CHAPTER 3: ANTI-REALISM AND THE PROBLEM OF THEORY CHOICE

3.1 Introduction

Explanatory and predictive powers are always the important features of a successful theory. They are vital in promoting the development of science. Scientific realists relate explanatory and predictive power of a theory to the truth about the corresponding reality. They hold that only a true theory which is corresponding to the reality can explain its predictive success. Though anti-realist may advise realists to embrace the notion of approximate truth for the explanation of the predictive success, a serious realist may reject such a move, because the replacement of definitive truth with approximate truth may dampen the claim that there is a definite correspondence between theory and reality. Realists have the reason to worry that the account of approximate truth would introduce attenuated degree of certainty in the correspondence relationship between theory and reality. It is undeniable that scientific realists' criteria of theory choice have a strong ontological commitment on the reality. The choice of a theory among its rivals, according to scientific realist position, requires distinguishable data. This position is vulnerable in replying to the challenges of underdetermination thesis (See Chapter 2).

Anti-realists deviate from scientific realists in epistemology and metaphysics. Although it is a common practice to equate anti-realists with instrumentalists (Rosenberg 2005), we should categorize pragmatists as anti-realists too. It is because pragmatists do not commit to correspondence truth in their epistemology. Nonetheless, it does not mean that pragmatists do not believe in true theory and true existence of observables and unobservables at all. They will have this commitment provided such belief is pragmatic. The highest principle of pragmatists is usefulness. The most favorable theory is not a true theory, but a pragmatic one. Hence, it is reasonable to categorize pragmatists as anti-realists.

In this chapter, the challenges of theory choice that posed to anti-realism will be discussed. The solution of anti-realism to the problem will be elaborated. The exposition is divided into instrumentalist and pragmatist position. It is necessary to make this separate discussion because both positions, though they are of anti-realism, have different set of problems and solutions to the theory choice thesis.

Van Fraassen's constructive empiricism, which has an important place in the realism/anti-realism debate, is discussed in the next section. Next, the anti-realist position of Nancy Cartwright, Hilary Putnam, and Larry Laudan will be explicated in turn.

3.2 An Overview of Constructive Empiricism

Constructive empiricism was proposed by van Fraassen as a tenet which rejects metaphysics. Van Fraassen rejects metaphysics for two reasons. First, he argues that metaphysical solutions to philosophical problems are unsuccessful (van Fraassen 2003). Under the influence of ordinary language philosophy, Van Fraassen construes metaphysics as 'word play'. He argues that there is no way to check for a metaphysical claim. This vulnerability of metaphysics always introduces stalemate in the debates because the opposite metaphysical claim has the equal explanatory power (van Fraassen 2003). Van Fraassen holds that there is no way to break the stalemate because metaphysics requests for interpretation, not for factual information (van Fraassen 2003). Apparently, the nature of metaphysics does not help in theory choice since it does not produce factual information about the nature. Moreover, van Fraassen goes further to hold that empiricists cannot even launch a radical critique of metaphysics about the nature (van Fraassen 2004). It is because such critique will be inevitably taking a metaphysical position (van Fraassen 2004), which should be rejected in the first place. Hence, metaphysical questions should be suspended by constructive empiricists.

The second reason of van Fraassen's rejection to metaphysics is that metaphysical presuppositions of nature are unnecessary in science. Van Fraassen holds that the acceptance of a scientific theory does not require the unobservables to exist, neither require the belief in unobservables. His constructive empiricism does not require real existence of theoretical entities and true claims of a theory (which is the stance of the correspondence theory of truth) as necessary presuppositions for the consideration in theory choice. The rejections of metaphysics and correspondence theory of truth form the pillar of van Fraassen's anti-realist position.

From the perspective of epistemology, van Fraassen's constructive empiricism is a variant of instrumentalist position that views theories from the perspective of pragmatism (Losee 2001, 257). The explanatory power should only be construed as a pragmatic virtue for theories. Van Fraassen asserts that theories are devices that facilitate the organization about observation.

Van Fraassen holds that realism is unnecessary in science (Giere 1985, 85). Science is not an enterprise that seeks for true theories, but for empirically adequate theories. Van Fraassen has distinguished between theory acceptance and belief. He holds that the criterion of accepting a theory does not require the belief in its truth (van Fraassen 1980).

> … an antirealism is a position according to which the aims of science can well be served without giving such a literally true story, and acceptance of a theory may properly involve something less (or other) than belief that it is true.
>
> (van Fraassen 1984, 250)

The distinction between theory acceptance and belief implies the abandonment of scientific realist approach in theory choice. However, van Fraassen shares realists' belief that the most successful theory should be the chosen theory among its rivals. They disagree about the definition of "successful theory". Scientific realists argue that a successful theory should be corresponding to the reality. On the contrary, van Fraassen argues that a successful theory is measured in terms of empirical adequacy. Van Fraassen holds that a theory is empirically adequate if it rightly attributes observable properties to the observable things (Psillos 1999, 196).

According to the constructive empiricist position, empirical adequacy serves the purpose of science (Losee 2001, 257). Van Fraassen is obliged to justify theory choice in the light of empirical adequacy. Although van Fraassen devotes lesser attention to the justification of theory choice (Giere 1985, 75), he does distinguish between the empiricist and realist approach to the acceptance of a theory (Giere 1985, 85). Accepting a theory does not involve realist existential belief of the observables and unobservables. However, theory acceptance does involve rational belief that the theory is empirically adequate (van Fraassen 1985, 247-252 and Worrall 2002, 33). The degree of belief involved in theory acceptance is lesser than that of realist position and tends to be more pragmatic in its nature (van Fraassen 1980, 13).

Van Fraassen claims that theory acceptance involves rational commitments. However, it is not a realist commitment which is pertaining to true or false theory. According to van Fraassen, theory acceptance implies a confidence that the chosen theory will be vindicated in terms of empirical adequacy (van Fraassen 1980, 12-13).

3.2.1 The Ontological Commitment of Constructive Empiricism

In van Fraassen's account, observability is an adequate criterion for the ontological commitment of constructive empiricism. He has distinguished between observables and unobservables. This distinction is necessary as van Fraassen insists that it is the observables which fulfill the criterion of empirical adequacy. Unobservables, which are posited by scientific realists, are to be excluded in constructive empiricism. Hence, the ontological commitment of constructive empiricism is confined to observables only.

Van Fraassen holds that observables are phenomena or appearances. The ontological status of observables is important in the sense that they are isomorphic to the abstract entities such as models (van Fraassen 2006, 539).

> For empirical adequacy uses unquestioningly the idea that concrete observable entities (the appearances or phenomena) can be isomorphic to abstract ones (sub-structures of models).
>
> (van Fraassen 2006, 539)

By equating observables to models isomorphically, van Fraassen needs to explain how so. Observables are concrete whereas models are abstract. They are two different kinds of entities.

> Indeed, how can we answer the question of how a theory or model relates to phenomena by pointing to a relation between theoretical and data models, both of them abstract entities? The answer has to be that the data model represents the phenomena…..
>
> (van Fraassen 2006, 544)

Van Fraassen asserts that observables and models are isomorphic in the sense that the latter represents the former. The representation theory of van Fraassen requires one to differentiate between "observing" and "observing that" in the first place.

"Observing" is a physiological behavior that see something as it is. It does not involve an interpretation of the observed phenomena. On the contrary, "observing that" is a behavior that involves interpretation of the perceived phenomena. In van Fraassen's example, a Stone Age person recently found in modern age was shown a tennis ball. He "observes" a tennis ball, but he does not "observe that" it is a tennis ball because he has not learned anything about tennis ball (van Fraassen 1980, 15). Hence, "observing that" is more than mere "observing" the facts.

To claim that observables and models are isomorphic, van Fraassen has to recourse to "observing that", which is the interpretation of the observables. Van Fraassen does not mean that the observables are isomorphic with models ontologically, as they are observed as they are. On the contrary, they are isomorphic in terms of the description of empirical contents made by models. Hence, the isomorphic relation between observables and models is not of ontology, but of epistemology. However, the ontology of observables and models serve as the pre-requisite for the existence of isomorphic relation.

However, the ontological commitment of observables is claimed problematic by Friedman. Friedman holds that van Fraassen's supposition of observables is inevitably assuming the existence of unobservables, which is an assumption that has been rejected by van Fraassen in the first place (Friedman 1982; cited in Muller 2003). The contradiction in van Fraassen's ontology will render constructive empiricism untenable.

> The observable objects are themselves characterised from within the world picture of modern physics: as those complicated systems of elementary particles of the right size and configuration for reflecting light in the visible spectrum, for example. Hence, if I assert that observable objects exist, I have also asserted that certain complicated systems of elementary particles exist. But I have thereby asserted that (individual) elementary particles exist as well! I have not, in accordance with Van Fraassen's constructive empiricism, remained agnostic about unobservable part of the world.
>
> (Friedman 1982, 278; cited in Muller 2003, 2)

Friedman's objection to van Fraassen's ontological distinction between observable and unobservable is based upon the holism of scientific systems. According to the holistic approach, the unobservables and observables make up the reality. This approach is of realist position on the nature of reality. This explains why holistic approach, which is held by Friedman, assumes the existence of unobservables that lay the foundation for the observable objects. However, there is no reason for one (such as Friedman) who is subscribing to a holistic view about unobservables and observables, to deny that unobservables and observables are ontologically distinguishable. To illustrate, it is conceivable for one to claim that a physical entity, such as a chair, to be ontologically distinct from its constituent subatomic particles. For a chair is not the same thing as the subatomic particles that constitute it (an atom may decay, or lose/gain its electrons, during the life span of a chair). Hence, Friedman's objection is untenable. Van Fraassen may refute the accusation of Friedman by repudiating the holistic picture of reality which comprises of unobservables. Friedman's argument is not detrimental to the ontological commitment of constructive empiricism.

Another important feature van Fraassen has attributed to observables is that they are theory-independent (van Fraassen 1980, 57). He asserts that one should not attribute conceptual framework or theory as the constituent of observable entities (van Fraassen 1980, 58). Theory is merely a tool that describes an observable entity. Furthermore, theory "does not obliterate the distinction between what is observable and what is not" (van Fraassen 1980, 58) because it is "an empirical distinction." (van Fraassen 1980, 58).

Van Fraassen's distinction between observable and unobservable is based on the human biological limitation in the presence of observational instruments. Van Fraassen's principle of observability is:

> X is observable if there are circumstances which are such that, if X is present to us under those circumstances, then we observe it.
>
> (Van Fraassen 1980, 16)

Van Fraassen has given two examples to illustrate his principle of observability. He claims that the moons of Jupiter are observables because they can be observed using a telescope (van Fraassen 1980, 16). However, observation of micro-particles in a cloud chamber is not a case of the particle's being observed (van Fraassen 1980, 17), because what is observed is a (observable) trail rather than the (unobserved) micro-particle itself. The unobserved micro-particles can only be inferred from the observation reports of the trail, which may vary among different observers.

> Suppose I point to such a trail [path of the ions] and say: 'Look, there is a jet!'; might you not say: 'I see the vapour trail, but where is the jet?'
>
> (Van Fraassen 1980, 17)

The observation of micro-particles in a cloud chamber yields multiple views of the observed phenomenon. One cannot conclude what exactly the perceived phenomenon *is*. If there is no agreement on the perceived phenomenon, one cannot conclude that he "observes that" thing (particle, etc). Observable does not allow vagueness in the observation report. According to van Fraassen's distinction between "observing" and "observing that", scientists whom do not have an ontological consensus on the entity under their observation may still come to a consensus on the interpretation of that entity. In other words, scientist *A* may observe the vapor trail while scientist *B* observes a jet in a cloud chamber. Both scientists may come to an agreement to claim that they have observed that entity *as* particle. Such agreement on the interpretation is based on a variation in the observation. According to the constructive empiricism, both scientists have not observed particles, but have observed that (phenomena) *as* particles. The particle, which is not observed but observed *as* such entity, according to van Fraassen, does not assume the real existence of such thing called ‘particle’ in space. It is because the particles are mere models that depict the observed phenomena.

Van Fraassen treats the observed phenomenon as what it has been observed. In the example of cloud chamber, different observed phenomena (observation of vapor trail or jet) need not have a unified common unobservable entity to account for such differences in observation. Van Fraassen is an agnostic in unobservable entities because they are not observable. In fact, constructive empiricists do not have to assume a unified framework of reality (as their scientific realist counterparts do) because they do not assume the strict one-to-one correspondence between ontological entity and theory. The move to exclude the realist correspondence theory is fully reflected in van Fraassen’s principle of “to save the phenomena” that excludes the role of unobservables in accounting for the reality.

3.2.2 Empirical Adequacy

Being agnostic about unobservable entities, van Fraassen holds that a theory saves the phenomena when it is empirically adequate, that is, when “what it says about the observable things and events in this world, is true.” (van Fraassen 1980, 12). Truth and empirical adequacy coincide for theories about the observable (Musgrave 1985, 198), but this coincidence should be interpreted in a pragmatist way. For van Fraassen, saying that an empirically adequate theory truly describes observable entities is amounting to saying that what is perceived is entailed by the theory. An empirically adequate theory does not describe the thing-in-itself, but the phenomenon.

> A little more precisely: such a theory has at least one model that all the actual phenomena fit inside. I must emphasize that this refers to *all* the phenomena; these are not exhausted by those actually observed, nor even by those observed at some time, whether past, present, or future.
>
> (van Fraassen 1980, 12)

The difference between scientific realism and constructive empiricism in terms of truth is that the former asserts that the observable entities are underlain by unobservable entities, while the latter remains agnostic about the unobservables. When the empirically equivalent rival theories emerge, scientific realists find themselves trapped in the dilemma of theory choice. Constructive empiricists, on the contrary, have lesser pressure to answer to the same problem. They may assert that empirically equivalent data are observable phenomena, which could serve as unambiguous evidences (unlike scientific realist assumption of unobservables) for theory evaluation. Empirically equivalent rival theories are empirically adequate. A scientist may adopt a pragmatic approach in theory choice since the rival theories are all empirically adequate. The choice of any rival theories which are empirically adequate and empirically equivalent can be justified, as all of them are "true" description of phenomena.

> While the only belief involved in acceptance [of a theory], as I see it, is the belief that the theory is empirically adequate…. To accept a theory is to make a commitment…. Commitments are not true or false; they are vindicated or not vindicated in the course of human history.
>
> (van Fraassen 1980, 88)

> Briefly, then, the answer is that the other virtues claimed for a theory are pragmatic virtues. In so far as they go beyond consistency, empirical adequacy,

> and empirical strength, they do not concern the relation between the theory and the world, but rather the use and usefulness of the theory; they provide reasons to prefer the theory independently of questions of truth.
>
> (van Fraassen 1980, 88)

Van Fraassen has developed a pragmatic account of explanation to reject realists' one. Realists believe that the explanation of a theory requires true premises (van Fraassen 1980, 97). They hold that the theory which has higher explanatory power should be chosen among the rivals because of its truer depiction of reality. The mistake of realists, pointed out by van Fraassen, is that they insist that an explaining theory must be acceptable (true) before it can explain legitimately (van Fraassen 1980, 97-99). Van Fraassen criticizes this view by stating that truth should not be presupposed in explanation. A false theory could have high explanatory power initially but proved to be false in the later stage (van Fraassen 1980, 98). For example, van Fraassen states that Newton's theory which was widely accepted as a true theory in explaining celestial systems fails to explain the perihelion of Mercury (van Fraassen 1980, 98).

> The important point is that the mere statement 'theory *T* explains fact *E*' does not carry any such implication: not that the theory is true, not that it is empirically adequate, and not that it is acceptable.
>
> (van Fraassen 1980, 98)

What van Fraassen attempts to elaborate is that explanation is neutral. "So scientific explanation is not (pure) science but an application of science." (van Fraassen 1980, 156). An explaining theory is not always a true theory that has ruled out all alternative theories (van Fraassen 1980, 128-130) prior to explaining. Explanation is pragmatic because it is context-dependent.

> Being an explanation is essentially relative, for an explanation is an answer… Since an explanation is an answer, it is evaluated vis-à-vis a question, which is a request for information. But exactly what is requested, by means of the question 'Why is it the case that *P*?', differs from context to context.
>
> (van Fraassen 1980, 156)

Van Fraassen holds that a theory with more explanatory power is also more informative (van Fraassen 1985, 294). However, the relevance of information needs not be objectively determined. Theory with more explanatory power always has pragmatically determined content (van Fraassen 1985, 294) that adequately describes the phenomena. A successful explanation is a success of adequate and informative description of the phenomena (van Fraassen 1980, 157). The explanatory power of theories is the criterion of theory choice in the sense that it is inline with the aim of science—"to give us theories which are empirically adequate" (van Fraassen 1980, 12)

The problem of theory choice requires constructive empiricists to justify that the selected theory is empirically adequate. However, the problem of theory choice remains unsettled when all of the rival theories are empirically adequate. Van Fraassen does not hold that good fit with empirical data is the only criterion for theory choice. However, it seems that empirical adequacy is the minimal criterion for a theory to be considered for acceptance. Indeed, van Fraassen adopts a pragmatic approach towards theory acceptance (van Fraassen 1980, 88).

> When a theory is advocated, it is praised for many features other than empirical adequacy and strength: it is said to be mathematically elegant, simple, of great scope, complete in certain respects: *also* of wonderful use in unifying our account of hitherto disparate phenomena, and most of all, explanatory.
>
> (van Fraassen 1980, 87)

From the above quoted passage, it is apparent that an accepted theory should possess an array of virtues on top of being empirically adequate. Van Fraassen asserts that these virtues are “human concerns, a function of our interests and pleasures, which make some theories more valuable or appealing to us than others.” (van Fraassen 1980, 87). It should be noted that van Fraassen does not view these virtues as compulsory criteria for theory acceptance. If two rival theories are both empirically adequate, theory *A* has some virtue *x* but theory *B* has some virtue *y,* both theories are acceptable. Even if both rival theories do not have any additional virtues on top of empirical adequacy, they are still acceptable. Hence, it is clear that van Fraassen’s account of theory acceptance is not amounting to theory choice in the sense of singling out *one* theory among rivals.

> Values of this sort [simplicity, of great scope, completeness, explanatory power and etc], however, provide reasons for using a theory, or contemplating it, whether or not we think it true, and cannot rationally guide our epistemic attitudes and decisions.
>
> (van Fraassen 1980, 87)

It is important to distinguish theory acceptance from theory choice, and further to distinguish “acceptable choice” from “available choice”. The best choice among several rival theories does not always imply that it is an acceptable choice. The best choice of theory among unsatisfactory rivals could be an unacceptable theory. Hence, the acceptable choice is depending on the available choice. To choose a theory, one may adopt a pragmatic approach if there are no satisfactory candidate theories to be chosen. However, to accept a theory (i.e., an acceptable theory), one needs a more rigorous criterion (than choosing a theory).

Van Fraassen does not see the difference between theory choice and theory acceptance. When he holds that other non-epistemic virtues always accompany an accepted theory, he has a loose definition of theory acceptance. He does not set a rigorous criterion for a theory to qualify the acceptance. His pragmatic approach in determining which theory to be accepted is rather context-dependent, which has a tendency to slide to relativism. For he holds that the virtues of theory provide reasons for using a theory, but "cannot rationally guide our epistemic attitudes and decisions" (van Fraassen 1980, 87).

The context-dependent pragmatic approach in theory acceptance is maintained when van Fraassen discusses on the rationality of theory preference:

> … if it matters more to us to have one sort of question answered rather than another, that is no reason to think that a theory which answers more of the first sort of questions is more likely to be true… It is merely a reason to prefer that theory in another respect.
>
> (van Fraassen 1980, 87)

In 1980s, van Fraassen does not make the meaning of "context-dependent" clear. It is a concept that was always linked to pragmatism. Van Fraassen rejects the idea that pragmatics can only be interpreted as a generalization of semantics (van Fraassen 1980, 89). He holds that the pragmatic dimension in accepting a theory is the commitment in that theory (van Fraassen 1980, 88).

> To accept a theory is to make a commitment, a commitment to the further confrontation of new phenomena within the framework of that theory, a commitment to a research programme, and a wager that all relevant phenomena can be accounted for without giving up that theory…. Commitments are not true or false; they are vindicated or not vindicated in the course of human history.
>
> (van Fraassen 1980, 88)

The commitment in accepting a theory is the pragmatic dimension which is not truth-borne. Van Fraassen takes it further to claim that the virtues of theory are pragmatic virtues (van Fraassen 1980, 88). He asserts that pragmatic virtues are going beyond consistency, empirical adequacy, empirical strength, and world-theory relation (van Fraassen 1980, 88). Van Fraassen concludes that pragmatic dimension or contextual factor of theory acceptance concerns only the use and usefulness of the theory (van Fraassen 1980, 88). In van Fraassen's account, all of the candidate rival theories must fulfill the minimum requirement of being empirically adequate in the first place. The decision of accepting a set of theories while rejecting others is context-dependent. If one wishes to limit the acceptable theory to only one, he could use his pragmatic approach to simply accept one among the rivals in the current context. It seems not irrational for van Fraassen as which theory to be accepted is not a big deal within a specific context because all rival theories are pragmatically and epistemically equivalent.

Van Fraassen attempts to elaborate his meaning of "context-dependent of theory acceptance" in terms of the linguistic context between speaker and audience. Van Fraassen holds that such linguistic context forms a tacit agreement between speaker and audience in evaluating a theory.

> The pragmatics of language is also the place where we must locate such concepts as immersion in the language, or world-picture, of science. The basic factors in the linguistic situation, pragmatically conceived, are the speaker or user, the syntactic entity (sentence or set of sentences) uttered or displayed, the audience, and the factual circumstances.
>
> (van Fraassen 1980, 91)

It is worth noting that van Fraassen has been associating linguistic context to the notion of world-picture. He does not specify how linguistic context resembles the world-picture. Furthermore, he does not explain the meaning of world-picture. It seems that a macroscopic world-picture is the linguistic framework of phenomena because van Fraassen does not recognize the effect of an underlying reality in theory construction. However, even if the linguistic context is governed by world-picture, van Fraassen is still obliged to explain how is the tacit agreement between speaker and audience (in the linguistic context) so governed. He is obliged to account for the mechanism of the phenomenal world-picture, which fulfills the requirement of empirical adequacy, that supports the inter-subjectivity of linguistic context. However, van Fraassen provides no clues towards the issue.

Notwithstanding the efforts to uphold pragmatism in theory acceptance, van Fraassen insists that empirical adequacy is the minimal requirement of theory acceptance. Once this requirement is fulfilled, other pragmatic virtues (simplicity, explanatory power, and etc) would be the additional merits that favor a theory among its rival. If there are rival theory *A* and *B*, pragmatic virtues should be taken into consideration in theory choice if both theories are empirically adequate. However, van Fraassen provides no clues for (1) judging the merits of pragmatic virtues and; (2) which pragmatic virtue has more weight in theory acceptance. It seems unpromising in providing an objective rank-order for the rival theories. Hence, van Fraassen's constructive empiricism can hardly account for rational theory acceptance along the dimension of pragmatism.

3.2.3 Explanation as Model

Pragmatism is held by van Fraassen to against the role of semantics in theory acceptance. However, the early van Fraassen's pragmatism is vaguely defined when it is applied in theory acceptance, as elaborated in Section 3.2.2. However, van Fraassen's use of pragmatism in constructing a new model of explanation is more well-defined than in explaining the criteria of theory acceptance. Van Fraassen's pragmatist account of explanation is closely related to his view on model.

In his new theory of explanation, van Fraassen asserts that explanation is not a proposition or an argument, but an answer (van Fraassen 1980, 134). To provide an answer to a why-question, one needs pragmatics. Pragmatics is context-dependent in providing an answer as an explanation (van Fraassen 1980, 134). Van Fraassen defines context-dependence as sentence-dependence. He characterizes the context of explanation with modality of world. He holds that the context of an explanation may entail a set of possible worlds (van Fraassen 1980, 135).

> What must the context specify? The answer depends on the sentence being analysed. If that sentence is
>
> > Twenty years ago it was still possible to prevent the threatened population explosion in that country, but now it is too late
>
> the model will contain a number of factors. First, there is a set of possible worlds, and a set of contexts, with a specification for each context of the world of which it is a part. Then there will be for each world a set of entities that exist in that world, and also various relations of relative possibility among these worlds….. When we evaluate the above sentence we do so relative to a context and a world.
>
> (van Fraassen 1980, 135-136)

Van Fraassen interprets the explanations as models. He holds that a model should fit the phenomenon (van Fraassen 1980, 137), without extending the explanatory power of model to the deep structure of the reality. The explanatory relation between models and phenomena depends on two factors, which are (1) the truth-value of the propositions in the models and; (2) the context of propositions. The factor (1) is indexical whereas the factor (2) is of pragmatics. The example given by van Fraassen for factor (1) is the sentence "I am here". He holds that it "is a sentence which is true no matter what context of usage we consider." (van Fraassen 1980, 136). The example for factor (2) given by van Fraassen is "van Fraassen is in Vancouver". He claims that this sentence is not necessary true because it is context-dependent (van Fraassen 1980, 136) where the contextual variables are "van Fraassen" and "Vancouver". Van Fraassen further holds that these contextual variables are not featured by truth value but other 'pragmatic presuppositions' such as:

> …. the assumptions taken for granted, theories accepted, world-pictures or paradigms adhered to, in that context.
>
> (van Fraassen 1980, 137)

The factor (2), which is the pragmatic presuppositions, is grounded in factor (1), the indexical factor. Since van Fraassen interprets explanations as models (which are the answers to account for phenomena), models are characterized by an indexical factor and a pragmatic factor. To a certain extent, van Fraassen implicitly agrees that the model and phenomenon is isomorphic, as he holds that the concrete phenomena *can be* isomorphic to abstract models (van Fraassen 2006, 539). For a model to be accepted among its rival, pragmatic factor plays the crucial role while the effect of a priori factor can be ignored. It is because the criterion to determine how satisfactory a model fits a phenomenon is lying in the pragmatic presuppositions. In other words, the explanatory power of a model springs from the pragmatic presuppositions, which are a wide range of variables that depends on context.

The criterion for accepting a model is beyond empirical adequacy. Explanatory power is the crucial criterion in determining which model is to be accepted (van Fraassen 1980, 154). However, van Fraassen contends that the explanatory power is irreducible to empirical data (van Fraassen 1980, 154-155). There are two factors to consider when evaluating a model. Firstly, empirical adequacy is the minimal requirement for a model to be accepted. Secondly, explanatory power determines the winning model among the rivals. The irreducibility of explanatory power to empirical data is not contradicting with the minimal requirement of empirical adequacy. For the explanatory power is determined by contextual variables which are not mere empirical data but grounded in the empirical adequacy that characterized by isomorphic relation between models and phenomena from the a priori perspective. Both empirical adequacy and explanatory power constitute the criteria of model acceptance. However, explanatory power has the final say if all of the rival models fulfill the requirement of empirical adequacy.

It is worth noting that van Fraassen does not say that explanatory power *is* the truth of the theory. In fact, as an antirealist van Fraassen would not grant that explanatory power is indispensable in theory acceptance. Hence, van Fraassen's account of theory acceptance does not confer the truth status to the accepted theory, with regard to the deep structure of reality. Van Fraassen's account of theory acceptance is based on the good will in believing that the capability of explanation justifies the acceptance of a theory. For van Fraassen, a theory is accepted because there are pragmatic reasons to believe that it is true. This pragmatic belief does not contradict with his principle of "acceptance is not belief", for what van Fraassen objects is the belief in the correspondence truth of the deep structure of reality. Van Fraassen rejects the idea that a true theory must correspond to the reality. The correspondence is implausible because the explanatory power is irreducible to empirical data (van Fraassen 1980, 154-155).

> The discussion of explanation went wrong at the very beginning when explanation was conceived of as a relationship like description: a relation between theory and fact. Really it is a three-term relation, between theory, fact, and context.
>
> (van Fraassen 1980, 156)

Context-dependent explanatory power allows van Fraassen's account of theory acceptance being escaped from the attack of underdetermination thesis. However, the pragmatic contextual variables of explanatory power render van Fraassen's account of theory acceptance to the risk of relativism. Van Fraassen admits that the contextual variables comprise of the assumptions that are taken for granted, world-pictures and paradigms (van Fraassen 1980, 137), which could be subjectively or inter-subjectively formed.

3.3 Cartwright's Antirealist Position of Laws and Theories

Nancy Cartwright cannot be categorized as a total realist or antirealist. Indeed, she holds an antirealist position about scientific laws and theories, whereas a realist position about theoretical entities. In viewing herself as an antirealist of theories, she distinguishes herself from two types of antirealists.

The first kind of antirealists is agnostic about the existence of unobservable entities (Cartwright 1983, 56). Such agnostic attitude is based on the argument that there is no evidential ground to support the theoretical claims about the unobservable entities. Cartwright identifies van Fraassen as the modern representative of this kind of antirealist. Cartwright is different from van Fraassen in the sense that she does not question about the existence of theoretical entities. The recognition of the existence of theoretical entities makes Cartwright an entity realist. However, Cartwright's denial of the ability of the fundamental theories in providing true descriptions of reality makes herself an antirealist of scientific theory. She even goes further to claim that fundamental theories do not describe the facts.

> The fundamental laws of physics, by contrast, do not tell what the objects in their domain. If we try to think of them in this way, they are simply false, not only false but deemed false by the very theory that maintains them.
>
> (Cartwright 1983, 56)

> If the right kinds of descriptions are given to the phenomena under study, the theory will tell us what mathematical description to use… But the 'right kind of description' for assigning an equation is seldom, if ever, a 'true description' of the phenomenon studied.
>
> (Cartwright 1983, 132-133)

The second kind of antirealists, asserted Cartwright, rejects the factual representation of laws about reality (Cartwright 1983, 56). They maintain that nothing in theory represents facts about reality except "the basic equations of modern physics" do (Cartwright 1983, 56). Putnam is identified by Cartwright as the representative of this kind of antirealist. Cartwright rejects this kind of antirealist position too. On the contrary, she holds that all sorts of statements in theory are able to represent facts of nature except the fundamental explanatory laws (Cartwright 1983, 56). Cartwright holds that it is the nature of explanation that contributes to the failure of factual representation of the fundamental laws about reality (Cartwright 1983, 58).

> I said that the fundamental laws of physics do not represent the facts, whereas biological laws and principles of engineering do. This statement is both too strong and too weak. Some laws of physics do represent facts, and some laws of biology—particularly the explanatory laws—do not. The failure of facticity does not have so much to do with the nature of physics, but rather with the nature of explanation.
>
> (Cartwright 1983, 58)

The failure of fundamental explanatory laws in explaining the reality has its cause partly in the nature of reality and partly in the nature of fundamental laws. Cartwright states that the reality is complex but "not fundamental" (Cartwright 1983, 58), whereas the fundamental laws are of universal and simple. To explain complex reality, fundamental laws are combined in the explanation. Cartwright calls this kind of combined fundamental laws as 'explanation by composition of causes' (Cartwright 1983, 58). The salient feature of combined fundamental laws is that it is assumed that the explanatory power of each component laws "act in combination just as they would act separately" (Cartwright 1983, 59). It is false, according to Cartwright, because the effects of the complex reality cannot be reduced to any one of the laws separately (Cartwright 1983, 59). Using Psillos's term, the explanatory power of combined laws does not "cover" the facts to be explained (Psillos 2008, 177).

Cartwright goes further to repudiate the explanatory power of a single fundamental law about the reality. She claims that a standalone fundamental law (such as the law of universal gravitation) may be true but not useful in terms of explaining the reality. It is because a standalone fundamental law is too general. It "can explain in only very simple, or ideal, circumstances" (Cartwright 1983, 58). It cannot explain (or in Psillos's term, 'cover') more complex phenomena in the reality.

> [The law of universal gravitation] can account for why the force is as it is when just gravity is at work; but it is of no help for cases in which both gravity and electricity matter. Once the *ceteris paribus* modifier has been attached, the law of gravity is irrelevant to the more complex and interesting situations.
>
> (Cartwright 1983, 58)

The idea of capacity or cause of objects contributes to the antirealist stance of Cartwright on the explanatory power of laws. Capacities are the abilities that cause an object to behave in certain ways. Cartwright holds that a standalone fundamental law is always corresponding to a single cause. Attempts to use a standalone fundamental law to explain multiple causes (in the case of complex phenomena) would inevitably result in the failure of explanation.

> If we state the fundamental laws as laws about what happens when only a single cause is at work, then we can suppose the law to provide a true description. The problem arises when we try to take that law and use it to explain the very different things which happen when several causes are at work.
>
> (Cartwright 1983, 72)

Cartwright distinguishes between phenomenological and theoretical laws. She favors the former and rejects the latter. The phenomenological laws are context-specific whereas the theoretical laws are abstract and universal (Cartwright 1983, 8). The phenomenological laws take the form of causal explanation about the reality, where the capacities or causes of the objects are explained within the specific context. Theoretical laws, on the contrary, are unable to explain the reality in two situations:

(i) A standalone theoretical law is too abstract to explain a simple phenomenon where there is only a single cause at work. The law is not context-specific to explain for the concrete cause underlying the phenomenon. In this sense, theoretical law is not useful.

(ii) A combination of theoretical laws fails to explain a complex phenomenon where there are multiple causes at work. In this situation, theoretical laws are not true.

In Cartwright's account, the difference between phenomenological laws and theoretical laws is the degree of abstractness, or applicability. The more abstract a law is, the more universal and less applicable it is in the specific scientific context. Cartwright defines abstractness of theories relative to the concreteness of theory application in a particular context.

> First, a concept that is abstract relative to another more concrete set of descriptions never applies unless one of the more concrete descriptions also applies. These are the descriptions that can be used to "fit out" the abstract description on any given occasion. Second, satisfying the associated concrete description that applies on a particular occasion is what satisfying the abstract description consists in on that occasion.
>
> (Cartwright 1999, 39; cited in Frigg 2006, 55)

Cartwright's antirealist position can be summarized in this way: neither a standalone nor combined fundamental/theoretical laws can explain (or cover) the complex reality. She rejects the idea that theoretical laws go hand-in-hand (Cartwright 1983, 8). She holds that theoretical entities and theoretical properties, which are the postulates about the complex phenomena, are not explained by theoretical laws but by phenomenological laws. Cartwright claims, consistent with her entity realism, that we have good reasons to believe in the truth of theoretical entities if we have a successful causal explanation given by phenomenological laws.

> Although I claim that a successful causal explanation gives good reason to believe in the theoretical entities and theoretical properties it postulates, I have repeatedly said that I do not believe in theoretical laws…. the propositions to which we commit ourselves when we accept a causal explanation are highly detailed causal principles and concrete phenomenological laws, specific to the situation at hand, not the abstract equations of a fundamental theory.
>
> (Cartwright 1983, 8)

For Cartwright, the issue of theory choice is relevant only to phenomenological laws. Because Cartwright has thought that theoretical laws are either not true or not useful in explaining the complex reality, the issue of theory choice between rival theoretical laws is thus meaningless. A successful phenomenological law is characterized by successful causal explanations (Cartwright 1983, 8). Hence, causal explanation plays an important role in determining the winning theory among its rivals.

3.3.1 Causal Explanation, Theory, and Model

Causal explanation lays the grounds for one to believe in theoretical entities (Cartwright 1983, 6). However, causal explanation always takes the form of model or theory when accounting for the phenomenon of empirical world.

In tradition, theories are thought as carriers of knowledge whereas models are regarded as hypothetical or heuristic tools (Bailer-Jones 2008, 17). However, Cartwright against this thought. Bailer-Jones argues that Cartwright indeed favors models over theories as the carriers of knowledge about the empirical world (Bailer-Jones 2008, 17). In her reply to Bailer-Jones, Cartwright stresses that she has no “real philosophic views about truth” although she thinks that models do provide truth claims (Cartwright 2008, 38). What does Cartwright mean is that although models are used to provide truth claims, they do not necessarily achieve this objective in order to fulfill the epistemic function. Furthermore, she holds that models should depict the causal relationship correctly but need not accurately.

> …. I do think that scientific models sometimes provide claims about the world, that sometimes these claims are meant to be true or approximately true, that sometimes they might well be true, and that sometimes we have good evidence to suppose them to be true. Often even when models are intended literally, not everything in the model is meant to depict something in the world and certainly not everything in the world….
>
> (Cartwright 2008, 38)

> It is important that the models we construct allow us to draw the right conclusions about the behaviour of the phenomena and their causes. But it is not essential that the models accurately describe everything that actually happens…
>
> (Cartwright 1983, 140)

In addition, Cartwright further asserts that some phenomena are too complex to be explained by a single model. She holds that it is not uncommon to have several models working together to provide an account for reality. There is no single model that comprehensively explains the complex phenomena.

> …perhaps not even everything relevant to the phenomenon under study—is meant to be depicted in the model.
>
> (Cartwright 2008, 38)

> We construct different models for different purposes, with different equations to describe them. Which is the right model, which the 'true' set of equations? The question is a mistake. One model brings out some aspects of the phenomenon; a different model brings out others… No single model serves all purposes best.
>
> (Cartwright 1983, 11)

Cartwright's denial of a single true model which is able to explain the phenomena in a comprehensive way has led her to conclude that sometimes it is impossible to pick out one right model among the rival models. In the face of the complex reality, and of the "dappled world" (Cartwright 2005), there could be a case that more than one theory/model should be chosen in order to account for the physical phenomena. If one views the problem of theory/model choice as an issue of choosing only *one right* theory/model, he may face the risk of having a distorted account of reality. Cartwright explicitly holds that sometimes, depending on purposes, different and incompatible models should be chosen to explain the phenomena. Such pragmatic account of model has been one of the major doctrines in recent years in the debates on scientific representation and model (e.g., Suárez 2003, Giere 2004, and Knuuttila 2005, 2011).

> For different purposes, different models with different incompatible laws are best, and there is no single model which just suits the circumstances. The facts of the situation do not pick out one right model to use.
>
> (Cartwright 1983, 104)

The complexity of phenomena is the reason why Cartwright upholds a multiple-model view instead of a single-model view in scientific explanation. In line with her anti-fundamentalist position towards the fundamental theories and laws, Cartwright rejects the idea that phenomena can be accounted by a single comprehensive and coherent theory. In fact, she does not assume the significance of theory in scientific explanation as she holds that "explanations in physics generally begin with a model." (Cartwright 1983, 103) and "the fundamental equations of our theories cannot be taken to govern objects in reality" (Cartwright 1983, 131). Theory is an abstract construct that "has a very limited stock of principles for getting from descriptions to equations, and the principles require information of a very particular kind, structured in a very particular way" (Cartwright 1983, 131). This structure of information is provided by models. It is the model that explains phenomena. Hence, the thesis of theory choice in Cartwright's philosophy is a less important thesis compared to model choice, in two senses. First, theory has no practical usefulness or direct helps in explaining the phenomena; second, theory does not provide true or approximately true description of phenomena (Cartwright 1983, 54).

The relatively insignificant role of theories in explaining phenomena, as compared to models, does not make theory choice totally unimportant in scientific activities. Cartwright does admit that despite the fundamental laws of physical theory do not represent the facts, theories can explain in simple and ideal circumstances (Cartwright

1983, 58). The thesis of theory choice in Cartwright's philosophy is relevant to the choice between the fundamental theories, for example, a choice between Einsteinian physics or Newtonian physics in accounting for mechanical phenomena. Scientists have to decide which background theory to be used as the framework before they work out their models.

> It is customary to take the fundamental explanatory laws of physics as the ideal. Maxwell's equations, or Schroedinger's, or the equations of general relativity, are paradigms, paradigms upon which all other laws—laws of chemistry, biology, thermodynamics, or particle physics—are to be modelled.
>
> (Cartwright 1983, 54)

In Cartwright's account, models are connected to theories via causes. However, this connection does not imply that models are derived from theories (Suárez and Cartwright 2008, 66-68). Causes have two sides, one at model and another at theory. Causes at theory's side take the form of theoretical explanation, whereas causes at model's side take the form of causal explanation. Theory decides the nature of causes that contribute to explaining phenomena, whereas model decides the composition of causes.

> One of the important tasks of a causal explanation is to show how various causes combine to produce the phenomenon under study. Theoretical laws are essential in calculating just what each cause contributes.
>
> (Cartwright 1983, 12)

Putting up all the (visible or invisible) causes together, the produced effects can be observed. These effects can be predicted in model through causal reasoning. The causal reasoning of model is bound to the laws of theory that account for the causes. In later Cartwright's account, the causes at theory's side are called capacities, which inform us the general features of entity. Capacities can only illustrate the tendency of cause in general, but not in actual phenomena. In an example of the capacities of electron, Cartwright elaborates:

> An electron, it seems, always repels another electron; it "tends" to cause the second electron to move away. This is true despite the fact that in some causal structures moving the first electron towards the second will cause the second to move even closer; in others it will cause a particular motion; in others no motion at all.
>
> (Cartwright 2008a, 135)

While Cartwright explicitly implies the causes at theory's side as capacities, she implicitly extends the meaning of capacities to describe the causes at model's side. However, there is no clear boundary demarcated by Cartwright in using capacities to denote fundamental laws of theories and causal reasoning of models. This obscure notion of capacities sparks Psillos's doubts on Cartwright's position of anti-fundamentalism, which entails the objection of fundamental laws (Psillos 2008, 168). Psillos asserts that Cartwright's objection of theoretical laws does not hold because laws "are still the most plausible candidates for explaining why objects have the capacities to do what they can do" (Psillos 2008, 168).

Psillos's doubts will be dismissed if we interpret Cartwright's notion of capacities from another perspective. Theoretical laws describe capacities as general tendency with idealization. However, theoretical laws alone are incapable of explaining or predicting the actual tendency of entities. The causal reasoning of models is supplemented in order to describe how capacities are put up to work in a specific phenomenon. Scientists use theoretical laws as a paradigm of capacities, whereas models as a theoretical construct that patching up the picture of reality. Throughout Cartwright's career, she consistently confers models more significant role as compared to theories, for she holds that theory does not help much in explaining phenomena.

3.3.2 Choosing the Right Model

Cartwright's version of model choice thesis relies on the effects imposed by causal power on the entities. Her anti-fundamentalism has ruled out the practical effects of theoretical laws on the entities. Cartwright always argues for the multiple-model view and against the single-model view. It is because a phenomenon is complex and a single model alone is incapable to explain it. The adoption of multiple-model view implies that multiple causal powers produce the observable effects jointly. Hence, Cartwright's thesis of model choice is a choice of model combinations that possess a combination of causal reasoning. In choosing the right model combination, one has to evaluate the effects which are produced by a range of causal powers. The winning model combination is determined by the fact that it is able to produce the desirable effects that constitute the phenomenon.

There are two approaches to the concept of causation, viz., extrinsic or intrinsic view of causality. Extrinsic view holds that a causal connection is incurred by the external force that imposes on two events. According to this view, the notion of cause and effect "must betoken some factual property of natural processes" (Norton 2003, 3). Intrinsic view holds that causation is an innate property of two events. The typical representative of this view is Kant. He develops a theory of causation by holding that the necessary connection between causes and effects is determined a priori, which is also the nature of pure time

(Melnick 2006, 203-205). In general, it is fair to conclude that empiricists are extrinsic causalists, while idealists are intrinsic causalists.

One of the famous advocates for extrinsic view of causality is Hume. He advocates an empiricist approach to causality (Loux 2006, 187). Hume rejects the view that (i) there are necessary connections between events (ontological links between cause and effect); (ii) there is any possibility for human to know such necessary connections between events. After his definition of cause from the perspective of metaphysics and epistemology, he immediately writes:

> But though both these definitions be drawn from circumstances foreign to the cause, we cannot remedy this inconvenience, or attain any more perfect definition, which may point out that circumstance in the cause, which gives it a connexion with its effect. We have no idea of this connexion;
>
> (Hume 2007, 56)

However, it is unclear if Hume is a causal realist. It is primarily because of his skeptical position on the existence of causal connection between events. Although he rejects the view that there are necessary connections between events, he still believes in real causal powers (Beebee 2006, 173). Hume claims that the causal relation between events is a "secret power" or "secret cause" (Hume 2007, 24; Dicker 1998, 108). Humean theory of causation renders the necessary causal relation between events implausible, but Hume does not reject the possibility of probabilistic causal relation. It seems that Hume's recognition of "secret cause" has made him a causal realist.

However, some scholars hold that Hume is not a causal realist. The first evidence presented by them is that Hume does not admit the objectivity of causation (Chakravartty 2007, 93). They further back their argument with the second evidence that Hume has rejected necessary connection between causes and effects (Chakravartty 2007, 93).

Cartwright is not an intrinsic causalist because she states that "most causal relations we study are not absolutely fundamental" (Cartwright 2008b, 240). She also against the view that causation is a priori. Similar to Hume, Cartwright is an extrinsic causalist. However, opposed to Hume, Cartwright believes that there is a necessary connection between cause and effect, not from the metaphysical but scientific point of view. She introduces a term "nomological machine" to describe the mechanism of causation. Cartwright defines nomological machine as a generator of laws that

> … deploying and harnessing capacities, getting them situated in just the right circumstances, in just the right connections with each other, keeping the whole thing stable enough and shielding it and setting it running, and then we can get regularities emerging.
>
> (Cartwright 2003, 201)

Nomological machine is a mechanism that generates laws and deploys capacities. Sometimes Cartwright explicitly equates nomological machine with capacities (Cartwright 2007a, 6). In *The Dappled World*, Cartwright attributes to nomological machine both tangible and intangible properties, as she defines it as:

> … a fixed (enough) arrangement of components, or factors, with stable (enough) capacities that in the right sort of stable (enough) environment will, with repeated operation, give rise to the kind of regular behaviour that we represent in our scientific laws.
>
> (Cartwright 1999, 50; cited in Hoefer 2008, 10-11)

Nomological machine is the joint product of nature and human (Cartwright 2003, 201). Nomological machines are sometimes recognized as models (Cartwright 2002, 242). One of the examples of nomological machines as models given by Cartwright is the planetary system (Cartwright 2003, 201). The arrangement of components or factors that constitute a nomological machine "has a stable capacity" (Cartwright 2003, 201), and the capacity attributes properties to a nomological machine. The position of planets is an example of the properties of a planetary system, which has been viewed as a nomological machine (Cartwright 2003, 201). The behavior of things or events in space-time has causal properties, which is attributed by causal capacity of a nomological machine.

The joint interaction of multiple capacities can be explained by the phenomenological laws in the model. Phenomenological laws are tied to reality.

> The route from theory to reality is from theory to model, and then from model to phenomenological law. The phenomenological laws are indeed true of the objects in reality—or might be; but the fundamental laws are true only of objects in the model.
>
> (Cartwright 1983, 4)

In Cartwright's account of causation, causal capacities that induce cause-effect are not bound by universal laws.

> I don't think there's anything in general we can do [if causes interact]. I don't believe in universal methodology…. There are cases where we have interaction—the notorious cases being in chemistry—where one comes to an understanding of how they operate. There are other cases where interactions [of causes] may not follow any rules.
>
> (Cartwright 2003, 202)

> … I genuinely believe that most things that happen in the world can't be subsumed under a regularity, or ought to be subsumed under a regularity. A lot of what happens simply is a result of interaction which we can't have a handle on.
>
> (Cartwright 2003, 202)

Instead, Cartwright holds a skeptic view on the intelligibility of causation in terms of fundamental laws and theories. However, her concept of nomological machine seems to imply, albeit she does not claim explicitly, that she has shared Hume's view that causation is intelligible in the common sense.

> I think most cases of causation are cases of interaction and that they're not intelligible in a scientific way. That is, not much of what happens in the natural world is governed in a systematic way.
>
> (Cartwright 2003, 202)

In the effort of constructing a model, the natural and artificial capacities of nomological machine are put together. Capacities are stable in the sense that they will regularly exercise themselves in a canonical way if they are properly triggered (Cartwright 2007a, 19). These capacities can be easily recognized as they "typically have visible markers" (Cartwright 2007a, 6), which are the observable traits. Cartwright holds that "capacities are often tied to markers by well-established empirical laws" (Cartwright 2007a, 6). Empirical laws (phenomenological laws) are the key factors in arbitrating between rival models. As Cartwright adopts a pragmatic approach in model choice, she stresses that the empirical laws must be practically useful in describing a phenomenon.

Cartwright is not a model realist (Cartwright 2007c, 4, 217, 219). She holds that some models, especially of economics, are making highly unrealistic assumptions that are useful for one to learn about causes (Cartwright 2007c, 4, 219, 220). Causal reasoning used in a model is practical in the sense that it is the “inference to most likely cause” (Cartwright 1983, 6). The most likely cause is “the only practical possibility” (Cartwright 1983, 6). However, Cartwright does not claim that the most likely cause is necessarily the actual cause in nature. Cartwright claims that one can only know the phenomenological cause through scientific practice. She holds that a most likely cause should be verified by experience (Cartwright 1983, 6) through backward reasoning (from effects to causes).

> I have sometimes summarized my view about explanation this way: no inference to best explanation; only inference to most likely cause. But that is right only if we are very careful about what makes a cause ‘likely’. We must have reason to think that this cause, and no other, is the only practical possibility, and it should take a good deal of critical experience to convince us of this.
>
> (Cartwright 1983, 6)

> Causal reasoning provides good grounds for our beliefs in theoretical entities. Given our general knowledge about what kinds of conditions and happenings are possible in the circumstances, we reason backwards from the detailed structure of the effects to exactly what characteristics the causes must have in order to bring them about.
>
> (Cartwright 1983, 6)

As Cartwright points out, prediction of effects is capacity-dependent. To predict the effects we need to ascertain “when a capacity obtains and when it does not” (Cartwright 2007a, 20). In an experiment, expression of capacities is measured, “and what scientists want to know is which capacities objects will express in which circumstances.” (Paul 2002, 249)

In Cartwright’s example of the composition of forces (capacity) that constituted by gravity and electricity (Cartwright 1983, 59), a resultant force (capacity) is produced artificially by human calculation using vectorial addition of the natural gravitational force and electric force. According to Cartwright, the capacities and causes are knowable and calculable, in an additive fashion. If she is right, it would appear that model choice is not a difficult task. For, we can adjudicate rival models by analyzing the composition of capacities, which is calculable. However, Lipton opposes to Cartwright's view of the calculable additive capacities. He holds that the composition of capacities is produced in much complicated ways than additive fashion (Lipton 2002, 257).

> Certainly forces and capacities may interact in complicated ways that are not in any intuitive sense 'additive'. This is easiest to see in cases where one capacity changes another: elastic bands become brittle, food becomes inedible and drugs lose their potency. The simple picture of a composition of forces as vector addition is inapplicable to most interactions between capacities.
>
> (Lipton 2002, 257)

However, Lipton overlooks the fact that notwithstanding Cartwright claims that individual capacity is calculable, she holds that the composite capacity is incalculable.

> Even in the case of the magnet this picture seems suspicious. To be sure, there are cases where all the causes affecting the motion of a metallic object can be represented neatly as vector forces, the magnetic force among, and the resultant motion calculated via vector addition and the rule that the acceleration of the metallic object equals the resultant force divided by the object's mass. But it is a huge leap of faith to suppose that the dust and spider webs between the floorboards can be regimented into this neat picture. The best Cartwright would be prepared to bet is true is that the magnet *could well* lift the earring. And this remains a weak prediction!
>
> (Cartwright and Efstathiou 2007b , 23)

> Capacities on the contrary are good for scope. Once established capacities can be carried to new settings…For example, now that it has been established that magnets have the capacity to attract metallic objects, the attraction may be confidently relied on in new settings. But capacities are not as good as we might hope for prediction. What is guaranteed with a capacity is that it will produce a fixed *contribution*… What actually happens is far harder to predict since it

> depends on what other causes are operating and what all their contributions together add up to.
>
> (Cartwright and Efstathiou 2007b, 22-23)

Since Cartwright has denied the predictive power of the composite capacities, it makes no sense to believe that rational model choice can be made on the grounds of predictive power. The unpredictability of capacities leads Cartwright to hold that, given certain causes and conditions, the repeatability of an effect is not guaranteed (Cartwright 2002a, 274-275). She further claims that there is no law to determine an effect. Though Cartwright has granted the practical use of phenomenological law, she delimits its function to causal reasoning that explains cause-effect. She does not claim that a phenomenological law is able to predict accurately an effect based on the cause. Cartwright's pessimistic stance on the predictive power of capacities results in a dilemma in model choice for mathematical sciences such as theoretical physics (though it may not be the case for empirical sciences which are probabilistic in most of its methodologies). Notably, her view about explanatory power, which is the notion of "inference to the most likely cause", does not relieve this dilemma. As the name suggested, inference to the most likely cause leads one at best only to the most *likely* cause, without a clue of which cause is the *actual* cause of an observed phenomenon. In short, there is no concrete guide for model choice in Cartwright's account because the exact connection between causes and effects is indeterminate.

3.4 Putnam's Antirealist Position About Theory

Notwithstanding Putnam regards himself a persistent realist, most scholars have disagreement. Putnam's debatable notion of realism has gone through two stages, which can be identified as the stage of pro metaphysical realism and of pro internal realism. Putnam's rejection of "metaphysical realism" labels him an antirealist, as he defines metaphysical realism as scientific realism which assumes the referential relations between theory and mind-independent reality. Later, his turn to internal realism has been interpreted as a shift to relativism, though he has attempted to distance himself from it (Ben-Menahem 2005, 5). However, the issue of Putnam's realism/antirealism position is complicated. It is because his alleged new notion of realism deviates from the traditional outlook.

Putnam is an antirealist and pragmatist about scientific theory. He has associated metaphysical realism with independence, uniqueness, bivalence, and correspondence. Putnam rejects all four attributes of metaphysical realism because they lack a human perspective.

> In various places I have described metaphysical realism as a bundle of intimately associated philosophical ideas about truth: the ideas that truth is a matter of Correspondence and that it exhibits Independence (of what humans do or could find out), Bivalence, and Uniqueness (there cannot be more than one complete and true description of Reality)
>
> (Putnam 1996, 107)

> In fact, metaphysical realist definitions of 'objectivity' are easily seen to be failures in their own terms. Re 'something's being the case is independent of how anyone would regard it', it suffices to note that reality does *not* have an existence and character wholly independent of human practices, beliefs, and evidence for the simple reason that human practices beliefs and evidence are a very large part

> of the reality we talk about, and reality would be quite different were they different.
>
> (Putnam 2005b, 18)

Putnam stresses that objective truth of science has its human perspective, implying that observers' role is a necessary factor in constructing scientific theory (Putnam 1990, 7). Human perspectives are indispensable because they are a part of the reality. Putnam takes scientific realism as a version of metaphysical realism which presupposes a God's eye viewpoint—that is, to study nature as it is, without human's perspective— in science (Sankey 2004, 1). Putnam rejects metaphysical realism (scientific realism) because he holds that it is impossible to do science without a human perspective embedded. However, Putnam's rejection of scientific realism does not imply a relativists' move. He argues that we need not to reject the realists' mind-independent truth. What Putnam advocates is a notion called "internal realism", which rejects a subset of realism, that is, scientific realism, without rejecting realism as a whole (e.g. Putnam still endorses the view about theoretical entities). It is in this sense Putnam views himself a realist. However, Putnam is a realist about nature (he adopts Kantian metaphysics to a certain extent), but an antirealist about scientific theory.

Being an antirealist about scientific theory, Putnam shares the identity with van Fraassen and Cartwright. However, his pragmatic view of theory distances himself from them, who are firmed empiricists. Putnam's pragmatist and fallibilist view of theory lead him to the rejection of empiricism (Mueller and Fine 2005, 86). The version of empiricism that is rejected by Putnam is a tenet which (1) allows only a single interpretation of reality (Putnam 1990, 5) which does not admit fallibility of such interpretation; and (2) which is skeptical.

> Pragmatism in general….. is characterized by being simultaneously *fallibilist* and *anti-skeptical*, whereas traditional empiricism is seen by pragmatists as oscillating between being too skeptical, in one moment, and insufficiently fallibilist in another of its moments.
>
> (Putnam 2004a, 99)

Putnam argues that empiricism presupposes a single God's eye view. He holds that the empirical world should be interpreted from multiple perspectives, and pragmatic attitude should be applied in doing so. According to Putnam, the pragmatic attitude in science is regarded as a spirit of readily altering one's view when it is found false or dubious. The fallibility of theory is always assumed in Putnam's pragmatist view (Mueller and Fine 2005, 86).

> Pragmatism tells us that we have to take seriously the beliefs that we find indispensable in our lives. That doesn't mean that we must always retain such beliefs unaltered. If there is a devastating criticism of a belief that has been fundamental to our practice up to now, then we must alter the belief (and that usually means altering the practice as well).
>
> (Putnam 2005a, 38)

Putnam's pragmatism is manifested clearly in the choice of physical laws that describe the phenomena (Putnam 1975, 102). He has sometimes used "pragmatic realism" to replace "internal realism" (Putnam 1996, 108). However, Putnam's pragmatic approach to theories has always been misread as relativistic. Hirsch is one of the scholars who interpret Putnam's pragmatic realism as "quantifier relativism" according to which different conceptual schemes are all acceptable and adequate to describe the world (Hirsch 2004, 229). Sosa even goes further by attributing Putnam's internal realism a radical perspective view of truth, reality, reference and causation (Putnam 2004, 234). In fact, Putnam's pragmatic account of theory asserts that there are non-relative cultural and historical dependent norms that govern all rational scientific activity (Żegleń 2005, 4). Putnam seems to hint that norms have both a relative and a universal side. He does not deny that cultures and histories are relative in terms of the contents of norms. However, he implicitly contends that there are also universal characteristics of norms across cultures and histories. The most apparent evident of the universal side of norms is his assertion of the revisibility of theory.

> Any principle in our knowledge can be revised for theoretical reasons.
> (Putnam 1962, 48; cited in Mueller and Fine 2005, 86)

> Pragmatists hold that there is never a metaphysical guarantee to be had that such-and-such a belief will never need revision.
> (Putnam 1994, 152; cited in Warner 2005, 25)

Putnam's antirealism is fit into Horwich's definition. In this definition, the role of human perspective is stressed in antirealist account of knowledge.

> The scientific anti-realist… cannot see how it is possible for there to be theoretical facts that, on the one hand, are within the reach of our methods of conceptualization and investigation but, on the other hand, exist independently of them. Thus, for a scientific anti-realist, the paradigm of knowledge is of observed facts, which are regarded as dependent upon human capacities.
> (Horwich 2004, 35)

Putnam antirealist position has its lead in his stance of anti-metaphysical (scientific) realism and anti-empiricism (of single interpretation of theory). Taken all his positions together, he adopts a version of antirealist cum pragmatist position called "internal realism" according to which human perspective rather than God's eye view is adopted in doing science. Internal realism is not a version of scientific realism about theory; for it holds that conceptual relativity should be adopted as a pragmatist approach in promoting human perspective in scientific theory. Conceptual relativity is closely linked to objectivity, according to Putnam, rather than to relativist sense of relativity. Putnam has made a distinction between objectivity and Objectivity. The latter with a capital “O” implies the absolute mind-independent truth and phenomena (Putnam 1996, 109). The former, with lowercase “o” which is upheld by Putnam, is a property of truth that is logically independent of the belief of people (Putnam 1996, 109). The capital letter Objectivity is a concept that operates at metaphysical level; whereas the lower case objectivity operates at semantic level. Objectivity at metaphysical level presupposes God’s eye view; while objectivity at semantic level allows human perspective of theory construction with the aid of conceptual relativity. Putnam holds that objectivity is “not a solution to the grand metaphysical question of Realism or Idealism, but simply a feature of our notion of truth.” (Putnam 1996, 109). Putnam states that truth is pluralistic, for objectivity has many dimensions.

> ‘Value judgments’ are not a homogeneous class, and different sorts of judgments possess different sorts of objectivity.
>
> (Putnam 2005b, 17)

Conceptual relativism suggests perspectivism in theory interpretation, always denoted as “internal realism” by Putnam. Conceptual relativism does not lead to relativism because truth is not interpreted in a relativistic way.

> If you have a world in which there are two black “atoms” and one red one, you can either say that there are three objects (the atoms), or that there are seven objects (the atoms and the various aggregates of two or more atoms). How many

> objects are there "really" in such a world? I suggest that *either way of describing it is equally "true"*.
>
> (Putnam 1995, 120)

However, Putnam does not take conceptual relativism to mean that anything goes in science. He asserts that human perspective in theory interpretation is constrained by the reality.

> Nevertheless, the phenomenon of conceptual relativity does have real philosophical importance. As long as we think of the world as consisting of objects and properties in some one, philosophically preferred sense of "object" and "property"—as long as we think that reality itself, if viewed with enough metaphysical seriousness, will *determine* for us how we are to use the words "object" and "property"—then we will not see how the number and kind of objects and their properties can vary from one correct description of a situation to another correct description of that same situation.
>
> (Putnam 1995, 122)

In the quoted paragraph above, Putnam uses italic font style to stress the word "determine". He implies that, if we believe that reality will determine the theory, the available perspective of interpretation we have in science is delimited by reality, for reality "will determine for us how we are to use the words 'object' and 'property'" (Putnam 1995, 122). Though conceptual relativity is contextually dependent on reality, Putnam does allow freedom in the way of describing reality (that is what he calls "human perspective"). Putnam against the view that there is only one correct way of describing reality, which is the view embraced by scientific realists.

> Although our sentences do "correspond to reality" in the sense of describing it, they are not simply copies of reality.
>
> (Putnam 1995, 122-123)

Thus, conceptual relativism is of antirealism in the sense that Putnam opposes scientific realists' one-to-one correspondence between theory and reality. Conceptual relativism is incompatible with correspondence theory of truth (Żegleń 2005a, 90). Putnam calls this antirealist position "internal realism", implying that interpretation of theory can be carried out in many ways (internal) as long as it is contextually dependent on the reality (realism). Putnam advances this antirealist position about theory by resorting to pragmatism. Notably, it is implausible for a scientific realist who subscribes to the correspondence theory to relativize correspondence truth within alternative conceptual schemes, for this move makes scientific realism incoherent.

However, Putnam does not reduce pragmatism to instrumentalism. He rejects the view that beliefs are true if they are useful (Putnam 2005c, 64). Indeed, pragmatism is a blend of fallibilist attitude and action orientation.

> Pragmatists, I explained, see *active intervention, intelligently directed experimentation* and *attempting to falsify even 'highly confirmed' hypotheses* as essential to rational belief fixation; I criticized logical positivists precisely for writing as if scientists viewed the universe from outside ('through a one-way mirror', which allowed them to look in without interacting….
>
> (Putnam 2005, 80-81)

The above passage suggests three meanings of Putnam's pragmatism. Firstly, pragmatists view theory as a dynamic product of human interaction with reality. Opposing to realist and positivist God's eye view of theory—there is a final theory which is infallible, pragmatists hold that such a final and static theory does not exist. Theory is in flux, which is shaped by human's action and interaction with reality. In his criticism on Popper's theoretical interest in the scientific prediction, Putnam stresses that theories are not just theories per se. They have pragmatic uses in guiding the actions, such as observations and further experiments.

> When a scientist accepts a law, he is recommending to other men that they rely on it—rely on it, often, in practical contexts…… Ideas are not *just* ideas; they are guides to action. Our notions of 'knowledge', 'probability', 'certainty', etc., are all linked to and frequently used in contexts in which action is at issue: may I confidently rely upon a certain idea? Shall I rely upon it tentatively, with a certain caution? Is it necessary to check on it?
>
> (Putnam 1975, 251)

Secondly, experimentation plays a determining role in theory construction and falsification. Putnam moves further to hold that the empirical science which is largely constructed on the foundation of experimentation has laid part of the foundation for mathematics too.

> I have not argued that mathematics is, in the full sense, an *empirical* science, although I have argued that it relies on empirical as well as quasi-empirical inference….my expectation is that as physical science develops, the impact on mathematical axioms is going to be greater rather than less…much of mathematics too is 'empirical'.
>
> (Putnam 1975, 77)

If mathematics contains empirical elements in its foundation, its axioms can hardly be a priori in nature. Although Putnam does not claim explicitly that mathematics is fallible, he does imply that mathematical knowledge is not final and static. However, since Putnam dubs himself a pragmatist, he has to accept that mathematics is fallible. For pragmatists are against the notion of the infallibility of knowledge. By asserting that mathematics is somehow empirical in nature, Putnam implies that neither mathematics nor empirical science is absolute knowledge. Furthermore, he has rejected mathematical logic "as a primary model for analyzing the norms governing our concepts" (Floyd 2005, 38). Putnam implies that conceptual norms have pluralistic source. Since mathematical axioms, logical reasoning and physical theories are fallible and epistemically non-absolute, a pragmatic pluralism about truth should be advocated. From here it brings out the third meaning of Putnam's pragmatism—all theories are fallible.

> …… knowledge can, and according to pragmatists *should*, produce a healthy awareness of human fallibility;
>
> (Putnam 1996a, 68)

> but pragmatists have never believed in infallibility, either in perception or anywhere else.
>
> (Putnam 2005b, 20)

In Putnam's opinion, the fallibility of theory is applicable universally, even to the fundamental theories. All theories and beliefs are fallible, except the fallibilism. Hence, Putnam's thesis of theory choice is of antirealist for two reasons. Firstly, one cannot test rival candidate theories by checking their correspondence to reality. It is because Putnam has rejected the correspondence theory of truth in his repudiation of metaphysical realism. Secondly, there is no absolutely reliable fundamental theory for one to choose as a framework for the comparison between rival theories. Putnam's thesis of theory choice has a "human face" as he holds fast to pragmatism and the fallibility of theory.

This "human face" of Putnam's theory choice thesis consists of fallibilism and pragmatism, or using Putnam's term, internal realism. Putnam has compared the positivists' and scientific realists' ways of theory appraisal with that of pragmatists in his "Lezione italiane" lecture at the Universita degli Studi di Roma, Italy. He concludes that positivists and realists appraise theory in an algorithmatic way, whereas pragmatists depend on human interaction with reality.

> Fundamentally, the standpoint is that of a single isolated spectator who makes observations through a one-way mirror and writes down observation sentences. Appraising theories for their cognitive virtues is then simply a matter of using an algorithm to determine whether a sentence has a mathematical relation to another sentence (the conjunction of the observation sentences the observer has written down), on this picture.
>
> (Putnam 1996a, 70)

> The pragmatist picture is totally different. For Peirce and Dewey, inquiry is cooperative human interaction with an environment; and both aspects, the active intervention, the active manipulation of the environment, and the cooperation with other human beings, are vital.
>
> (Putnam 1996a, 70)

Putnam stresses that active intervention of the environment and the cooperation with other human being are two indispensable aspects in pragmatist way of theory appraisal. Putnam regards himself a realist because he holds that intervention of the environment is the first aspect of scientific quest. In addition, Putnam regards himself an internal realist because he holds that conceptual relativity—which can be promoted via cooperation with other human beings—is another indispensable aspect. Positivists and scientific realists, on the contrary, do not appreciate these two aspects. Although they do interact with reality in the form of experiment and observation, such interaction has assumed a so-called neutral/*O*bjective (which is different from *o*bjective) standpoint. This neutral interaction with reality is lacking of human element, for the reality is regarded as independent from human's manipulation.

Since the human aspect is vital in theory appraisal, Putnam's thesis of theory choice has a social dimension, though he does not spell it out explicitly. In general, Putnam has abolished all kind of dualism such as fact/value, mind/body, observation/theory, truth/convention, and mind/world dichotomy (Ben-Menahem 2005, 13). So we may well assume that he will not argue against the abolishment of the social dimension/theoretical dimension dichotomy in theory choice.

Putnam's implicit objection to social dimension/theoretical dimension dichotomy in theory choice is a good strategy to save him from being accused as relativist. On the one hand, Putnam advocates an autonomous society which would not block "the path of inquiry", a society in which scientific liberty (i.e. academic freedom) is achieved (Putnam 1990, 203). He holds that an autonomous person who has scientific liberty will respect others in scientific inquiry (Putnam 1990, 203). Putnam implicitly implies that an autonomous society helps to promote conceptual relativity in science. It is especially helpful in appraising rival theories, as authority's opinion is not to be sought for, and what is essential is an array of opinions and perspectives from peers. With the abolishment of social/theoretical dimension, Putnam may argue that the peer's influence (e.g., persuasion, etc) on theory choice is not an irrational factor, for the peer's influence has theoretical grounds (e.g., persuasion which is based on scientific reasons). Despite the abolishment of social/theoretical dimension, Putnam provides no operating guideline to coach theory choice. As such, Putnam's argument has a gloomy danger of falling into relativism, for one can argue that such absolute scientific liberty can potentially paralyze the mechanism of rational theory choice, with regard to the fact that the theory choice has no decisive criterion.

On the other hand, Putnam states that the social dimension of theory choice has a theoretical dimension, which jointly makes possible a choice among rival theories. With regard to the fact that Putnam has rejected dualism in general, it is reasonable to infer that he will accept Wittgenstein's maxim "the limit of my language is the limit of my world". Wittgenstein's maxim states that whatever that constitutes the world is describable by language. What is unspeakable, or unknowable, does not belong to the world. Wittgenstein's maxim denies that there is an insurmountable gap between language and world. Putnam's rejection of theory/reality dualism is parallel to Wittgenstein's rejection of language/world dualism. Substituting Wittgensteinian "language" with "theory" and "world" with "reality", Putnam's notion of theoretical dimension has its foundation in the reality. Now, reality is the basis for rational theory choice. Although scientific liberty, as the social dimension, is permitted in theory choice, reality does filter the opinions and perspectives that are irrelevant. By this way, relativism is thus avoided in Putnam's thesis of theory choice. However, Putnam's solution is still problematic as he needs to explain how reality arbitrates which theory to be chosen. It is unlikely that he will resort to correspondence theory of truth, for it is the very idea of metaphysical (scientific) realism that he has rejected throughout his career.

> But the usefulness of true ideas is the result of their "agreement" with reality; their usefulness alone does not constitute that agreement.
>
> (Putnam 1990, 221)

The term "agreement" is enclosed within double quote in the above quotation. This indicates that Putnam has the word "correspondence" in mind but avoid to use it, so that he can escape from falling back to metaphysical (scientific) realism. In short, Putnam's rejection of metaphysical (scientific) realism and adoption of pragmatism serve no good strategy to provide a convincing thesis of theory choice. His repudiation of dualism does save him from relativism but not from the problem of theory choice.

3.5 Laudan's Antirealist Position

Laudan has repudiated scientific realists' claim that an empirically successful theory is true, and vice versa. He calls this realist claim "convergent realism" or "convergent epistemological realism", which is grounded in empiricism (Laudan 2002, 212-213). As an antirealist, Laudan does not repudiate the view that scientific theories are true or approximately true. Indeed, he is skeptical about the human's ability in knowing the status of truth of theories.

> There is nothing in this model which rules out the possibility that, for all we know, scientific theories are true; equally, it does not preclude the possibility that scientific knowledge through time has moved closer and closer to the truth…… But what I am suggesting is that we apparently do not have any way of knowing for sure (or even with some confidence) that science is true, or probable, or that it is getting closer to the truth.
>
> (Laudan 1977, 126-127)

Laudan holds that it is problematic—as in the realist's account—to maintain that empirical success entails the truth of the theory. He points out that there are two conditions to qualify a theory as empirically successful, in the convergent realist account (Laudan 2002, 213). First, the theories are approximately true. Second, the referents are genuinely referred by the central terms in theories. An empirically successful theory, according to convergent realists, is attributed by the ability of providing "detailed explanations and accurate predictions" (Laudan 2002, 213) in relation to reality. Realists believe that if a theory is approximately true, it will be explanatorily successful (Laudan 2002, 221). In addition, realists hold that the referential central term is the evidence of accurate predictions.

Laudan rejects the realist view that an approximately true theory is always explanatorily successful. He claims that an approximately true theory may not logically entail that the theory's entailments (such as the observable consequences) will be true (Laudan 2002, 222).

> Indeed, it is entirely conceivable that a theory might be approximately true in the indicated sense and yet be such that *all* of its thus far tested consequences are *false*.
>
> (Laudan 2002, 222)

Furthermore, Laudan holds that connecting approximate truth to the success of theory is not a promising enterprise. The primary reason is that none of the realists have provided a coherent account of approximate truth that explains the predictive success of approximately true theories (Laudan 2002, 223). A coherent account of approximate truth requires the "central explanatory terms genuinely refer" (Laudan 2002, 224). Unfortunately, history of science shows that there were successful theories which do not entail referential terms.

Laudan holds an antirealist position against convergent realism. He claims that empirical success of a theory is not always evidenced by the reference of theoretical terms to the reality (Ladyman and Ross 2007, 84). Besides, Laudan argues that empirical success of a theory does not warrant the unshakable position of theory during theory-change. In other words, Laudan argues that empirical success of a theory is not necessarily connected to truth and referenced referents. However, in the conventional realist account, a true/approximately true theory requires its central theoretical terms to refer (Laudan 2002, 223). Laudan contends further that scientific realism is untenable because the history of science offers a non-referential picture of successful theory (Laudan 2002, 223-224).

> Realists have no explanation whatever for the fact that many theories which are not approximately true and whose "theoretical" terms seemingly do not refer are nonetheless often successful.
>
> (Laudan 2002, 228)

Laudan strikes realists' empirical foundation by pointing out that many empirically successful theories were turned out to be false in the history of science, such as aether theory and phlogiston theory. It is because the posited unobservable entities do not exist. However, these empirically successful but false theories were able to make correct prediction.

> …. optical aether theories had also made some very startling predictions, e.g., Fresnel's prediction of a bright spot at the center of the shadow of a circular disc; a surprising prediction which, when tested, proved correct. If that does not count as empirical success, nothing does!
>
> (Laudan 2002, 218-219)

If past successful theories could be false, Laudan holds that it is reasonable for one to suspect that the current successful theories may probably be false too. If the current successful theories are the successors of the past successful but false theories, it is unlikely that the former will be an approximately true theory. However, Laudan does not explicitly claim that the former is always false if their predecessors are false. His pessimistic induction implies that there is a valid reason to assume that the current successful theories may as well be false. A past successful but false theory does not guarantee its successor to be true, but the latter can be successful regardless of its truth or falsity. Laudan's pessimistic induction is pessimistic about the truth of the successor theories, but neutral about the possibility of success of the latter. Pessimistic induction has undermined realist view that "the greater success of current theories shows that they are more truth-like than their less successful predecessors." (Doppelt 2003, 5)

> Most of the past theories of science are already suspected of being false; there is presumably every reason to anticipate that current theories of science will suffer a similar fate.
>
> (Laudan 1977, 126)

> If a theory has once been falsified, it is unreasonable to expect that a successor should retain either all of its content *or* its confirmed consequences *or* its theoretical mechanisms.
>
> (Laudan 2002, 229)

It is seemingly that Laudan does not deny that a false theory in the history of science may have legitimate successors (empirically successful theories). What has Laudan rejected is the view that the successor of a false theory will be free from the inheritance of the false content (consequences or theoretical mechanisms).

Recently, Laudan's pessimistic induction was challenged by Psillos's so-called "divide et impera move". It is a realist view which objects the abandonment of all theoretical constituents of a rejected past theory (Psillos 1996, S308). Psillos contends that there are stable theoretical constituents retained notwithstanding the past theories are proved false. These stable theoretical constituents have truth-like claim, and thus scientific realism is still defensible (Psillos 1996, S308).

Psillos's rebuttal to Laudan's pessimistic induction is not convincing. The survival of stable theoretical constituents from the rejected past false theory fails to account for the reason of the rejection of this theory. Notwithstanding the existence of stable and true theoretical constituents, Laudan may argue that their truth is not significant to the extent to save the theory from being proved false, as a whole, and from being rejected. Furthermore, Psillos's account does not support the realist account of scientific progress in terms of verisimilitude of truth. Realists hold that the empirical success of theories is proportionate to the accumulation of truth. They hold that science is progressive as evidenced by the fact that current theories are more truth-like than their predecessors. Psillos's "divide et impera move" defeats the realist picture of scientific progress, for he does not account for the accrued stable theoretical constituents that passed from false and rejected theories to the succeeding theories. Psillos can hardly argue that succeeding theories, which are derived from the false predecessors, progress in terms of having more truth-like claims, for he has no way to deny that a rejected past theory must have more false than true theoretical constituents.

Laudan contends that the objective of scientific theories is problem-solving. The problem-solving capability of a theory is not wholly, and mainly, determined by its status of truth. In other words, Laudan denies that a theory which is effective in problem-solving is necessarily true or approximately true. The progress of science is not determined by the verisimilitude of truth, but the problem-solving effectiveness. Realists see the progress of science as "the accumulation of new truths" (Laudan 1981, 227), while Laudan asserts that science progresses as the solved problems accumulated.

> Determinations of truth and falsity are *irrelevant* to the acceptability or the pursuitability of theories and research traditions.
>
> (Laudan 1977, 120)

> The *acceptability* of a research tradition is determined by the problem-solving effectiveness of its latest theories.
>
> (Laudan 1977, 119)

> Within the problem-solving model, however, we make no assignments of truth or falsity; there is nothing in the structure of deductive logic which precludes the localization of properties such as problem-solving effectiveness.
>
> (Laudan 1977, 43)

By exhibiting the counterexamples in history of science, Laudan concludes that realist account of empirically successful theory is untenable. He holds that a theory should be chosen based on its problem-solving capability, not on its empirical success.

> ..... *choose the theory (or research tradition) with the highest problem-solving adequacy*.
>
> (Laudan 1977, 109)

> ….. the rationale for accepting or rejecting any theory is thus fundamentally based on the idea of problem-solving *progress*.
>
> (Laudan 1977, 109)

As a problem-solving enterprise, science progresses as the solved problems are accumulated. Laudan claims that a theory is regarded as rational and progressive if it is effective in problem-solving.

> I propose that the rationality and progressiveness of a theory are most closely linked—not with its confirmation or its falsification—but rather with its *problem solving effectiveness*. I shall be arguing that there are important *nonempirical*, even "*non-scientific*" (in the usual sense), factors which have—and which should have—played a role in the *rational* development of science.
>
> (Laudan 1977, 5)

On the one hand, Laudan attempts to blur the traditional distinction between scientific progress and scientific rationality (Laudan 1977, 5). In the traditional view, progress is a temporal concept whereas rationality is atemporal (Laudan 1977, 5). Laudan holds that these concepts are not distinct, but intersecting. He rejects the realist view that a theory is rationally reliable which is independent of its historical context (Laudan 1977, 5). Laudan implies that the atemporal nature of scientific rationality has its root in the temporal nature of progress. On the other hand, Laudan attempts to establish the distinction between scientific progress and rationality in his own way. He repudiates the idea that progress of science implies the consequences of a series of rational theory choice. He holds that not all of the scientific progresses are driven by rational choice, for the rationality of the criteria of theory choice does not always warrant scientific progress.

> Insofar as rationality and progressiveness have been linked at all, the former has taken priority over the latter—to such a degree that most writers see progress as *nothing more than* the temporal projection of a series of individual rational choices. To be progressive, on the usual view, is to adhere to a series of increasingly rational beliefs. I am deeply troubled by the unanimity with which philosophers have made progress *parasitic* upon rationality.
>
> (Laudan 1977, 5-6)

Notably, Laudan does not deny the importance of rationality in science. Laudan's rebuttal to the epistemic relativism in his attack of global underdetermination thesis shows that he is not a relativist (Laudan 1998, 320-353). The concept of rationality which is repudiated by Laudan is truth-oriented. Laudan demonstrates that there are no necessary connections between rationality and truth. Laudan defines rationality as good and sound reasons for a given action or belief (Laudan 1977, 123). He proceeds to hold that scientific rationality has two dimensions, which are 'rationality outside science' and 'rationality within science'.

> It is vital to be clear at the outset that many things that would count as good reasons *outside* science cannot constitute good reasons *within* science. To take a trivial example, I might have a good reason for saying that "2+2=5," if I know that someone will punish me severely if I refuse to say it.
>
> (Laudan 1977, 123)

'Rationality outside science' is a notion of rationality that is grounded in *good personal reasons* of doing science in a certain way, which is not the rationality constitutive in science. Laudan differentiates these two dimensions of scientific rationality by admitting that 'rationality outside science' is not necessarily the constitutive rationality within science.

> But what can count as a good personal reason for doing something does not necessarily count as a good *scientific* reason for doing it.
>
> (Laudan 1977, 123-124)

Laudan holds that the 'rationality within science' should be answered in terms of the aims of science—problem-solving (Laudan 1977, 124). The effectiveness of problem-solving of a theory is the indicator of 'rationality within science'. In other words, the good reason for advocating a theory is not its truth contents, but its problem-solving effectiveness. The good reason (rationality) for saying that "1+1=2" is not because it is true, but because it is able to solve arithmetic problems. The problem-solving model of scientific progress is of pragmatism which should be embraced, according to Laudan, because it is rational. The realist truth model of scientific progress, on the contrary, should be abandoned because it is irrational, for getting closer to truth is not the aim of science.

> I have tried to argue that the single most general cognitive aim of science is problem solving….. *the chief way of being scientifically reasonable or rational is to do whatever we can to maximize the progress of scientific research traditions*.
>
> (Laudan 1977, 124)

Laudan's view on scientific rationality is countenanced by structural realists. Structural realists are realists who only accept partial realism (Votsis 2004, 16), which is a version of realism that emphasizes on the mathematical or structural content of theories (Ladyman 1998, 409). However, structural realists are realists in the sense that they countenance the mind-independent world (Psillos 2001, S13).

Structural realists do not converge with realists on the correspondence theory of truth. Worrall holds that truth should not be a criterion for choosing a theory. Empirical adequacy, without truth, constitutes a good reason for accepting a theory.

> However, to explain the rationality of what goes on in science, there is no need to involve considerations of whether such a theoretical claim *is* true (indeed as we have been seeing such involvement poses major problems for ideas about rationality). Scientists should be seen as "accepting" theories, not as true, but only as *empirically adequate*.
>
> (Worrall 2002, 33)

Ladyman asserts that a structural realist approach to scientific theories is a semantic approach (Ladyman 1998, 416). Theories are the presentation of structures or models whose mathematical content is unchanged across theory change. Notwithstanding the empirical contents of a theory are changeable, the mathematical content is stable in the evolution of theories. Structural realists attempt to use the invariable mathematical content to refute Laudan's pessimistic induction. They claim that the invariable part of the theory preserves through theory change. A past theory which has been proved false may not necessarily induce a false successor theory, for both theories still share the common invariable structure.

Psillos is dubious about the defensibility of structural realism. He contends that the only knowable knowledge in structural realist account—the structure—is too ambiguous (Psillos 2001, S18-S19). Knowing the formal structure of a theory is insufficient for scientists, for one cannot use it to explain and predict the phenomena (Psillos 2001, S21). Psillos concludes that structural realists cannot establish the correspondence relationship between formal structure and natural structure of phenomena (Psillos 2001, S21-S22). Based on Psillos's assertion, it is apparent that the ambiguity of formal structure casts no light on the question of which rival theory is correct and thus favorable. In addition, the failure to establish a correspondence relation between formal structure of theories and natural phenomena renders it implausible for structural realists to apply a specific formal structure to the phenomena concerned. Theory choice based on the formal structure is impossible, for the correspondence relationship between formal structure and phenomena is ambiguous. There is no way to choose between rival theories because the structural relation between formal structure and natural structure is void.

> If structures are independent of an ontology of individuals and properties, then we cannot even speak of any structural relation (be it isomorphism, or embedding or what have you) between structures.
>
> (Psillos 2001, S22)

The implausibility of choosing between rival theories poses a threat to the structural realist's notion of theory change. Structural realists hold, in their rejection to Laudan's pessimistic induction, that the invariable formal structure of theory is preserved in the course of theory change. However, the predicament of theory choice, as depicted by Psillos, implies that scientific progress is impossible in structural realist account.

Dawid provides a structural realist account of string theory that is likely to refute Laudan's pessimistic induction. Dawid states that string theory is purely theoretical and void of empirical content (Dawid 2004). This theoretical form makes string theory a pure structure without a correspondence relation to reality. Dawid holds that if string theory is true, it is then the most fundamental theory in physics (Dawid 2004). String theory will become the most likely candidate of the final theory as it is a milestone toward the grand unification theory that unifies gravity, quantum mechanics, and particle physics (Zwiebach 2004, 6-8). Dawid implies that physical science will converge on the final truth if string theory is true, and thus it counters Laudan's pessimistic induction which claims that there is no reason to believe that the successor theories (string theory, in this case) are true if their predecessors are false.

However, Dawid's claim of the pure theoretical structure of string theory is rejected by physicists. String theory requires a ten-dimensional spacetime which is unable to be detected in experiment. Physicists disagree with Dawid that string theory is of pure theoretical structure, because the unobservable is posited by string theory. Physicists sometimes claim that string theory has already successfully predicted the effects of gravity (Zwiebach 2004, 10). As such, there is a correspondence relation between string theory and reality. Although thus far there has been no experimental verification of string theory due to the immaturity of experimental techniques, it is believed that experimental verification is possible in future (Zwiebach 2004, 8). The possibility of experimentation in future may possibly render string theory false in its prediction, and thus succumbs to the criticism of Laudan's pessimistic induction. For Laudan may use his pessimistic induction to argue that there is no way to assure that experimentally falsifiable string theory is closer to truth if the past successful predecessor has been proved false.

In short, Laudan holds that a theory should be chosen based on its problem-solving capability. A winning theory is not necessarily a true theory, but an effective theory in solving scientific problems. However, it is worth noted that Laudan does not deny that a winning theory can be a true theory as well as an effective problem-solver. In Laudan's account, the effectiveness of problem-solving is the primary and the only necessary criterion for rational theory choice.

3.6 Conclusion

This chapter explored four anti-realists' primary philosophical account (that of van Fraassen, Cartwright, Putnam and Laudan) and its relationship to the problem of theory choice. One of the salient common characteristics of anti-realists is the rejection of scientific realism. However, anti-realists do not appear as a relativist or a skeptic in their attitude towards the knowledge and truth. They hold that the realist belief of the existence of unobservable is untenable in terms of empiricism (as in the writings of Laudan) and unnecessary as a presupposition of a scientific theory (as held by van Fraassen). The rejection of unobservable by anti-realists is connected to their rejection of metaphysics. Van Fraassen holds that there is no way to empirically validate a metaphysical claim. Cartwright, as an antirealist of scientific laws whereas a realist of theoretical entities, asserts that metaphysical claim about the existence of a fundamental reality is false. She argues that the reality represented by model is complex and cannot be united into a single universal law.

The claim of the non-existence of a 'deep' reality is used by anti-realists to rebut the realist claim of a fundamental and unified reality. Deep reality, a term used by Hooker, denotes the underlying unobserved real entities that appear as causes of the observable phenomena (Hooker 1985). Cartwright, Putnam and Laudan assume the existence of a pluralistic reality in which the correspondence theory of truth fail to hold; while van Fraassen, though does not explicitly make his stance clear, apparently rejects the realist position of the fundamental reality. This difference between anti-realists and realists contributes largely to their position on the issue of theory choice. For anti-realists, deep reality does not assume a role in rational theory choice. Some anti-realist, including van Fraassen, even was agnostic about deep reality. They claim that scientists do not need to establish a strict one-to-one correspondence between an array of rival theories and the reality, for the so-called ultimate reality does not exist, hence the search for a true theory in terms of the correspondence to reality is a mistaken enterprise. As such, anti-realists are not exposed to the pressing situation to answer the question of underdetermination thesis. Indeed, an underdetermination thesis is espoused by anti-realists. This thesis maintains that crucial experiment is impossible because theory is underdetermined by the evidence. Unlike realists, anti-realists are not bothered by the problem of the impossibility of crucial experiment that has been posed by underdetermination thesis.

Anti-realists have a common tendency of resorting to historical development of science as a strategy when they are confronted with the problem of theory choice. In applying this strategy, anti-realists first relate to their claim of the non-existent of an ultimate reality. They argue further that this ultimate claim is of a priori, which cannot be validated empirically, and has never been supported by the history of science thus far. From this point, anti-realists push their argument further to assert that there is no perfect theory and model, even in principle (van Fraassen 2008, 45; Cartwright 2007c, 28; Laudan 1977). It is partly because of the recognition of the non-existent of crucial experiments in history (Lakatos 1978, 86). They claim that the problem of theory choice is always resolvable in the development of science, as the unfavorable theory which was chosen at a particular point of time was replaceable, without posing the problem of the rationality of choice. Lakatos even suggests using normatively interpreted history to evaluate two rival methodologies of science (Lakatos 1976).

Anti-realist's strategy in using the instances in history of science to account for the possibility of rational theory choice can be explained by the fact that they have upheld perspectivism in theory interpretation, which is subsequently leading to the claim that the chosen theory must not necessarily a true one in the realist sense. However, anti-realists do not hold a relativistic view in their perspectivism. They tend to see the possibility of the co-existence of equally fit theories that are rationally adoptable. For instance, Putnam's notion of conceptual relativism, also known as internal realism, is a sort of perspectivism that permits rational choice to be made between two equally fit theories.

The main attribute that distinguishes anti-realists from relativists is that the former recognizes the role of rationality in theory choice, while the latter does not. However, the rationality that assumed by anti-realists is not truth-oriented as in the realist sense. On the contrary, anti-realists' notion of rationality is conceptually broader in scope. For example, Laudan defines rationality loosely as good and sound reasons for a given action or belief (Laudan 1977, 123), which is a definition which will not be agreed upon by scientific realists. It is because, as asserted by Laudan, a good reason may not be sound in science, especially if one has good reason to accept a false theory (one such example given by Laudan is that one may have good reason for resurrecting the Ptolemaic theory if one is poor and the Vatican is awarding grants for such research). The fact that a broader definition of rationality has been given in anti-realist account relieves the dilemma of theory choice that is faced by scientific realists. Anti-realists do not need to be confined to arbitrating the true theory among its rivals, which is corresponding to the reality. On the contrary, anti-realists tend to defend their criteria of theory choice by detaching correspondence truth from the rationality of choice. As for Laudan, a choice is deemed rational if the chosen theory is more effective in problem-solving as compared to its rivals. In Laudan's account, problem-solving is a concept that is not equivalent to the correspondence between theory and reality; For van Fraassen, empirical adequacy is the criterion of theory choice. An empirically adequate theory fits the reality well at the level of phenomena, but there is no correspondence relationship with the unobservables. Van Fraassen's tenet of "acceptance is not belief" (van Fraassen 1985, 246) clearly illustrates that theory choice does not involve realist existential assumption of the unobservable entities and processes.

It is noteworthy to mention that anti-realists embrace some extent of pragmatism in their accounts for scientific theory and theory choice. For example, van Fraassen's account of theory acceptance is based on the good will in believing that the capability of a scientific explanation justifies the acceptance of a theory. For van Fraassen, a theory is accepted because there are pragmatic reasons to believe that it is 'true'. Similarly, Putnam asserts that scientific activity has its human perspective, implying that observers' role is a necessary factor in constructing scientific theory (Putnam 1990, 7). For Putnam, theory choice is rational so long as it is pragmatic. Multiple interpretations of theories are allowed—consequently, there are multiple possibilities in theory choice—because Putnam has rejected the correspondence theory of truth. In the face of rival theories, one may evaluate the merits of them from various perspectives (e.g. simplicity, robustness etc), based on the multiple interpretations of theories. Given an agreed criterion for theory choice, one may have different opinion from his peers, as they interpret the theories from their own perspective. One such example is the disagreement between Schrödinger and the Copenhagen school on quantum physics, which leads them to favor different theories. Putnam points out that "'the way the theory is understood' can't be discussed *within* the theory." (Putnam 1979, 136) Hence, if a theory can be understood in a pragmatic way beyond its content, it may as well be chosen based on its pragmatic merits.

Anti-realists are exposed to answering a specific question: Given an array of rival theories that equally fulfill the criterion of rationality (be it fulfilling van Fraassen's empirical adequacy, Laudan's effectiveness of problem-solving, Putnam's internal realism/pragmatism, or Cartwright's pragmatic approach in model choice), how does a choice to be made among them? The question is rather tricky to answer as anti-realists' definition of rationality is broad in scope. If the answer is that any one choice is acceptable in this situation, scientific realists may claim that the anti-realist criterion of theory choice fails to be decisive under certain circumstances. However, although anti-realists' solution to the problem of theory choice may not appear decisive, there is no threat of incurring relativism because rationality is the working principle of anti-realism.