply our findings on that front within this analogy too, any conspiracies such as compasses actually designed to be showing East and previous travellers were being deceived for centuries, are unfounded; there may be many broken compasses and naval accidents caused by them in the past, but they are not constituting a majority to warrant such conspiracies.

inition of a satisfying definition stems from a not meetable demand, for instance, you want a definition of a concept called ‘approximating X’ that doesn’t include ‘X’, then your expectations from a definition might be too high and theoretically impossible to meet. My take of a satisfying definition would be: a concept can be considered to be defined sufficiently clearly if that definition lets you use the same concept without any contradictory results, across the domain wherever it occurs and explains what the word or word group is representing clearly enough, that is considered a satisfying definition. Since Niinuluoto’s verisimilitude definition does not create any such conflicting occurrences or paradoxes, is sufficient to gather the difference between the two theories, in alignment with the claim in NMA and sufficiently accounts for the relationship with success and approximate truth, it is acceptable by the aforementioned more realistic definition of a definition. The criticism around T_1, starting with such concerns around the definition of the concept of verisimilitude should be left aside.

5.3 Conclusion

Overall, we have seen that a formal and rigorous definition of approximate truth, which is cleared of all scrutiny, is not possible, but it is not needed either. The merits of defining this concept clearly cannot be denied, and it would strengthen the realist claim in NMA indefinitely if it were indeed possible. However, the lack of a formal and strict definition is not a central problem to what has been tried to be conveyed in the argument NMA, which is that success implies an approximation to the truth. Methods like Niiniluoto’s metric for *verisimilitude* could help with the comparison of theories and assessing the degree of approximation and comparing them with the amount of overall success a theory has; without the need for formal and strict definitions like Tarski’s truth definition. Also, the concept is a qualitative adjective and can be grasped intuitively, and Niiniluoto’s quantitative definition is in alignment with that common-sensical and intuitive understanding too. We can work with this definition to show that the realist claim is holding by comparing theories according to this metric, similar to people who have been using compasses to guide their travels for centuries. Those people were travelling in the directions they wanted, without knowing anything about magnetic fields and the underlying mechanisms of how the compasses work, and they would have every right to claim that their compasses worked. We can utilize the concept of approximate truth in our arguments without providing an exact definition for it, and still can claim what NMA claims, without an exact definition for approximate truth at hand, a working definition is more than enough to establish the claim in consideration.

CHAPTER 6

DISCUSSION: TOWARDS A SELECTIVE PRAGMATIC REALISM

In the earlier chapters, we've analyzed the major fronts of the scientific realism debate as of today. We've looked upon objections from theory change, rule circularity, approximate truth and the base rate fallacy. On the issue of base rate fallacy, some realist responses to this criticism have shown that the debate is actually reducible to one's opinion to begin with, for instance, the theory pool to define the base rate was kept very small by the realists where's it's been considered to be larger by the anti-realist and this choice was reflecting their own optimism or pessimism, to begin with. When it comes to pessimistic induction; although we've seen with Moti Mizrahi's random sampling demonstration (2013) that the claim about change in most theories turned out to be a false assumption, nevertheless significant revolutionary changes have occurred throughout the history of modern systematic science. Our exponential re-drawing of the history of science and Mizrahi's statistical analysis gives us grounds for a realist commitment but doesn't fully discard the antirealists'concerns. On the other hand, the debate is dissolved in a stalemate in the rule circularity confrontation. Whether we pick an externalist or internalist approach in epistemology determines our tolerance for rule circularity and the debate is transferred to the epistemologists on that side. Another way to discuss this rule circularity was to dig into the roots and discuss in general whether a meta-theory should be more stringently proven than a theory in consideration. That perspective was opened up by Fine, in which we accepted his invitation and took a closer look into the philosophy of mathematics, turned out to be also matter of taste. Whether one is willing to loosen their demands when that demand is rigorously shown to be non-meetable by logical limitations, is some form of an aesthetic choice and in turn depends on how much you are willing to trust humans'mathematico-logical intuition in a sense. Therefore the rule circularity in using IBE to support scientific realism can neither be justified nor be condemned according to this analysis. Finally, when we've analyzed the debates surrounding the concept of approximate truth, we've seen that some rigorous attempts to define the concept - to be able to use it in connection with success- are given. However, some underlying logical and philosophical concerns remain no matter how rigorous a definition has been. In response to the begging the question around assuming truth to deduce whether we're approximating it, we've suggested utilizing the concept of approximate truth, just like a compass; rather than an inherently perfect definition we can approximate the concept of approximate truth itself, as best as we can and use it to compare different truths.

All of the previous observations and fruits we've gathered from the major debates surrounding NMA suggest one thing: there's a stalemate in the debate and the conversations just stuck on different fronts. We can't justifiably defend realism in the traditional sense, at least not from this argument but anti-realist criticisms are not also reaching their full effect and therefore best way to follow is to incorporate some of anti-realists'epistemic humility to realism, to respond to their major concerns and get one step closer to solve or step forward from this decades long debate. This chapter synthesizes these discussions into a coherent defence of selective pragmatic realism, a position that emerges from the fusion of standard realism's strengths with pragmatic and heuristic considerations. We argue that while a full-blooded commitment to scientific realism is not tenable, a selective, pragmatic approach offers a middle path that acknowledges the merits of realism in a manner that remains sensitive to its philosophical challenges.

6.1 Reasons for Suggesting SPR

6.1.1 From Predictive Success to Pragmatic Realism

The foundational argument for scientific realism has been NMA, as we've mentioned earlier. NMA argues that science's immense success in its predictions would be a miracle if the theories were not pointing out truths, or at least converging towards a truth with time. In my view, this success doesn't necessarily indicate truth but gives us excuses to hold our belief in scientific theories as representing truths, approximately. A complete realist commitment to every dimension or aspect of theories is not dictated. We've seen with the PMI objection that many old theories were discarded later on. Although this observation doesn't ground a meta-induction as we've seen that the claim about "most" of them being changed, is a false accusation; nevertheless this pessimistic observation warrants consideration when we re-assess our realism. The resolution of this tension lies in accepting that not every aspect of every scientific theory is approximately true, but there's a tendency to retain most theories as Mizrahi demonstrated (2013, pp. 3220-3223). With a selective filter when committing to realism, we're acknowledging that not all aspects and not all theories survive.

The key to resolution lies in taking predictive success as a heuristic, utilizing it as a navigation tool when we try to distinguish between theories which will survive the harsh tests of time. This strategic tool lets us be realists about certain aspects of theories that have been responsible for the theories'success and withhold judgment about other parts which are not essential to its success. The difference with a full realist commitment here is that we're taking predictive success as suggestive rather than indicative of truth, so this is a weak and flexible commitment which is open to changes by future evidence, while we're also diverting from an anti-realistic full-blown rejection of an underlying truth.

6.1.2 Heuristic Approaches and Survivorship

One approach we've discussed as a response to theory change observation was to restrict our realism span. We've mentioned two opposing views which are applying this strategy. One was recommending belief in the reality of the structure whereas the other to the entities. Both views have their own justifications for doing so, and usually, they demonstrate why the aspect they're focused on should be preferred over the other by pointing out historical examples. The shared characteristic is, though, that both sides actually track the elements that have resisted the change, the surviving parts of theories. The conflict between these two major branches of realism stems from the fact that it's impossible to demonstrate a general claim by giving one or two case histories. The opponents of both sides try to inductively generalize from some cases of structure retention or entity retention during scientific theory changes, to an overall survival of one of these two. General statements can only be falsified by counter-examples rather than being supported by such case examples. Since it's an impossible and futile project to scan all the successful and alive scientific theories to check whether the structure or the entities outlive the other; I suggest taking the common denominator of these two branches, namely the survivorship, as another heuristic; which suggests there's an underlying reality corresponding to that surviving part of the theory. This approach both incorporates the theory change criticism into realism and also guides us within our choice of branches within realism. It lets us approach theories locally and discuss in a more fruitful way rather than trying to reach an epistemic over-generalization sweeping across the whole science. Note that this is not only a compromise to satisfy the instrumentalist critics, this is a refinement that counts for occurrences of theory change while providing a subtle, improved account of realism. We're avoiding the pessimistic pitfall of over-generalization from past failures without falling into the pitfall of overgeneralizing from past successes by incorporating this concept as a metric that's suggestive of converging towards some truth without necessarily implying there's an underlying truth. We're leaving behind the false dichotomy of realism & anti-realism behind.

6.1.3 Pragmatic Realism and the Utility of Scientific Theories

We've depicted there's a stalemate in the debate between realists and anti-realists; in the chapters that we've examined rule circularity, base rate fallacy and approximate truth issues we've seen two sides often talked past each other and the core conflicts couldn't be resolved in rule circularity and approximate truth debates. Taking a pragmatic turn to shift our focus from whether scientific theories are strictly true to whether they're useful in guiding scientific practice is helpful to direct future research into a more effective realm and focus on conflict resolution rather than this deadlock. The success of a theory can be explained by its practical utility and doesn't necessarily solely depend on its reality. Similar to our block of code which reliably gives the desired outcome, some scientific theory reliably predicts phenomena. Some other theories prove themselves useful in aiding the development of new technologies, some provide satisfying explanations of phenomena. These kinds of theories deserve some respect, or namely realist commitment, even if every aspect of them is not going to be ultimately true. Here we're choosing to hold our virtual cup with the handle that can carry it which is the pragmatic consideration, rather than the useless handle that won't carry the weight, which is epistemic justification.

6.1.4 Synthesizing Realism and Anti-Realism: A Unified Approach

One point we've emphasized in the introduction of this study was that realism and anti-realism are not contradictory positions, although the naming is unfortunate, they are only contrary. Both sides accept the success of science and diverge on how to explain that success, or whether it warrants an explanation at all. For instance, instrumentalists and constructive empiricists argue that scientific theories are mere tools for organizing our experience, without commitment to their truth. Selective pragmatic realism synthesizes elements of both positions by carefully allocating our realist commitment -as it seems that we can only have a limited amount of it if we want to avoid being metaphysically too generous and annoying the anti-realists- among the parts of scientific theories that have proven themselves and turned out to be resilient .

We've left the problem of underdetermination mostly out of our discussion, but we've already seen in eliminative inference considerations when responding PMI, that it's closely related to our discussion. Addressing that problem is also possible via a selective approach. Underdetermination stems from the observation that multiple theories can explain the same data. We've mentioned that not all theories are created equal while discussing eliminative inference and attrition in science. But to refresh ourselves, let me emphasize that a set of possible theories is distributed on a bell curve and not every hypothesis has the same capabilities: while we were discussing PMI we mentioned some metrics to assess theories like empirical success, coherence with other facts, explanatory power and such. Selective realism acknowledges the fact that not all theories are equally plausible. A reasonable suggestion in that case is that the ones which provide the most successful predictions should be granted a higher degree of realist commitment and the degrees of this realist commitment should be constantly updated, and re-evaluated. We should view our realist commitment not as a binary function with two values but as a continuous variable that provides the base rate or prior probability in a subjective Bayesian sense. This approach balances the epistemic humility of anti-realism with the explanatory power of realism.

6.1.5 Cognitive and Heuristic Contributions to Selective Realism

One aspect of the science that's been overlooked in the realism vs. anti-realism debate is the scientists who are conducting the research. Kuhn pointed out to this dimension (1962) but his considerations emphasized the social dimension of science. We can also recognize the fact that science is a cognitively constrained process. As such it's impossible for science to meet unrealistic demands like being entirely true, inherently such criteria is doomed to fail. For instance, we have seen while analysing rule circularity that some form of unproven or not-yet-justified axioms are necessary in any epistemic system. Also, science doesn't have to perfectly mirror reality, we're beings that operate on species-specific perceptive limitations (Giere, 2006), but that doesn't mean that we can't latch onto any truth regarding the outside world or patterns we've depicted are surreal. Mitchell for instance, accepts the observations on our cognitive perceptive limitations and how science has to simplify phenomena by modelling, but still argues for a selective realism (2009). Following similar veins, I'm proposing evaluating theories based on their success in explaining existing phenomena and predicting future phenomena reliably. This heuristic-driven approach allows us to retain a commitment to the most reliable elements of scientific knowledge while remaining open to revision and improvement.

6.1.6 Conclusion: Pragmatic Realism as a Path Forward

The aforementioned arguments suggest a defence of a position that balances the strengths of scientific realism with the fruits that can be gathered from the critics of the view. This way, we can retain realist commitments when they are justified while also accepting the limitations set by the evolving nature of scientific knowledge. This perspective allows us to transcend the philosophical ennui in the realism vs. anti-realism debate by concentrating on the pragmatic value of scientific theories in guiding the research in future. Selective pragmatic realism thus provides a dynamic framework for understanding the success of science without the perils of following traditional realism.

6.2 A Selective Pragmatic Realism

The No Miracles Argument, uttered by Putnam as a significant support for scientific realism, has been criticized from different grounds since its birth. We have seen that major fronts of confrontation have been the criticism from theory change, the base rate fallacy in the argument, the circularity happening due to the usage of inference to the best explanation and the discussion surrounding the concept of verisimilitude. Throughout the survey of objections to this influential argument, one thing appears to be sure: scientific realism, as it is formulated initially, is not anymore supportable but needs some modulations based on the rightful criticisms if it is going to be a serious position in the scene of philosophy of science today. Also, the ongoing rapid changes in methodology and technological improvements in the new millennium should have their weight in this debate, and the new science requires a new philosophical stance as we've seen with most realist responses an emphasis on the improvement of modern methods and a date and context-sensitive dedication to realism rather than a continual optimism on the whole history of science. Taking all of these into consideration, I would suggest some modifications to standard realism.

6.2.1 Selective Realism

When we are discussing theory change, one of the reasonable positions to take into account for the mentioned change is structural realism. Following that line of reasoning and combining it with the conclusions drawn from other criticisms, the most reasonable stance is to be selective of what we are going to be realistic about. While some aspects of scientific theories warrant realist commitments, not all of the components are created equal within a theory. A localized, individualistic approach to theories rather than a blanket generalization would be helpful at this point. As Henderson offered to solve the ennui (2014), we can take the globally optimistic position based on the original NMA to support local theories, but we must be really careful in the process. Within a specific theory, the reality of specific theoretical entities, mechanisms, or structures that have demonstrated consistent empirical success and explanatory power could be taken literally, while an agnostic or "suspending judgment" approach is applied to the remaining parts. Most of the time, this retainable part is the theoretical structure, but it does not always have to be the case. To ground the selection of which items to choose in this commitment, the approximate truth definition given by Niiniluoto (1987) could be a useful compass. I am not suggesting a strict and general commitment to the mathematical structure or entities, rather I'm advocating for approaching case by case to the theories to pick the most commit-worthy items. Let us have a look at two examples to see how this method would be beneficial:

From Newton to Einstein

Newtonian laws of motion and universal gravitation were the *modus operandi* of mechanics for a long time. The laws were very reliable with one little caveat: They described the motion of objects in everyday circumstances, the objects were moving with ordinary speeds, and they were of the usual size. Space and time were absolutes, and action at a distance was a surprising but accepted phenomenon pertaining to forces. Einstein's general relativity changed that picture drastically, depicting space-time as a 4-D continuum, and the gravitational attraction was nothing else than a result of the curvature of space-time created by masses. Therefore, no action-at-a-distance was occurring; the gravitational force was only a geometrical consequence of the space-time structure. On the surface level, these two theories seem to contradict each other so radically that no one would even bother to check their genetics to make sure whether they are related. However, when we do apply genetic testing, that is, looking closer at the mathematical structure, surprising results await us.

Just like the case with Fresnel and Maxwell's theories of light, Einstein's theory is reduced to the good old Newtonian mechanics - to the relief of a lot of freshman STEM students- in certain limits: low speed and weak fields. So Newton's equations are an approximation that is occurring in the limited case of GR. For most predictive and pragmatic concerns, classical mechanics is still valid. A Niiniluotan metric of approximate truth can verify this since the difference between the GR result and Newtonian result in everyday cases is so small that the metric of verisimilitude would give us a distance that is incredibly small in this case. The mathematical structures governing relationships between entities, such as energy-momentum relations, are equivalent in both frameworks. Even though the interpretation of the underlying physics changes, we can be certain that structural relations remain the same. Furthermore, both theories predict elliptical orbits for planets around the Sun. However, Einstein's theory provides a more precise description, accounting for phenomena like the precession of Mercury's orbit, which Newton's theory could not fully explain.

The Case of Electron

Electrons are one of the unluckiest entities in science; their definitions have kept changing since their discovery. Nonetheless, one thing remains the same: We can manipulate electrons and detect them reliably by their effects on the outcome of certain experiments. Regardless of the theory surrounding the electron, the thing itself keeps existing.

Evolution of Our Understanding of Electrons

- *J. J. Thomson (1897)*: In Thompson's cathode ray experiments, electrons were discovered as negatively charged and indivisible objects a lot less massive than the protons, so they have been recognised as a distinct entity by the calculations' indication of a tiny mass.
- *Quantum Mechanics (1920s)*: With the emergence of Quantum Mechanics, electrons have become a wave-particle duality like many other tiny and speedy objects; in fact, with De-Broglie's equation, everything turned out to be wave-y when there is enough speed.
- *Quantum Field Theory (Mid-20th Century)*: A quantum field can be thought of as a medium that permeates the entire universe, and particles are nothing more than quantized excitations or disturbances in these fields. To visualize this, imagine the quantum field as the surface of a calm ocean. In its lowest energy state (the vacuum state), the field is flat and stable. When energy is introduced into the system, the field is perturbed, leading to localized disturbances or "ripples". These ripples correspond to particles. An electron is created by excitations in a Dirac field. When enough energy combines in a local discrete

packet, it behaves like a particle, and it is quantized in distinct amounts that specify the type of particle.

Although the theoretical description radically shifts, the electrons remain there; we can manipulate them in different ways, like in Stern-Gerlach or Young's Double-Slit experiments. We can measure their attributes and use them in the application, the microscopes that let us see inside of a cell or cathode ray tubes in old TVs; even the hardwares in my computer that lets me type these lines are called *electronics*, quite meaningfully. Physicists can still accelerate the electrons in particle accelerators independent of QFT describing them as oscillations in the field, or Thompson describes them as indivisible particles. Therefore, we can see that the electron itself survives the theory change related to the concept.

As we have seen with these two cases, there are incidences where different parts of theories are more resistant to the corrosion of time, and strict adherence to the entity, or structural realism, is not dictated by the history of science. Being selective and leaning towards one strong part of a theory, which is empirically adequate and adheres to the heuristics I will be providing in the next section and believing in that part's reality is a reasonable optimistic approach to take. Given the last century's events and the ongoing acceleration in new and more reliable methods, as discussed in attrition considerations and eliminative inference responses to PMI, we have sufficient ground to stay optimistic. Sherilyn Roush's observation about how new methods are more reliable (2003) combined with the weighted time-axis perspective suggested by the exponential growth of science (Fahrbach 2011), solve the theory change issue within a realistic framework. Moti Mizrahi's random sampling results (2013) and the previous two studies sufficiently clear up anti-realists'pessimistic worries and suggest an optimistic attitude and even an optimistic induction instead of a pessimistic one. We are justified in believing a correspondence between what theories suggest and reality, but at the same time, anti-realist objection from theory change was not in vain. Although they are not making up the majority of past theories as implied by PMI, there are indeed some jumps in history of sciences where paradigms of the field change, in Kuhn's words. What we can learn from the history of science is, then, not all parts of a successful theory are retained over time, and we can be realists still, since the majority are indeed retained. Nevertheless, we are justified to be picky and suspicious. Therefore we should be smart about our choices and selective in the right direction. The realist would be better supported in a localized, case-by-case approach to theories with the global NMA as a supporting optimistic induction behind her back. In this new framework, the restrictive sub-types of realism must serve as methods to approach different theories rather than big -isms to commit to on a general level.

6.2.2 A Cognitive Perspective to Guide Our Selection

The traditional debate between realism and anti-realism seems to be resulting in an ennui on different fronts: defining verisimilitude in a persuading way or overcoming the rule circularity involved. Even on the fronts that seem to be solvable, like the PMI discussion or the BRF criticism, the matter can be reduced to a decision between keeping the theory pool large enough to include all the falsified ones or not. Therefore, looking from a perspective different from classical epistemological discussions would be helpful. One of the ways we can transcend this ennui is taking a cognitive turn. Up until this point, we have considered theorems themselves, but what about the scientists themselves? The mechanisms of cognition that direct scientists while they are conducting experiments, hypothesizing new proposals or choosing one theory over another can enlighten the issue surprisingly and simplify some seem to be unsolvable issues.

Examining the relationship between theories and the actual world was the main way the realism debate was carried out. However, there is also the relationship between the theorizer and the theorems. The cognitive frameworks that have been utilized in the process of research gain importance in understanding the truthlikeness of theories. Considering the evolutionary process behind humans' first and second-level cognitive processes, we can infer the degrees of assimilating truth or truth-tracking. Vlerick, for instance, points out the distinct role logic and mathematics play in a human's conceptualization of the world. Mathematical -or structural- thinking, in general, lets us reach beyond species-specific pragmatic information gathering that other mammals and primates are capable of (2022). Since it is a culturally built, abstract way of forming beliefs and lets us examine our first level of intuitive thinking from a more reliable and measurable point of view, it is beyond the skewed lenses of perception that can represent reality differently for each species.

Another issue that is brought up by cognitive approaches, like Vlerick's or Giere's, is that truth is not mirrored but represented by science. By deciphering how scientific model building works by examining underlying cognitive processes, we can assess which parts of the models correspond with real structures in the universe. One important feature of modelling is that there might be more than one representation of phenomena, but this does not make all of them pointless or false. For instance, a dog and a human might see different colours when they are out in the park playing fetch. Nonetheless, the perception of both of them latches on to some structure or pattern that is real. For instance, when we look at cherry trees, we see a red and green pattern representing fruits and leaves; another mammal that cannot distinguish the red part of the light spectrum might not see the fruits distinguishably. A honeybee for instance sees more colours than us and has a different, more interesting picture. All three species grasp reality in their unique way, but none of us has experiences that are "unreal", like a hallucination. Evolution donated all living beings in a way that we are navigating in the world and surviving, so we must have been gathering something of the truth; otherwise, a chaotic and mismatching pattern between cognition and reality would not let us survive. So, an approach to truth-tracking that focuses on fitting rather than mirroring comes in handy when explaining what science actually does.

By focusing on cognitive processes, we can look at the heuristics, cognitive limitations, and biases and refine our realist position accordingly. The constructs that survive despite cognitive limits and biases are more worthy of our realist commitment, like Verlick suggested with focusing on structural or mathematical thinking which is a cultural convention that is beyond first level cognitive processes. In contrast, we can be more cautious about some entities that are posited by theories if they reflect much of the aforementioned biases. All in all, re-evaluating the debate with new lenses and not accepting it as a purely philosophical problem is helpful, considering how the traditional debate is stuck on almost every point of confrontation between the realist and the anti-realist. The cognitive approach is giving us the leverage we need and directs us to look at the heuristics.

6.2.3 Taking a Heuristic Approach

The Symbolic Utility of Realism

it is undeniable that scientific realism donates the scientists with a purposefulness that is lacking in an empiricist frame of approach. Following Cintora's (2004) suggestion, there is a symbolic utility with the realistic frame of mind. Since the epistemic grounds for taking a complete realist stance in the classical sense turned out to be inadequate, especially with the problems of rule circularity and defining approximate truth, we can look into other concerns to tip the scale to one side. The motivation and dedication of scientists seem to be essential pragmatic concerns. Taking into consideration that, from sophists in Ancient Greece -who rendered it impossible to do any intellectual inquiry- to the logical positivists of the 20th century - who almost killed major branches of philosophy with their high demands- nobody has benefited from an extreme suspicion, therefore following a realistic philosophical approach would be the more reasonable and realistic attitude among the alternatives, from pragmatic grounds, as we've already seen in rule circularity and approximate truth discussions how unrealistic and perfectionistic the epistemic demands raised by the anti-realist critics and if those standards were applied as general rules of thumb how other branches in philosophy would also get stuck with infinite and unsolvable regresses to foundations.

The Heuristic Value of Realism

Since realists believe that their postulates correspond with reality and that they are unraveling the mysteries of the cosmos by conducting scientific research, they are prone to be more imaginative and bold. A realistic philosophy pushes the researcher to look for buried layers of explanation rather than being satisfied with a surface-level one, pursue novelty in their lines of research and overall be more enthusiastic. At the same time, the same heuristic drive is lacking in the scientists who are only occupied with the idea of empirical adequacy and acting like they are playing a matching items game with hypothesis and data obtained empirically. For instance Cíntora acknowledges that epistemic arguments alone cannot definitively establish realism over empiricism. However, from a pragmatic standpoint, realism is the more fruitful stance

(2004). On the other hand David Deutsch brings up a clever fortune-teller analogy to explain the difference between an empirically adequate and predictively successful theory vs. a theory that gives explanations. According to this analogy, a science without necessary underlying mechanisms and correspondence with reality would be equivalent to having a prophet that always gives you true predictions but can't help you with building an aeroplane; since you have to know which questions to ask and which order to ask them and apply all the possible combinations before you actually gather the necessary info to build an aeroplane (1996), that kind of prophet is equivalent to nature itself, natural phenomena always gives you the correct result when you gone to them with your hypothesis, but that is not science, science is what is helping you come up with relavant hypothesis, for instance aerodynamics knowledge build up on classical and fluid mechanics provides you with a layer of explanation other than correct prophecies. Although empiricism, which suggests an agnostic stance on the truth value of theories and reality of unobservable entities posited by science, avoids some pitfalls in the epistemological realm, it has diminished returns when pragmatic considerations like the progress of science and giving adequate explanations are considered. Since epistemological assessments turned out not favouring either realism or anti-realism in the end, and we have seen that it is almost reduced to a matter of choice to be optimistic or pessimistic about our theories' or undetectable objects' power of matching with reality, there is a significant value in considering pragmatic issues instead to resolve the ennui. Whichever side we pick, nevertheless, we cannot deny that a scientifically curious mind of a researcher is already twisted towards accepting a realist attitude even though they do not explicitly acknowledge it; it is intrinsically what motivates their endeavour. Otherwise, the research process would be dreadful and pointless. The belief in uncovering an underlying reality that is independent of our minds helps drive science forward, resulting in new discoveries.

6.2.4 The Heuristic Framework for Realist Commitments

Combining the suggestions of the previous chapters, I advocate for a heuristic approach to realism which has more room for flexibility and provides a selective frame that is context-sensitive. Heuristics are defined as problem-solving strategies to guide our decision-making in case of uncertainty. To decide when applying a realist approach is justified and when we should be careful and withhold our judgement, as an agnostic empiricist stance would suggest; we can apply the following heuristics.

i. **Success Heuristic**
As predictive success is the strongest reason for suggesting realism, which is the central pillar of NMA, we can safely rely on it while assessing the degree of realist commitment we should entertain for a theory. What we have learned from the discussions about theory change and base rate fallacy is that the relationship between empirical success and coherence with reality is not easily deniable and confrontations on that front turned out to be defeatable. What we mean by success here should be refined a little bit; on top of predictive accuracy, we can also check for coherence in the resulting explanation of phenomena and also the theories'success in guiding the experimental design and re-directing research if necessary. For instance, Q. Mech. is consistently successful in explaining events at the subatomic level. That provides a justification for realist commitment to some of its core structural aspects even though we cannot successfully describe the fundamental entities pertaining to the theorem.

ii. **Survivorship Heuristic**
To accommodate for the theory change and so-called Kuhnian jumps in the history of modern sciences, we can restrict our realist commitment to the 'fittest' theorems in the history of mature sciences. Taking into account how much of a theory has sustained for how many eras in that particular science is a good indicator. We are looking for not-yet-discarded theories that are minimally revised throughout their lives when new evidence is accumulated. This heuristic takes into account the evolutionary explanation of van Fraessen, although not as a suggestion of anti-realism but as a measure of correspondence with reality instead. For instance, the atoms, as building blocks of matter or theory of atoms if you like, survived through all the shifts in modern physics and, hence, chemistry. Whether we take a classical mechanics approach or a quantum mechanical one, the fact that matter is comprised of matter remains the same. This longevity supports a realist commitment to atoms as real entities in this case. Another case that is more in accordance with minimal revision could be Newtonian mechanics. The minimal revisions required to incorporate relativity

suggest that the core of Newtonian mechanics reflects real aspects of physical laws.

iii. **Coherence Heuristic**

While we were discussing definitions of approximate truth, we mentioned that the coherence between other established facts or successful theories is a nice metric to guide our way- towards North-. If a scientific hypothesis or theorem blends in smoothly with the cocktail of existing theorems and makes the taste better, we can increase our faith in it is future. For example, evolution is in accord with molecular biology, genetics and archaeological fossil records. Hence, we are supported in our belief that it corresponds with reality.

6.2.5 Navigating Rule Circularity and Approximate Truth Issues

A heuristic approach allows us to circumnavigate unresolved issues like rule circularity and approximate truth. In both of these objections, we have seen the ultimate perspective one adopts become a matter of choice of whether we want to be optimistic or pessimistic, rather than a certain epistemic indication favouring one view over another.

Rule Circularity

Since using inference to the best explanation to support a realistic approach creates a rule circularity, which turned out to be indispensable, a heuristic approach is helpful in this regard. The justification of our commitment is based on practical success and evidence rather than a self-validating principle by accepting such an approach.

Approximate Truth

We have arrived at the conclusion of using the definitions of approximate truth as a compass to guide our next step, utilizing the concept as a tool rather than an epistemic principle to adhere to. Heuristics is parallel to this line of thinking, suggesting a commitment by focusing on empirically successful parts of theories using metrics like coherence, longevity, minimal improvisation and so on; in the same way, we can treat approximate truth as a working assumption rather than an absolute criterion.

Pragmatic Realism through Heuristics

To conclude, we have established that a cognitive turn on the issue is helpful and scientific realism can be approached pragmatically via heuristics. Rather than a blanket commitment to classical realism, we can decide whether a theorem deserves our commitment, case by case, through certain heuristics. Taking into account the dynamic nature of science, which also increases exponentially day by day, we need to adjust our philosophy with equal dynamism and leave aside dogmatic, pretentious philosophical positions of the last century. With the new realistic approach with heuristics and cognitive turn, our commitments are not rigid and unchanging but open to revision in the face of new evidence, in accord with the nature of the scientific method itself.

6.3 Difference with pragmatism

6.3.1 Pragmatism

Originating in the 19th century with Dewey, Peirce and James's ideas; Pragmatism advocates for, at it's core that the measure we should be evaluating our theories with should be their capacity to be of use in problem-solving and guiding future research. The correspondence with a mind-independent reality and describing it as it is; is left aside.

The Pragmatic Maxim of Peirce asserts that the meaning of a concept or theory is to be found in its practical effects. Hence we should be focusing on the practical consequences of believing a theory or utilizing a concept without worrying ourselves with an underlying reality (1878). To exemplify we can look at the concept of energy in physics; it's accepted to be meaningful because it helps us predict outcomes and guides experimentation, Peirce's maxim cuts away any further questioning of its truth in an ontological sense. James' idea of truth as what works builds on top of this suggested dis-worry about underlying reality or correspondence. Truth is not a static function between our scientific descriptions and reality but what works in practice according to James (1907). A theory is true in this definition if it helps to navigate the world by solving problems and leading to successful predictions. It's not an intrinsic property of statements but originates in the engagement with reality. James'truth is significantly different from the standard realist conceptualisation of truth, where the theories are literally true or false and evaluated according to their correspondence with the external world. James' truth is evolving and dynamic, and within that framework, theories are judged according to pragmatic consequences.

Dewey follows a similar line with utilizing scientific theories as toolkits in the scientific inquiry which is regarded as some sort of a puzzle-solving related to problems we face in our everyday life (1938). The inquiry is not about discovering the subtleties of universe or reaching some immutable truths, hence theories are not mirroring reality but help us navigate and cope with our environment. Knowledge generated by this inquiry is tentative and always revisable as new problems accumulate. We're improving our methods and models to adapt to the world more effectively according to him.

Rejection of Metaphysical Realism

Pragmatism is more in alignment with anti-realist positions than realist ones. As realists have a positive claim about correspondence with a mind-independent reality in scientific knowledge and pragmatists find this concern irrelevant and stays agnostic or downright reject engaging with the issue. As the name is self explanatory, no point in emphasizing they're only concerned themselves with the theories'success in serving human needs and carrying us to our aimed positions, wherever that may be. For instance, quantum mechanics with its entanglement concept letting us build quantum computers and the quantum logic behind that development is helpful in carrying our computation theory forward, so we don't need to concern ourselves with the great break of reality around this entanglement concept and explaining very anti-intuitive consequences of quantum mechanics according to Pragmatism.

The Evolutionary Nature of Truth

Truth is an evolving concept in pragmatism as we've seen with James'truth as a working tool definition. According to pragmatists new observations of phenomena and new problems surrounding them prompt us to discard or revise our old theories. The evolutionary process is always ongoing and there's no ultimate truth to reach. Our current theories or any past theories for that matter, are useful or have been useful, complete their duty and cease to be relevant in the future and they become replaced with new and better ones. As we've discussed pessimistic meta induction in detail in one of the previous chapters, I will avoid raising the pitfalls and fallacies in this kind of thinking around theory change again. How the history of systematic modern science actually unfolds should be clear by now, from our previous discussions around PMI and base rate fallacy. Turning back to pragmatism, the philosophy is consequence and future-oriented, scientific research is depicted as a process of continual refinement. Theories are always being tested and improved. Note that, this emphasis on consequences and the future doesn't relieve one from explaining past successes, it's a shift of focus only. In rejecting metaphysical realism, pragmatism shifts the focus from seeking ultimate truths to understanding how theories function as instruments for navigating the world. This view, contrasted with standard realism, offers a dynamic and adaptable framework for thinking about the nature of scientific inquiry and its goals.

6.3.2 Selective pragmatic realism vs Pragmatism

SPR incorporates elements of realism and pragmatism but differs significantly from classical pragmatism in the philosophy of science in several key aspects. Below is a detailed analysis of the differences:

1. Commitment to Truth vs. Utility

SPR retains a commitment to the truth of scientific theories, even though it limits this commitment's scope to some particular aspects, taking into account success and longevity. Whereas in pragmatism we've seen the practical consequences of scientific theories are highlighted and utility became synonymous with validity. With Peirce's pragmatic maxim and James'working truth definition, we're observing that the meaning of a concept is directed to its practical effects. As opposed to that what we're doing with SPR: suggesting to commit a mind-independent reality, albeit selectively and using pragmatic concerns to guide our selections. We're utilizing success as one of our metrics of measuring whether we're approximating truth while they- the pragmatists- are equating utility itself with truth and don't concern themselves with an underlying truth. To get a clearer picture, let's think of a scenario where you're a deep marine researcher on a ship, the ocean beneath has 4 layers of water but your equipment which is donated with the best technology available in your field, can only reach the 3rd level under the surface. In this analogous case, if you're a supporter of Scientific Pragmatism: you would just re-label the 3rd layer you could found, as the 'deepest level' and get on with other parts of your mission. Because, you'd think, for all practical concerns, that's what is reachable to humankind right now and anything below that is considered irrelevant and unknowable to you or anyone else. Whereas a proponent of Scientific Realism suggests belief in a 4th level, even though we can't reach it with our equipment, since our 3rd level shows clear signs of being sitting above some other layer: for instance, we haven't detected the sand but only water in the 3rd strata of the ocean or there are some flow patterns in the water that necessitates another stratum below the current one.

Turning back to our version of realism, our position acknowledges the debate between realism and anti-realism. We aim to synthesize elements from both camps. We acknowledge the realist notion that some parts of science reveal truths about an external reality but we're integrating anti-realist concerns by limiting which aspects of a theory receive realist commitment. By focusing on the practical utility of theories, we navigate between the extreme poles of both positions. On the other hand, Pragmatism tends to bypass the realism vs. anti-realism debate by focusing on the practical success of science. We maintain an interest in the ontological status of scientific theories. While pragmatists suggest shifting the focus to practical consequences without needing to engage deeply with metaphysical questions. Since we've also suggested a shift into the practical realm when epistemological discussions resulted in a stalemate it might raise the worry in the careful reader's mind that what we're doing is just applying pragmatism. But we're turning to pragmatism, cognitive science and heuristic strategies to engage with the problem and show how they also support a realist commitment, rather than a suggestion of leaving the question of reality aside altogether. Hence the key difference between pragmatism and SPR is that: SPR does not reduce truth entirely to practical success but uses practical and predictive success as an indicator of truth in specific theoretical contexts while pragmatism, typically regards truth as synonymous with practical efficacy, avoids a deeper metaphysical commitment to (or even suggesting any implication of) reality.

2. Theory Change and Approximate Truth

We've examined the theory change criticism in Chapter 4 and acknowledged that some of the scientific theories may change over time. But we argued that the claim about most theories being changed has been falsified, therefore, it turned out that a statistical meta-induction relying on the "most" theories, is not grounded. When we carefully examine the history of science with incorporating exponential growth of science, improvement in methodology and attrition of theories; we've reached the opposite conclusion that an optimistic induction could be made because important core aspects are being retained and science is maturing into a stable period nowadays. Thus most resilient aspects of successful theories (such as mathematical structures, core mechanisms or sometimes entities, depending on the theory) should be considered approximately true. We suggest that these surviving elements reveal truths about reality, even if some entire theories are later revised or discarded. The Kuhnian or Laudanian history of science is an almost constant function with some huge discrete jumps and falls occurring frequently at random times, whereas in our picture, there's a continuously growing and mostly exponentially increasing function which has some bumps every now and then, in its crawling period at the first two centuries. Thus, selective realism, uses theory change as an opportunity to refine which parts of a theory deserve realist commitment, rather than interpreting it as a serious setback that can hinder the realist claim. While pragmatists align with the anti-realists and suggest that there are only tentatively working hypotheses at every point of history which are destined to change later on. They share the same illusion of anti-realists that science constantly drastically changes, which we've shown to be not even close to the real picture when careful close analysis is given to the matter. By careful analysis, we mean: a time axis, when drawing the picture of the history of theory change, that is re-evaluated logarithmically to incorporate the growth rate of science, and random sample analyses that are conducted to assess whether most theories do actually change; rather than buying into some long lists of dead or almost dead theories as justifications for a melancholic induction about the whole scientific inquiry.

Also we are suggesting a belief in the concept of approximate truth within selective realism, even though an exact definition of the concept is not yet given. Pragmatists do not focus on the concept of approximate truth in a similar manner. For them, the idea of truth is more fluid and contingent upon ongoing inquiry and practical outcomes. John Dewey in particular viewed scientific theories as working hypotheses rather than as steps toward an ultimate truth(1938), whereas we're viewing the concept of approximate truth as defined by Niinuluoto as a working hypothesis, a navigation tool which is tuned with the power of coherence. We're choosing to utilize the concept of approximate truth rather than utilizing theories themselves and not caring about their truth value, this is one of the key differences. To sum up, SPR retains a realist interpretation of certain enduring elements of scientific theories, despite acknowledging theories are not ultimate truths or perfect and complete pictures of reality and they're prone to some change or refinement. Pragmatism does not seek an approximation to an ultimate truth but rather focuses on how well theories function as instruments for addressing current problems.

3. Role of Heuristics and Cognitive Constraints

Selective Pragmatic Realism: We introduced heuristics as a way to navigate which parts of a scientific theory deserve our commitment. By this approach we recognize the borders of our perception and knowledge while still proposing a realist interpretation of core components. Durability, predictive success and coherence heuristics are suggestive for scientists when deciding which aspects within a theory are commitworthy. Although pragmatists also recognize the role of cognition in shaping scientific research, they are not concerning themselves with which parts or aspects of theories are real. The scientific theories are refined and adjusted based on what is working, like a self-feeding feedback loop; without external heuristics like we proposed as sorting algorithms of commit-worthy and unworthy items.

6.4 Conclusion

To sum up our previous points, although our pragmatic realism shares some commonalities with pragmatism like focusing on the practical success of theories, our selective commitment to realism distinguishes this view from pragmatism at its core. Pragmatism reduces truth to being a function of practical efficacy, we do the opposite of making practical efficacy one of many functions of measuring degree of converging to truth, thus we are not collapsing reality to utility. We use heuristics such as resiliency of theories or endurance of the aspects of theories as guidelines to direct our realist commitment while supporters of pragmatism suggest an indifference towards the issue of reality. With SPR we are trying to carve out a middle path, respecting the utility-focused insights of pragmatism and taking their suggestion into account, we're utilizing this view itself in our approach while retaining a selective and limited realist commitment to the truth of our theories. That's why we're still committed to three pillars of realism, on metaphysical, semantic and epistemological dimensions; we're not giving up on one of them in our selective elimination; we're just suggesting filtering theories according to a metric which would be a carefully weighted product of suggested heuristics. In this elimination, we're focusing on the components of a specific theory in question to assess the ultimate commit-worthiness of a theory. Determining the exact metric and a whole list of the heuristics to utilized in our approach, are beyond my expertise and this study is not intended as a conclusion of this decades-long debate but a step towards its solution to direct future research. Relevant questions to focus philosophical research on in the future would be determining a list of relevant heuristics, assessing their weights in the culminating truth value of a theory which is supposed to be on a scale that's changing from 0 to 1 in a continuum, rather than a binary semantic evaluation of standard realism.

CHAPTER 7

CONCLUSION

In this thesis, we surveyed through a systematic and in-depth exploration of historical scientific realism and anti-realism debate, with focusing on NMA particularly. My main goal has been to re-evaluate the status of scientific realism, and assess whether it can still be upheld, despite significant criticisms, in some form. We've analyzed a large spectrum of objections raised by anti-realists, like problems of circularity, probabilistic porblems like base rate fallacy and a deeper concern raised with historical narrative, the Pessimistic Meta Induction. We've left some major issues in the realism debate like the under-determination of theory by data, and anti-realist explanations of scientific practice out of our analysis, to be able to both them and the ones we have chosen to analyze justice. Throughout the study we aimed to get a grasp of the new position realism has been left after each criticism is taken into account and given their due as much as they necessitate. Each chapter aimed to provide a robust analysis of these objections and we've concluded with a discussion of how selective pragmatic realism might offer a balanced resolution.

In our introductory chapter, we have refreshed our memory about Scientific Realism and had a brief look at its dimensions and sub-types such as Entity Realism, Ontic and Epistemological Structural Realism. We have established the foundation of the discussion by prompting ourselves with the question of how to interpret the success of scientific theories. Realism in its standard form suggests that the legendary predictive success of science is not a miracle but stems from a pointing out to an underlying truth, albeit approximately. This idea is culminated in an argument, first coined together by Putnam, known as NMA and has been the driving force of the debate of realism, it prompted lots of criticisms and some major criticisms became the topic of our main chapters.

In Chapter 2, we addressed one of the central critiques, the Base Rate Fallacy accusation raised by critics like Colin Howson and Magnus & Callendar. According to this criticism, realists ignore the prior probability of a theory being true (in a set of successful theories) when defending their position by NMA. So there's the potential of erring on the side of optimism and assuming high probabilities by disregarding the mentioned base rates. We examined how Bayesian reasoning plays a role in this criticism and discussed responses from realists. We have explored how a realist response can be formulated by pointing out the already selective nature of scientific inquiry, which is biased towards truth and therefore has a historical record of having successful results. Some realists like Psillos, also emphasized the complexity of the concept of base rates, how it is not context-free and case histories are of importance. Our base rates turned out to be dependent on our definition of a successful theory, to begin with, and the suggestive pool that the realist would be content with is much much smaller than what critics of realism had in mind when formulating this BRF accusation. We've concluded that the accusation of a base rate fallacy can be mitigated through careful probabilistic analysis.

In Chapter 3 we tackled the Circularity Charge, which suggests that the NMA is circular, it uses an abductive form of reasoning to prove the truth-conveying status of scientific research which is also based on this type of abductive reasoning, particularly inference to the best explanation. Epistemic circularity would be a serious challenge for scientific realists if it turns out to be occurring on a premise circularity level, where the conclusion is already assumed in one of the premises. But a close look has shown that what occurs is a rule circularity and it is a topic of debate whether it's vicious or benign. Not all circular reasoning is inherently fallacious. Through an analysis of externalist and internalist epistemic frameworks, we explored how realists might avoid the circularity charge by relying on externalist theories of justification. This approach helps defend the NMA from the charge of circular reasoning, though it does not entirely eliminate concerns about the underlying assumptions of realism. Furthermore, we've tackled the side-quest created by Fine, when he prompted the realist with a questioning from the meta-philosophical grounds. Applying the demands of Hilbert about the strengths of meta-theorems to the Scientific Realism debate as

Fine requested, we've found out that his prompt is not well-intended and inherently unanswerable by the very nature of the inquiry as shown by Gödel decades ago, so if such a demand were indeed seriously uttered to undermine the claim of Scientific Realism, it would have cut everything else in the field with equal force, from rules of first-order logic to inductive inference.

In Chapter 4, we have laid out the ongoing debate about Pessimistic Meta Induction (PMI), one of the most known arguments of the critics of realism. The PMI points to the historical record of scientific theory change, where many once-successful theories have been discarded, hence it opposes the implication of NMA from success to truth and actually infers the opposite conclusion that most of the time, present successful theories of science are destined to be false. If past successful theories were later found to be false, why should we believe that current successful theories are -approximately-true?

We reviewed a range of realist responses to this challenge, including the argument that certain core elements of discarded theories —such as mathematical structures—have been retained through theory change, indicating that scientific progress still tracks truth in some meaningful way. Additionally, we explored how the growth of scientific knowledge, changes the graph of progress of science drastically and makes the period where significant theory changes indeed occur just the crawling period of systematic science. Also we have seen methodological improvements lead to more reliable theories today than in the past, a careful reader might object that our predecessors have claimed the same for theories, and we as realists concluded that they were also justified in claiming that. Finally, we've mentioned Mizrahi's statistical analysis by random sampling which turned out to be giving the opposite of the assumption of the anti-realists: rather than discarding or changing the most successful theories of the past we humans have turned out to be keepers and not disposing of our precious items that fast when it comes to the domain of science.

Although this criticism has turned out to be mitigated by aforementioned realist responses and the central axiom- that most successful past theories have been discarded and changed with new ones- turned out to be false, it still raises some distrust against the current scientific theories, that many theories have been changed.

In Chapter 5, we delved into the issues raised by the concept of Approximate Truth, which is often invoked by realists to explain the success of discarded theories. While past theories may have been false in some respects, they were close enough to the truth in key areas to allow for their success. However, defining approximate truth rigorously have been challenging. We explored some attempts to formalize the concept of approximate truth, including Niiniluoto's framework, which seeks to quantify how close a theory is to the truth. Despite these efforts, the notion of approximate truth remains problematic because it often relies on vague or ambiguous criteria. Nevertheless, the concept continues to be a valuable tool for realists, as it allows them to account for the success of past theories while acknowledging their limitations.

In chapter 6 we culminated our discussion in the proposal of Selective Pragmatic Realism. This is a modified version of realism that aims to address the challenges raised in the preceding chapters. Our approach recommends that applying some filters to our realism and narrowing its scope efficiently, on a case-by-case basis. This approach also takes a pragmatic stance when choosing that filter, rather than committing to absolute truth and a necessary implication from success to truth as standard realism was suggesting. SPR focuses on the parts of theories that have proven to be reliable and successful in practice while remaining agnostic or sceptical about other aspects.

SPR approach is motivated by the anti-realist critique that scientific theories are revised or discarded in the past, sometimes despite their success; but it also retains the realist commitment to the idea that certain aspects of successful theories—particularly those that contribute to their predictive and explanatory success—are likely to correspond to reality. By weaving pragmatic considerations into the debate, Selective Pragmatic Realism offers a balanced approach that respects the historical contingency of scientific theories while maintaining that some core elements of these theories which are truth-tracking.

In conclusion, we have critically analyzed the major objections to the NMA and hence realism in the Philosophy of Science. By focusing on the selective endorsement of certain theoretical entities and structures, this thesis proposes a more flexible and context-sensitive realism that can navigate the complexities of scientific theory change without abandoning the realist project altogether. By engaging with challenges such as Rule Circularity, PMI, Base Rate Fallacy and Definition of Verisimilitude we have found out weaknesses and emphasized the strengths of realism simultaneously. Ultimately we have synthesized our findings in a suggestion of Pragmatic Selective Realism, which uses core concepts from both sides like predictive success, survivorship and coherence as heuristics to strategically select where our commitment should lie in a particular theory. In doing so, we aim to move beyond the traditional stalemate in the realism debate and offer a constructive path forward for understanding the success of science. Although we have left out the discussions about the underdetermination of theory by data due to the scope of the thesis and trying to make a closer analysis of the NMA in-depth, our final suggestion of a heuristic approach could be utilized in that facade of the debate too. Ultimately, we have argued that Selective Pragmatic Realism offers a promising way forward, allowing us to preserve the explanatory power of realism while addressing the legitimate concerns raised by anti-realists. This pragmatic approach recognizes the limits of our knowledge but remains committed to the idea that science, in its most successful forms, reveals something about the structure of reality. Future research can focus on refining the heuristics and adding new ones if necessary, finding appropriate weights for specific heuristics and utilizing the final metric in the assessment of theories among a candidate pool of theories that's successful and empirically adequate in the explanation of certain phenomena.

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APPENDICES

A TURKISH SUMMARY

Giriş

Bu tezde, bilim felsefesindeki temel tartışmalardan biri olan : bilimsel teorilerin, empirik ve kestirimsel başarısının ardında bir gerçeklik olup olmadığı konusu ele alınmaktadır. Bilimsel teoriler nesnel ve zihinden bağımsız bir gerçekliği tanımlar mı yoksa sadece duyusal deneyimlerimizi düzenleyen ve anlatı şablonlarımızı organize eden faydalı araçlar mıdır? Bilimsel realizm ve anti-realizm kampları arasındaki bu tartışma, hem bilim insanları hem de filozoflar tarafından 19. yüzyılda ele alınmış ve Kuantum Fiziği alanındaki bazı gelişmeler üzerine 20. yüzyıl başlarında yeniden alevlenmiştir.

Bilimsel realizm akımı, başarılı bilimsel teorilerin en azından yaklaşık olarak doğru olduğunu ve dış dünyaya karşılık geldiğini savunur. Öte yandan, anti-realizm ise teorilerin bir gerçekliği yansıtmasına gerek olmadığını, yalnızca gözlemlerimizi düzenlemek ve tahminlerde bulunmak için kullanıldıklarını vurgular. Özellikle anti-realist kamptaki Enstrümentalismi benimseyen filozoflar bilimin tamamen araçsal olduğunu ve empirik yeterliliğinin, ya da tahminsel gücünün bu gözlemlerin kendisinden öte bir şey anlatmadığını söyler. Bu tartışma, özellikle Thomas Kuhn'un paradigma değişimleri ve bilimsel ilerlemenin kümülatif yapısını sorgulayan çalışmalarıyla 19. yüzyıl sonlarından itibaren yükselmiştir ve 20. yüzyıl başlarında mantıksal pozitivistlerin tüm önermeleri duyusal deneyimin organizasyonu olarak gören felsefesi, anti-realizmin en güçlü formu olarak felsefe tarihi sahnesindeki yerini almıştır.

Modern tartışmada ise önemli bir merkez noktası, Mucize Yok Argümanı'dır (MYA). Bu argüman, eğer bilimsel realizm doğru olmasaydı bilimin kestirimsel başarısının bir mucize olacağını ortaya koyar. MYA, bilimsel teorilerin neden bu kadar başarılı olduğuna dair, yani doğru tahminler yapıp karmaşık teknolojiler geliştirmemize olanak sağladığına dair, bilimsel realizmin sunduğu “bilimsel teoriler tarafından, altta yatan bir gerçekliğe dair açıklama yapılıyor olduğu” önerisinin, en iyi açıklama olduğunu savunur.

Ancak realistlerin bu argümanına; anti-realistler tarafından dairesel akıl yürütme, temel oran yanılgısı ve Kötümser Meta-Tümevarım (KMT) gibi çeşitli itirazlar getirilmiştir. Bu tarz itirazlarla bilimsel realizmin bilimin başarısından, bilimsel teorilerin doğruluğuna dair yaptığı iddianın altı kazınmış ve bu itirazların bazıları literatürde oldukça geniş yer bulmuştur. Yukarıda saydığımız itirazlar dışında bilimsel gerçekçiliğin bu başarıyı açıklamak için gerçekten en iyi ya da tek açıklama olup olmadığı sorusu da MYA’ya getirilen önemli bir anti-realist meydan okumadır. Ancak anti realist kamptan getirilen Van Fraessen’in evrimsel açıklaması ya da Jarrett Leplin’in ‘-mış gibi’ açıklaması gibi diğer felsefi ekoller bu tezin dışında bırakıldı. Özellikle Van Fraessen’in inşacı empirisizmi gibi başlı başına bir kitap konusu olabilecek geniş akımları burada hakkını vererek irdeleyip realist açıklama ile kıyaslamak mümkün olmayacağından, bunların yerine MYA’ya getirilen diğer eleştirileri derinlemesine irdelemek tercih edildi.

Tezin temel amacı, MYA’ya olan bu ana itirazları ele almak, itirazların realizmin iddiasına dair çeşitli haklı noktaları işaret ettiği yerlerde realizm anlayışımızda değişiklik ve sınırlamalara giderek, bu tartışmayı yapıcı bir şekilde sonuçlandırmaktır. Bu analizler sonucunda seçici pragmatik realizm, pek çok noktada tıkanmış olan bu tartışmayı aşmak için bir çözüm önerisi olarak sunuldu. Seçici pragmatik realizm, realizm ve anti-realizm arasında bir orta yol sunar ve anti-realist itirazları yapıcı eleştirilere dönüştürerek temel realist iddiamızdan vazgeçmeden ama realizm anlayışımızda gerekli değişiklikleri de yaparak yola devam etmeyi önerir. Epistemik tartışmanın teraziyi, iki taraftan birinin lehine çeviremediği noktada; pragmatik kaygılar ve kognitif açıklamalar gibi yeni cephelerden konuya yaklaşarak gerçekçiliğimizi stratejik bir şekilde yönlendirir. Bu strateji; kestirimsel başarı, hayatta kalma başarısı ve ilgili alanlardaki diğer bilgilerle uyumluluk gibi bazı kriterleri teorilerin gerçekliğe yakınsama oranına dair bir metrik tanımlamakta kullanmaktır. Selektif yaklaşım ise bahsedilen yeni metriğe göre teorileri birbiri ile kıyaslamak ve bir teorinin içerisindeki öğeleri de bu metriği oluştururkenki kriterleri sağlamada ne kadar önemli olduklarına göre pragmatik bir şekilde ele almaktır. Yapısal gerçekçilik veya obje gerçekçiliği gibi kısıtlı gerçekçilik önerilerine benzer olan ancak onlardan farklı olarak yapıya ya da nesnelere genel bir adanma önermeyen bu lokal yaklaşım, bilimsel realizme getirilen geleneksel itirazları çözerken realizmin temel görüşlerini de korur.

Temel Oran Yanılgısı İtirazı

Mucize Yoktur Argümanı'na (MYA) yöneltilen en büyük itirazlardan biri, bu argümanın, olasılıksal düşünme sırasında yaptığımız hatalardan biri olan temel oran yanılgısına düştüğüne dair suçlamadır. Colin Howson gibi filozoflar tarafından öne sürülen bu itiraz; MYA'nın kurulumunda, teorilerin doğru olma olasılığının en baştan çok düşük olduğunun göz ardı edilmesine odaklanır. Olasılık terimleriyle açıklanacak olursa, temel oran yanılgısı, bir olayın gerçekleşmesi ihtimalinin yüksek olduğuna karar verilirken; o olayın ele alındığı genel populasyon içinde ne kadar yaygın ya da nadir olduğunu hesaba katmadan çıkarım yapılması anlamına gelir. Bu ihmal çeşitli hastalık senaryolarındaki yanlış pozitif bulunma ihtimallerine uygulandığında, hastalığın tüm nüfustaki yaygınlığı göz önüne alınmazsa, söz konusu test sonucu 95% ihtimalle doğruymuş gibi görülürken, genl nüfustaki yaygınlık dikkate alındığında aynı pozitif test sonucu 0.02 ihtimalle gerçekten hastalığın varlığına işaret etmektedir. Aradaki bu bariz fark, Bayes'in olasılık formülündeki baz oran dediğimiz bir kısmın, ilk hesapta tamamen devre dışı bırakılmış olmasından kaynaklanır.

Howson ve diğer eleştirmenler, bilimsel teorilerin çoğu zaman başarılı olmasının onların doğru olduğu anlamına gelmediğini savunur. Bir teorinin başarısı rastlantısal olabilir ya da başka nedenlere bağlı olabilir ve MYA, başarılı teorilerin gerçekte ne kadar nadir olduğunu göz ardı ederek temel oran yanılgısına düşmektedir. Örneğin, doğru tahminlerde bulunabilen çok sayıda yanlış teori olabilir, bu nedenle başarı tek başına doğruluğu garanti etmez.

Howson, MYA'yı Bayesçi olasılık teorisi ile yeniden formüle eder. Bayesçi terimlerle, MYA şu şekilde ifade edilebilir:

- P(Başarı|Doğruluk) yüksektir, yani bir teori doğruysa, başarılı olma olasılığı yüksektir.
- P(Başarı|Yanlışlık) düşüktür, yani bir teori yanlışsa, başarılı olma olasılığı düşüktür.

Bundan sonra MYA, P(Doğruluk|Başarı)'nın yüksek olduğu sonucuna varır; yani bir teori başarılıysa, doğru olma olasılığı yüksektir. Ancak Howson, bu formülasyonun doğru teorilerin tüm teoriler içindekitemel oranını, yani P(Doğruluk)'u göz ardı ettiğini savunur. Howson'a ve benzer eleştirilerde bulunmuş anti-realistlere göre eğer doğru teorilerin tüm bilimsel teoriler arasındaki temel oranının düşük olabileceğini hesabınıza katmazsanız, MYA'nın vardığı sonuç geçersiz olacaktır. Bayesçi terimlerle ifade edecek olursak, önsel olasılık olan P(Doğruluk) dikkate alınmadan, P(Doğruluk|Başarı)'ya varılan sonuç hatalı olur.

Bu itiraza tezde , bilimsel yöntemin teori seçimindeki rolünü vurgulayarak yanıt verildi. Tıpkı bir doktorun tahliller yaptığı hastaları tüm nüfus içinden rastgele seçmeyip, önden şikayetlerine göre elediği ve bu yüzden pozitif sonuç veren tıbbi testlerin gerçekten hastalığa işaret ediyor olma oranının oldukça yüksek olduğu gibi; bilimsel teoriler de rastgele üretilmez, ortaya atılmış hipotezlerin test edilmesi ile ampirik doğrulama ve iyileştirme süreçlerinden geçerler. Bu süreçler, en başarılı ve sağlam teorilerin hayatta kalmasını sağlayan bir filtre işlevi görür. Bu nedenle, Bayesçi bir olasılık formülünde baz oran olarak Howson'un savunduğu kadar düşük bir doğru teori oranı zaten yoktur, bu oran oluşturulurken tarihin herhangi bir döneminde dile getirilmiş tüm bilimsel teorileri genel populasyon olarak dikkate almaya da gerek yoktur. Çünkü bilimsel yöntem zaten zaman içinde yanlış teorileri eleyerek doğru teorilerin oranını artırır, realistler de başarılı teorilerden gerçekliğe dair bir kestirim yaparken, MYA içerisinde zaten bilimin metodunun işe yaradığına dair bir iddia savunmaktadır. Realistlerin 'başarılı' sıfatını tanımlamak için kullandığı kriterler, ele alınan teori havuzunu oldukça daraltmaktadır.

Bu yanılgı iddiasına dair tezde sunulan başka bir savuşturma da MYA'nın küresel ve yerel versiyonları arasında bir ayrım yapmaktır. Küresel MYA, bilimin genel başarısının (birçok teori ve alan arasında) bilimsel realizmi destekleyen güçlü bir kanıt sunduğunu ileri sürer. Yerel bir MYA ise tek bir başarılı teoriyi ele alıp -mesela T1 olsun- buna odaklanır ve T1'in başarısının doğruya işaret ettiğini savunur. Küresel MYA'nın yapısının olasılıksal bir kurulum değil bir modus tollens olduğunu ve bu modus tollensin içerisinde de sadece en iyi açıklamaya atıf yapan abduktif bir çıkarım olduğunu görürsek, olasılıksal çıkarımlarda işleyen temel oran yanılgısı itirazının bu durumda küresel MYA'ya işlemediğini görürüz. Henderson'un ileri sürdüğü bu lokallik/küresellik ayrımına göre herhangi bir T teoremine dair yerel anlamda okunabilecek MYA'lar, küresel MYA'nın bilimin filtreleme metoduna dair olan bu iddiası ile baz oran yanılgısından zaten korunmaktadır. Yani global MYA bilimsel yöntem gerçekliği tespit etmekte başarılı oluyor iddiasını ortaya koyarak, spesifik teoremlerle ilgili yerel MYA'lar için bir kalkan görevi görmektedir. Bilimin genel başarısı, bilimsel yöntemin güvenilir bir şekilde doğru veya yaklaşık doğru teoriler ürettiğini göstermektedir, bu da yerel MYA'yı bireysel teoriler için destekler.

Sonuç olarak, tezin bu kısmında temel oran yanılgısı itirazının bilimsel teorilerin seçici doğasını göz ardı etmekte olduğu, bu sebeple kritikler tarafından iddia edilen düşük baz oranların, ancak anti-realistlerin pesimizmini kabul ettiğimizde ortaya çıkabilecek gerçeklikten uzak başlangıç oranları olduğu ortaya konmuştur. Bilimsel yöntem, başarılı teorilerin doğru olma olasılığını artırır, bu nedenle doğru teorilerin temel oranı Howson'un varsaydığından çok daha yüksektir. Bu durumda, MYA, bilimsel realizmi destekleyen geçerli bir argüman olmaya devam eder.

Döngüsellik İtirazı

Mucize Yok Argümanı'na (MYA) getirilen diğer önemli bir itiraz, döngüsellik içerdiği suçlamasıdır. Bu itiraz, Larry Laudan ve Arthur Fine gibi filozoflar tarafından ortaya atılmıştır. Döngüsellik itirazı, MYA'nın kanıtlamaya çalıştığı şeyi -yani bilimsel realizmin doğru olduğunu- önceden varsaydığını öne sürmektedir. Bu tür bir dairesel akıl yürütme, eleştirmenlere göre MYA'nın geçerliliğini zayıflatmaktadır.

Mucize Yok Argümanı, abduktif akıl yürütme ya da diğer adıyla en iyi açıklamaya çıkarım olarak olarak bilinen yöntemle ortaya konmuştur. MYA bilimin başarısının, bilimsel realizm doğru değilse bir mucize olacağını söyler. Mucizeler olmadığına göre, bilimin başarısını açıklamamız gerekir, bilimsel realizm bu başarıyı en iyi açıklayan duruş olacağından MYA burada abduktif çıkarım yaparak bilimsel realizmin benimsenmesi gerektiğini ileri sürer. Ancak Laudan gibi eleştirmenler, abduktif akıl yürütmeyi kullanarak realizmi savunmanın sorunlu olduğunu, çünkü abduksiyonun zaten bilimsel realizmin güvenilirliğini varsaydığını öne sürer. Bu da onlara göre, realizmin doğruluğunun abduktif bir süreçle meşrulaştırılmaya çalışılmasıyla ortaya çıkan bir dairesellik oluşturur, yani MYA'da döngüsellik safsatası vardır.

Arthur Fine bu eleştiriyi, Hilbert'in matematikteki programını kullanarak genişletir. Hilbert, matematiğin temellerine dair; herhangi bir matematiksel teoremin doğruluğunun söz konusu edildiği meta-teoremlerin kanıtının; üzerine konuştukları teoremlerden daha güçlü şekilde yapılması gerektiği ve başka teoremlerle kanıtlanması gerektiğini savunur. Fine, bilimsel realizmin MYA'dan savunulması için; Hilbert'in bahsettiği bu kriteri sağlamayan bir durum olduğu tespitini yapmıştır. Tıpkı küme teorisini kanıtlamak için küme teoretik aksiyomlar kullanılınca olacağı gibi, Fine abduksiyon metodu ile bilimsel realizm savunusunda da benzer bir sorunun var olduğunu Hilbert'in bu meta-teoretik taleplerine dayanarak öne sürer.

Tezde bu konuya Fine'ın önerdiği gibi mantıksal temellendirme zemininden bakmak için Hilbert'in bu meta-mantıksal talebi biraz daha yakından incelenmiştir. Hilbert yukarıda bahsi geçen meta-teoremsel talepleriyle aslında bütün önermelerin, başka önermelerle temellendirilebileceğini varsaymaktadır. Onun programı tüm matematiğin temeli olabilecek sonlu, tutarlı ve eksiksiz bir formel sistem geliştirmeyi amaçlamaktaydı. Ancak daha sonra Gödel'in eksiklik teoremleri, aritmetiği ifade edebilecek kadar güçlü herhangi bir sistemin, sistem içinde kanıtlanamayan ama doğru olan ifadeler içereceğini göstermiştir, yani herhangi bir sistemin Hilbert'in istediği gibi tam ve aynı zamanda tutarlı olamayacağı Gödel tarafından ortaya koyulmuştur. Buradan yola çıkarak yeterince güçlü her epistemik sistemde, ki birinci derece mantıktan daha fazlası olan bütün epistemik sistemler bu 'yeterince güçlü' tanımının kapsamına girer, sistemin içerisinden kanıtlanamayan veya temellendirilemeyen bazı önermelerin illa ki olacağını görebiliriz. Bu durumda insanlığın yapmakta olduğu matematik, felsefe gibi akıl-yürütme içeren uğraşılar tam olarak mekanize edilemez ve sadece deduksiyondan fazlasını içermek zorundadır. Bu durumda Kant'ın 'öngörü' olarak adlandırdığı başka unsurlar bu uğraşılarda mecburen etkili olmaktadır. Yani içerisindeki tüm akıl yürütme kurallarını, döngüselliğe düşmeden kanıtlayabildiğimiz ve hiçbir şeyin aksiyom olmadığı bir sistem mümkün değildir. Bunu zaten felsefe tarihinde tümevarımsal çıkarım gibi başka akıl yürütme kurallarının sorunsallaştırılmasında da görürüz ve Carnap gibi filozoflar bir tür kural döngüselliğinin hem kaçınılmaz hem de zararsız olduğunu bu vaka üzerinden göstermişti .

Öte yandan tezde kural döngüselliği itirazına yanıt olarak, dışsalcı epistemolojiyi kullanarak da MYA'nın savunulabileceği gösterilmiştir. Dışsalcı epistemoloji, inançların içsel gerekçelendirilmesine değil, inançların oluşturulma sürecinin güvenilirliğine odaklanır. Dışsalcı bakış açısından, bilimin başarısı, bilimsel teorilerin doğru ya da yaklaşık doğru olduğuna inanmak için güvenilir bir temel sağlar. Bu yaklaşım, abduktif akıl yürütmenin kullanıldığı bu argümanın, belirli bir döngüsellik barındırmasına rağmen, bu daireselliğin sorun yaratmadığını öne sürer. Kuralların, kendi kendilerinin geçerli olduğunu gösteren başka argümanlarda bir çıkarım metodu olarak kullanılması, amplifikasyon türünde yapılmış zararsız bir döngüselliktir ve ön-sayımların sonucu direkt kabul ettiği safsatalı bir düşünce tarzı olan öncül döngüselliğinden bu noktada ayrışırlar.

Kural daireselliği ile zararlı dairesellik arasındaki ayrımı şu şekilde yapabiliriz: Kural daireselliği, bir kuralın kendisini doğrulamak için kullanıldığı durumu ifade eder; ancak bu, kuralın güvenilir bir sürecin parçası olması durumunda sorunlu değildir. Bilimsel realizmde, bilimsel yöntemin güvenilirliği abduktif akıl yürütmenin kullanılmasını meşrulaştırır ve bu döngüsel akıl yürütmeyi zararlı bir dairesellik olmaktan çıkarır. Bilimin zaman içindeki başarısı, yöntemin güvenilir olduğunu ve belirli durumlarda kullanılan akıl yürütme süreçlerinin başarıyla sonuçlandığını gösterir, bu da yöntemin güvenilirliğine dair bize gereken zemini sağlar.

Sonuç olarak döngüsellik itirazı MYA'nın doğasını yanlış anlamış ve başka kurallar için çoktan kabul görmüş olan ve alanın işleyişi için mecburi bir taviz olan kural döngüselliğine imkan tanımayı, abduktif çıkarım(veya en iyi açıklamaya atıf) için sorunlaştırarak, ikilik teşkil eden bir meta-felsefi yaklaşım gütmüştür. MYA'da abduktif akıl yürütme kullanılıyor olsa da, bu, zararlı bir döngüsellik teşkil etmez çünkü argüman, bilimin genel başarısı tarafından desteklenmektedir. Tezin bu kısmında bilimsel yöntemin güvenilirliğine ve Fine'ın meta-teoretik eleştirisinin çıkış noktasının zaten karşılanması imkansız bir talep olmasına odaklanarak, MYA dairesellik suçlamasına karşı savunulmuştur.

Kötümser Meta- Tümevarım (KMT) İtirazı

Kötümser Meta-Tümevarım (KMT) belki de bilimsel realizme yönelik en meşhur itirazlardan biridir. Larry Laudan tarafından popüler hale getirilen KMT, bilim tarihinin başarılı bilimsel teorilerin nihayetinde yanlış olduğu konusunda bol miktarda kanıt sunduğunu iddia eder. Geçmişte başarılı olan ve doğru kabul edilen pek çok teorinin—kalorik ısı teorisi, flogiston teorisi ve Ptolemy'nin astronomik modeli gibi—günümüzde yanlış olduğu düşünülmektedir. KMT, bu geçmiş örneklerden yola çıkarak, günümüzün başarılı teorilerinin de gelecekte yanlışlanacağını ve MYA'nın iddia ettiğinin tam aksinin gerçekleşeceğini öne sürer.

KMT, bilimsel realizmin temel varsayımına, yani bir teorinin başarısının onun doğruluğuna işaret ettiği fikrine meydan okur. Buna göre, geçmişte başarılı olan teoriler yanlışlanmışsa, günümüz başarılı teorilerinin de gelecekte yanlışlanacağı varsayılmalıdır. Bu itiraz, Mucize Yok Argümanı'nın temelini sarsar çünkü bilimsel başarının gerçekliğe işaret ettiği veya teorilerin doğruluğunu gösterdiği fikri MYA'nın ana fikriyken, KMT buna karşıt örnekler sunmakla kalmayıp, bunlar üzerinden bir meta-tümevarım yaparak realizmin savının tam aksini vurgular.

Tezde bu itiraza birkaç yanıt sunuldu. İlk olarak, bilimsel yöntemin zaman içinde önemli ölçüde geliştiği vurgulandı. Geçmişteki teoriler, genellikle daha sınırlı boyutta verilere dayanarak ve daha az titiz olan yöntemlerle geliştiriliyordu. Bugün ise bilimsel teoriler çok daha büyük veri setlerine dayanmakta ve çok daha sıkı bir şekilde test edilmektedir. Bu durum, günümüzdeki başarılı teorilerin, geçmişteki teorilere kıyasla, gelecekte yanlışlanma olasılığını azaltır. Dikkatli okuyucular, geçmişteki insanların da kendi günlerindeki bilimsel teoriler ve yöntemler için aynısını söyleyebileceğini fark edip, buna itiraz edebilir. Oysa onlar da bunu demekte gayet haklıydılar ve gerçekten onların metodolojileri de daha önceki yöntemlere göre daha iyiydi. Burada bilimi, sürekli yeni metotlarla kendi kendisini iyileştiren ve geribildirimlerle yanlış teoremleri elemekteki yetkinliğini artıran bir oto-besleyici geribildirim-loopuna benzetebiliriz.

İkinci bir yanıt olarak, pek çok geçmiş teorinin tamamen yanlış olmadığı savunuldu. Yanlış bulunmaktan ziyade, belirli bağlamlarda teorilerin bazı kısımlarının korunduğu ve bu çekirdek üzerine, daha geniş kapsamlı fenomenleri açıklamak üzere yeni teoriler inşa edildiği görüldü. Örneğin, Laudan gibi anti-realistlerin büyük teori değişimleri listesinde sıklıkla anılan Newton mekaniği hala düşük hızlar ve zayıf yer çekimi alanları gibi gündelik durumlarda son derece doğrudur ve pek çok mühendislik ve uygulamalı bilimde bu mekanik anlayışının ilkeleri kullanılmaktadır. Einstein'ın görelilik teorisi Newton mekaniğinin yerini almış olsa da, Newton mekaniği birçok pratik bağlamda yararlı ve yaklaşık olarak doğru kabul edilmektedir. Bu nedenle, bilimsel ilerleme, teorilerin tamamen ortadan kaldırılmasından ziyade, zaman içinde daha doğru hale getirilmesi süreci olarak görülebilir. Hatta bu yöntem fizikte denk-düşme ilkesi olarak bilinir ve yeni teoremler inşa etmenin bir metodu olarak da kullanılmaktadır.

Ayrıca bilimsel ilerlemenin devrimsel olmaktan ziyade kümülatif olduğu vurgulanıldı. Kuhn'un öne sürdüğü gibi bilimsel alanlarda paradigma değişimleri olabilir; ancak bu değişimler eski teorilerin tamamen reddedilmesini gerektirmez. Bunun yerine eski teoriler genellikle yeni teorilerin içine özel durumlar ya da yaklaşık doğruluklar olarak entegre edilir. Örneğin kuantum mekaniği, klasik mekaniği tamamen ortadan kaldırmamış; onun büyük ölçeklerde geçerli bir özel durumu olarak kabul etmiştir. Yani Kuhn'u baz alan anti-realist eleştirmenlerin çizdiği kesikli ve sıçramalar ya da düşüşlerle dolu bir bilim tarihi grafiği yerine aslında olan exponensiyel ve devamlı bir artış gösteren, yer yer de ufak tümsekler yaparak artarak artan bir fonksiyondur. Teori değişimleri olsa da bu tümsekler sanıldığı gibi ani kesinti ve sıçrayışlar değildir, bir tür devamlılık ve kestirimsel başarı için asli önemi olan çekirdek yapıların korunması söz konusudur. Bunlara ek olarak, Moti Mizrahi yaptığı rastgele seçilmiş bir örneklem üzerinden, kötümser meta indüksiyonun çıkış noktası olan geçmiş teorilerin çoğunun değiştiği önkabulünün yanlış olduğu göstermiştir. Yani istatistiksel bir meta-tümevarım olan "geçmişte çoğu... öyle ise günümüzde çoğu..." şeklindeki düşüncedeki "çoğu" tespiti asılsızdır. "Pek çok" teorinin anti-realistlerin yaptığı gibi değişim örnekleri olarak listelenmesi, "çoğunluğunun" bu şekilde değişime uğradığına kanıt değildir, bu iki kelime eş anlamlı gibi kullanılamaz.

Son olarak bir de, bilim tarihindeki devrimsel sayılabilecek teori değişimleri üzerinden kötümser bir çıkarım yapmaya kalkışmadan evvel bilimsel bilginin üstel büyümesinin önemini vurgulamak gerekir. Son yüzyılda, teknoloji ve deneysel yöntemlerdeki gelişmeler sayesinde bilim insanlarının elindeki ampirik veri miktarı dramatik bir şekilde artmıştır. Bu bilgi artışı, günümüz teorilerinin geçmiş teorilere göre daha doğru olma olasılığını artırmaktadır. Koroborasyon prosesinden pek çok kez geçmiş ve sürekli pek çok farklı grup tarafından sınanmakta olan günümüz teorileri, örneğin 300 yıl önce modern bilimin emekleme dönemlerinde ortaya çıkan akranlarına kıyasla aslında oldukça 'olgun' kabul edilebilir. Günümüz teorileri sayısı exponensiyel olarak artmış binlerce sınamada hayatta kalmış veya bunların sonucuna göre revize edilmiş, oturmuş bir çekirdeğe sahip teorilerdir, aynısı 18. yüzyıl içerisinde ortaya atılıp benzer yaşta olan bir teorem için o günlerde söz konusu değildi.

Yukarıda sayılan sebeplerle, günümüzde bazı teorilerin belirli yönlerinin revize edilmesi ya da değiştirilmesi mümkün olsa da, eskisi kadar büyük sıçramaları, bilimsel çalışmaların 90%'ınının yapılmış olduğu son 50 yıl içerisinde gözlemlemek imkansızdır. Yani bilim tarihini ele alırken zaman eksenimizi bu exponensiyel büyümeyi karşılayacak şekilde logaritmik olarak orantılandırmak gerekir. Bu yapıldığında büyük ve kaotik değişikliklerin olduğu dönemin bilimin 10%unun üretildiği emekleme periyoduna denk geldiği görülür. Bilimsel ilerlemenin genel eğrisi günümüzde oldukça stabilleşmiştir.

Sonuç olarak tezde Kötümser Meta-İndüksiyon'un, bilimsel yöntemler ve bilgi birikimindeki niteliksel gelişmeleri hesaba katmadığı gösterildi. Geçmişte başarılı olan bazı teoriler yanlışlanmış olabilir; ancak bu, çoğunun yanlışlandığını göstermediği gibi öyle olsaydı bile günümüz teorilerinin çoğunun da aynı kaderi paylaşacağını da kanıtlamazdı, bu tarz kıyasların ciddi ve anlamlı olabilmesi için benzer objeler üzerinden kurulmuş olması gerekir. Günümüzdeki bilimsel teoriler geçmişteki yanlışlanmış teorilere, bu tarz bir çıkarsamanın yapılabileceği kadar benzerlik taşımamaktadır. Eğer bunların aynı grup altında kümelenmelerinin ve bir meta çıkarıma tabi tutulmalarının doğru olduğu savunulacaksa, hem bu benzerliği göstermek böylece üzerine tümevarım yapılan teoriler örnekleminin yeterince rastgele olduğunu kanıtlamak, hem de değiştiği iddia edilen teorilerin 'çoğunluğu' oluşturduğu yönündeki varsayımlarını temellendirmek, yine anti-realistlerin üzerinde bir kanıtlama yükümlülüğü olarak durmaktadır. Aksi takdirde realistlerin iddia ettiği gibi günümüz teorilerinin daha büyük olasılıkla yaklaşık olarak doğru olduğu ve bilimsel ilerlemenin kümülatif bir süreç olduğu savunulmaya devam edilebilir.

Yaklaşık Doğruluk Tartışması

Bilimsel realizm tartışmasındaki en önemli kavramlardan biri, yaklaşık doğruluktur. Realistler, bilimsel teorilerin her zaman tam anlamıyla doğru olmasalar bile en azından yaklaşık doğru olduklarını ve gerçeğe yaklaştığını savunurlar. Bu kavram, realistlerin, bilimin giderek doğruya yaklaştığını ve zihinden bağımsız bir gerçekliği betimliyor olduğunu savunmalarına olanak sağlar, şu anki teorilerimiz mükemmel olmasa bile. Ancak, yaklaşık doğruluk kavramı anti-realistler tarafından eleştirilmiştir. Örneğin Laudan gibi filozoflar tarafından bilimsel başarı ile yaklaşık doğruluk arasında realistlerin savunduğu gibi bir gerektirmenin olmadığı, çünkü her şeyden önce bu kavramın açık ve kesin bir tanımının bulunmadığı dile getirilmiştir. Mesela bir örnek senaryo oluşturmak gerekirse, standart modelden bir obje eksilterek, diyelim ki elektronu çıkartarak 16/17 objenin yerinde kaldığı yaklaşık bir tasvir elde edebiliriz. Ama dünyadaki medeniyet bir kıyamet-sonrası senaryosundaki gibi yok olmuş olsa, gezegenimizde elektronun silindiği böylesi bir standart model tasviri bulan uzaylı fizikçiler, bu resme dayanarak tutarlı ve başarılı bir teoremi yeniden inşa edemezdi. Yani yaklaşık olarak doğru bir tasvir onlara başarıyı garanti edemezdi. Böylesi bir kurguya ise bir realistin itirazı, zaten bu resmin en başından "yaklaşık doğru" kabul edemeyeceği olurdu. Bu alanda çalışan herhangi bir bilim insanı da bize 17 objeden birini eksiltmekle, modelin doğruluk oranı arasında böylesi lineer bir bağ olmadığını söylerdi. Peki öyleyse yaklaşık doğruluk tam olarak nedir? Bir teorinin yaklaşık doğru olup olmadığını nasıl belirleyebiliriz? Ve bir teorinin "gerçeğe yakınsıyor" olması ne anlama gelir?

Yaklaşık doğruluk kavramının geçerliliğine dair bu sorular, bazı filozofların yaklaşık doğruluk fikrinin bilimsel realizmi savunmak için anlamlı bir zemin sunup sunamayacağını sorgulamalarına neden olmuştur. Tezde bu kavramı tanımlama girişimlerine kısa bir değini yaparak bu anti-realist kaygılar yanıtlanmaya çalışıldı. Yaklaşık doğruluğu tanımlamaya yönelik çeşitli girişimlere bakılınca, bu konuda Ilkka Niiniluoto'nun yaklaşık doğruyu tanımlamaya en çok yaklaştığı görülür. Niiniluoto, bir teorinin verisimilitude veya gerçeğe yakınlık derecesinin nasıl ölçülebileceği konusunda formel bir çerçeve geliştirmiş ve hem teoremlerin yapısındaki önermelerin her birinin geçerli olduğu dünyanın, aktüel dünyadan ne kadar uzak olduğuna bakarak, hem de bu önermelerin teorem içerisindeki önemine göre ağırlıklandırma yapmıştır. Niiniluoto'ya göre, bir teorinin gerçeğe yakınlığı, dünyanın yapısını ne kadar iyi yakaladığı ve gözlemlenebilir olayları ne kadar doğru tahmin ettiği ile ilgilidir ve bu tarz bir tanım Popper'ın orijinal tanımında gördüğümüz paradoksal sonuçlardan da kaçınmamızı sağlar.

Yaklaşık doğruluğun tanımlanmasındaki zorluklara rağmen, bu kavram bilimsel süreçleri anlamlandırmada önemli bir araçtır. Teorileri tamamen doğru ya da tamamen yanlış olarak değerlendirmek yerine, yaklaşık doğruluk sayesinde teorilerin ne derece doğru oldukları birbirlerine kıyasla ölçülebilir ve bir fenomeni açıklamada yarışan alternatif teoriler olması durumundaki eksik-belirlenim problemi de bu şekilde aşılabilir. Bu yaklaşım, bilimsel teorilerin daha nüanslı bir şekilde değerlendirilmesine olanak tanır ve bilimsel bilginin karmaşıklığını kabul eder. Popper'in tanımında önermelerden beklenen, ikili mantığa dayanan kesin doğruluk veya tamamen yanlışlık atıfını ve bunun etrafında kurulan yanlış dikotomiyi de reddeder. Dolayısıyla bilimsel sürecin kendisini olasılıksal bir mantıkla ele alarak MYA'da iddia edilen başarı ile doğruluk arasındaki ilişkiyi de kıyassal olarak kurmuş olur. İki teoremi Niinuluoto'nun yaklaşık doğruluk metriği üzerinden kıyasladığımızda elde ettiğimiz sonuç ile başarıları üzerinden kıyasladığımızda alacağımız sonuç benzer olur.

Tezde Niinuluoto'nun tanımına dayanılarak yaklaşık doğruluğun bir ikilik değil, bir süreklilik olarak anlaşılması gerektiğini savunuldu. Teoriler, daha az ya da daha çok doğru olabileceği ve bilimsel ilerlemenin, teorilerin bu süreklilikte gerçeğe daha fazla yaklaşmasıyla tanımlanabileceği tartışıldı. Örneğin, Newton mekaniği tam olarak doğru olmayabilir; ancak birçok bağlamda hala oldukça doğrudur ve bu bağlamlarda yaklaşık doğru olarak kabul edilebilir.

Bu daha esnek doğruluk anlayışı benimsenerek, bilimsel realizmin bilimsel çalışmaların nasıl işlediğini neden daha iyi açıkladığı daha net görülebilir. Bilimsel teoriler genellikle tam, kesin veya nihai değildir; ancak yine de oldukça başarılı olabilirler ve belirli alanlarda yaklaşık doğru olabilirler. Bu görüş, realizmin bilimsel teorilerin değişimini kabul etmesine olanak tanır ve yine de bilimin doğruya yaklaştığını savunur. Yaklaşık doğruluk(verisimilitude) kavramı da kavramın neliğine dair keskin bir içsel tanım vermek yerine; eldeki önermelerin veya teorilerin bu kavramı ne kadar karşıladığına dair kıyası sağlayan dışsal ölçütlerle oluşturulmuş bir metrikle ele alınması ile yetinilebilecek bir kavramdır. Tezde bu tanıma dayanarak, kavramın nihai hedef olmaktan çıkarıp araçsallaştırılarak MYA'daki iddianın hala bu tarz bir araç ile kontrol edildiğinde geçerli olduğu gösterildi.

Seçici Pragmatik Realism)

Tezin son büyük bölümünde, seçici pragmatik realizm; realizm ve anti-realizm tartışmasına bir çözüm olarak sunuldu. Bu yaklaşım, hem realizmin hem de anti-realizmin unsurlarını bir araya getiren ve her ikisinin de güçlü yönlerini kabul ederken zayıflıklarından kaçınan bir görüştür.

Seçici pragmatik realizm, bilimsel teorilerin tüm yönlerinin doğru olmasına gerek olmadığını savunur. Bunun yerine, teorilerin özellikle doğru tahminlerde bulunma ve teknolojik ilerlemeleri sağlama konusundaki pratik başarılarına odaklanarak; bunların bir önceki kısımda tanımlanan şekilde, sürekli bir spektrumda çeşitli değerler alabilecek bir gerçeklik oranına işaret ettiğini kabul eder. Bilimsel teorilerin faydasına vurgu yaparak epistemik tartışmanın bizi çıkmaza sürüklediği veya başlangıçta kabul ettiğimiz tutumu, kendi kendisini gerçekleştiren bir kehanet gibi doğruladığı yerlerde pragmatik açıdan yaklaşarak realizmi savunur.

Seçici pragmatik realizmin en önemli avantajlarından biri, geleneksel bilimsel realizme yönelik başlıca itirazlardan bazılarını bertaraf etmesidir. Örneğin, tarihteki teori değişimlerine dayanan pesimistik meta indüksiyon itirazı, seçici pragmatik realizm altında daha az önemlidir. Çünkü amaç, tarihteki tüm başarılı bilimsel teorilerin tüm yönleriyle doğru olduğunu kanıtlamak değildir. Bunun yerine, başarılı bulunan her bir teori, temel bileşenlerinin pratikte ne kadar iyi işlediğine göre değerlendirilmelidir. Benzer şekilde, döngüsellik itirazı da zayıflar çünkü seçici pragmatik realizm, bilimsel metodun doğasındaki abduktif çıkarım yönteminin bütün epistemik kaygılardan uzak ve saf olarak kanıtlanabilir bir üst-kural olmasını beklemez, bunun yerine bu metodun da kullanıldığı bilimsel akıl yürütmenin pratikteki başarısına odaklanarak, "bir şeyleri doğru yapıyor olmamız gerektiği" demek ki bu çıkarım tarzının işlevsel olduğu sonucuna varır.

Seçici pragmatik realizm, bilimsel teorilerin neden başarılı olduğunu açıklayabilecek bir yaklaşımdır, hatta bu teoriler daha sonra revize edilse ya da değiştirilse bile. Örneğin, flogiston yanma teorisi sonunda terk edilmiş olsa da, yanmanın madde transferi içerdiği fikri gibi bazı unsurlar daha sonraki oksidasyon teorilerine dahil edilmiştir. Bu örnek, bilimsel teorilerin tamamen yanlış olmaktan ziyade belirli yönlerde doğru veya bazı unsurlarının yaklaşık doğru olabileceğini gösterir. Bu unsurları takip ettiğimizde, devamlı olarak bir gerçekliğe yakınsayan ve giderek altta yatan gerçekliğin dokusunu daha yakından tanımlamaya yaklaşan bir bilim tarihi resmi görürüz.

Seçici pragmatik realizm ayrıca, bilimsel teorileri değerlendirirken tahmin ve teknolojik uygulamaların önemini vurgular. Bu yaklaşım, özellikle mühendislik ve tıp gibi alanlarda, teorilerin sonuçları kontrol etme ve tahmin etme amaçlı etkili modeller geliştirilmesinin daha önemli olduğu durumlarda, bilimsel teorilerin başarısını anlamak için oldukça faydalıdır.

Burada çözüm olarak ortaya sunulan Selektif Pragmatik Realizm(SPR)'in instrumental bir yaklaşımdan veya pragmatizmden farkı ise kestirimsel veya uygulamadaki başarının altta yatan bir gerçekliğe işaret ettiğini hala kabul ediyor ve savunuyor olmasıdır. Bu realist kabulün benimsenmesinin gerekçesi de, önceki kısımlarda realizmin temel argümanına dair tek tek ele alınan pek çok anti-realist itirazın:

-Ya aslında geçerli olmadığının (temel oran yanılgısı, pesimistik meta indüksiyon)
- Ya en başından anti-realist olarak başlayan bir varsayıma dayanarak bunun üzerine olasılıksal kestirimler yapılarak ortaya atıldığının, yani bu durumda kanıt yükümlülüğünün anti-realist kampta kaldığının (akıtan vazo modeli, pesimistik meta-indüksiyon, subjektif Bayesyan yaklaşım)
-Ya da temelinde karşılanması güç beklentilerden yola çıktığının, meta-felsefi olarak bu tarz beklentilerin zaten karşılanamayacağının (kural döngüselliği, yaklaşık gerçekliğin içsel bir tanımının verilmesi); gösterilmiş olmasıdır.

Sonuç olarak bu tezde; realist yaklaşımın epistemik, metafizik ve semantik boyutlarının hepsine sadık kalırken, hangi teorilere veya teorilerin içerisinde hangi unsurlara bu realist adanmanın yöneltilebileceğine dair şüpheci ve titiz bir tutum gütmenin realizm tartışmasına makul bir çözüm olduğu gösterildi. Seçici pragmatik realizmin, realizm ve anti-realizm tartışmasına daha nüanslı ve pratik bir yaklaşım sunduğunu savunuldu. Bilimsel teorilerin faydasına ve pratikteki başarılarına odaklanarak;ve gerçekliğe yakınsama kavramını bir süreklilik ve ölçülebilir bir kıyas unsuru halinde algılayarak, seçici pragmatik realizm, bilimsel teorilerin fenomenleri açıklamadaki ve tahmin etmedeki rolünü anlamak için daha esnek ve bilimin doğası ile daha uyumlu bir yaklaşım sunuyor. Bu yaklaşım, bilimsel bilginin karmaşıklığını kabul ediyor ve realizm tartışmalarında sıklıkla görülen ya hep ya hiç anlayışından kaçınıyor.

Sonuç

Sonuç olarak, bu çalışmada seçici pragmatik realizm, uzun süredir devam eden realizm ve anti-realizm tartışmasına bir çözüm olarak sunuldu. Bilimsel teorilerin pratik başarılarına odaklanarak, seçici pragmatik realizmin; realizmin güçlü açıklayıcı yönünü hem de anti-realist eleştirilerin epistemik mütevaziliğini bir araya getirdiği vurgulandı.

Pragmatik gerçekçi yaklaşım, bilimin sürekli revize edilen ve geliştirilen teorilerle ilerlediği gerçeğini standart gerçekçiliğe göre daha iyi yakalıyor. SPR çeşitli heuristikler ile bu ilerlemeyi takip edip, teorilerin içerisindeki gerçekliği bulmaya yakınsayan çekirdek öğelere realist bir yaklaşımı, geri kalanlara ise agnostik yaklaşmayı ya da yargıda bulunmayı ötelemeyi savunuyor. Seçici pragmatik realizm, bilimsel ilerlemenin dinamik doğasını kabul eder. Bu bakış açısı, bilimsel metodun işleyişini; teorilerin bazı kısımlarının korunurken diğer kısımlarının değiştirilerek gerçeğin dokusuna gittikçe daha yakınsamak olarak yorumlar. Bilimsel araştırmayı, gözlemlerimizin altında yatan yapının çeşitli enstantanelerinin yakalanmaya çalışıldığı bir süreç olarak görür. Burada realiteyi bir ayna gibi yansıtmak değil, bir kamera objektifinden izlemek söz konusudur, yani lensin yapısına göre gerçekte olan enstantane farklı şekillerde yakalanabilir.

Selektif gerçekçi tutumda, elimizdeki araçların ve biyolojik koşullarımızın izin verdiği ölçüde gerçek ile etkileşebildiğimizi ve türümüze özgü duyusal ve kognitif sınırlamalar dahil pek çok sınırlamayla belirlenmiş bir perspektiften bakmaya mahkum olduğumuzu da kabul ederek, realitenin ancak bir temsilini bilebileceğimizi baştan kabul etmek söz konusudur. Bu sayede, standart realizmin, fazla genelleyici ve epistemik tuzaklara düşmeye müsait optimizmine karşın, pragmatik bir yaklaşımla, yine altta yatan bir gerçekliği takip ettiğimiz iddiasından da vazgeçmeden, bilimin nasıl çalıştığını ve kestirimsel başarının altında yatan süreci daha iyi anlamlandırmış oluruz. Gelecek araştırmalarda ise SPR'da bahsi geçen heuristiklerin tam bir listesini oluşturmaya ve listedeki uyumluluk, hayatta kalma oranı, kestirimsel başarı vb. maddelerin bir teoremin gerçeğe yakınsaklığını belirlemedeki ağırlığının ne olduğunu belirlemeye odaklanılıp burada ana hatları sunulan görüş şekillendirilerek daha keskin bir tablo sunulabilir.

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