CHAPTER 4: RELATIVIST ACCOUNTS OF THE PROBLEM OF THEORY CHOICE

4.1 Introduction

Relativism and realism are rival views of science. The history of relativism is as old as that of realism, both of which having their root in the Ancient Greek philosophy. Relativism, according to the historian of philosophy, can be traced back to the time of the Sophists. Protagoras, one of the greatest Sophists in the fifth century BC, who claimed that man is the measure of all things has set the basic principle for the relativism ever since—"there are two arguments on every subject" (Guthrie 1977, 24). According to Guthrie, a great historian of Ancient Greek philosophy, Protagoras's 'man-measure' claim is a form of "an extreme subjectivism according to which there was no reality behind and independent of appearances,…, and we are each the judge of our own impressions." (Guthrie 1977, 186). The criteria of knowledge, according to Protagoras and Greek Sophists, are relative to each man. It is conceivable that the criteria of theory choice, according to the Sophists' tenets, indeed lie in the subjectivity of the agents who adjudicate between rival scientific theories. As every man is the measure of things, there would be no objective and standardized criteria for theory choice. All conceivable criteria would be on an epistemic par.

Skepticism is always connected to relativism in one of the two ways: either (1) from skepticism to relativism or (2) from relativism to skepticism. The (1) is exhibited by those who doubt the existence of reality or the knowability of the reality, and go further to maintain that any theory that accounts for the reality is no matter of fact. This group of skeptics-relativists views scientific theory as a tool rather than emphasizing the dimension of objectivity/truth of a theory. The (2) is demonstrated by those who hold that the merits of scientific theory are framework-based. This group of relativists rejects the notion that there exists fundamental scientific theory across scientific communities and historical epochs. There is a danger to glide into skepticism when these relativists draw a conclusion that the reality cannot be disclosed neutrally (objectively) by any scientific theory. Notably, not many relativists are radical skeptics about reality and scientific theories. For radical skepticism can hardly be a consistent doctrine. It is always found self-refuting in the radical version of skepticism.

This chapter aims to discuss the doctrine of relativism and its connection to the problem of theory choice. I discuss Nelson Goodman's relativism at length in Section 4.2. His notions of world, world-versions, and world-making are elaborated in connection to the problem of theory choice. Section 4.3 is allocated for the discussion of Kuhn's relativism, with the emphasis on his notion of paradigm. The issues of theory choice are discussed in connection to different phases of science. The relation between incommensurability and theory choice is also discussed. Finally, Feyerabend's relativist position is elaborated in Section 4.4.

4.2 Goodman's Conception of World and World-versions

Goodman is a nominalist who espouses individuals (particulars) over classes (universals) (Eberle 2009, 451). In the classical debate between realists and nominalists since the middle age, these two camps converge on the point that substances are real. However, they disagree on the ontological status of properties. Realists hold that properties are real, which are universals in the sense that multiple instance of properties is possible (Campbell 2009, 606). Nominalists, however, claim that properties are not real, "being mere shadows cast by resemblance, or language, or habits of classification" (Campbell 2009, 606).

Goodman's nominalism adheres to the basic tenet of classical nominalism, and transcends to accept individuals of any kind, both of tangible and abstract (Eberle 2009, 451). Tangible and abstract individuals are real, according to Goodman, because they are conceptually human-made. This marks a difference between Goodman and other nominalists who maintain that individuals are naturally real. For Goodman, the world is made up of various individuals that can be conceived of, be it concrete or abstract, natural or manufactured. Such fabrication is done by human's mind. For Goodman, world cannot exist independently without cognitive activities (Nolt 2004, 71). World encompasses not only natural things and artificial things, such as mountain and chair. The Goodmanian world also comprises of abstract things such as numbers, fiction, arts and music. Unlike other philosopher such as Lewis who takes world as a function of propositions to truth-values (Berto 2010, 471), Goodman's does not take this semantic approach to account for the ontology of world.

However, Goodman does not hold that the everyday world, be it natural or artificial, is the ultimate reality. He views the everyday world in terms of a multiplicity of different worlds. A poet lives in the world of poetry, whereas a scientist lives in the world of science. Goodman stresses that he is by no means a modal realist who embraces the existence of possible worlds when he speaks of an array of multiple worlds. All worlds are actual worlds (Goodman 1978, 94). These actual worlds are not multiple alternatives reducible to a single actual world.

> We are not speaking in terms of multiple possible alternatives to a single actual world but of multiple actual worlds.
>
> (Goodman 1978, 2)

The world is made, not found (Goodman 1978, 22). It is noteworthy that Goodman does not account for the way to individualize the abstract individuals that constitute the world. Goodman concerns what can be made of abstract individuals rather than what are the basic constituents of them (Elgin 2009, 290) and what way to differentiate between them. Although Goodman does not make explicit what he means by abstract individuals, we can rightly assume that symbols, triangles, and musical notations that have been enumerated by him in *Ways of Worldmaking*, are examples of abstract individuals. Goodman's recognition of abstract individuals as real particulars in our residing world marks a divergence from the standard metaphysical claim that abstract individuals are mere possible that exist only in the possible world (Heller 2007, 78). This departure from the standard view of abstract individuals is also shared by Kripke in the context of the existence of fictional entities, where the latter are "in some sense" abstract entities that exist in our concrete world (Thomasson 2009, 14-15). The difference between Goodman and Kripke is that the latter claims that fictional entities are abstract entities existing contingently in a concrete activity of writing or story telling (Thomasson 2009, 15), while the former does not explicitly attribute existential contingency to abstract individuals.

However, Goodman implicitly implies that abstract individuals can be expressed only through tangible medium, such as sounds, pictures, and words. The example given is the abstract paintings that have no subject which "may still *refer* by exemplification or expression" (Goodman 1978, 105). All of the abstract individuals lie within the actual world where human reside (Goodman 1978, 104).

> Fiction, then, whether written or painted or acted, applies truly neither to nothing nor to diaphanous possible worlds but, albeit metaphorically, to actual worlds.
> (Goodman 1978, 104)

Goodman's concept of abstract individuals is not purely formal objects as numbers are. It shares the characteristics of "earthy abstracta" proposed by Peter Simons, which implies that the ontological difference between abstract individuals is the difference of materiality in connection to their relevant concrete domain (such as music and art works) in the actual world (Simons 2003, 491-492). That is to say, abstract individuals can be instantiated as concrete individuals. For both Goodman and Peter Simons, the difference between abstract individuals can be identified in their different ways of concrete instantiation (e.g. Beethoven's third symphony and fifth symphony can be differentiated when they have been instantiated by musicians, viz., being performed in a concert). Interpreting Goodman's concept of abstract individuals with this "earthy" flavor (i.e. abstract individuals can be instantiated as concrete individuals) may save him from being accused as a self-contradictory nominalist, for abstract individuals are universal substances that embraced by realists.

According to Goodman, saying that individuals and worlds are made is not amounting to holding that (i) they are mere artifacts without natural existence; (ii) one can create individuals and worlds in any arbitrary way. Goodman does not say that an individual, for instance, a mountain is human-made, in such a way that the individual (eg. Mountain) that fits into the world is fabricated. Worlds are made "by making versions" (Goodman 1978, 94). It is the versions that are made prior to any scientific activity and consequently determine the worlds one resides. Hence, Goodman has placed an emphasis on the conceptual context of the worlds—versions—rather than the entities in the worlds. The versions are abstract individuals in the Goodmanian sense; whereas the entities in the worlds, which are the contents of the versions, are the instantiated concrete individuals. This interpretation of versions-contents relation is inline with Goodman's nominalism. In fact, the contents are dependent on the versions not only ontologically, but their meanings are also determined by the versions. The contents of the versions are causally connected to the contents of the worlds. It can be concluded that when Goodman uses the term "worldmaking", he implies "version-making", or "world-version making" to be more specific. We may reasonably question how it is possible for Goodman's world-version not to be a concept of class (universal), which is supposed to be rejected if he is a serious nominalist. However, Goodman seems taking this issue lightly by treating anything, perhaps including the concept of world-version, as an individual. Goodman's strategy is to reduce all substances, be it concrete or abstract, to "physical particles or phenomenal elements" (Goodman 1978, 95). However, such strategy is not successful in defending the universal-like concept of world-versions as a consistent nominalist tenet, because world-versions are not substances (that is, it cannot be localized in time and space). Goodman is aware of this plight as he declares that he does not impose nominalistic restrictions in his exposition of worldmaking (Goodman 1978, 95).

> I am sometimes asked how my relativism can be reconciled with my nominalism. The answer is easy. Although a nominalistic system speaks only of individuals, banning all talk of classes, it may take *anything whatever* as an individual. [my emphasis]
>
> (Goodman 1978, 94)

> Nominalism of itself thus authorizes an abundance of alternative versions based on physical particles or phenomenal elements or ordinary things or whatever else one is willing to take as individuals.
>
> (Goodman 1978, 95)

Worlds are fabricated in a way that conforms to the versions, which are the frame of reference. Version as a form has world as its content. Such form and content are bound to form the world-version, which is a frame of reference fabricated by human before any intellectual activity can be carried out. However, world-versions are not arbitrarily made. The fabricated worlds are "actual words made by and answering to true or right versions." (Goodman 1978, 94) True or right versions are made based on the entities in our residing world, such as "physical particles or phenomenal elements" (Goodman 1978, 95). Goodman proceeds further to claim that right versions may also be based on "whatever else one is willing to take as individuals" in the version-making (Goodman 1978, 95). This implies that although Goodman holds that his acceptance of countless right world-versions "does not mean that everything goes" (Goodman 1978, 94), his willingness to allow any individual things to be used as the constituents of world-versions is still amounting to radical relativism. Granting that anything can be the constituents of world-versions, Goodman claims that worldmaking may culminate in different world-versions.

> This does not mean….that worlds are built from scratch. We start, on any occasion, with some old version or world that we have on hand and that we are stuck with until we have the determination and skill to remake it into a new one… Worldmaking begins with one version and ends with another.
>
> (Goodman 1978, 97)

> The uniformity of nature we marvel at or the unreliability we protest belongs to a world of our own making.
>
> (Goodman 1978, 10)

Goodmanian world-version rejects the notion of an ultimate reality and a single interpretation of the world. Instead, the Goodmanian world-version allows a pluralistic way of interpreting the world and its constituents. Goodman rejects the reduction of pluralistic world-versions to a single world (Goodman 1978, 4). Pluralistic world-versions, which are independent from one another, have independent importance and thus irreducible to the world-version of physics (Goodman 1978, 4-5). Notably, multiple world-versions are not always homologous. They are permissible to be in conflict.

> The pluralists' acceptance of versions other than physics implies no relaxation of rigor but a recognition that standards different from yet no less exacting than those applied in science are appropriate for appraising what is conveyed in perceptual or pictorial or literary versions.
>
> (Goodman 1978, 5)

> In one world there may be many kinds serving different purposes; but conflicting purposes may take for irreconcilable accents and contrasting worlds… Grue cannot be a relevant kind for induction in the same world as green, for that would preclude some of the decisions, right or wrong, that constitute inductive inference.
>
> (Goodman 1978, 11)

The irreducibility of pluralistic world-versions to a single fundamental world-version identifies Goodman as a relativist. The irreducibility of pluralistic world-versions implies that there are no unique truth and objective reality, where this absence of uniqueness is the core doctrine of relativism (Gellner 1982, 183). For Goodman, reductionist approach is impossible because it undermines the diverse-yet-integrated world-versions that one possesses. In a relativist's account, world-versions are conceptual schemes, which are sets of fundamental beliefs about the world (Baghramian 2004, 165). The reductionist approach that undermines the organic world-versions will thus cast partiality in one's world-version.

> To demand full and sole reducibility to physics or any other one version is to forego nearly all other versions.
>
> (Goodman 1978, 5)

World-version is taken by Goodman as a function of how worlds are made, with the individuals in the worlds assuming the role of variables. By dissolving individuals into the function of world-version, Goodman has abstained from answering the problem of individualization of abstract individuals that he claims exist.

> With false hope of a firm foundation gone, with the world displaced by worlds that are but versions, with substance dissolved into function…… we face the questions how worlds are made, tested, and known.
>
> (Goodman 1978, 7)

Goodman constructs a theory of pluralistic world-versions by dissolving substance (individual) into function. World-version as function implies conceptual frameworks and properties. As a nominalist, Goodman is not supposed to endorse the existence of properties. Instead, Goodman's endorsement of pluralistic world-versions also implies an implicit endorsement of the multiplicity of properties, which is a metaphysical realist doctrine of the existence of universals. According to metaphysical realist, universal as a shared entity encompasses the properties of individuals and the kind to which they belong (Loux 2006). The conception of world-version which presupposes a multiplicity of properties has allowed metaphysical realism to sneak into Goodman's nominalist thought.

> Talk of unstructured content or an unconceptualized given or a substratum without properties is self-defeating; for the talk imposes structure, conceptualizes, ascribes properties.
>
> (Goodman 1978, 6)

> Predicates, pictures, other labels, schemata, survive want of application, but content vanishes without form.
>
> (Goodman 1978, 6)

Content consists of individuals or substances. It is this content, which is also known as fact, that constitutes the world. In the post-linguistic turn in the analytic tradition, facts are truth-bearers that have no independent existence in the world (Hossack 2007, 32). However, Goodman's conception of facts is departing from this traditional standard interpretation, for he speaks of physical and perceptual facts that are independent of language.

Although "facts constitute the one and only real world" (Goodman 1978, 91), many facts that fit the past may diverge in the future, as stated by Goodman (Nozick 1993, 123). The belief in the divergence of past facts into different future facts thus reasonably accounts for the existence of multiple worlds and multiple world-versions. For Goodman, worlds are made from other worlds (Goodman 1978, 6).

> The many stuffs—matter, energy, waves, phenomena—that worlds are made of are made along with the worlds. But made from what? Not from nothing, after all, but *from other worlds*.
>
> (Goodman 1978, 6)

The existence of multiple worlds and world-versions can also be accounted from the perspective of culture and society. Men live in different society who inherited different cultural background see things differently. Furthermore, "worlds may differ in that not everything belonging to one belongs to the other" (Goodman 1978, 8). Cultural or societal impact on the multiplicity of worlds and world-versions is an unavoidable practical need (Goodman 1978, 9). Though Goodman does not say much about the actual constructional process of worldmaking, Hacking insists that Goodman's constructivist conception of worldmaking ought to be interpreted as a social process because it is people who make the world (Hacking 1999, 45). Recently, Boghossian has identified this constructivist approach as "constructivism about facts" which states that "all facts are socially constructed in a way that reflects our contingent needs and interests." (Boghossian 2008, 377)

> Eskimo who has not grasped the comprehensive concept of snow differs not only from the world of the Samoan but also from the world of the New Englander who has not grasped the Eskimo's distinctions.
>
> (Goodman 1978, 9)

> Since the constructions in question are socially contingent, it looks as though one society may construct the fact that p even while another one constructs the fact that not-p. But it couldn't be the case both that p and not-p.
>
> (Boghossian 2008, 378)

Goodman points out that "we make worlds by making versions" (Goodman 1978, 94). Worlds are constructed through the lens of world-versions. Without the latter, one cannot perceive and know the former. One may rebut by giving an example, making an analogy between worlds and buildings, and between world-versions and blueprints, that building can be perceived or known without a blueprint. It is true to say so, to a certain extent, that one does not need to have a detailed blueprint in mind prior to perceiving a building, but it is not true to claim that one needs not to possess the idea of what constitutes a building. Such idea, which is the Goodmanian world-version, defines the nature of a building. Without a world-version of police station, one may not be able to differentiate between police station and other buildings (say, a hospital). More seriously, without world-version, one cannot live a normal life because he is unable to perceive something *as* something. To illustrate, a cat on the street may perceive the building of a police station; yet this animal is unable to perceive that building *as* a police station because it does not have a Goodmanian world-version.

Facts are the contents of world-versions, which is relative from one world to another. Goodman repudiates that there are absolute and objective facts. Instead, facts are abundant and varied in the world. Therefore, the world can be perceived in various ways by different perceivers. However, Goodman permits the similarity of facts and world-versions (Godfrey-Smith 2009, 106) in order to dampen down the occurrence of exclusively inconsistent and conflict worlds. Facts are translatable into each other because most of them are either similar or not exclusively incommensurable. If the facts are mutually translatable into each other, they are commensurable varieties of the same fact.

> The chapter before began with the rather reproachful question “Can’t you see what’s before you?”, and arrived at the illuminating answer “That depends…”…. one thing it depends on heavily is the answer to still another question: “What do you make of it?”
>
> (Goodman 1978, 91)

> Did the sun set a while ago or did the earth rise? Does the sun go around the earth or the earth go around the sun? Nowadays, we nonchalantly deal with what was once a life-and-death issue by saying that the answer depends on the framework….the geocentric and the heliocentric versions, while speaking of the same particular objects—the sun, moon, and planets—attribute very different motions to these objects. Still, we may say the two versions deal with the same facts if we mean by this that they not only speak of the same objects but are also routinely translatable each into the other.
>
> (Goodman 1978, 93)

Goodman’s conception of world-version is a contextualist approach. Two scientists may make observation with different world-versions, reaching at different theories, while working with the same facts of the same world. Chappell has distinguished two types of contextualist, namely of semantic and of inferential (Chappell 2008, 532). Semantic contextualists hold that the meanings of epistemic words such as ‘know’ vary across contexts; while inferential contextualists hold that the epistemic problem varies across contexts primarily due to the variation in the constitution of contexts (Chappell 2008, 532). Goodman is apparently an inferential contextualist, for he holds that multiple worlds—as the constitution of world-versions— exist. Goodman argues that both geocentric and heliocentric world-versions are right versions based on their own context of argument. He reaches an extreme conclusion that one may adopt any world-version and theory as one deems fit to suit her purposes.

> In practice, of course, we draw the line wherever we like, and change it as often as suits our purpose. On the level of theory, we flit back and forth between extremes as blithely as a physicist between particle and field theories.

(Goodman 1978, 119)

As a contextualist who holds that there are multiple right world-versions that exist legitimately, Goodman inclines to embrace content relativism. According to Cappelen, content relativism states that interpretation of content/fact varies across interpreters (Cappelen 2008). An observed fact may be interpreted differently by two scientists who are working with different world-versions. A physicist may interpret the term "time" in the context of Newtonian linear framework while his colleague may interpret the same term in the context of Einsteinian spatial-temporal framework. Following Goodman's train of thought, a scientist may switch between different world-versions, but he cannot hold multiple conflicting world-versions simultaneously about the same fact. It is because the same fact should exist in the same world, and conflicting world-versions imply the conflict between different worlds (Goodman 1978, 116). A particular investigated fact is not permissible to exist in two different possible worlds (but it is permissible to exist in different actual worlds, such as world of science and arts), for Goodman rejects the existence of possible worlds. When confronted with the plight of world-version choice, the scientist will go through a process called reflective equilibrium, which is a process of adjustment to achieve a stable coherence among different judgements (Goodman 1953; cited Kelly 2005). Hence, at least at the high level, choice among competing world-versions is possible by using the method of reflective equilibrium—which is a concept introduced in Goodman's *Fact, Fiction, and Forecast* (surprisingly, it was disappeared in the later *Ways of Worldmaking*). Reflective equilibrium is a method that operates contextually (Kelly 2005, 185), which is perfectly apt to be applied as a strategy to choose among an array of rival world-versions.

However, Goodman provides no clue to implementing reflective equilibrium when one is confronted with conflicting world-versions about the same fact.

4.2.1 Ways of World-making

Goodman elaborates at length five ways of world-making. Although world-making is a process that discards the fundamentalist conception of truth, one should not deliberately ignore the place of truth in world-making. The general notion of truth is sometimes a required but insufficient standard in the process of world-making. Furthermore, truth is relative to the worlds.

> Insofar as a version is verbal and consists of statements, truth may be relevant. But truth cannot be defined or tested by agreement with 'the world'; for not only do truths differ for different worlds but the nature of agreement between a version and a world apart from it is notoriously nebulous.
>
> (Goodman 1978, 17)

The nebulous nature of agreement between a world-version and its relevant world allows flexibility in the way of world-making. Such obscurity is the immediate result of the absence of an ultimate truth. There is no ultimate standard in making a world. Hence, a multiplicity of worlds can co-exist legitimately.

Although Goodman allows the multiplicity of worlds to be fabricated, he stresses the importance of creating right versions of worlds. 'Right version' does not mean 'true version'. For Goodman, a right version of world means an apt version that suits the purpose of world-making. For example, the world with the version of molecules is not the apt world for the everyday world (Goodman 1978, 21), yet it is relevant only to the physicist world.

> That right versions and actual worlds are many does not obliterate the distinction between right and wrong versions, does not recognize merely possible worlds answering to wrong versions, and does not imply that all right alternatives are equally good for every or indeed for any purpose.
>
> (Goodman 1978, 20-21)

Thus, when speaking of ways of world-making, Goodman emphasizes on the ways of making right version of world for the right purpose. The rightness is a relative standard which does not preclude conflicting world-versions.

> The dramatically contrasting versions of the world can of course be relativized: each is right under a given system—for a given science, a given artist, or a given perceiver and situation.
>
> (Goodman 1978, 3)

A right world-version is important because it determines the rightness of the world. Goodman holds that there is no foundation for us to arbitrate the rightness of a world without the lens of a right world-version. However, there is no objective way to arbitrate the rightness of a world-version. The only standard for rightness is a pragmatic one—fitting the purpose of the person who lives in that world.

> Yet doesn't a right version differ from a wrong one just in applying to the world, so that rightness itself depends upon and implies a world? We might better say that 'the world' depends upon rightness. We cannot test a version by comparing it with a world undescribed, undepicted, unperceived….. While we may speak of determining what versions are right as 'learning about the world', 'the world' supposedly being that which all right versions describe, all we learn about the world is contained in right versions of it.
>
> (Goodman 1978, 3-4)

Goodman's treatment of the notion of world and world-version inherits much of the continental philosophical style, which can hardly be analyzable to a specific referent. He claims that there are five indispensable ways of world-making. Firstly, world can be made by "composition and decomposition" (Goodman 1978, 7-10). It is an approach that requires one to assemble and dismantle facts. A distinction between part and whole needs to be drawn in the first place. In orthodox account, part-whole relation is applied to any kind of objects (Uzquiano 2006, 137), being physical or abstract, observable or unobservable. Goodman raises no argument against this orthodox view. On the one hand, connection between parts should be identified in order to recognize the whole; On the other hand, identifying features of the part is the way to decomposing the whole. Both composition and decomposition requires identification of the relationship between facts. It is made possible "by the application of labels: names, predicates, gestures, pictures, etc." (Goodman 1978, 7-8) Goodman's suggestion of using proper name for the labels in composing and decomposing the world is consistent with his nominalism.

World can be made by composing and decomposing. However, Goodman holds that not every single composing or decomposing activity is tantamount to creating a brand new world. This may prevent the existence of an infinite number of worlds. What Goodman expects is the construction of right worlds, not numerous worlds which do not serve the purpose of their creator.

> We do not make a new world every time we take things apart or put them together in another way.
>
> (Goodman 1978, 8)

Goodman treats pictures and predicates equally important in the process of world-making. Along with gestures and names, they are labels that one can use to compose and decompose the world. These labels denote the facts uniquely (Goodman 1978, 102) when they are decomposed; while denote generally when they are composed.

> Thus pictures may make and present facts and participate in worldmaking in much the same way as do terms.
>
> (Goodman 1978, 8)

The right world that is constructed with the aid of any labels—predicates, gestures, pictures, etc—has a salient feature of consistency. There is consistency if one's world exhibits regularity.

> Only so, for example, do our observations of emeralds exhibit any regularity and confirm that all emeralds are green rather than that all are grue… The uniformity of nature we marvel at or the unreliability we protest belongs to a world of our own making.
>
> (Goodman 1978, 10)

Thus, consistency is a measure that determines if the right world is made by the ways of composing and decomposing. It is apparent that two scientists may hold rival yet consistent (at least, partially) theories. According to Goodman's way of world-making, both rival theories are right (Goodman avoids using the term 'true') and worth pursuing as long as internal consistency can be achieved by each of them.

> …. worlds differ in the relevant kinds they comprise.
>
> (Goodman 1978, 10)

So, the world of scientist *A* differs from that of scientist *B* in the sense that they subscribes to different theories. Rival theories are different in the sense that they have different theoretical contents. However, they are relevant in the sense that both are relevant to the same facts. Hence, rival theories are relatively right (Goodman foregoes the term ‘true’) theories if they are internally consistent and relevant to the facts. Theory consistency and fact relevancy are the criteria of theory choice.

The second way of world-making proposed by Goodman is a process called weighting. It is a process of placing the emphasis differently in world-making. Certain facts are emphasized more than others, for they are more relevant for the intended world.

> While we may say that in the cases discussed some relevant kinds of one world are missing from another, we might perhaps better say that the two worlds contain just the same classes sorted differently into relevant and irrelevant kinds.
>
> (Goodman 1978, 10)

Weighting is also a process of changing interests and developing new insights (Goodman 1978, 11). New insights can always be obtained when there is a shift in interest. Consequently, a new world is made from the old one. The difference between geocentric and heliocentric views is the difference of emphasis rather than the difference in entities. Both views are right from their own perspective.

> Some relevant kinds of the one world, rather than being absent from the other, are present as irrelevant kinds; some differences among worlds are not so much in entities comprised as in emphasis or accent, and these differences are no less consequential.
>
> (Goodman 1978, 11)

> With changing interests and new insights, the visual weighting of features of bulk or line or stance or light alters, and yesterday’s level world seems strangely perverted—yesterday’s realistic calendar landscape becomes a repulsive caricature.
>
> (Goodman 1978, 11)

Goodman enumerates the histories of Renaissance as another example:

> one that, without excluding the battles, stresses the arts; and another that, without excluding the arts, stresses the battles. This difference in style is a difference in weighting that gives us two different Renaissance worlds.
>
> (Goodman 1978, 101-102)

These different Renaissance worlds are different in world-versions yet converging on the same facts. Both historical accounts of the Renaissance admit the facts of the battles and the arts. However, they assign different weightage to these facts, by so doing the worlds of Renaissance are created in different ways. The Renaissance world of arts places heavier weight on the aspect of arts, while largely ignoring the war affairs. The Renaissance world of battle places heavier weight on the aspect of war affairs, while ignoring the achievement of arts. Due to this difference in weighting, the world-versions of historians vary. It is possible that the theories about Renaissance are contradictory among historians who hold different world-versions. However, these conflicting theories are right theories if they describe the facts that do exist in the Renaissance time. Goodman may think that the conflicting theories, which are the conflicting world-versions, are the right theories as they are the versions of the world on which they converge. Following Goodman’s line of thought, a historian is free to construct any theory about the past by using weighting approach on the facts. Theory choice does not appear as a problem as long as the choice is to be made based on the relevant world.

Ordering is the third way of world-making. Far before his publication of *Ways of Worldmaking* in 1978, Goodman has recognized the significance of ordering as an opportunity for cognitive advancement in his 1954's masterpiece *Fact, Fiction, and Forecast* (Elgin 2000, 13). Ordering is a process where men apply a frame of reference on a set of same entities. An entity can be ordered in different way by different persons in their perception and cognition. Different orderings inevitably result in different world-versions. There is no issue of primitive or derivative world-version generated from ordering. The apparent effect is the different constructional systems of the world. They "differ in order of derivation" (Goodman 1978, 12) from the same entity of the world.

Ordering as a way of world-making is somehow radical. Goodman denotes radical ordering as a way of reconstructing the world-version that is unbounded by a primitive guideline. According to Goodman, there is no absolute guideline of ordering in perceptive and cognitive activity. Goodman uses the construction of a comprehensive image of a city as an example of radical ordering (Goodman 1978, 13). The information about the city is temporally, spatially, and qualitatively heterogeneous. There exists no single guideline of ordering to reconstructing the comprehensive information of a city. Different ordering of city reconstruction will yield different image of the city. Besides, radical ordering is also exemplified in the map reading (Goodman 1978, 13). Different spatial order in reading a map results in different temporal sequence of a trip.

Although radical ordering is unbounded by any specific guideline, it is not carried out without any guideline at all. At the minimal, ordering is made possible provided that it adheres to the "suitable arrangements and groupings" (Goodman 1978, 13), that is, the ordering must be confined to the nature of the ordered entities in the world. A typical example can be found in map reading, an activity in which the ordering of a route is confined by the spatial order. The route can be reasonably constructed in any order as long as the ordering is carried out in terms of the spatial order (left, right, south, and etc). Non-spatial order in map reading for street direction, such as the order of brightness of the color on the map, is an instance of wrong ordering. For it is not a conventional way to use the color brightness as a guide to search for the street direction. A legitimate ordering activity consists of the choice of the right order type, based on the nature of the ordered entity. As such, travelers are deemed rational in choosing any spatial order in map reading. They are irrational if their choice of route is based on the non-spatial order, such as the order of brightness.

The fourth way of world-making is deletion and supplementation. It is an approach of modification that applied on the created world. It is to be adopted to suit the purpose of a world-maker by polishing the created world in his desired form.

> ….the making of one world out of another usually involves some extensive weeding out and filling—actual excision of some old and supply of some new material.
>
> (Goodman 1978, 14)

Goodman contends that deletion and supplementation are common in everyday life, art works and scientific practices (Goodman 1978, 14-15). This approach is carried out subconsciously in everyday life and deliberately in the scientific practice. One tends to dismiss what he has perceived as illusory or negligible if it does not fit into his world-version (Goodman 1978, 15). Scientists, on the other hand, tend to deliberately reject or purify the entities and events of the world in order to conform to his world-version

(Goodman 1978, 15). Nonetheless, deletion and supplementation as ways of world-making must conform to the coherence of world-version.

Deletion and supplementation imply choice which must be cohered with the existing world-version. Goodman illustrates the replacement of analog system by digital system as an example of deletion. He argues that the use of digital thermometer with readings in tenths of a degree is a deletion of the temperature readings that lie between 90 and 90.1 degrees (Goodman 1978, 15). As for the case of supplementation, Goodman states that experiment has shown that observers creatively perceive two spots of light as moving along a path when they are flashed in quick succession (Goodman 1978, 15-16). The perceived phenomenon of moving light is a supplementation to the perceived flash of light at two distant spots.

The fifth, and the last, way of world-making is deformation. Deformation is defined as correction or distortion of the constructed world-version (Goodman 1978, 16). Deformation is adopted by scientists in re-adjusting the curve of a graph in order to fit the data (Goodman 1978, 16). This approach of world-making is similar to the approach of deletion and supplementation. It is carried out pragmatically to perfect the constructed world-version.

4.2.2 The Problem of Theory Choice in Goodman's Relativism

Apparently, Goodman's constructivism is a version of contextualism. The context of world-making involves perception, cognition and social factors. Hence, it is impossible to create an objective world-version which is faithfully corresponding to the reality-as-it-is. Indeed, such objective world-version is repudiated by Goodman in his stance of anti-fundamentalism. Firstly, Goodman holds that there is no single fundamental reality and fact as they are diverse in multitude. Goodman rejects the idea that there is an ultimate reality in which all other facts supervene on it. Secondly, Goodman holds that all facts that constitute world-version are fabricated. Although the fabrication of world-version follows five general ways as outlined in Section 4.2.1, Goodman denies that two persons may always obtain the identical world-version using the same facts as the material for construction.

The rejection of a single objective world-version does not lead Goodman to deny the existence of a mind-independent reality. The non-existence of an ultimate reality implies that the constituents of reality exist on the same par in terms of ontology, where conflict of worlds is inevitable. A multiplicity of possibly conflicting world-versions created with different purposes is also permitted. A wrong world-version should not be chosen, according to Goodman, from the rival world-versions. Although the wrong world-versions are unfavorable, right yet conflicting world-versions are permitted.

> Under "rightness" I include, along with truth, standards of acceptability that sometimes supplement or even compete with truth where it applies…
>
> (Goodman 1978, 109-110)

Goodman does not worry about the flowering of competing right world-versions. As a pluralist, he holds that "conflicting versions often present good and equal claims to truth" (Goodman 1978, 110). At the semantic level, conflicting world-versions render the respective statements in a state of conflict. Conflicting statements about two conflicting world-versions cannot be both true. One possible solution is to permit the conflicting statements to be true in different possible worlds.

> We can hardly take conflicting statements as true in the same world without admitting all statements whatsoever (since all follow from any contradiction) as true in the same world, and that world itself as impossible. Thus we must either reject one of two ostensibly conflicting versions as false, or take them as true in different worlds…
>
> (Goodman 1978, 110)

An advocate of possible worlds does not face problem in choosing among conflicting world-versions. For, she may contend that these conflicting world-versions reflect a clash in the world we inhabit, but they are true and non-conflict if they reflect multiple possible worlds which are different from our actual world. She may follow David Lewis in holding that such possible worlds are no less actual than the world we inhabit, for "the inhabitants of other worlds may truly call their own worlds actual" (Lewis 2001, 161), and importantly, 'actual' is indexical (Lewis 2001, 161). Each possible world consists of one world-version, and an alternative world-version implies the existence of another possible world. Thus, the problem of choice of world-versions does not arise for the advocate of possible world because there is only one world-version in each possible world. Since Goodman repudiates the theory of possible world, two conflicting statements are not to be taken as true in different possible worlds.

Goodman's rejection of the theory of possible world is a metaphysical rather than a semantic stance, that is, he denies the existence of the worlds which are different from the one we inhabit. In spite of such metaphysical rejection, Goodman is a proponent of epistemic possible worlds—though he does not make explicit of it. The theory of epistemic possible worlds dictates the varied epistemic possibilities of one's knowledge about the world. A statement or theory may bear different epistemic meanings in different epistemic context—that is, a theory can manifest many distinct epistemic possible worlds. Notably, an advocate of the epistemic possible worlds does not need to be a modal realist who holds fast to the existence of metaphysical possible worlds.

> In addition to metaphysical possibilities—ways the world might have been—there are also epistemic possibilities—ways the world might be, for all we know. For example, it is now widely accepted that however things might have been, water would still be H_2O: that is, it is metaphysically necessary that water is H_2O. However, for all people in the eighteenth century knew, water was not H_2O: that is, it was epistemically possible that water was not H_2O.
>
> (Whittle 2009, 265)

In the epistemic context of the eighteenth century, the progress of science was not up to the level to enabling scientists to recognize water as H_2O. According to the theorists of epistemic possible worlds, the claim that "water is not H_2O" is legitimate in the epistemic context of the eighteenth century. Water could be possibly and legitimately recognized as a compound of anything which is other than the chemical bonding between Hydrogen and Oxygen atoms. Advocates of epistemic possible worlds can be divided into two groups: one holds that the notion of epistemic possibility is subject-relative; another holds that this notion is subject-independent (Whittle 2009, 269). The former group is typical relativists who claim that there is no consensus about the truth of a proposition or theory, for the epistemic possible worlds are relative to subject. For example, in the eighteenth century, scientist *A* may hold that water is a compound *P* while scientist *B* may assert that water is a compound *Q*, where *P* and *Q* are not H_2O. According to the relativist of epistemic possible worlds, both claims are legitimate. The proponents of rival theories do not necessarily share a common belief. Choice between rival theories can be made arbitrarily in the epistemic context of the eighteenth century. On the contrary, the advocates of subject-independent notion of epistemic possibility are non-relativists who hold that there is a common belief among different people on a proposition or theory (Whittle 2009, 269). Choice between rival theories is thus constrained by the shared belief.

Goodman belongs to the camp of relativist of epistemic possible worlds. Two apparently conflicting propositions can be both true in their respective epistemic possible worlds. He asserts that sometimes propositions appear to be in conflict because they are elliptical (Goodman 1978, 111). The conflict can be resolved if the propositions are expanded by the addition of more details.

> Statements affirming that all soldiers are equipped with bows and arrows and that none are so equipped are both true—for soldiers of different eras; the statements that the Parthenon is intact and that it is ruined are both true—for different temporal parts of the building; and the statement that the apple is white and that it is red are both true—for different spatial parts of the apple.
>
> (Goodman 1978, 111)

Goodman thinks that it is epistemically possible that two conflicting propositions describing the same object are both true. Proposition *P1* "Parthenon is intact" and proposition *P2* "Parthenon is ruined" appear contradictory. It is so because they are elliptical, held Goodman. The conflict is resolved when a temporal perspective is introduced. Temporal perspective is part of world-version. By adding the temporal details to these conflicting propositions, the original elliptical propositions are expanded, and become different in meaning. *P1* can be expanded to a new proposition *P1'* "Parthenon is intact at the time of Socrates"; while *P2* can be expanded to *P2'* "Parthenon is ruined in 2010". The expanded proposition *P1'* and *P2'* resolve the conflict between *P1* and *P2*.

According to Goodman, choice between conflicting propositions or world-versions is impossible, as in the case of *P1* and *P2*. It is because these elliptical propositions cannot be both true in our residing world. For theory choice to be possible, the original propositions or world-versions have to be expanded by adding more details. Such addition implies the adoption of additional perspective (i.e. of temporal, spatial etc)—that is, additional world-versions. Multiple perspectives can be added to the original propositions or world-versions, as Goodman holds that one world is always made from other worlds, and world-making is a remaking (Goodman 1978, 6). However, Goodman has recognized that this solution is an ad hoc approach, because the expanded propositions always carry different meaning from the original one. The expanded propositions "speak of different things or different parts of things" (Goodman 1978, 111). Furthermore, triviality of proposition or world-version is introduced in Goodman's solution. The original *P1* and *P2* may be expanded into infinitely many true proposition $P1^n$ and $P2^n$, rendering the choice trivial too. It is because Goodman confers all right proposition/world-version an equal epistemic status.

The choice between two propositions/theories shall be made within the respective world-version. It is illustrated explicitly by Goodman in his example of the motion of the earth.

> On the face of it, the two statements
>
> (1) The earth always stands still
> (2) The earth dances the role of Petrouchka
>
> conflict since the negate of each follows from the other. And they seem to be about the same earth. Yet each is true—within an appropriate system.
>
> (Goodman 1978, 111)

Goodman claims that statement (1) and (2) are elliptical and thus can be "expanded by explicit relativization" (Goodman 1978, 112) within the respective world-version. Goodman expands (1) and (2) into (3) and (4), respectively, as below:

> (3) In the Ptolemaic system, the earth stands always still
> (4) In a certain Stravinsky-Fokine-like system, the earth dances the role of Petrouchka.
>
> (Goodman 1978, 112)

The new proposition (3) and (4) mean differently from (1) and (2), because the addition of extra details to the former has altered the original meaning of the latter. The proposition (3) and (4) do not tell us if the earth actually moves. What they claim is that the answer to the question of motion/motionless of earth depends on a particular world-version/perspective (either Ptolemaic or Stravinsky-Fokine-like system). Without these added world-version, choice between (1) and (2) does not have a rational ground. It is because (1) and (2) are conflicting, and the fact that both propositions are epistemically possible makes them possess equal epistemic status.

The choice is possible within a particular world-version. Taking the world-version of Ptolemaic system as illustrated in proposition (3), suppose that two scientists arrive at two conflicting propositions:

(3a) In the Ptolemaic system, the sun moves.

(3b) In the Ptolemaic system, the sun stands still.

It is apparent that proposition (3a) and (3b) are derived from (3), and they are subsumed under the same world-version—the Ptolemaic system. Using Goodman's terminology, (3a) and (3b) "fit" to the Ptolemaic world-version. Rational choice between (3a) and (3b) is possible within the Ptolemaic world-version, because the same world-version is used as the background for choice. Hence, one can reasonably conclude that, within the world-version of the Ptolemaic system, proposition (3a) is true while (3b) is false. It is because (3a) is consistent with the geocentric doctrine of the Ptolemaic system. Hence, (3a) is more favorable to be chosen over (3b) in the world-version of the Ptolemaic system.

According to Goodman, world-versions are distinct. They can be either conflicting or non-conflicting. Notwithstanding for the case of non-conflicting world-versions, they may not necessarily manifest the overlapping content. Using Kuhn's term, distinct world-versions may appear to be incommensurable. Hence, it is impossible to make a meaningful point-to-point comparison between two world-versions. It is because such comparison requires one to take a perspective—a higher world-version, without which the comparison cannot be carried out. When a higher world-version is adopted as a perspective for comparison, issue of the distinctiveness of this higher world-version raises in the same way as faced by those world-versions under comparison. The higher world-version, which serves as a framework for specific comparison between two world-versions, does not warrant the plausibility of point-to-point comparison. Furthermore, meaningful comparison between distinctive world-versions is hampered by the infinite recursive steps to higher world-version. Since Goodman has repudiated the conception of ultimate reality and world-version, there is no absolute world-version for one to adopt as a final frame of reference in choosing between two world-versions.

In a nutshell, Goodman's conception of world-versions renders him in a difficult state to answer to the problem of theory choice, where two rival theories are derived from different world-versions. In Goodman's philosophy, rational theory choice is only possible provided that the rival theories are derived from within the same world-version. Goodman does not think that the rightness of a theory is vindicated by the corresponding reality. On the contrary, he claims that "total and permanent acceptability [of a theory], though, may be taken as a sufficient condition of rightness" (Goodman 1978, 139). The criteria of theory acceptability are relativistic, for it is a matter of fit to the world-version (Goodman 1978, 138).

4.3 Kuhn's Conception of Paradigm and the Problem of Theory Choice

4.3.1 Theory Choice in the Transition from Immature Science to Mature Science

The problem of theory choice in Kuhn's philosophy is closely related to his conception of paradigm. Paradigm is a putative vague notion used in many contexts by Kuhn.

According to Masterman, this term was used in at least twenty one different ways by Kuhn, in a somewhat quasi-poetic style (Masterman 1970, 61). In the broadest sense, it is a framework that characterizes the development of science. It has been used to distinguish different discrete stages of a discipline. Each discrete stage is characterized by a set of governing beliefs, theories, and standard practices.

> …the term 'paradigm' is used in two different senses. On the one hand, it stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community. On the other, it denotes one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science.
>
> (Kuhn 1970, 175)

Kuhn holds that the first sense of 'paradigm' is sociological in nature (Kuhn 1970, 175). It is a communal consensus achieved by the scientists who are working within the same paradigm. The notion of paradigm as a social framework has been exploited by sociologists of science to account for the scientific activities and outcomes from the perspective of social interaction. The ambiguity of the notion of paradigm is also attributed to its application in scientific activities at the micro-level, that is, the impacts of paradigm-governed psychological state of individual scientist in scientific activity.

The second sense of 'paradigm' is more ambiguous and elicited plenty of criticisms. Its meaning is always associated with the introduction of subjectivity and irrationality into science (Kuhn 1970, 175). The second meaning of 'paradigm' denotes the function of paradigm, that is, as a guide for scientific quests. Paradigm as a research guide is grounded in the social environment—based on the definition of Kuhn—which is shaped by shared beliefs and values. Paradigm as a value-bound research guide provides the legitimate problem-solution for the practitioners in their subsequent research. Hence, paradigm has always been interpreted as a relativist concept, for the underlying shared values and beliefs are relative with different scientific communities. A scientist from a different community who has no social bond with a competing paradigm will face the problem in understanding and accepting it (Demir 2008, 140-142).

Kuhn has divided scientific development into three stages: immature science, normal science, and revolutionary science. However, there is a cycle between the period of normal science and revolutionary science. Such cycle is driven by accumulated anomalies that lead to crises, which induce scientific revolution. Science resumes to the state of normal science after scientific revolution, and the cycle goes on.

Paradigm is a marker that distinguishes three stages of science. The stage of immature science is characterized by the absence of paradigm. In this period, a divergence of beliefs, theories, practices, and methods prevail. There is no consensus of the standard problem-solution set. A wide variety of choices in pre-scientific activity emerges in the practitioners' community. These choices of theory and method are carried out without the guidance of paradigm, thereby the outcomes can possibly be contradictory. Interestingly, Feyerabend was in disagreement with Kuhn about the non-existence of paradigm during the period of immature science. Feyerabend, in a letter to Kuhn, maintained that paradigm "existed long before modern science" (Hoyningen-Huene 2006, 617). Such divergence between Feyerabend and Kuhn lies in the different understanding of paradigm. For Feyerabend, paradigm is being understood as ways of thinking or methods. It is unnecessarily scientific in nature. Nevertheless, Kuhn takes paradigm to be scientific which denotes methods, laboratory protocols, theories, laws and scientific beliefs.

However, Kuhn does not regard immature science as an enterprise that is always consisted of unscientific elements. For Kuhn, the determinant of being scientific is not the outcome of a prediction. That is, a realized prediction that fits well with the reality is not the only criterion of being scientific. He claims that not all sciences are predictive, nor are experimental (Kuhn 1983, 568). The determinant of being scientific is far more than accurate prediction, involving also the complexity of skill, sensitivity to minor errors, and the precision of the input for prediction (Kuhn 1970a, 8).

Kuhn distinguishes between precision as a goal and precision as an achievement. A research that pursues precision as its goal is not unscientific notwithstanding that it fails to achieve precision as the outcome (achievement). The example given by Kuhn to support his claims is the study of astrology, which is regarded as an immature science. Although astrologers failed to predict precisely, astrology was an enterprise that pursues precision as a goal in its endeavors. It is in this sense Kuhn holds that astrology is not mere unscientific.

> Astrology cannot be barred from the sciences because of the form in which its predictions were cast. Nor can it be barred because of the way its practitioners explained failure. Astrologers pointed out…… the forecast of an individual's future was an immensely complex task, demanding the utmost skill, and extremely sensitive to minor errors in relevant data. The configuration of the stars

> and eight planets was constantly changing; the astronomical tables used to compute the configuration at an individual's birth were notoriously imperfect; few men knew the instant of their birth with the requisite precision. No wonder, then, that forecasts often failed.
>
> (Kuhn 1970a, 8)

Kuhn holds that astrology is a discipline that strives for the accurate prediction (precision as a goal). The failure of astrologers in obtaining accurate predicted outcome (precision as an achievement) was held accountable not by the astrology itself, but accountable by the complex variables of prediction (i.e. "few men knew the instant of their birth with the requisite precision"). Hence, Kuhn asserts that astrology is not a sheer unscientific discipline. The goal of astrology to pursue accuracy is a scientific element.

Although astrology, as an immature science, is not totally unscientific in its research activity, it is "not a science" (Kuhn 1970a, 8). Astrology is a craft that resembles engineering, meteorology, and medicine (Kuhn 1970a, 8). Despite the fact that astrologers had rules to apply in their practice, "they had no puzzles to solve and therefore no science to practice" (Kuhn 1970a, 9). Apparently, Kuhn thinks that it is not the imprecision of astrology that makes it an immature science. Astrology is not a mature science because it has no puzzles.

Immature science is not always necessarily leading to normal science, although Kuhn holds that normal science must derive from immature science. The route to normal science is affected by societal-value-bound rational choices which may lead to the emergence of first paradigm that governs normal science. Normal science is a mature science, which is not only striving for precision as a goal, but it is also based on the past achieved precision about the studied phenomena (precision as an achievement):

> [its] research firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice.
>
> (Kuhn 1970, 10)

Based on Kuhn's distinction between precision as a goal and precision as an achievement, it is clear that immature science is always characterized by pursuing precision as its goal but fails to realize it as an achievement; normal science, on the contrary, must pursue precision as its goal and achieve it eventually. This is evident in Kuhn's claim that normal science is "based upon one or more past scientific achievements" (Kuhn 1970, 10). On the grounds of past achievements, normal science proceeds to make more achievements.

Mature science is also characterized by the existence of routine problems and problem-solving activity (Nickles 2003, 144). In immature science, the problem-solving activity is different from that of mature science in the sense that the choices made (choice of theory, methods, belief and etc) by different practitioners are not converging to a consensus. In other words, the real problem-solving activity is not deemed to exist in immature science because there is disagreement about what constitutes a problem. Thus, the linear model of problem-solving activity, that is, one solution leads to another problem, does not exist in immature science. Linear model of problem-solving activity is important in mature science as it drives the development of science. The absence of such model makes the accumulative progress of immature science to mature science impossible, as there is no direction for such progress to take place due to the disagreement about the nature of problem.

As problem-solving activity is an indication of the existence of consensus in a particular discipline, that is, the characteristic of normal science, the absence of consensus is the salient feature of immature science. Kuhn has conceived of this idea even before the publication of *The Structure of Scientific Revolution*. In *The Essential Tension*, which was published three years ahead (in 1959) of *The Structure of Scientific Revolution*, Kuhn called the phase of immature science as “preconsensus phase” and the phase of normal science as “firm consensus phase” (Kuhn 1959; cited in Hoyningen-Huene 1993, 133). It is apparent that the maturity of a scientific discipline is depending on the communal consensus, which is the social factor. In *The Structure of Scientific Revolution* and his 1963 article *The Function of Dogma in Scientific Research*, Kuhn also used “preparadigm period” and “paradigm period” to denote “preconsensus phase/immature science” and “firm consensus phase/normal science” (Hoyningen-Huene 1993, 133). Thus, notwithstanding the ambiguity of the concept of paradigm, a paradigm-governed normal science is always characterized with the existence of communal consensus of problem-solving activity. A paradigm defines the legitimate problem-solution in normal science.

The consensus of problem-solution must fulfill two requirements to maintain the paradigm and normal science. Firstly, the achievement derived from such consensus must be “sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity” (Kuhn 1970, 10). Secondly, the problem-solution set must be “sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve.” (Kuhn 1970, 10). The first requirement implies two things. First, the scientific achievement derived from practitioners who are working under a background consensus on a standard problem-solution should be sufficiently extraordinary, which is always made possible by the emancipation of scientific practice from traditional dogmas, subsequently allowing novel ideas to emerge. This implies that, for novel ideas to emerge abundantly, the problem-solution as a research guide should not be too specific and restrictive. Problem-solution is expected to sketch the research direction in a broad rather than in a specific way. A problem-solution that assumes a role as a specific guide to every single detail will restrain the creativity of scientists and thus deter the subsequent achievements, which in turn losing its attractiveness to the adherents. A scientist is more likely to be committed to a paradigm which is unrestrictive and tends to lead to significant new discoveries. The commitment of adherents to a paradigm is important for a normal science to sustain. Second, the standard problem-solution should be far more robust than the competing problem-solution in order to keep its adherents away from the competing paradigm.

The second requirement implies that the openness of problem-solution allows sufficiently wide horizon for subsequent theory applications and development. In normal science, practitioners “force nature into the conceptual boxes” (Kuhn 1970, 5) with their creativity that bound by the paradigm. In the case where there is insufficient openness of a guiding problem-solution, the effectiveness of problem-solving will be affected. The abundance of hurdles might be encountered as the consequence. The nature of openness of problem-solution characterizes normal science with the paradigm-bound element of arbitrariness (Kuhn 1970, 5) and subjectiveness (Kuhn 1970, 156).

Paradigm is maintained by the faith of its advocates. In the transition from immature to mature science, scientists must have a faith that the first "paradigm will succeed with the many large problems that confront it" (Kuhn 1970, 158). The initial faith in the first paradigm is derived from the recognized problem-solving ability demonstrated in the paradigm that drives transition from immature science to mature science. It is a rational faith—a faith that can be reasoned by the problem-solving ability of paradigm. It is this rational faith demonstrated in the cumulative nature of normal science research, which distinguishes it from the period of immature science.

> Normal research, which *is* cumulative, owes its success to the ability of scientists regularly to select problems that can be solved with conceptual and instrumental techniques close to those already in existence.
>
> (Kuhn 1970, 96)

The rational faith of practitioners and the emergence of problem-solution distinguish mature science from immature science. The practitioners in the immature science do not have a rational faith because there is no problem-solution. The faith of practitioners in any given theory in immature science is thus irrational, for the theory that they subscribed to does not have a paradigmatic problem-solution. Hence, theory choice of practitioners in immature science is irrational and groundless from the viewpoint of a scientist who is working in the normal science. No practitioner can legitimately proclaim that his theory is privileged over the rivals, for there is no paradigm to legitimate the choice.

Furthermore, the absence of paradigm also implies the impossibility of theory comparison in the immature science.

The faith of practitioners in mature science implies loyalty to a single paradigm. According to Feyerabend, Kuhn denies the possibility of psychological commitment to more than one paradigm (Hoyningen-Huene 1995, 356). Hence, in Kuhn's opinion, the faith in a single-paradigm marks the completion of transition from immature science to mature science. Both faith and problem-solution drive the cumulative growth of normal science within a paradigm. The choice of the first paradigm in the immature science/mature science transition period is somehow arbitrarily dependent on the faith and the communal consensus on problem-solution set.

4.3.1.1 Theory Choice in the Period of Immature Science

In the period of immature science, there is no paradigm that determines the problem-solution, methodology, and research direction. Strictly speaking, it is impossible to have a scientific theory in the period of immature science, for paradigms, which are absent, are deemed to exist prior to theory (Masterman 1970, 66). A theory cannot exist without a paradigm. The absence of a theory, in the light of paradigm, is not the absence of any theory per se but the non-existence of a paradigm-governed theory. As pointed out by Bird, the absence of a paradigm-governed theory implies that each researcher will have to start from scratch in his research (Bird 2000, 30). There might have numerous existing theories but there exists no single governing theory. It is in this sense when Kuhn asserts that paradigm, as a concrete scientific achievement, is prior to the various theories (Kuhn 1970, 11).

In spite of this, theory choice can still be found in immature science. However, theory in immature science is not the Kuhnian paradigm-governed systematic theory, but a trivial one. A trivial theory is not necessarily an insignificant theory. It is a theory that is irrelevant to any paradigm, that is, a paradigmless theory. It does not have problem-solution. In Kuhn's word, immature science does not have puzzle to solve.

Theory in immature science is defined loosely, which denotes an unsystematic thought on a phenomenon. It is more akin to speculative opinion or thought fragment. Sometimes Kuhn refers to this kind of theory as "speculative and unarticulated theories" (Kuhn 1970, 61). The example of the theory in immature science is the theory of basic substance in ancient Greece. Thales, the founder of ancient Greek philosophy, holds that the basic constituent of all things is water; Anaximenes, another ancient Greek philosopher, thinks that it is air that constitutes the things. Apparently, the water theory and the air theory are different theory, regardless of the fact that they are not being considered as a scientific theory in modern science. The fluid theory of electricity is another example of immature science theory given by Kuhn. The adherents of this theory believed that electricity is fluid because they experienced a severe shock when touching the conductor-connected water (Kuhn 1970, 61-62).

In his reply to critics in *Criticism and the Growth of Knowledge,* Kuhn coined a new term "proto-sciences" to denote immature sciences whose theories and practices resemble "philosophy and the arts rather than the established sciences in their development patterns" (Kuhn 1970b, 244). Clearly, proto-sciences are immature sciences in the context of the development patterns which are characterized by the lack of clear-cut progress. The examples given by Kuhn include chemistry and electricity theory before the mid-eighteenth century, and today's social sciences.

Kuhnian conception of progress cannot be construed in terms of the truth. Rather, it must be understood in terms of puzzle-solving. According to Alexander Bird, Kuhnian conception of progress is a functional approach because a scientific progress is measured by the success in fulfilling the problem-solving function (Bird 2007, 67). The success of problem-solving is an evidence of scientific progress. The magnitude of success thus constitutes Kuhnian conception of "clear-cut" progress.

A progress is achieved where a new paradigm is more capable in terms of problem-solving compared to its predecessors. The progress takes a zigs and zags course rather than a linear accumulation of knowledge (Scheffler 2000, 164). In proto-sciences, the absence of paradigm and puzzle-solving imply the impossibility of clear-cut progress. Hence, the progress from proto-science to mature science is not warranted.

> I claim no therapy to assist the transformation of a proto-science to a science, nor do I suppose that anything of the sort is to be had.
>
> (Kuhn 1970b, 245)

There seems to be contradictory when Kuhn claims that proto-sciences lack a clear-cut progress on one hand, while admitting that some proto-sciences may advance into a state of normal science on the other hand. This seeming contradiction is dismissed by a careful interpretation of Kuhn's statement where he claims that proto-sciences have no clear-cut progress. It is apparent that the absence of clear-cut progress in proto-sciences does not imply the absence of *any* progress. Progress is still possible in Kuhnian proto-sciences, as it is evidenced by the fact that some proto-sciences have progressed into mature sciences. Hoyningen-Huene points out that Kuhnian progress is marked by "an increase in articulation and specialization" (Hoyningen-Huene 1998, 4). Progress in proto-sciences, interpreted as such, is not a form of progress in terms of puzzle-solving. Unfortunately, Kuhn did not provide further elaboration.

However, there are some clues we may pick up. In immature science, progress can be seen as driven by individual rather than by community. It is because the non-existent of paradigm in this period signifies the lack of collective forces (shared agreement, protocol, apprenticeship, problem-solution, etc) on individual practitioners. The variation of each individual on theory choice is enormous, for there is no shared agreement on the criteria (i.e. lack of a paradigm). However, the absence of a paradigm alone is insufficient to account for the maturity of a discipline. In addition to the establishment of a paradigm, it is a necessary requirement that the paradigm is not too dogmatic to restrict the ideas of its practitioners. For Kuhn thinks that a paradigm should grant, to certain extent, its practitioners freedom in their research to make progress. In comparison with the immature science, Kuhn recognizes that the (paradigm-permitted) variability of judgment in theory choice is essential to scientific advance in the normal science (Kuhn 1970b, 262). By the same token, the same must hold in the immature science, for otherwise there is no way to account for the transition of some immature sciences (e.g. fluid theory of electricity) to normal science (e.g. modern theory of electricity).

Theory choice in the period of immature science is characterized by the lack of consensus among the researchers about what constitutes the phenomena under study. This disagreement places a rather loose definition of theory choice, in which the legitimacy of a choice is not an agreed matter. In fact, the legitimacy of theory choice is not an important issue in immature science. A practitioner may claim that criterion c_1 should be adopted as a legitimate criterion in theory choice, whereas another practitioner may have another criterion, say c_2, for a legitimate theory choice. No party can persuasively convince their opponents about the superiority of their criteria for theory choice, since a paradigm does not exist in the period of immature science.

Thus, criterion of theory choice in immature science is not bound by any agreed standard. However, the non-existent of any agreed theory choice criteria does not imply the absence of any criteria. These criteria are non-objective due to the lack of a paradigm, that is, they are not universally acceptable by the community. However, these non-objective criteria of theory choice might be rational if a practitioner bases his theory evaluation on the today's agreed virtues of science, such as accuracy, coherence, generality, and so on. These criteria are the good reasons (rational) for theory choice in the normal science (Kuhn 1970b, 261; Kuhn 1983, 568). An astrologer is rational if his choice of theory *T* over the rival *T'* is based on the reason that *T* is more accurate, or more general, and so on. The criterion of theory choice is pluralistic, as there are no universal criteria of theory choice.

Though the criteria of theory choice might be rational in the immature science, Kuhn does not claim that it actually will. In fact, he does not elaborate on the criteria of theory choice in the immature science. To him it seems that the criteria are not a matter, for the theories are no matter in this period of science.

However, in his article *Reflections on My Critics,* Kuhn was seemingly against the position that there exist any rational criteria of theory choice in the immature science. He asserts that "if they [scientists of mature science] did not hold values [accuracy, scope, simplicity, etc] like these, their disciplines would develop very differently" (Kuhn 1970b, 261-262). Kuhn implies that rational criteria of choice are required for the mature science to exist. Since the immature science is very different from the mature science, it is reasonable to infer that Kuhn does not think that there are any rational criteria of theory choice in the immature science. Furthermore, it is also reasonable to claim that the non-existence of rational criteria of theory choice contributes to the non-existence of paradigm in the immature science.

4.3.1.2 Theory Choice in the Immature-Mature Science Transition

Contrary to many philosophers, Kuhnian mature science is not definable in terms of predictive and experimental activities (Kuhn 1983, 568). Nor the terms and languages of a field can be used to define a science (Kuhn 1983, 568). For Kuhn, there is no simple definition about science. He sometimes says that whether an activity is science "need not have an answer" (Kuhn 1983, 568). However, Kuhn admits that it is possible to distinguish mature science from non-science. Kuhnian science is necessarily characterized by the possession of a paradigm (Stone 1991, 179). The immature-mature science transition is a process of paradigm acquisition.

The transition from immature science to mature science requires three criteria: (1) The emergence of puzzles (Kuhn 1970a, 5); (2) the abandonment of Popperian critical discourse (Kuhn 1970a, 6); (3) and the abandonment of Popperian scientific testing (Mayo 1996, 271). These criteria will be discussed in this section. It is noteworthy that these three criteria are interdependent. However, Kuhn holds that puzzle is more fundamental to the emergence of science (Kuhn 1970a, 7).

Kuhnian puzzles do not amount to any problem that arises in a discipline. A field-oriented problem does not necessarily characterize a puzzle. Astrology, which is not a science, has plenty of problems (such as the configuration of the stars) but no puzzle, according to Kuhn (Kuhn 1970a, 8). Hence, Kuhnian puzzles are narrower in the meaning. Puzzle is not field-oriented but paradigm-oriented. A puzzle in physics is identified so not in virtue of it being a problem about physical phenomena, but in virtue of it being paradigm-oriented (e.g., wave theory or particle theory of light). A puzzle is relevant to paradigm in the sense that it can be conceptualized and operational in the framework of paradigm.

> A paradigm can…. .even insulate the community from those socially important problems that are not reducible to the puzzle form, because they cannot be stated in terms of the conceptual and instrumental tools the paradigm supplies.
>
> (Kuhn 1970, 37)

The nature of the puzzles that marks the successful immature-mature science transition is best illustrated by Kuhn in his comparison between astronomy and astrology. Astronomy as a mature science constitutes "calculational and instrumental puzzles" (Kuhn 1970a, 9).

> For more than a millennium these were the theoretical and mathematical puzzles around which, together with their instrumental counterparts, the astronomical research tradition was constituted.
>
> (Kuhn 1970a, 9)

The puzzles of astronomy arise from the past failures. The failures encountered by astrologer, by contrast, did not give rise to puzzles (Kuhn 1970a, 9). It is so for the astrology because "there were too many possible sources of difficulty" (Kuhn 1970a, 9). Kuhn holds that the persistent failures encountered in astrology are due to the difficulty in the application of the theory by the astrologer to explain and predict the astronomical phenomena (Kuhn 1970a, 9). The non-mathematical nature of astrological theories is the main factor contributing to the failure in the explanation and prediction of the astronomical phenomena. Besides, astrologers were facing difficulties in providing a coherent set of astrological theories. It is not the incompetence of the astrologer, as many of them, such as Kepler and Ptolemy, were astronomers too (Kuhn 1970a, 9). The failure of astrology to transit to mature science has its cause in the discipline itself. The nature of astrology, unlike astronomy, is doomed to fail in acquiring a first paradigm.

For a successful immature-mature science transition to take place, it is essential that the particular discipline is capable of supporting a puzzle-solving tradition. Such tradition allows the practitioners to constructively revise the discipline when facing with difficulties and failures (Kuhn 1970a, 6). To make a transition successful, a discipline must provide resources to overcome the failures. By overcoming the failures, new problems emerge that lead the practitioners to refine their techniques, tools and theories. The more failure is surmounted, the better chances a discipline can support the puzzle-solving tradition. Astrology failed to transit to a mature science because it was haunted by the same fundamental failures for centuries. No improvement had been made to overcoming these failures. The stagnant state of astrology results in no further new problems in the field. Hence, the fact that astrology is not able to support a puzzle-solving tradition leads it no where to a state of mature science.

Besides, a successful immature-mature science transition requires a wide range of possible solutions to the failure. The solutions can take the form of alternative calculations, theories, or instruments. The more alternative to the solution is available, the more likely a problem can be solved. Take astronomy as an example. Kuhn contends that astronomy has more resources to handle a failure than astrology does. An astronomer can adjust his theory, make a new measurement, or reform his astronomical technique (Kuhn 1970a, 9). On the contrary, astrology did not have a variety of solution to the failure. Astrologer could not make use of the failures "in a constructive attempt to revise the astrological tradition." (Kuhn 1970a, 9)

The emergence of a puzzle-solving tradition, as a requirement of a successful immature-mature science transition, implicitly allows a "quasi-revolution" to happen. In fact, Kuhn does not think that revolution happens in any period of science except in revolutionary science. He has never explicitly claimed that a revolution is required for the formation of a mature science. However, the requirement of a puzzle-solving tradition in mature science somehow demonstrates that such tradition must occur out of sudden. It is so because Kuhn asserts that there is only problem but no puzzle exists in the period of immature science. For the first puzzle to emerge, which is the first sign of the emergence of mature science, it is reasonable to conceive of a quasi-revolution that facilitates such transition. A quasi-revolution is a revolution that encompasses revolutionary breakthrough in the choice of theory, in the advent of new instrument, and in methodology used. Consequently, problems evolve into puzzles in a way that the territory of the discipline continuously expanded and enriched with more puzzles that drive the discipline forward to a mature state.

The quasi-revolution that drives the immature-mature science transition is not a full-fledge revolution as what happens during the replacement of paradigms in the revolutionary science period. However, it is akin to the paradigmatic revolution in the sense that the change of world view is ensued. Practitioners start seeing nature in a new way. By such a gestalt switch of world view, practitioners working in the immature science apply new theories, instruments, and methods in their attempts to surmount the fundamental failure. The criteria of theory choice in this stage are largely irrational, for a rational criterion requires some maturity of the discipline to serve as a basis for a choice to be made. It is dubious that the practitioners in an immature science could arrive at a rational agreement about what elements constitute such basis for theory choice. Indeed, Kuhn contends that any reasonable agreement among practitioners is more likely to occur in the mature science (Kuhn 1982).

In addition, quasi-revolution shares two characteristics with scientific revolution. First, in quasi-revolution and scientific revolution, a paradigm gains its status because it is more successful than its rivals in tackling the critical problems. Second, both quasi- and scientific revolution provide a paradigm that promises a good prospect for future research. Scientists believe that this promise of success is realizable in normal science. Wide spread confidence in a new paradigm among the members of a scientific community may drive a quasi-revolution in the formation of the first puzzle in normal science; and drive a scientific revolution in the formation of the new paradigm-puzzle in the period of revolutionary science.

> Paradigms gain their status because they are more successful than their competitors in solving a few problems that the group of practitioners has come to recognize as acute…..The success of a paradigm…..is at the start largely a promise of success discoverable in selected and still incomplete examples.
>
> (Kuhn 1970, 23-24)

The quasi-revolution in immature science may or may not lead to the successful transition to mature science. It is a necessary but insufficient condition for this transition. If the fundamental failure in the immature science has been overcome, a new horizon may open and the first puzzle would emerge. The solution to the first puzzle leads to new puzzles, in which a new solution awaits. The puzzle-solution cycle continues, which is a sign of the formation of puzzle-solving tradition, and eventually the transition from immature science to mature science is realized.

Once the puzzle has emerged, a "common body of belief" (Kuhn 1970, 13) is established. The shared puzzle is important in the immature-mature science transition, for it serves as a foundation for the accumulation of scientific knowledge. The practitioner is free in building "his field anew from its foundations" (Kuhn 1970, 13) in the immature science, but he is restricted to a common body of accumulated knowledge in the mature science.

What is the role played by theory choice in immature-mature science transition? In the immature science, theory choice is "relatively free" (Kuhn 1970, 13) in the sense that it is not governed by a paradigm. Theory choice in the immature science is not a choice between competing paradigms, for paradigm has not emerged. However, given that a quasi-revolution is reasonably posited to drive immature-mature science transition, it is apt to consider the emergence of a quasi-paradigm during such transition. It is a reasonable consideration, for Kuhn explicitly admits that a new paradigm often emerged ahead of the crisis in normal science.

> Often a new paradigm emerges, at least in embryo, before a crisis has developed far or been explicitly recognized.
>
> (Kuhn 1970, 86)

Thus, a quasi-paradigm may well explain the existence of theories in the immature science. Without a paradigm, or a quasi-paradigm, Kuhn has difficulty to account for the theory choice in immature science. Without theory choice justified in the period of immature science, it is inconceivable that a transition from immature science to mature science could take place.

Kuhn does not elaborate much on the role of theory choice in the immature-mature science transition. By assuming the legitimacy of quasi-paradigm in immature science, we may as well assume that the practitioners have been facing multiple quasi-paradigms for them to choose from to overcoming the encountered problems in the field. A correct theory choice is essential for the first puzzle to emerge. However, theory choice takes no full responsibility on the immature-mature science transition, for a successful transition is primarily dependent on the nature of the internal difficulty of the discipline itself. It need not be rational, for the standard-governing paradigm has yet to emerge.

Apart from the emergence of puzzle, the abandonment of Popperian critical discourse (Kuhn 1970a, 6) constitutes the second criterion for immature-mature science transition. Kuhn's understanding of critical discourse is "the tradition of claims, counter-claims, and debates over fundamentals" (Kuhn 1970a, 6). For Popper, critical discourse consists of bold conjectures and criticism (Popper 1970, 55). Kuhn agrees with Popper that critical discourse, whose origin can be traced back to Greek philosophy, is a part of science (Kuhn 1970a, 6). However, Kuhn does not grant that critical discourse is all of science (Kuhn 1970a, 6). Saying of critical discourse being a part of science, Kuhn means that it "recurs only at moments of crisis" (Kuhn 1970a, 6), when scientists "must choose between competing theories" (Kuhn 1970a, 7). In other periods of science, critical discourse does not occur. It is the abandonment of critical discourse in favor of puzzle-solving that "marks the transition to a science" (Kuhn 1970a, 6)

> Already by the Hellenistic period mathematics, astronomy, statics and the geometric parts of optics had abandoned this mode of [critical] discourse in favour of puzzle solving. Other sciences, in increasing numbers, have undergone the same transition since. In a sense, to turn Sir Karl's view on its head, it is precisely the abandonment of critical discourse that marks the transition to a science.
>
> (Kuhn 1970a, 6)

Interestingly, the fact that Kuhn asserts that critical discourse occurs only at moments of crisis when scientists must choose between rival theories implies two things. First, Kuhn does not think that theory choice genuinely occurs in other periods of science except in the period of crisis. Second, Kuhn implicitly concedes that theory choice is not rational (which is quite opposite to his later view on the rationality of theory choice), for he contends that critical discourse (which is rational according to Popper) needs to be abandoned for the sake of practicing normal science. The reason of abandoning critical discourse perhaps, according to Kuhn's line of thought, lies in the view that it would impede the development of science. It is fair to hold that any factor that would impede the development of science is an irrational factor. In view of the fact that critical discourse in normal science is accompanied with deciding between fundamental theories, and given that critical discourse is irrational if it has been practiced in normal science, we may conclude that Kuhn would concede that theory choice (of fundamental theories) is irrational in normal science.

Lastly, the abandonment of Popperian scientific testing constitutes the third criterion for immature-mature science transition. Scientific test, according to Popper, is an indispensable component of scientific activity that fuels the progress of knowledge (Gattei 2009, 2). Testability constitutes the criterion of demarcation between science and pseudoscience. For Popper, scientific test is crucial to determine the fate of a theory. Hence, it would appear absurd to Popper if one contends that scientific test is unimportant in the mature science.

Contrary to Popper, Kuhn claims that Popperian scientific testing has no role to play in the mature science. By saying so, Kuhn does not mean that all types of test are unimportant in science. The type of scientific test which is essential, according to Kuhn's understanding, is not a Popperian crucial test, for it plays no decisive role as what has been claimed by Popper (Kuhn 1970a, 7). Kuhnian scientific testing is a test of the capability of the practitioners, not of the theory (Kuhn 1970a, 7). Speaking of testing, Popper refers to the test of a theory, while Kuhn refers to the test of the practitioners who use the theory. To abandon Popperian testing is to abandon the crucial test for theory, but not the Kuhnian test. The main reason to give up Popperian testing, though not spelled out by Kuhn, perhaps is that it is of the nature of critical rationalism. As I have elaborated above, Kuhn views critical rationalism (expressed in critical discourse) an impediment to puzzle-solving, and thus it must be forgone for the realization of immature-mature science transition. Thus, Popperian testing being characterized by critical rationalism must be abandoned for the same reason.

During the immature-mature science transition, Kuhnian test is expected to prevail over Popperian test. The quality of a practitioner that is subject to the Kuhnian test includes the capability of making a right theory choice, skillfulness in experiment, and so on. The criterion of theory choice would be pragmatic, for after all it is the practitioner who is subject to the test. To show that he is capable, a practitioner would choose a theory which has more prospects in bringing out a solution to puzzle.

4.3.2 Theory Choice in the Normal Science

A mature science is "normal", according to Kuhn, in two senses. First, a mature science is normalized by its tradition, which is called "paradigm". Kuhn always relates a normalized mature science to paradigm in which a set of standard scientific practices and common beliefs are provided to guide research. Following from this meaning of mature science, major disagreement among scientists will not arise. The second sense of "normal" is that in a mature science there would be no crisis. No major anomalies are expected in a normalized mature science. Taken these two meanings together, a mature science is a normalized discipline without crisis.

> In this essay, 'normal science' means research firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice.
> (Kuhn 1970, 10)

> Achievements that share these two characteristics I shall henceforth refer to as 'paradigms', a term that relates closely to 'normal science'. By choosing it, I mean to suggest that some accepted examples of actual scientific practice—examples which include law, theory, application, and instrumentation together—provide models from which spring particular coherent traditions of scientific research.

(Kuhn 1970, 10)

To stress the relation between mature science and normalized paradigm, Kuhn always uses the term "normal science" to refer to mature science. Normal science is stable, largely because the paradigm that dominates it is widely accepted by the scientists. The stability of normal science implies that, unlike in immature science, scientists do not rebuild their theories from scratch every time they do research. They inherit the standard of practice (theory, methodology, laboratory protocol, puzzle-solution and etc) from their predecessors who are working in the same paradigm. Scientists are heavily dependent on their community (an environment that makes possible research fund, research team, peer reviewer, journal, conference and etc) in carrying out their research. Without scientific community, there would be no normal science. For paradigm that governs normal science is community-based. It is the community that normalizes the scientific practice and sustains the scientific tradition (paradigm).

> In the sciences… , the formation of specialized journals, the foundation of specialists' societies, and the claim for a special place in the curriculum have usually been associated with a group's first reception of a single paradigm.
> (Kuhn 1970, 19)

A scientific community can only hold at least and at most one paradigm at a time. For Kuhn, a paradigm is closely related to the nature of a scientific community. One could study the history and characteristics of a community in order to understand the nature of a paradigm. In other words, since normal science is characterized by paradigm, normal science could be examined via the perspective of history and sociological characters of a scientific community. It is this historical and socio-psychological approach advanced by

Kuhn in his elaboration of normal science in his philosophy. Notably, this approach of Kuhn also invites a fame of relativism.

> History and social-psychology are not, my critics claim, a proper basis for philosophical conclusions…. But unlike most philosophers of science, I began as an historian of science, examining closely the facts of scientific life…. My criterion for emphasizing any particular aspect of scientific behaviour is therefore not simply that it occurs, nor merely that it occurs frequently, but rather that it fits a theory of scientific knowledge.
>
> (Kuhn 1970b, 235-237)

To sustain a highly united scientific community in normal science, a certain extent of dogmatism is required in science so that no major disagreement would emerge among scientists. This dogmatism emerges in peer-review system (e.g. which article to be accepted as publishable), research fund (e.g. what topic of research is prioritized), text-book writing (e.g. what theory to impart), laboratory protocols (e.g. which genome screening approach is used), definition of a scientific term (e.g. phylogenetic or recombination species concept for bacteria), and so on. Every decision made by the members of a scientific community is inevitably dogmatic in certain extent, partly due to the trends of research and the limitation of resources. Dogmatism also exists in the adopted approach to solving a puzzle. In view of the external constraints (e.g. limitation of fund resources), a scientist may choose a method or equipment over another in his research. Different method/equipment being chosen already presupposes different background theory of this method/equipment. The observed results might be different, and this would contribute as a variable to the choice of the explanatory theory.

Extending this dogmatic practice to theory choice, Kuhn has concluded that logic and observation alone are necessary but insufficient criteria (Kuhn 1970b, 234; Kuhn 1970a, 16-19).

> The criteria with which scientists determine the validity of an articulation or an application of existing theory are not by themselves sufficient to determine the choice between competing theories.
>
> (Kuhn 1970a, 19)

Kuhn asserts that the socio-psychological factors, which are subjective and dogmatic in nature, of scientist are complementary to logic and observation. Throughout his career, Kuhn has never given up this socio-psychological aspect of theory choice. It is this socio-psychological factor that has invited the accusation about Kuhn's being a subjectivist. Kuhn rejects this accusation, and makes explicit in his article "Objectivity, Value Judgment and Theory Choice" that his criteria for theory choice are objective. In that article, Kuhn stresses that each of the five mentioned criteria—accuracy, consistency, scope, simplicity, and fruitfulness—is an objective basis for theory choice (Kuhn 1977). However, Kuhn points out that, though the criteria for theory choice are objective, it is implausible for a scientist to *use* them without being influenced by socio-psychological factors. This subjective aspect of the use of criteria is inevitable, for "the criteria are imprecise" and "when deployed together, they repeatedly proved to conflict with one another" (Kuhn 1977, 322). Though Kuhn has not denied the role of logic and observation in normal science, Kuhn's stress on the historical-socio-psychological dimension of science has subsumed the former under the latter. It is evident as Kuhn holds that community-based paradigms "are prior to theory" (Masterman 1970, 66) and "may be prior to, more binding, and more complete than any set of rules for research." (Kuhn 1970, 46)

> Paradigms *could* determine normal science without the intervention of discoverable rules.
>
> (Kuhn 1970, 46)

> While paradigms remain secure, however, they can function without agreement over rationalization or without any attempted rationalization at all.
>
> (Kuhn 1970, 48-49)

This community-based paradigm allows a greater extent of arbitrariness in theory choice when Kuhn holds that a paradigm "is not the same paradigm" for individual scientists in a community (Kuhn 1970, 50). What he means is that individual scientists in the same community are working under a different subset of paradigm. Scientists share a common paradigm, but do not share the same subset of that same paradigm. It is in this sense that Kuhn holds that a paradigm "is not the same paradigm" for each scientist. This sociological characteristic is inevitable, for each scientist receives training in different institution, working with different supervisor, and reading different journal. Such differences constitute different subsets of the same paradigm.

> Consider….the quite large and diverse community constituted by all physical scientists. Each member of that group today is taught the laws of, say, quantum mechanics, and most of them employ these laws at some point in their research or teaching. But they do not all learn the same applications of these laws…. What quantum mechanics means to each of them depends upon what courses he has had, what texts he has read, and which journals he studies…. In short, though quantum mechanics (or Newtonian dynamics, or electromagnetic theory) is a paradigm for many scientific groups, it is not the same paradigm for them all.
>
> (Kuhn 1970, 49-50)

This conception of paradigmatic subset could explain the dogmatism and arbitrariness that are granted by Kuhn in normal science. Though paradigm is prior to rule and logic, it is speaking so from the perspective of holism, which I shall call "sociological holism of paradigm". However, from the perspective of localism, rule and logic (along with the psychological state and experience of individual scientist) are internalized subset of a holistic paradigm. Though not stated explicitly by Kuhn, this localized paradigmatic subset consists of two types. The first type of paradigmatic subset is objective in nature (i.e. rule and logic), while the second type is subjective (i.e. personal value and experience). The general agreement among scientists in holding a shared paradigm is maintained both by the sociological bonds (sociological holism of paradigm) and by the objective subset (rule and logic). However, the application of paradigm in scientific practices is determined by the localized paradigmatic subsets, i.e. personal value, experience and the learned rule and logic. The diverse paradigmatic subsets that possessed by scientists account for the variety of observation and conclusion made by different scientists who are working under a common paradigm. However, this divergence is minor and does not lead to crisis, according to Kuhnian definition of normal science. Scientists seek to reconcile their disagreement by adjusting their paradigmatic subsets (e.g. arriving at a consensus of the equipment being used).

4.3.3 Paradigm Shift and Theory Choice

Kuhn maintains that a paradigm is stable in the period of normal science. However, a paradigm becomes questionable and in threat when anomalies accrued. However, Kuhn holds that not all anomalies will lead to paradigm change. Some anomalies are quickly resolved by the scientists using the puzzle-solution set provided by the existing paradigm. Those anomalies which cannot be resolved will be accumulated, eventually resulting in crisis. Scientific revolution, where change of paradigm occurs, will be the consequence if the crisis persists.

Hence, it is apparent that anomaly is the initial cause which may lead to paradigm shift. There are two conditions for anomaly to emerge. First, scientists are well-trained and capable in puzzle-solving activity. A scientist knows "with precision what he should expect, is able to recognize that something has gone wrong." (Kuhn 1970, 65) An experienced scientist is unlikely to observe irregular phenomena due to the improper execution of laboratory procedures. For Kuhn, anomaly is by no means *any* observed irregularity, especially those results from the aberrant observation recorded by inexperienced practitioners due to their lack of skills in experimentation. An anomaly is an irregularity that deviates from the puzzle-solution set which is familiar by a well-trained scientist. Because of his knowledge and skills in the field, a well-trained scientist can distinguish genuine anomaly from the pseudo-anomaly which arises from the experimental mistakes.

The second condition for anomaly to emerge is the maturity of a discipline, which is a characteristic of normal science. The instruments and knowledge have been extensively developed in mature science, and it would make an observed irregularity an anomaly, for the irregularity cannot be explained by the well-developed pool of knowledge. There is no way one could recourse to the existing puzzle-solution set to account for the irregularity. In view of the fact that anomaly emerges when the standard puzzle-solution fails to be effective, it is unlikely that anomaly would emerge at the initial stage of normal science. It is because the knowledge domain is yet to be explored at the early stage of normal science. Any irregularity observed in this early stage is expected to be solved with reference to puzzle-solution.

> Whatever the level of genius available to observe them, anomalies do not emerge from the normal course of scientific research until both instruments and concepts have developed sufficiently to make their emergence likely and to make the anomaly which results recognizable as a violation of expectation. To say that an unexpected discovery begins only when something goes wrong is to say that it begins only when scientists know well both how their instruments and how nature should behave.
>
> (Kuhn 1977, 173-174)

> Anomalies, by definition, exist only with respect to firmly established expectations. Experiments can create a crisis by consistently going wrong only for a group that has previously experienced everything's seeming to go right.
>
> (Kuhn 1977, 221)

The emergence of anomaly is always an indicator of new and unexpected discovery. For Kuhn, unexpected discovery is not supposed to find its place in normal science. It is because all discoveries are expected within the paradigm. The emergence of an unexpected discovery in a normal science is an indication of the challenge posed by an anomaly to the normal science. What Kuhn means is that the discovery in normal science is a normal (i.e. expected) one, not an abnormal discovery. However, the emergence of anomaly which leads to the abnormal discovery is the sign of the instability of paradigm. Scientists start to realize that the existing paradigm does not fit the reality.

> Discovery commences with the awareness of anomaly, i.e., with the recognition that nature has somehow violated the paradigm-induced expectations that govern normal science.

(Kuhn 1970, 52-53)

Anomaly which leads to new discovery may or may not lead to a crisis in normal science. If the new discovery could be assimilated with the existing paradigm, anomaly ceases without crisis. The new discovery is assimilated with the paradigm in a destructive-constructive way (Kuhn 1970, 66), that is, precision is enhanced while some standard beliefs are abandoned. The corollary of the assimilation is the paradigm adjustment, where the fundamentals are still remained.

However, if the new discovery could not be assimilated with the existing paradigm, accrued anomalies would render normal science in a state of crisis. Scientists diverge in their opinion regarding the solution to anomalies. Fundamental theories are challenged and no consensus is reached among scientists.

> In the physical sciences disagreement about fundamentals is, like the search for basic innovations, reserved for periods of crisis.
>
> (Kuhn 1977, 222)

The phenomenon of prolonged disagreement among scientists on the fundamental theories is a sign of the end of the monopolized single paradigm. Multiple paradigms, each of which contains rival theories, emerge. There is no ruling paradigm at this period of crisis. Each paradigm is on an epistemic par with its rivals, each of which possesses different advantages over others. Some paradigms may appear favorable in terms of its puzzle-solving potentials, while others may have exclusive advantage in terms of non-cognitive value such as simplicity. Some scholar, such as Kuukkanen, argues that Kuhn has put more emphasis on the puzzle-solving capability of a theory/paradigm over other virtues (Kuukkanen 2007) when deciding between rival theories/paradigms. If this argument is sound, one may hold that Kuhn would have claimed that puzzle-solving may serve as a decisive criterion for the theory/paradigm choice. However, Kuhn does not proclaim that puzzle-solving capability is the decisive criterion for scientist to arbitrate between rival theories. Notwithstanding, as pointed by Alexander Bird (2003), Kuhn has recognized puzzle-solving as a driving force that propels science to progress, other non-cognitive virtues such as simplicity, scope, and aesthetics play important role in theory choice as well. It is partly because of these non-cognitive virtues and partly because of the socio-psychological account of paradigm shift, that incur the charge of irrationalism and relativism on Kuhn's thought. Despite Kuhn has denied the charge, he fails to provide a rational account of paradigm shift and theory choice.

The dilemma that Kuhn faces could be accounted by the fact that his concept of paradigm is, as claimed by O'Malley and Boucher, too rigid to explain the rapid conceptual adjustments (O'Malley and Boucher 2005). For Kuhnian paradigm has a defined set of theories, laws, and methodologies. Kuhn always identifies theory with a paradigm, such that they are interchangeable concepts. However, in the history of science, scientific terms and theories were always found to be shared by different paradigms. For example, the theory of finite-age universe was subsumed under a pair of rival paradigms—the paradigm of static universe and the paradigm of expanding universe (Kragh 2007). However, Kuhn denies the overlapping of paradigms. Consequently, Kuhnian choice of one paradigm is inevitably forgoing the useful theories which are subsumed under a rival paradigm.

4.3.4 Incommensurability and Theory Choice

The incommensurability thesis is a problem with regard to the comparability and translatability between two theories. It was raised independently by Kuhn and Feyerabend in 1962 (Sankey 1994, 2). Feyerabend's notion of incommensurability[1] is his ground to argue against the reducibility of earlier theories to the later ones (Sankey 1994, 2). For Kuhn, he uses incommensurability to account for the discontinuity between rival paradigms in the history of science. In addition, Kuhnian incommensurability implies the difficulty in evaluating rival paradigms due to the absence of shared standards and concepts (Chen 1997, 258). Broadly speaking, incommensurability also implies communication breakdown[2] between members of competing scientific communities (Mößner 2011, 367).

> In the first place, the proponents of competing paradigms will often disagree about the list of problems that any candidate for paradigm must solve. Their standards or their definitions of science are not the same…. The transitions to Lavoisier's paradigm had, like the transition to Newton's, meant a loss not only of a permissible question but of an achieved solution.
>
> (Kuhn 1970, 148)

Kuhn's notion of incommensurability has evolved from a broader to a narrower sense in the course of his philosophical career. In *The Structure of Scientific Revolution*, incommensurability was applicable to methods, problem-solution, theories, and world-change. In the broadest sense of incommensurability, the worlds of the rival scientists are incommensurable[3]. Notably, the latter Kuhn has reformulated his notion of incommensurability in response to severe criticism. In "Commensurability, Comparability, Communicability", Kuhn has explicitly espoused a much weaker thesis of incommensurability, namely local incommensurability. It is a version of incommensurability that claims that communication is possible across incommensurable theories, therefore theory comparison is possible (Kuhn 1982b). As such, Kuhn claims that incommensurability of theories does not make theory choice irrational.

According to Sankey's studies on Kuhn, the "transition between incommensurable paradigms is a transition from the 'world' of one paradigm to the 'world' of another" (Sankey 1994, 21). However, this broader sense of incommensurability received extensive attacks with the charge of relativism. To respond to his critics, Kuhn has redefined the notion of incommensurability in a narrower, linguistic sense[4], in the late 1970s. In the preface to *The Essential Tension*, Kuhn claims that such a redefinition is persuaded largely by the work of Quine (Kuhn 1977, xxii). Despite the narrower definition, Kuhnian incommensurability retains the residue of the earlier notion of incommensurable world. The advocates of different paradigms, according to the later version of incommensurability thesis, speak "different languages—languages expressing different cognitive commitments, suitable for different worlds." (Kuhn 1977, xxii-xxiii)

Incommensurability thesis has introduced irrationality to theory choice. It is so because there are no common empirical consequences between two rival theories, for the terms of both theories cannot be translated point-by-point into a neutral language. The absence of such neutral common language thus renders theory choice irrational.

> The point-by-point comparison of two successive theories demands a language into which at least the empirical consequences of both can be translated without loss or change.
>
> (Kuhn 1970b, 266)

It is noteworthy to point out that, despite Kuhn's recognition of the difficulty in theory translation, translation between rival theories is by no means completely impossible. Partial translation is possible as evident by the fact that scientists from competing schools may exchange their opinions. However, Kuhn's assertion that such partial translation is error-prone (Kuhn 1970b, 268) might, as viewed by rationalists, render theory choice irrational. It is because theory choice is probably made on the basis of the mistranslation between two rival theories.

The partial translatability of rival theories has its cause in the incommensurable worlds in which scientists operate. Kuhn holds that scientists from competing schools operate in incommensurable worlds, that is, they perceive and interpret the phenomena differently. The same phenomenon means different things in the incommensurable worlds, which is best illustrated by Kuhn's duck-rabbit pattern of perception. A duck may appear as a rabbit if viewed from a different angle. One cannot see both duck and rabbit at a single point of time. Similarly, a phenomenon may appear as a duck-pattern to scientist *A*, while appear as a rabbit-pattern to scientist *B*. The switch from one pattern to another requires a gestalt-switch, implying the complete switch of paradigm and world-view. The replacement of paradigms is necessary, for worlds that entail the phenomena are incommensurable.

> Practicing in different worlds, the two groups of scientists see different things when they look from the same point in the same direction….That is why a law that cannot even be demonstrated to one group of scientists may occasionally seem intuitively obvious to another. Equally, it is why, before they can hope to communicate fully, one group or the other must experience the conversion that we have been calling a paradigm shift.
>
> (Kuhn 1970, 150)

The incommensurable worlds do not imply the Leibnizian[5] or Lewisian[6] possible worlds. Kuhnian worlds are actual worlds that we reside. Scientists are free to operate in different worlds by changing the lens of perception and interpretation. However, a scientist cannot work in two worlds at the same time, according to Kuhn. It is because any two worlds are inevitably incommensurable. Some philosopher, such as Alexander Bird, claims that Kuhn's thesis of world-change has "a quasi-idealist, neo-Kantian slant" in his later philosophical development (Bird 2002, 453), which is inferior to his earlier contribution in *The Structure of Scientific Revolutions* (Bird 2002).

The incommensurable worlds lead to an unattractive consequence of theory choice. The principle of rationality requires the empirical observation to serve as a necessary criterion for theory choice, for it is deemed objective. Though the empirical observation is not equivalent to the reality, realists assume an objective correspondence between them (they also assume an objective correspondence in the unobservable domain). The principle of rationality held by realist is that the correspondence between observation and reality is strictly a one-to-one relation. No variation is allowed in the empirical observation of scientists, despite the possibility that these scientists may belong to rival schools. Kuhn, as an anti-realist, does not assume this realist position. Though he denies that the incommensurable worlds permit scientists to "see anything they please" (Kuhn 1970, 150), there is no strictly one-to-one correspondence relation between observation and reality. Though scientists are confined by the reality in their experiment, they can perceive and interpret the phenomena in more than one way. The variation in observation is the consequence of the variation of reality (incommensurable worlds). Kuhn has no means and intention to decide which reality (incommensurable world) is more real. For him, all incommensurable worlds are equally valid. This position results in the incomparability of the incommensurable worlds, in the sense that one cannot decisively weigh the value of the competing worlds. Kuhn maintains that all incommensurable worlds should be relatively weighted[7]. As a consequence, theory choice is inevitably relative and non-rational, for Kuhn assumes that scientists are influenced and confined by the world in which they operate.

Many scholars have refined and developed the notion of incommensurability ever since its proposal. Alasdair MacIntyre, a well-known historian and philosopher of ethics, has criticized that it is the conceptual incommensurability that was adopted by Kuhn that led him into the dilemma of irrationalism (MacIntyre 1980). Conceptual incommensurability, MacIntyre argues, presupposes Cartesian account of epistemological crises. It is an account that puts everything in question simultaneously, meaning to say that the totality of knowledge is under examination simultaneously without discrimination. It is shown so, according to MacIntyre, in Kuhn's exposition of incommensurability. MacIntyre highlights that Kuhn's conceptual incommensurability assumes the absence of complete contact between the view points of the proponents of rival paradigms. It follows that the transition from one paradigm to another requires a leap of faith, which is a conversion that is characterized as irrational. MacIntyre argues that despite Kuhn has rejected the accusation of irrationalism, the Cartesian flavor of conceptual incommensurability allows no rational continuity between two rival paradigms. It is because the totality of knowledge "is put in question simultaneously" (MacIntyre 1980, 68) and "since reason operates only *within* traditions and communities….such a transition [between paradigms] or a reconstruction could not be a work of reason"[8] (MacIntyre 1980, 67).

However, MacIntyre argues, Kuhn may resort to historical truth to get rid of the accusation of irrationalism. History, according to MacIntyre, is presupposed by Physics. By history MacIntyre means the scientific tradition which encompasses scientific theory. As a historian of ethics, MacIntyre claims that histories can be constructed and rationally compared with each other. It follows that incommensurable theories, or paradigms, are rationally comparable too. Implicitly, theory choice has a rational ground as well.

> I am suggesting, then, that the best account that can be given of why some scientific theories are superior to others presupposes the possibility of constructing an intelligible dramatic narrative which can claim historical truth and in which such theories are the subject of successive episodes. It is because and only because we can construct better and worse histories of this kind, histories which can be rationally compared with each other, that we can compare theories rationally too.
>
> (MacIntyre 1980, 73)

The prevalent view amongst philosophers of science is that the incommensurability thesis applies only to revolutionary but not normal science. However, Szumilewicz argues that this is not the case. Szumilewicz launches two criticisms to the received view of the incommensurability thesis (Szumilewicz 1977). Her first criticism was plotted against the view that the incommensurability thesis is applicable only to the revolutionary science. According to this view, two theories are rationally comparable in normal science, while they could not be rationally compared in the revolutionary science. On the contrary, Szumilewicz holds that the correspondence between two theories, regardless whether during the normal or revolutionary science, has tied incommensurability to all phases of science. It is so because Szumilewicz interprets incommensurability as meaning variance, that is, the meaning of two successive theories, whether in normal or revolutionary science, "may be significantly different" (Szumilewicz 1977, 346). Szumilewicz's argument has two parts. First, she holds that the language of science is constantly changing. The variability of scientific language inevitably results in the meaning variance of two theories. Second, Szumilewicz argues that the divide between normal and revolutionary science is arbitrary. Taken the first and the second argument together, Szumilewicz argues that meaning variance of theory occurs in all phases of science. Since Szumilewicz takes incommensurability to mean meaning variance, it is apparent that incommensurability is applicable to all phases of science. It is noteworthy to mention that Caneva shares Szumilewicz's objection to the distinction between normal and revolutionary science, but with different reason. Caneva argues that the existence of small-step continuity during paradigm shift has undermined "the historical relevance of incommensurability" and "undercut the distinction between normal and revolutionary science." (Caneva 2000, 100).

The implication of Szumilewicz's argument is that if one holds that incommensurability of theories implies the irrationality of theory choice (e.g. theories are not rationally comparable) in revolutionary science, he must have to forgo the traditional view that the scientific enterprise in normal science is rational (e.g. theories are rationally comparable). For Szumilewicz has tied the incommensurability to all phases of science, and rejected the clear-cut divide between normal and revolutionary science. In the same vein, if one holds that theories are rationally comparable in normal science, he must approve the view that the same is applicable in revolutionary science. In short, Szumilewicz's argument does not decide whether incommensurability is rational or irrational. Her argument implies that if one takes incommensurability to form a rational basis for theory choice, he has to admit that theory choice is rational in *both* normal and revolutionary science; if one takes incommensurability to form an irrational basis for theory choice, he has to admit that theory choice is irrational in *both* normal and revolutionary science.

> My first criticism will be that if *TI* [Thesis of Incommensurability] were true it would apply not only to revolutionary periods but to *all* phases of theoretical change in science. Most defenders of *TI* would allow that, during periods of non-revolutionary or "normal" science, an improved theory *T'* is comparable with the earlier theory *T* from which it was developed; so that rational appraisal is possible in these cases. I agree. But I claim that *T'* and *T* will be "incommensurable" in these cases no less than in cases of revolutionary change: thus "incommensurability" either excludes rational comparison and appraisal in non-revolutionary cases, or allows rational comparison and appraisal in revolutionary cases.
>
> (Szumilewicz 1977, 345)

The second criticism of Szumilewicz was directed against the view that logical relations do not hold between incommensurable theories (Szumilewicz 1977). According to this received view, incommensurability implies incomparability of theories, due to the fact that the common terms of two theories have different meaning. Szumilewicz holds that, on the contrary, "logical relations may hold between two theories despite their incommensurability" (Szumilewicz 1977, 348). By resorting to formalizing the scientific terms, Szumilewicz claims that we can compare the mathematical functions of two incommensurable theories independent from the semantics of them. She suggests that scientists may use their intuitive understanding to map the formalized terms with their corresponding meanings. With such an optimistic view to relate the formal aspect with the content of the scientific terms, Szumilewicz concludes that "meaning variance need not render *T* and *T'* [incommensurable theories] rationally incomparable" (Szumilewicz 1977, 349). However, Szumilewicz's strategy is not without problem. On the one hand, she does not demonstrate how to warrant the precise mapping between formalized terms and their semantic content. On the other hand, Szumilewicz's strategy to compare the formal aspect of the scientific terms may not be viable in the cases where the scientific terms are lacking of mathematical form (e.g. scientific terms such as 'gene', 'organism', 'tarsal bone'). Carnap's failure in providing a logical construction of the world serves as a good example that it is impossible to formalize *all* scientific and ordinary terms[9].

4.3.5 Incommensurability and Philosophy of Biology

In Kuhn's writing, he never applied incommensurability to biology, though he uses biological concepts as an analogy to the lexical taxonomy. It is intellectually interesting to probe into this area, as biology has achieved significant breakthrough and rapid advancement since the proposal of the double helix model of DNA by Watson and Crick in 1953. The emergence of new discoveries, especially in molecular biology, has led to the establishment of many promising sub-disciplines. One of them is the emergence of evolutionary biology. However, the notion of biological process in evolutionary biology, which is a central concept, has not reached a consensus among scientists. Many incommensurable interpretations of this notion exist among different scientists (Delisle 2011, 57). The notion of gene trees serves as another example of incommensurability in biological science. In the paradigm of phylogenetics, gene trees refer to the gene hierarchy of an organism; whereas in the paradigm of population genetics, gene trees are gene hierarchy of species (Knowles 2009). The same word denotes different meaning in different paradigms, though its evolutionary meaning is retained. Tracing the history further backward to Darwinian period, gene trees, which was called heredity at that time, did not have evolutionary meaning. It was not a concept which "necessarily imply support for programmes of selective breeding" (Paul 2009, 234). Darwin's conception of heredity was material and particulate (Gayon 2000, 72), it "was less the past (ancestry) than the present structure of these collections of units [gemmules, which are small buds generated by cells]" (Gayon 2000, 73)

However, it was reported that there are terms and concepts which did not differ in their meaning under different paradigms, such as the term "substitution rate", which retained the same meaning under the paradigm of single calibration point model and the paradigm of relaxed-clock model (Ho and Phillips 2009).

The phenomenon of incommensurability also persists in systems biology, which is a new sub-discipline of biology that emerged in 2000s (Arkin and Schaffer, 2011). It is a discipline that encompasses cell biology, physics, chemistry, mathematics, information management, and genetics. Systems biologists aim to study how the biochemical processes of individual cell contribute to the behaviors and survival of an organism

(Arkin and Schaffer 2011). One of the challenges faced by systems biologists is to explain the cellular process in terms of information management (Nurse and Hayles 2011, 853). It is not an easy task, for to translate the biochemical laboratory results into the information processing data elements requires a new approach to bridge two distinct disciplines, which are completely incommensurable.

Calvert and Fujimura argue that the works of systems biologists involve paradigm shifts between different epistemic concepts, which occurred as the consequence of its interdisciplinary nature (Calvert and Fujimura 2011). In their interview, Calvert and Fujimura found that systems biologists were able to collaborate despite their difference in epistemic commitments and background training[10]. They thus conclude that the differences between paradigms do not present hurdle for the collaboration between systems biologists, which implies that distinct paradigms are not incommensurable. This conclusion is opposed to Kuhn's, as he claims that paradigms are incommensurable.

> Nevertheless, in all these cases, collaboration required a great deal of negotiation and labour at the borders [of different fields]. It may be that differences between paradigms, epistemologies, or methods do not constitute incommensurable boundaries to collaboration. With enough desire, commitment, and labour, these differences may not only be surmounted, they may be productive.
>
> (Calvert and Fujimura 2011, 162)

Calvert and Fujimura assert that the differences in paradigms held amongst systems biologists may be surmounted with enough efforts. They do not imply that the corollary of the collaboration amongst systems biologists is to adhere to a single paradigm. It is unlikely to happen because the research team is comprised of scientists from diverse fields. Conversely, multiple paradigms are in effect dominating the collaboration. Such situation is made possible by the mutual appreciation of the differences in paradigms and values.

> Communication across disciplines in systems biology could be made easier if members of the field have a greater awareness and appreciation of their different epistemic assumptions and values.
>
> (Calvert and Fujimura 2011, 162)

Burian takes incommensurability in biology to be interpreted against two backgrounds: the reduction theory and the referential discontinuity (Burian 2005). The reduction theory demonstrates that there is a fundamental theory to which other theories could be reduced to, or deduced from. The typical example of the reduction theory is the claim that Mendelian genetics could be reduced to molecular genetics (Burian 2005, 130). This claim is established on the assumption that the reduced theory has definitions and statements which are entailed by the fundamental theory. According to Burian, the mistake of reduction theory is its false presupposition of the continuity between two distinct theories.

> Were this account of reduction correct, the concepts of the reduced theory would be, in effect, definable within the more fundamental theory, and the claims of the reduced theory would be a subclass of the claims of the fundamental theory. In fact, as is now generally recognized, reduction of this sort virtually never occurs in science.
>
> (Burian 2005, 130-131)

Although Burian does not proclaim to hold a view of radical incommensurability, his argument against reduction theory has implied incommensurability in terms of scientific definition of the terms used in two theories. It is a kind of conceptual incommensurability. In fact, many opponents of the reduction theory[11] have also argued that “the key concepts of successive theories are in fact incommensurable in meaning.” (Rosenberg 2008, 550). Notably, Burian does not explicitly subscribe to the view of paradigm in biology. Nonetheless, he holds that the phenomenon of incommensurability implies that the relation between two biological theories could not be reductive[12].

> Worse yet, when one comes to cases like …. Mendelian versus molecular genetics, discontinuity theorists have put forth quite convincing arguments to show that the concepts of the theory to be reduced simply cannot be reproduced within the successor theory. This claim, as I argue, seems to be entirely in accord with the facts.
>
> (Burian 2005, 131)

Besides, Burian also interprets incommensurability in terms of referential discontinuity. Referential discontinuity is not the same with referential indeterminacy which had been made known by Quine and Donald Davidson. The proponents of referential indeterminacy hold that reference is, in an indefinitely many different referential relation, solely determined by the use of language (Nimtz 2005). According to the Davidsonian thesis of referential indeterminacy, interpretation is inevitably indeterminate, for it is hard to apply the rule precisely and arrive at the agreement on the observable (Davidson 2004, 157). The notion of referential indeterminacy does not entail incommensurability, but the notion of referential discontinuity does.

In Kuhnian thesis of referential discontinuity, a reference does not stay constant through paradigm shift, that is, a term may refer to different references in two paradigms. Burian argues, based on the history of genetics, that the referential discontinuity of genetic terms is held accountable by the scientific community. For “the terms are—in a certain sense—community property.” (Burian 2005, 133). The referential discontinuity happens when the refinement of method shifts the reference of a term. It follows that the references are incommensurable after the shift of reference. Burian implies that scientific community plays a major role in the incommensurability of reference.

> It is, of course, true that the procedures by which factors (or genes) are identified and individuated can be refined and improved in ways that may shift the reference of particular terms; such refinements can change altogether the set of entities to which a community refers by use of such terms as “gene” and “factor”. But, the terms are—in a certain sense—community property…. The point about the social character of the referential use of scientific and prescientific terms is rather stronger than it looks.
>
> (Burian 2005, 133)

Marcel Weber studies incommensurability in biology from three perspectives: translation failure, non-corresponding predictions, and referential discontinuity (Weber 2002). He examines the problems in light of the oxidative phosphorylation controversy[13] in the history of biochemistry. It was a dispute about how mitochondria, the organelle of cell which generates energy to sustain cellular activities, generate energy in the form of adenosine triphosphate (ATP). According to Weber, there were two incommensurable paradigms: The chemical paradigm proposed by Slater and the chemiosmotic paradigm proposed by Mitchell.

The chemical paradigm was the old paradigm, which conferred to enzyme-bound chemical compound, known as ‘high-energy intermediate’, a vital role in converting a molecule of adenosine diphosphate (ADP) to form energy—ATP. This paradigm was widely accepted in 1960s because it was modeled after a successful mechanism to generate energy from sugar, which was the ATP-generating step of glycolysis. However, there was no crucial evidence to prove that the chemical paradigm is true, for being true the hypothesized high-energy intermediate must exist. Unfortunately, this chemical intermediate was not found in the experiment.

The chemiosmotic paradigm did not postulate high-energy intermediate as the chemical paradigm did. It proposed that the electrochemical potential generated via diffusion of proton across mitochondrial membrane has been a factor that converts ADP to form energy—ATP.

Weber analyzes that both chemical paradigm and chemiosmotic paradigm encompass incommensurable terms, such as "high-energy intermediate" in the chemical paradigm and "proton-motive force" in the chemiosmotic paradigm, which differ in sense and reference. The fact that these terms do not logically contradict each other, Weber argues, leads to translation failure. Weber further demonstrates that an attempt to translate the theoretical statements of one paradigm into another will inevitably lose inferential relations.

Non-corresponding prediction is another consequence of incommensurability[14]. Weber holds that it is a situation where the predictions made by a paradigm have no corresponding ones made in another. Similarly, Hoyningen-Huene has highlighted the difficulty of the problem of non-corresponding prediction (Hoyningen-Huene 2000). He analyzes the issue of corresponding prediction for both commensurable and incommensurable theories. He argues that in comparing two commensurable theories, there is always a corresponding prediction. The difficulty lies only in the situation where each theory has its own strength. However, incommensurable theories have no corresponding prediction, because of "the difference in their concepts and the untranslatability of both theories." (Hoyningen-Huene 2000, 105). Weber's understanding of non-corresponding prediction as a feature of incommensurability in biology is inline with Hoyningen-Huene's. He argues with examples that both the chemical paradigm and the chemiosmotic paradigm predicted no common thing. The chemical paradigm predicted energy-link functions by which no corresponding prediction was found in the chemiosmotic paradigm. The chemiosmotic paradigm predicted proton translocation by which no corresponding prediction was found in the chemical paradigm. Hence, Weber concludes that non-corresponding prediction is the feature of incommensurability that occurred in the history of biochemistry.

The third, and the last, perspective of incommensurability studied by Weber was referential discontinuity. The high-energy intermediate in the chemical paradigm was not presupposed in the chemiosmotic paradigm. It was replaced by "a whole new kind of bioenergetic mechanicsm" (Weber 2002, 8), which was a proton-motive mechanism underlain by the intermediate of glycolysis and the citric acid cycle. The chemical intermediates in these paradigms were not a straightforward replacement, for they were two different entities. Hence, Weber concludes that the referential discontinuity was observed in the paradigm shift from the chemical to the chemiosmotic paradigm.

The kinds of incommensurability that are discussed in the above-mentioned literatures of philosophy of biology were elaborated, in a more general sense, by Sankey. He takes incommensurability to be a semantical problem which has to do with the languages of scientific theories (Sankey 1994). He has given a broad definition of incommensurability as such:

> Broadly speaking, to say that a pair of theories is incommensurable is to say that the theories do not share a common language, or that the terms they employ do not have common meaning.
>
> (Sankey 1994, 1)

Sankey analyzes that incommensurability stems from the semantic dependence of the scientific terms on the theoretical contexts. Contrary to Kuhn, Sankey holds that incommensurability is not a common phenomenon. He distinguishes two types of semantic incommensurability: semantic incommensurability due to variation of sense and semantic incommensurability due to discontinuity of reference (Sankey 2009). According to Sankey, it is the latter that presents challenges to scientific realism. However, Sankey does not worry about such challenge, as he argues that it is dismissible by a modified causal theory of reference (Sankey 1994).

Meaning variance of two theories, Sankey argues, gives rise to translation failure and referential discontinuity (Sankey 1994, 1998). He points out that Kuhn takes meaning change between theories to "include variation of reference as well as sense" (Sankey 2009, 197). Meaning variation of incommensurable theories results in the untranslatability of the theoretical terms.

> Untranslatability has a central role in the incommensurability thesis. If a pair of theories is incommensurable, then the languages employed by the theories are partially or wholly untranslatable. In addition, translation and content comparison have a close connection according to the thesis. Since the content of theories

> expressed in untranslatable languages is inexpressible within a shared vocabulary, it appears not to be directly comparable.
>
> (Sankey 1994, 73)

Sankey rejects the idea that incommensurable theories are wholly untranslatable (Sankey 1994). However, he claims that translation failure of incommensurable theories implies partial untranslatability, which is the impossibility of word-to-word translation. Further, Sankey argues that untranslatability does not imply communication failure between incommensurable theories, for the causal theory of reference "allows referential overlap and comparison even in the absence of translation" (Sankey 1994, 220). Translation failure does not present a hurdle for the comparison of rival theories. Hence, in Sankey's account, theory choice is possible in the presence of translation failure.

Sankey has defended Kuhn's later notion of taxonomic categories against translation failure. He calls this taxonomic account of incommensurability as taxonomic incommensurability (Sankey 1998). It is a thesis which "involves differences between the taxonomic categories which scientific theories employ" (Sankey 1998, 7). Entities are categorized according to taxonomy (Bird 2002). For Kuhn, taxonomic terms are also known as kind terms (Kuhn 2000, 92 cited in Hoyningen-Huene and Oberheim 2009, 206). Hence, it is apparent that Kuhn does not take taxonomy to be a mere semantic construct, but it also refers to entity or kind. According to Kuhn, the taxonomic categories are clusters that group the scientific terms (Kuhn 1983). Kuhn, in 1980s, has rejected the notion that a scientific term is to be understood and applied in a universal context. Instead, he claims that scientific terms are categorized into different taxonomic clusters and they should be interpreted in an interconnected way.

> Many of the referring terms of at least scientific languages cannot be acquired or defined one at a time but must instead be learned in clusters….. The Newtonian terms ‘force’ and ‘mass’ provide the simplest sort of example. One cannot learn how to use either one without simultaneously learning how to use the other. Nor can this part of the language-acquisition process go forward without resort to Newton’s Second Law of Motion. Only with its aid can one learn how to pick out Newtonian forces and masses, how to attach the corresponding terms to nature.
>
> (Kuhn 1983, 566)

Sankey concurs with Kuhn that taxonomic categories do not render complete untranslatability but local untranslatability (Sankey 1993). Since taxonomic categories are interconnected, the languages of two intertranslatable theories must have the same taxonomic structure. A successful translation requires the preservation of categories. However, argued Sankey, the interconnected categories undergo a radical change in meaning and reference after translation. Translation between local clusters of terms fails because those terms are interconnected and inter-defined.

> Translation between such local complexes of terms fails because the meaning of such terms is determined in relation to other terms of the interdefined set. Terms which are defined within an integrated set of concepts cannot be translated in piecemeal fashion into an alternative complex in which the necessary conceptual relations do not obtain.
>
> (Sankey 1993, 772)

Sankey (1998) criticizes Kuhn’s inference from the notion of untranslatability between theories to the denial of evaluating the merit of the theories in terms of truth. Sankey argues that “rival theories may make more or less true claims about the same entities, despite untranslatability” (Sankey 1998, 12). He asserts that at least some of the terms employed by the theories must refer to the same entities, thus making the rational theory choice plausible.

Lastly, I turn to Stegmüller's structuralist reconstruction of Kuhn's incommensurability thesis. Stegmüller proclaims that he has developed an "entirely new approach to the analysis of the structure of scientific theories" (Stegmüller 1976, vii), which could be used as a source of conceptual apparatus to rationally reconstruct Kuhn's notion of normal science and scientific revolution. This so-called "new approach" is a non-statement view of theories, that is, to interpret theories as set-theoretic predicates rather than classes of empirical statements. Contrary to the tradition, Stegmüller's approach does not characterize scientific theory as a linguistic entity. In other words, Stegmüller attempts to axiomatize scientific theory by understanding the empirical claims as Ramsey-Sneed-sentences (Stegmüller 1979). Stegmüller states that "instead of working with formalized languages and formalized theories of logic", non-statement view of theories "makes use of informal set theory and logic only". (Stegmüller 1979, 83)

> In contrast to the statement view, a theory itself is interpreted as a composite mathematical structure together with a class of intended applications.
> (Stegmüller 1976, 14)

> There are good reasons for not identifying a theory with the central empirical claim….. A theory itself is characterized as a nonlinguistic entity: namely, as an ordered pair consisting of a core *K* and the class of intended applications *I*.
> (Stegmüller 1976, 16)

> ….. empirical claims of theories are not to be formulated by infinite sets of sentences, but rather each by a Ramsey-Sneed-sentence.
> (Stegmüller 1979, 24)

> Now what about scientific revolutions? Can the logician also contribute to a better understanding of this phenomenon? Here I should like to begin with a confession so that you will not be too terribly disappointed with the following remarks: The logician can actually accomplish far less in this case than in the case of the phenomenon which Kuhn called normal science. This is not because scientific

> revolutions are in fact thoroughly irrational processes, but simply because many aspects of these phenomena lie outside the competence of the logician.
>
> (Stegmüller 1980, 83)

The reason that drives Stegmüller to formulate a non-statement view of theories lies in the problem of theoretical terms, which arises from the empirical statements of theory. Empirical statements contain theoretical terms, which could not be substantiated in the meaning. What Stegmüller means is that the effort to understand theoretical terms is enormously difficult, for theoretical terms are self-referential in the interpretation. Logical inferences are of no help here.

> The traditional idea concerning the empirical claims of a theory leads to a difficulty in all cases in which such claims contain theoretical terms. In order to substantiate a claim of the form "c_i is an *S*", one must always refer back to a statement "c_j is an *S*," i.e., to a statement of *exactly this form*.
>
> (Stegmüller 1976, 15)

Stegmüller holds that non-statement view of theories could be used as an approach to rationally defend Kuhn's notion of incommensurability. According to him, Kuhn's explication of incommensurability was based on the statement view of theories. The theoretical terms contained within the empirical statements are thus self-referential. At the macro level, any choice between paradigms is circular and indeterminate. Communication breakdown is inevitable in Kuhnian statement view of incommensurable theories, for there is no neutral meta-language.

> [Kuhn's] exposition of this sort of scientific revolution, which we will call the dislodging of a theory by a substitute theory, contains an exaggeration and a mistake.
>
> (Stegmüller 1976, 214)

> [In Kuhn's statement view of theories] we need only remember statements to the effect that any argument in favor of a paradigm (i.e., in favor of a theory <K,I_0>) is basically circular, that the proponents of various theories can not communicate due to the lack of a neutral metalanguage….
>
> (Stegmüller 1976, 215)

Kuhn's statement view of theories renders the inferential relations between theories impossible. Hence, Stegmüller points out, Kuhn argues that there is no logical deducibility between two theories. However, Kuhn's critics condemn his incommensurability thesis from the perspective of statement view of theories. Such critique is persuasive as, in the context of statement view, the failure of inferability and reducibility between theories renders scientific progress irrational. To salvage Kuhnian incommensurability from irrationalism, Stegmüller proposes that the reducibility of theories cannot be defined in terms of inference.

> Kuhn details there the reasons why Newtonian dynamics may *not* be said to be deducible from relativistic dynamics. This argument is a joke. Kuhn uses the metatheoretic 'paradigm' of his opponents to support the incommensurability thesis. For the 'statement view', reduction problems can only turn on one thing: namely, the 'inference relations between classes of sentences.' *However, instead of arguing from noninferability to nonreducibility, we will argue that an adequate concept of reduction cannot be defined in terms of inference*.
>
> (Stegmüller 1976, 216)

Stegmüller holds that the reducibility of one theory to another is important to account for scientific progress. It is this reason that Stegmüller thinks that a rational account of incommensurability should not evade from answering the problem of reduction. From the perspective of non-statement view, Stegmüller proposes an alternative notion of reduction, which is non-inferential in nature. He treats theories as sets to which the reduction relations, construed as functions, hold. In mathematics, function is an abstract construct that maps domain to range. In Stegmüller's account, reduction relation between two theories is interpreted as function mapping between two sets. The concepts and laws are the elements of theory-sets. The reduction relation is a set-theoretic (non-linguistic and non-logical) mapping of the concept-element and law-element of a reducing theory-set to the concept-element and law-element of a reduced theory-set.

By formalizing reduction in set-function, Stegmüller need not consider the meaning of theoretical terms. Hence, his account is free from the problem of translation failure, comparability, and referential discontinuity; not because these problems do not exist, but they are irrelevant in the context of set-function. For, in mathematics, the semantics of domain and range of two sets is not important. What matters is the mapping rule. Given a function and a mapping rule, the relation between two sets always holds. Stegmüller's account of non-statement view of theory transforms the reduction relation from the traditional logical and semantic relation to a mathematical function. The reduction relation is thus a mapping from domain of a theory to the range of another. The elements (i.e., concepts, law) of theory-set are mapped to the corresponding theory according to the mapping rule of the function. Hence, speaking of incommensurability between two theories, the charge of translation failure and referential discontinuity has nothing to do with the scientific terms (the element of theory-set). The cause of incommensurability lies in the structure of theory-set. For, in mathematics, given a mapping rule, two incommensurable sets could enter into a functional relation, which is the reduction relation in Stegmüller's case.

> The reduction is to be realized via a reduction relation which 'transposes' the basic concepts of *T* into those of *T'*, and indeed in such a way that the basic laws of *T* can then be 'mapped' onto those of *T'*.
>
> (Stegmüller 1976, 128)

> Thus it appears appropriate to construe the reduction relation as a one-many relation with the domain M_{pp} and the range M'_{pp}.
>
> (Stegmüller 1976, 128)

By showing that the reduction relation between two incommensurable theories is a set-function relation, and by rejecting the traditional idea that reduction is a logical relation, Stegmüller claims that his structuralist approach could salvage Kuhnian incommensurability thesis from the charge of irrationalism and relativism. Without rejecting incommensurability thesis, Stegmüller holds that set-theoretic interpretation of theory is rational, based on the assumption that the set theory and function in mathematics are a rational approach. It is a reasonable assumption because set theory "provides mathematics with its foundation" (Bagaria 2008, 616).

Stegmüller interprets incommensurability thesis as a problem which lies in the incommensurability of domain and range of two theories, which is different from the traditional view that incommensurability lies in the scientific terms of two theories. Thus, Stegmüller does not need to account for the translation failure between scientific terms in the context of semantics and logic. By resorting to the rationality of mathematics, incommensurability (i.e., translation failure, referential discontinuity) is unquestionably rational in Stegmüller's account. Hence, if one is to question Stegmüller's account, she should have looked into the legitimacy of axiomatizing theory as set[15] and reduction relation as function. However, if one is able to show that it is illegitimate to adopt

Stegmüller's approach, the most he can say is that Stegmüller's incommensurability thesis is illegitimate; he can not in the least claim that Stegmüller's incommensurability is irrational, because Stegmüller's incommensurability is modeled after mathematics, which is a rational enterprise.

4.4 Feyerabend and the Problem of Theory Choice

4.4.1 Feyerabend's Relativist Position

Feyerabend is a well-known relativist on scientific theory. He opposes the realist correspondence theory of truth, according to which there is a one-to-one correspondence relation between theory and reality. Feyerabend holds that such correspondence does not warrant truth, for "any false theory can be made to fit the facts" (Feyerabend 1981, 5). If any theory can be made in an ad hoc way to account for the facts, the credentials of the theory are thus questionable. According to Feyerabend, theory is an instrument that can be created in any way to explain the reality. That is why Feyerabend asserts that any false theory can be made to fit the facts. Hence, it is natural for Feyerabend to come to doubt about the realist account of objectivity of theories. He is a skeptic about realist objectivity, which does not question only the truthfulness of realist objectivity, but also confront the superiority of realist notion of objectivity.

> But if objectivism while perhaps acceptable as a particular point of view cannot claim objective superiority over other ideas, then the objective way of posing problems and presenting results is not the right way for the relativist to adopt.
> (Feyerabend 2002, 78)

The rejection of realist notion of objective theory does not imply the denial of objectivity in Feyerabend's thought. The very objectivity that is rejected is the assertion that scientific theories are universal and the reality can only be explained in one definite way. The proponents of universal scientific theory claim that a true scientific theory should be applicable to all phenomena spatio-temporally; whereas the proponents of definite reality maintain that the reality manifests itself in only one way. Feyerabend, as a skeptic who maintains that one can never know reality as it is, modestly asserts that we should not expect a universal theory could be formulated to account for the reality in a one-to-one corresponding way. He contends that "the world we inhabit is abundant beyond our wildest imagination" (Feyerabend 2001, 3)

> A relativist who deserves his name will then have to refrain from making assertions about the nature of reality, truth and knowledge and will have to keep to specifics instead. He may and often will generalise his findings but without assuming that he now has principles which by their very nature are useful, acceptable and, most importantly, binding for all. Debating with objectivists, he may of course use objectivist methods and assumptions; however, his purpose will not be to establish universally acceptable truths (about particulars or generalities) ……
>
> (Feyerabend 2002, 78)

Feyerabend complains that realist notion of objectivity renounces the subjective element of theory. Realists hold that a true theory must objectively reflect the corresponding reality. They reject the idea that scientific theory could be formulated based on personal bias. As a skeptic, Feyerabend suggests that both subjective and objective elements are indispensable in scientific theories.

> More recent developments in the interpretation of quantum mechanics suggest regarding such appearances [of atoms] as phenomena (Bohr's term) that transcend the dichotomy subjective/objective…. They are "subjective", for they could not

> exist without the idiosyncratic conceptual and perceptual guidance of some point of view (which need not be available in explicit form); but they are also "objective": not all ways of thinking have results and not all perceptions are trustworthy.
>
> (Feyerabend 2001, 143)

> The "subjective" side of knowledge, being inextricably intertwined with its material manifestations, cannot be just blown away.
>
> (Feyerabend 2001, 146)

The acceptance of subjective element of scientific theory shows that Feyerabend's conception of truth is very much different from that of realist. For realist, there is only one definite truth for each physical phenomenon; while for Feyerabend, truth is non-definite because he has conceived of a possible pluralistic reality according to which he has sometimes called "the abundance of reality". Though Feyerabend admits that simplification of the abundance of reality is needed in science, but there is certainly more than one way of simplification (Feyerabend 2001, 241). Even in the presence of a monist reality, truth could be manifested in more than one way. It is this belief that makes Feyerabend accepts non-scientific account (such as myth) of world picture.

> A myth can very well stand on its own feet. It *can give explanations*, it can reply to criticism, it can give a satisfactory account even of events which prima facie seem to refute it. It can do this *because it is absolutely true*. It has therefore, something to offer. It has to offer truth, absolute truth.
>
> (Feyerabend 1999, 64)

From the above quotation, it seems to Feyerabend that a true theory must be able to give explanations, reply to criticisms and provide a satisfactory account of the reality. Apparently, a true theory is measured by its capability of providing a *reasonable* picture of reality, not by its capability of providing a *matching* picture, as what scientific realist does, of reality. A matching picture of reality requires a one-to-one correspondence between theory and fact, which is a scenario where there is only a ruling theory. A reasonable picture of reality does not have realist objective truth as its prerequisite. It does not require a single ruling theory to explain the reality. In fact, a single ruling theory, Feyerabend argues, is detrimental. It is a new form of authoritarianism and superstition, which is a dogma scientists aim to obviate at the start. Therefore, a reasonable picture of reality requires pluralism of theory. That is, a physical phenomenon should be accounted not only by science, but also by non-science. Strikingly, Feyerabend states that these pluralistic pictures of reality are all true theories, because they are able to provide a reasonable picture of reality.

> A truth that reigns without checks and balances is a tyrant who must be overthrown and any falsehood that can aid us in the overthrow of this tyrant is to be welcomed.
>
> (Feyerabend 1984, 138)

> There is no idea, however ancient and absurd that is not capable of improving our knowledge. The whole history of thought is absorbed into science and is used for improving every single theory. Nor is political interference rejected. It may be needed to overcome the chauvinism of science that resists alternatives to the status quo.
>
> (Feyerabend 1978, 47)

The consequence of an explicit distinction between reality and picture of reality is that one cannot have a single definite knowledge, as what is pursued by scientific realists. Despite one cannot influence reality, his social and personal characteristics do penetrate into the picture of reality of which he conceives. Scientific theory, as one of the many pictures of reality, is inevitably influenced by the contingent factors of individual scientists and their community. Hence, Feyerabend contends that the blossom of a variety of (even contradictory) theories in science is expected and should be welcomed.

> Being tied to individuals and groups a world-view cannot be 'Platonized'—it cannot be presented as a person-independent entity that enters into relations with other person-independent entities such as facts and/or theories; it has to be related to the individuals and the communities that are affected by it.
>
> (Feyerabend 1994, 156)

It is interesting to compare the view of Feyerabend and Wittgenstein on the picture of reality. Feyerabend had shown great enthusiasm in Wittgenstein's thought since he was a university student. He was initially planned to study with Wittgenstein, but Wittgenstein died before his arrival in England (Preston 1997, 3). Consequently, Feyerabend studied with Popper whose "ideas were similar to those of Wittgenstein but they were more abstract and anaemic" (Feyerabend 1978a, 116). Undoubtedly, Feyerabend was familiar with Wittgenstein's works. He read "*Philosophical Investigations* in detail" and "rewrote the book so that it looked more like a treatise with a continuous argument."[16] (Feyerabend 1978a, 115-116). He even admitted that "there is much Wittgenstein in all my papers" (Feyerabend 1995a, 50).

Wittgenstein construes reality as atomic fact in his early philosophy. In this account, the picture of reality is represented by the atomic propositions.

> A proposition is a picture of reality: for if I understand a proposition, I know the situation that it represents.
>
> (Wittgenstein 1988, 39)

> A proposition *shows* its sense. A proposition *shows* how things stand *if* it is true. And it *says that* they do so stand.
>
> (Wittgenstein 1988, 41)

However, Wittgenstein has abandoned this idea in his later philosophy and turned to embrace a thesis of meaning-is-use (Hanna 2010). The later Wittgenstein claims that the pure referentialism that was embraced in his early philosophy does not account for the variation of meaning of the same referent (Hanna 2010, 19). Further, the picture of reality that is represented by proposition follows the principle of middle exclusion: "a proposition must restrict reality to two alternatives: yes or no" (Wittgenstein 1988, 41).

The objection to the principle of middle exclusion is apparent in Feyerabend's thought as well. His tenet of relativism rejects the kind of "yes or no" correspondence between reality and theory. There exists no absolute correspondence, and a relativist picture of reality should be adopted. Hence, in making a choice of theory, one should not be confined to the principle of middle exclusion. That is, one should not be tempted to think that a theory must be either true or false. Moreover, one should also not to conceive the reality as represented by only one theory. On the contrary, one should always presume that the reality can have more than one picture, viz., can be represented equally well by more than one theory. Such assumption would undoubtedly influence the choice between rival theories. Feyerabend's assertion that the interpretation of the reality is dependent on the theory which explains it is clearly a Wittgensteinian thesis of meaning-is-use. Whichever theory being chosen is not a matter about objective truth, for Feyerabend's conception of truth is not confined to accuracy and predictive success. What matters in theory choice is that the qualified theory should be able to picture the reality, in the sense that it "can give explanations, it can reply to criticism, it can give a satisfactory account" of the reality (Feyerabend 1999, 64).

> ….what is determined by the 'facts' is the acceptance (or rejection) of sentences *which are already interpreted* and which have been interpreted independently of the phenomenological character of what is observed. The impression that every fact suggests one and only one interpretation and that therefore our views are 'determined' by the facts, this impression will arise only when (with respect to the language used) the relation of phenomenological adequacy is a one-one relation.
>
> (Feyerabend 1981, 34)

The objection to realist correspondence theory of truth leads Feyerabend to conclude that truth is local. A theory is true in certain context while not so in others. The criterion of theory choice is thus local. There is no universal criterion to arbitrate amongst rival theories. A criterion used in a context (Feyerabend sometimes calls it 'tradition') as a rational standard for theory choice may be deemed irrational in another context. Feyerabend holds that the rationality of standard is an integral part of the context, which may vary from one context to another. Science is, as seen by Feyerabend, historically relative because the rationality that endorses it is relative.

> We have seen that rational standards and the arguments supporting them are visible parts of special traditions consisting of clear and explicit principles and an unnoticed and largely unknown but absolutely necessary background of dispositions for action and judgement. The standards become 'objective' measures of excellence when adopted by participants of traditions of this kind.
>
> (Feyerabend 1978a, 27)

> …….judgements are made by individuals who participate in traditions and use them to separate 'Good' from 'Evil'…… rationality is not an arbiter of traditions, it is itself a tradition or an aspect of a tradition. It is therefore neither good nor bad, it simply is.
>
> (Feyerabend 1978a, 27)

However, the notion of local criterion of theory choice is not without problem. Since the choice is context-dependent, and Feyerabend claims that truth is specific to context/tradition, the truth of a choice is inevitably context-dependent. For example, in the context of myth, metaphors and story-telling serve as the criteria for Feyerabendian truth. Scientific criteria of truth such as accuracy and simplicity play no role here, because they are not a part of myth. The criterion of theory choice in myth is dependent on the conception of truth implied by myth. That is, to choose amongst rival theories, one should not look at the accuracy of the story-telling but the richness of story or the greatness of the heroes. There would be no problem for a practitioner to make a context-dependent theory choice in a clear-cut discipline like myth. However, problem arises in new discipline which does not have a well-defined boundary and in the inter-disciplinary field. So long as the notion of local criterion of theory choice is held, the practitioners have to know what are the truth-criteria of these disciplines before they could determine the criterion of theory choice. However, it is unlikely that the truth-criteria of these disciplines could be known clearly, as in the new discipline much works on domain-charting is awaited. The notion of local criterion of theory choice faces no fewer problems in the interdisciplinary fields, for the domain of interest of these fields are not subsumed under a single local context/tradition. If a theory choice is, as maintained by Feyerabend, local to the context/tradition, the interdisciplinary domains will render theory choice impossible.

Feyerabend’s notion of local truth and local criterion of theory choice is the result of the non-existence of a universal rationality. As rationality is relative, as claimed by Feyerabend, scientists must have abandoned the notion of universal reason. In his book *Farewell to Reason*, Feyerabend urges that one should instead embrace relativism—a doctrine which has an implicit pragmatic concern in its core. The main reason for embracing relativism is for the good of human's welfare and progress of science. For relativism is open to alternatives, a virtue Feyerabend conceives as important when confronting the unknown possibilities in science.

> The way in which scientific problems are attacked and solved depends on the circumstances in which they arise, the (formal, experimental, ideological) means available at the time and the wishes of those dealing with them. There are no lasting boundary conditions of scientific research.
>
> (Feyerabend 2002, 304)

In Feyerabend's philosophy, relativism and the pluralism of theory are desirable for two reasons. First, Feyerabend claims that human can never know the reality as it is; second, it follows that there is no definite certainty in knowledge. It is the proliferation of theory that can prevent an accepted as true but indeed false theory (which is unaware by scientists) to dominate. Although Feyerabend has inherited much of Wittgenstein's thought, he does not resemble the latter in the least in the aspect of knowledge certainty. The later Wittgenstein holds that science has certain degree of certainty. Mathematics[17], being the pure science, has highest degree of certainty (Wittgenstein 1999, 226; Wittgenstein 1976, 131). We can also have legitimate empirical claims to knowledge in science, argued Wittgenstein, by perceiving and observing the reality (Grayling 1996). However, the certainty of knowledge that is granted by Wittgenstein is denied by Feyerabend.

> Of course, 'certain' may not always mean 'irrefutable'…, but whatever it means, '*X* is certain' cannot now make us complacent with respect to *X*, it cannot make us believe that the question concerning the truth of *X* is settled.
>
> (Feyerabend 1981, 153)

> … we may point out that induction does not get us very far, and does not provide certainty.

(Feyerabend 1995, 204)

As a skeptic of certainty, Feyerabend embraces relativism and pluralism to "check and balance" the dominating theory. Notably, relativism is not a final solution to the problem of certainty in science. Proliferation of theory in science will not increase the certainty of knowledge. On the contrary, it will decrease the certainty due to the absence of a dominating theory. With respect to increasing knowledge certainty, Feyerabendian theory choice plays little, if no, role. Regardless of whatever criterion of choice and whichever theory has been chosen, Feyerabend teaches us that the certainty of the explained reality will never be definite. If it is so, the tenet of "anything goes" perhaps is the most economic but unproductive approach in theory choice. "Anything goes" is economic in the sense that one does not need to literally "make" a choice, because Feyerabend teaches him to treat all available theories equally. Thus, epistemic effort is saved for the reflection during the process of theory choice; "Anything goes" is unproductive in the sense that the certainty of knowledge will not be increased as a result of theory choice. Since theory choice plays insignificant role in enhancing the certainty of knowledge, it is a meaningless endeavor in scientific practice. One of the main activities of scientists is making theory choice. If theory choice is meaningless in terms of increasing the certainty of knowledge, the whole scientific enterprise would be meaningless too. Besides, non-sciences such as myth and magic fare no better, for theory choice in these enterprises can increase no certainty (e.g. certainty about the detailed practice of magic). If certainty of knowledge could not be increased as a consequence of theory choice, the slogan of "anything goes" can hardly warrant progress in science.

> This is also what is meant by the slogan 'anything goes': there is no guarantee that the known forms of rationality will succeed and that the known forms of irrationality will fail. *Any* procedure, however ridiculous, may lead to progress, *any* procedure, however sound and rational, may get us stuck in the mud.
> (Feyerabend 1977, 368)

Notably, Feyerabend's relativism is not a doctrine which opens the door *only* to irrationality (though Feyerabend explicitly welcomes irrationality). Feyerabend's relativism is by no means rejecting rationality. What has been rejected is the realist notion of rationality, which is a universal and non-relativistic conception of rationality. In fact, Feyerabend's notion of relativism still encompasses rationality as its component—a relativistic rationality. The examples of this kind of rationality, as given by Feyerabend, are manifested in myth, magic, and traditional medicine. For Feyerabend, these fields are merit in their own context. They are rational in their respective field, for they can account for the reality in their respective context. They are not universally rational, but relativistically rational. The practitioners of different fields may accuse each other as being irrational. Such accuse assumes a universal notion of irrationality. Subscribing to their contextual notion of rationality, a scientist may reject magic as an irrational enterprise; however, a practitioner of magic may view the practice of science irrational.

By holding that rationality is relative, there would be no uniform pattern of theory choice in science. As pointed out by Feyerabend, the tradition of a field matter in scientific development. Since the criterion of theory choice is a part of the tradition, the relativistic outlook of different tradition will encompass different criterion of choice. The rationality of theory choice lies in its potentiality in advancing a discipline, for Feyerabend holds that proliferation of theories and methods is the key to the advancement of science. Hence, a rational choice should, at least, not deter or rule out rival theories, for the realization of the proliferation of theory. Without ruling out other rivals, a chosen theory, based on Feyerabend's criterion of rationality, should not be decisive and final. However, by choosing a non-decisive theory to save other rival theories from being eliminated, one may risk the possibility that the best theory may not be held and sustained. Further, a non-decisive theory, if being chosen, could be a disaster to the advancement of science due to its indeterminacy. It is because a non-decisive theory may fall short in its problem-solving capability. Hence, the principle of proliferation cannot, according to rationalist, serve as a rational criterion for theory choice.

However, Feyerabend may rebut that, to save the proliferation of theory, one need not choose a non-decisive theory among rivals. A decisive theory, which has the dominating role in persuasively accounting for a phenomenon, may still be chosen provided that it could warrant the proliferation of theories in science. Feyerabend may suggest that we have one decisive theory as a chosen one while keeping other rival theories in juxtaposition as reference to "check and balance" the status quo of science. The main reason Feyerabend advances the proliferation of theory is to prevent the so-called tyranny of a single ruling theory. However, it is not clear how such check-and-balance mechanism could take place if the chosen theory is more decisive than its rivals. For a check-and-balance mechanism to realize its practical power, the rival theories have to exert their impact (such as overriding or taking over the existing theory) on the dominating theory. If a decisive theory has been chosen, there would be no rational way that a check-and-balance mechanism could be exerted by the rival theories, for they are non-decisive. It would be irrational to use a non-decisive theory to account for the observed phenomena if one has a more decisive one as his choice.

Perhaps Feyerabend may still argue that epistemic decisiveness is not the criterion of theory choice. He may contend that anything goes so long as proliferation of theory is warranted in science. If it is so, scientists would be indecisive in their daily practice of problem-solving activity. There would have no tangible guideline to use a theory if one adopts the principle of anything goes. Major disputes amongst scientists will inevitably arise from time to time, for as long as the proliferation of theory is warranted, any decision on theory choice would appear to be rational; and follow Feyerabend's lines of thought, epistemic disputes are welcomed. There would be many solutions to a problem, but no scientist can decisively convince his peers if the principle of proliferation is to be held. In this scenario, many solutions imply no solution. Consequently, there would be no, or little, progress in science.

Hence, it is apparent that the principle of proliferation is detrimental to the progress of science. Unfortunately, Feyerabend has implicitly regarded his relativism as a rational approach to the advancement of science, through the proliferation of theories and methods. However, this version of rational relativism is not championed by many scholars. Achinstein has argued (Achinstein 2000), from a realist point of view, that relativism is untenable because the principle of proliferation of theory promotes the invention of any theory, even contradicting ones. He asserts that there is no reason one should accept all these contradicting and inconsistent theories as true or probable. Van Fraassen is dubious about how one could possibly accept the coexistence of alternative theories without giving up one's own (Van Fraassen 2000). It seems to him that to accept a theory implies the abandonment of the alternatives. Kekes rejects Feyerabend's understanding of rationality, and criticizes Feyerabend's relativism based on the logic-based rationality (Kekes 1991). Notably, these critics do not accept relativism as a tenable doctrine in the practice of science, nor do they accept the view that proliferation of theory is a rational approach to the advancement of science[18].

However, it is important not to view Feyerabend as an enemy of science, as claimed by Keeley, in order to understand the significance of his relativism in scientific practice (Keeley 2006). Feyerabend's slogan of "anything goes" is not all about anarchism in science, but also closely related to his conception of pragmatism and democracy.

4.4.2 Relativism as Pragmatism

Although Feyerabend does not openly profess the element of pragmatism in his relativism, pragmatism is the core tenet that fuels many of his important concepts. Pragmatism, according to Robert Brandom[19], asserts that goal achievement is the primary virtue to be emphasized (Brandom 2005). Feyerabend's pragmatic idea[20] lies in his stress on the success of science. He claims pragmatically that anything goes for the sake of the proliferation of theory, that is, a mark of the success of science. For Feyerabend, a scientist who subscribes to pragmatism would do science not based on truth, but based on the outcome. A pragmatic scientist would choose a theory with reference to the favored outcome. If theory *T* can best explain an observed phenomenon which could not be accounted by its rival theories, it will be chosen because of its pragmatic value. A theory that has been chosen is not because of its truth value. High explanatory power of a theory, in the light of pragmatism, implies epistemic success (i.e. goal achieved, the phenomenon is satisfactorily explained, etc.). In science, pragmatists may have different goals: theory to be accepted by peers, theory capable of predicting the phenomena accurately, constructing a simple theory, and so forth. Goal achievement, according to pragmatist, is an indication of success in science. A pragmatist will choose the *right* means, which may be more than one way, to achieve the desired success.

Although success is the goal pursued by both realists and pragmatists, they approach it in different way. A realist who subscribes to the 'no miracle' argument claims that the success of science is attributable to its approximate truth. To pursue success in science, one needs to choose, at least, the approximately true theory which can mirror the reality as detailed as possible. However, pragmatists dissent with this realist stance. They assert that the success of science is not only brought about by the true theory at hand. In other words, they believe that whatever theory it is, the one which can serve the intended purpose should be favored. Pragmatists incline to maintain that there is more than one theory which could serve the purpose of, and promote the success in, science.

A relativist cum pragmatist, such as Feyerabend, defines the success of science even more loosely. According to Feyerabend, success of science is the proliferation of theory, which means the success of a multitude of theory. He has tied the success of science to the success of individual theory. The success of a theory has two meanings in Feyerabend's account: (1) being accepted widely; and (2) does not rule out the proliferation of rival theories. At first it seems contradictory between these meanings. How could a successful theory be accepted widely and at the same time allows its rivals to proliferate? The contradiction diminished as one understands that Feyerabend allows no single theory to dominate, that is, a doctrine of anarchism in science. Apparently, Feyerabend's principle of proliferation has a relativist flavor. To achieve proliferation in science, pragmatism is the working principle. It is this relativist pragmatism that makes Feyerabend a radical relativist.

Feyerabend has made a pragmatic interpretation of the success of Galileo's theory (Feyerabend 1978). Galileo's argument, Feyerabend claims, is not completely rational and unquestionable. He points out that Galileo had made use of a large amount of common sense, which is irrational, in his science. This common sense, known as "natural interpretations", is "mental operations which follow so closely upon the senses, and which are so firmly connected with their reactions that a separation is difficult to achieve." (Feyerabend 1978, 73). Feyerabend claims that natural interpretations, which are common senses, were regarded in the history of thought as irrational a priori presupposition of science or prejudices which were supposed to be discarded. However, Feyerabend argues, Galileo's success lies mainly in the use of natural interpretations which had complemented the limitation of rationalism. The use of natural interpretations by Galileo was a pragmatic strategy—to render his new radical theory an outlook of common sense which could be more readily acceptable by the public.

> Galileo is one of those rare thinkers who neither wants forever to *retain* natural interpretations nor altogether to *eliminate* them. Wholesale judgements of this kind are quite alien to his way of thinking. He insists upon a *critical discussion* to decide which natural interpretations can be kept and which must be replaced….. The methods of reminiscence, to which he appeals so freely, are designed to create the impression that nothing has changed and that we continue expressing our observations in old and familiar ways.
>
> (Feyerabend 1978, 73)

Feyerabend argues that Galileo's theory appeared irrational to his contemporaries because it contained "absurd and counterinductive assertions, such as the assertion that the earth moves" (Feyerabend 1978, 81). Of course, today we know that the assertion that the earth moves is true, and thus a rational claim. However, it was not perceived as a true and rational claim in Galileo's age. What is implied by Feyerabend is that the false theory of the earth being static was widely conceived as true and rational during Galileo's time. The implication is that a perceived rational theory may not be a true theory. In the history of science, there were many theories perceived as rational at one time turned out to prove false eventually. Hence, rationality, for Feyerabend, is relative and separable from truth. To make his perceived irrational theory to appear rational, Galileo adopted non-scientific approach such as propaganda and psychological tricks. These approaches are pragmatic means for Galileo's theory to be accepted.

> How does he [Galileo] manage to introduce absurd and counterinductive assertions, such as the assertion that the earth moves, and yet get them a just and attentive hearing? One anticipates that arguments will not suffice—an interesting and highly important limitation of rationalism—and Galileo's utterances are indeed arguments in appearance only. For Galileo uses *propaganda*. He uses *psychological tricks* in addition to whatever intellectual reasons he has to offer.
>
> (Feyerabend 1978, 81)

However, Feyerabend argues that Galileo's theory is neither built upon realist sense of observation nor on other corroborated theory. Instead, it was a result of imagination. It was "a daring new suggestion involving a tremendous leap of the imagination" (Feyerabend 1978, 91)

> These [psychological] tricks are very successful: they lead him to victory. But they obscure the new attitude towards experience that is in the making, and postpone for centuries the possibility of a reasonable philosophy. They obscure the fact that the experience on which Galileo wants to base the Copernican view is nothing but the result of his own fertile imagination, that it has been *invented*.
> (Feyerabend 1978, 81)

> We can now add that it leads to the invention of a new kind of experience that is not only more sophisticated but also far more speculative than is the experience of Aristotle or of common sense. Speaking paradoxically, but not incorrectly, one may say that Galileo invents an experience that has metaphysical ingredients.
> (Feyerabend 1978, 92)

The message that Feyerabend wants to convey is that the success of Galileo's theory does not merely lie in its objective reflection of the reality and accurate prediction. Galileo's theory is an imaginative creation which serves a pragmatic goal. There are infinitely many scientific theories that scientist could produce imaginatively to depict the reality which was depicted by Galileo's theory. Feyerabend does not deny that there is more than one objective way of such depiction. That is, according to Feyerabend, there is no one true theory in science. For science to blossom, the principle of proliferation of theory, which is a pragmatic principle, should be endorsed.

Feyerabend contends that the standards of practice for a discipline need not always be rational. Rationality, as illustrated in the example of Galileo's science, is a relative concept which differs from time to time. Pragmatism alone constitutes a sufficient standard for scientific practice. The standard of pragmatism changes from time to time. Notably, Feyerabend does not think that a discipline must possess some necessary objective standards (such as rationality) so that it could claim for its legitimacy. Pragmatism as a sufficient standard will do. The pragmatic goal of a discipline lies not in attaining realist truth but in acquiring wide acceptance of a multitude of theory in the community, which culminates in proliferation of theory and scientific progress.

> I grant that business, religions, special professions such as science or prostitution, have a right to demand that their participants and/or practitioners conform to standards they regard as important, and that they should be able to ascertain their competence…. The standards taught need not be 'rational' or 'reasonable' in any sense, though they will be usually presented as such; it suffices that they are *accepted* by the groups one wants to join, be it now Science, or Big Business, or The One True Religion.
>
> (Feyerabend 1978, 217)

Historical examples provided by Feyerabend convey a message that the legitimacy of a discipline lies in its acceptance by public. To obtain wide acceptance for a theory, pragmatic means such as propaganda and psychological tricks, as used by Galileo, is useful. Rationality, argued Feyerabend, is a dispensable standard for a theory to get accepted. The core criterion for theory evaluation lies in the acceptability of a theory. That is to say, a reasonable scientist will choose a theory based on the consequences (e.g. the number of the followers) of such choice, not based on the causes (e.g. truth, coherence) that make such a choice rational. Hence, in the presence of two rival theories, one needs to evaluate that which theory may potentially draw more adherents if it is chosen. Scientist shall discard the theory which may not potentially draw as much support as its rivals. However, this criterion for theory choice is not without problems.

First, acceptability as a pragmatic criterion for theory choice poses the problem of the criterion for itself: what constitutes a reasonable acceptance? We may conceive of a radical situation where one may accept a theory blindly. Does it constitute an acceptable acceptance? If the answer is positive, the accepted theory can obtain no sustaining force because its adherents uphold it for no reason, not even an irrational one. He who chooses a theory over another for no reason may discard it easily at any point of time. A theory which has been accepted blindly by the majority will be in an unstable state and in a great risk of being abandoned. It does not seem rational to have one theory being chosen and discarded quickly due to losing support from the community. Imagine that if the majority of the scientists accept a theory blindly, the high turn over rate of theory would be detrimental to the progress of science. Hence, if acceptance of any theory is counted as an acceptable acceptance, there would be little progress in science.

However, Feyerabend may rebut that it is not the case that any acceptance of a theory, blind acceptance being one example, could be counted as an acceptable acceptance. He may say that an acceptable acceptance of a theory must be of reasons, regardless of rational or irrational ones. That is to say: any reason goes. However, it is a dangerous statement because one's reason for the acceptance of a theory may not be relevant to the context. Truth may constitute a reason for one to accept a theory; usefulness may constitute another reason; what if one's reason for accepting a theory is out of context, such as "I accept genetic theory *T* because the rainbow is colorful"—which is a statement that does not make sense because the reason for accepting a theory is not related to the context of the theory. Does an out-of-context reason constitute an acceptable acceptance?

If affirmative, the context to which a theory may apply is dubious, for the theory may appear as contextless. The reason of one's acceptance of a theory implies the applicability of that reason in the context of theory. A theory would progress nowhere if it has no defined context of application. However, if an out-of-context reason does not constitute an acceptable acceptance, it will be in contradiction with Feyerabend's claim that any reason will do. Thus, it is obvious that the assertion "any reason goes" fails to account for the acceptable acceptance of a theory.

The second problem of acceptability as a pragmatic criterion for theory choice is an operational one: how could one measure and predict the degree of public acceptance of a theory prior to making a choice? In Feyerabend's account, it is vital to predict the degree of public acceptance of a theory because his pragmatism is outcome-based. He argues that only the theory which could attract a large number of adherents should be chosen, for wide acceptance of a theory would promote proliferation of theory. Notably, it is impossible to have a precise measurement and estimation of the degree of public acceptance of any theory. Is there an objective way for an approximate measurement and estimation? Appealing to statistical methods perhaps is one of the ways. However, Feyerabend does not stress the importance of precise or approximate estimation of the public acceptance of a scientific theory. In various historical examples that Feyerabend has illustrated, successful scientists did not make an estimation of the degree of public acceptance prior to making a theory choice. What they did is, according to Feyerabend, that they made a choice with the *intention* to have their chosen theory accepted widely by public. To materialize their intention, scientists resorted to irrational alternatives such as propaganda and psychological tricks. These irrational alternatives were adopted because they are appealing to the public. Feyerabend did not state clearly whether it is implausible for scientists to make a prediction about the degree of public acceptance of their theory prior to theory choice, or they were simply unwilling to do so. However, the historical examples given by Feyerabend show that the degree of public acceptance for two rival theories could not be certain until a theory choice has been made. If a scientist follows Feyerabend's suggestion to choose a theory based on the estimated public acceptance, she will never be sure which theory should be chosen. Hence, based on the historical examples given by Feyerabend, public acceptability as a pragmatic criterion for theory choice is not feasible but a mere faith. Such a mere faith has no warranty for a chosen theory to succeed, for the public acceptance of a theory is dependent on various external factors, such as the educational level of the public, cultural background, personal belief and so on.

The third problem that arises against the acceptability as a pragmatic criterion for theory choice is related to the second problem. As argued above, acceptability as a pragmatic criterion for theory choice is a good faith. According to Feyerabend, scientists need to use irrational approaches to attract adherents. To do so, scientists have to understand the subjective aspects of the intended audiences. However, Feyerabend does not show us how to do so. In his writings, scientists are assumed to know how to use the right subjective approaches (i.e. propaganda, psychological tricks) if they are to succeed. They are assumed to already have possessed the understanding of the subjective aspects of their intended audience. Scientists are assumed to know what psychological approaches may draw wider public acceptance for their theory; they are also assumed to know how to apply those approaches in order to realize the wide acceptance for their theory. However, to know a psychological approach is one thing, to know how to apply it is another. To know what approach to use, one needs to know the subjective aspects of the audience; to know how to apply an approach, one needs to master the appropriate skills (e.g. persuasion, deception etc). Not to mention that the know-how requires complicated skills, knowing the audience is no simple task. It seems that if a scientist adopts Feyerabend's acceptability criterion for theory choice, she would have to consider the psychological states of her audiences, and master the publicity skills. However, there is no certain way that a scientist can acquire such knowledge and apply it timely and suitably. If acceptability could be a criterion for theory choice, it turns out that there is no certain way to achieve wide acceptance of a theory in scientific community.

In short, it is apparent that the outcome of a theory choice—degree of acceptance of a theory in the scientific community—fails to serve as a pragmatic criterion for theory choice. Feyerabend has no way to show that a theory choice could be made based on the predicted degree of acceptance in scientific community.

However, Feyerabend may still recourse to his slogan of "anything goes" to defend his principle of proliferation. He may argue that theory acceptability in a community is not the only condition to warrant the proliferation of theory, indeed any theory choice would do. This line of argument is a transformation of "anything goes" to "any choice goes". Now the question is: would this argument serve a pragmatic end—scientific progress?

For Feyerabend, proliferation of theory is equivalent to scientific progress. That is, scientific progress is measured by the quantity of the accepted theory. In this sense, the “any choice goes” argument would result in the increase of accepted theory.

Notably, the emphasis of Feyerabend on the quantity of the accepted theory would render him to embrace not “any choice” but “all choice” when a scientist is presented with a range of rival theories. For the “any choice goes” argument suggests that one can make any (one) choice among the rival theories, but for the sake of the proliferation of theory, one should choose all the rival theories instead of only one. So, Feyerabend’s slogan of “anything goes”, which says that all theories are on the same epistemic par, would lead to the conclusion that all theories should be accepted, when associated with the principle of proliferation. To have a maximum quantity of theory to proliferate, it is not good enough for one to choose any (one) theory, but he is obliged to choose all rival theories (albeit the contradictory ones). So, to adhere to the principle of proliferation, Feyerabend has to embrace “all choice goes”. However, choosing all rival theories implies no choice at all, for a choice constitutes two components: inclusion and exclusion. The “all choice goes” argument assumes no exclusion of theory. All theories are on the same epistemic par and equally merit to be chosen. In that case, scientific progress will be just a mere accumulation of theory and it is directionless, for choosing a theory implies choosing a direction. When all rival theories are embraced, such proliferation of theory is inevitably directionless. Using Kuhn’s term, there would have no paradigm. Perhaps Feyerabend is not bothered by the directionless of science in his principle of proliferation, as he has explicitly against the domination, which implies a direction, of a single theory in science.

4.4.3 Democracy and Relativism

Feyerabend's relativism in philosophy of science has been extended to his political thought in which he comes to embrace a notion of "free society". Free society, according to Feyerabend, "is a society in which all traditions should be given equal rights *no matter what other traditions* think about them" (Feyerabend 1995a, 75). It is a liberal society where democracy prevails and everyone has a right to choose his favorite tradition or theory, regardless it is scientific or non-scientific, rational or irrational.

Notably, Feyerabend asserts that the prevalence of science is a threat to democracy because the equality of traditions is destroyed (Feyerabend 1978a). Individual citizen has deprived of their right to choose the tradition which is other than science. In a society where science prevails, non-scientific traditions are viewed as irrational and should be discarded. It is thus not a free society because science is the only arrangement for all citizens. Science is enforced on all citizens without seeking their approval.

> We accept scientific laws and scientific facts, we teach them in our schools, we make them the basis of important political decisions, but without ever having subjected them to a vote…. Modern society is "Copernican' not because Copernicanism has been put on a ballot, subjected to a democratic debate and then voted in with a simple majority; it is 'Copernican' because the scientists are Copernicans and because one accepts their cosmology as uncritically as one once accepted the cosmology of bishops and cardinals.
>
> (Feyerabend 1978, 301-302)

As a democratic relativist, Feyerabend against the view that science should be favored due to its enormous success. In view of the fact that "science is not always successful"

(Feyerabend 1978, 306), Feyerabend insists that everyone has a right to reject science in a free society. Of course, it does not mean that individual is encouraged to embrace non-science rather than science. Rather, the main point is that individuals should be given the right to do so if they seem fit. For example, a patient should have the right to choose traditional medicine over modern medical treatment, an act which should not be deemed irrational. Any attempt to hold that *only* science is rational is a dangerous move, for it may lead to the abandonment of culture (as the customs, life styles, religions, etc cannot be rationalized in the ways the sciences do).

Feyerabend has distinguished between the rights of citizens and the consequences of exercising these rights (Feyerabend 1978a, 86). Though Feyerabend does not explicitly admit Lockean natural rights into his thought, he states that rights should be given to citizens for two reasons. The first reason is that "everyone must be able to pursue what he thinks is truth, or the correct procedure" (Feyerabend 1978a, 86). The second reason is related to his principle of proliferation: "because the only way of arriving at a useful judgement of what is supposed to be the truth, or the correct procedure is to become acquainted with the widest possible range of alternatives." (Feyerabend 1978a, 86).

The first reason of conferring the rights to citizens is fairly compatible with the notion of liberal society. A liberal society grants individual rights to think, say, and act, with a proviso that the granted freedom is not in violation with others' well-being. It is this proviso that is missing in Feyerabend's thought. The proviso of rights is always taking the form of laws in a liberal society. A law legitimately commands obedience (Held

2006), with no exception. However, Feyerabend does not set a boundary for the citizens' rights to operate. He has not considered the scenarios where the rights of citizen might be in conflict with ethics, laws and public welfare. Consider one of such scenarios:

> A man who was badly injured in a car accident expressed his will that he prefers traditional medical treatment to modern medical treatment. If he is not treated by modern medicine immediately, he will possibly die. The dilemma now is: should individual rights be emphasized over the ethical concern?

For Feyerabend, the rights of the injured man in the scenario above should be respected. Nobody should force him into receiving modern medical treatment. Feyerabend would have argued that the consequence of exercising this right should be taken separately. He holds that the consequence of exercising a right should not deny and override the right itself. That is, the consequence should not affect a decision of choice (e.g. science/non-science). In the scenario above, Feyerabend would have said that one should not force the injured man to receive modern medical treatment although he would probably die by insisting on traditional medicine.

> Scientists, of course, assume that there is nothing better than science. The citizens of a democracy cannot rest content with such a pious faith. Participation of laymen in fundamental decisions is therefore required even if it should lower the success rate of the decisions.
>
> (Feyerabend 1978a, 87)

Citizens' rights without proviso are extended to the making of scientific decision and policy. Feyerabend stresses that citizens' decision should be taken into consideration in science and education of science.

> Assuming this right, a citizen has a say in the running of any institution to which he makes a financial contribution, either privately, or as a taxpayer: state colleges,

> state universities, tax supported research institutions such as the National Science Foundation are subjected to the judgement of taxpayers, and so is every local elementary school. If the taxpayers of California want their state universities to teach Voodoo, folk medicine, astrology, rain dance ceremonies, then this is what the universities will have to teach.
>
> (Feyerabend 1978a, 86-87)

> Duly elected committees of laymen must examine whether the theory of evolution is really as well established as biologists want us to believe…. They must examine whether scientific medicine deserves the unique position of theoretical authority….. In all cases the last word will not be that of the experts, but that of the people immediately concerned.
>
> (Feyerabend 1978a, 96-97)

Apparently, Feyerabend suggests that science should be supervised by laymen in order to protect their rights and benefits. Furthermore, Feyerabend believes that laymen can spot the errors of specialists (Feyerabend 1978a, 97). Conceivably, Feyerabend would have suggested that theory choice of scientist should be subjected to public's supervision too, because there are theories, such as Darwin's theory of evolution, which are contradictory to individual's belief. When a layman has a say in scientific theory choice, which is deemed legitimate regardless of whether it is rational or irrational, the decision made by scientist would be governed largely by non-scientific factors. The choice of a theory over its rivals would be relative to the contemporary public's opinion. Scientist would incline to be a social pragmatist, rather than being a scientific pragmatist, in arbitrating between rival theories. It is highly improbable that a choice would be based on scientific consideration when laymen have a role to play in making a theory choice.

If that is the case, theory choice is no longer a scientific issue but a socio-political one. Even if a scientist would seriously take scientific factors into consideration, he still has to put the public's opinion at the forefront. Following Feyerabend's line of thought, a free society would have its citizens exerting fully their rights and opinions, overriding that of scientists, on scientific activities. Scientists are deemed to work for the citizens by adhering to their opinion and decision. Scientist would then assume a role more as a "socio-political scientist" than a professional scientist.

Thus, theory choice in a free society has no scientific base because it is arbitrary. According to Feyerabend, however, it is favorable because it would promote proliferation of theory, and consequently scientific progress. Feyerabend is right to say that theory will proliferate, but he is wrong to think that it leads to progress in science. The reason is simple: if there is a progress in any sense, it is not of science; because theory choice and other scientific activities are dominated by laymen's opinions which are not scientific. Although in a Feyerabendian free society everyone is granted equal rights to science and non-science, the progress of science would inevitably be deprived due to the domination of laymen's opinion.

Feyerabend may rebut that the participation of laymen in theory choice is beneficial in two senses: (1) it will ensure that no false theory can dominate and overrule the true rivals, in the case where a scientist has made a wrong choice; (2) it will ensure the progress of society. The first rebuttal assumes that human is fallible, including scientists. However, this rebuttal does not consider the fact that laymen are more likely to err than a well-trained scientist with regard to scientific matters. Laymen, with their own interests and beliefs, will not do justice to science. Assuming that they have good will to treat science fairly, they are not capable of doing so because they do not possess the required knowledge and skills. Hence, laymen's participation in theory choice is a disaster to science rather than saving science from domination by false theory. The second rebuttal assumes that progress of society is more important than of science. Though Feyerabend does not say that progress of science is unimportant, his thought has implicitly excluded the parallel progression of science and society. For science to progress, Feyerabend requires proliferation of theory. However, proliferation of theory does not increase the likelihood of the accumulation of true theory, which in turn does not drive science to progress. However, theory proliferation (i.e. prevalence of both scientific and non-scientific theories) may drive society to progress in a limited sense: the society becomes more liberal where everyone's rights are respected and realized. The society, as a whole, is unlikely to progress in terms of scientific advancement, for scientific progress is inconceivable in Feyerabendian free society. Feyerabend has overlooked the fact that the progress of the modern society is always achieved via the techno-scientific advancement. Progress in society in terms of the promotion of citizen's right without a simultaneous progress in science can hardly be justified as a real progress, at least in the modern sense. Hence, it is apparent that Feyerabendian free society is a free but unprogressive society. It can be concluded that participation of laymen in theory choice and other scientific activities is democratic but unfruitful.

4.5 Conclusion

This chapter explores three well-known relativists' philosophy and their stances on the thesis of theory choice. Although they belong to the same camp, their relativism differs in kind. Nelson Goodman and Thomas Kuhn are mild relativists who do not go against the rationality of science, whereas Feyerabend is a radical relativist who promotes the prevalence of irrationality and suggests that rationality should not have a dominating position in science. However, three of them share a common stance in anti-foundationalism, claiming that science should not be reduced to a single universal theory or law. They hold that a foundationalist view of science is detrimental to the progress of science.

Goodman has developed a pluralist theory of world-versions. It is a contextualist theory which asserts that there exist multiple right world-versions. World-versions are the perspectives through which one constructs her world. Hence, world-versions make plausible the interpretation or perception of the world in which one resides. Goodman denies the idea that there is only a single world. Indeed, there are multiple worlds which are distinct and equally real. Goodman holds that world-versions are different ways of interpreting the worlds, which could not be reduced to only one. World-version consists of not merely subjective elements; it is also objectively based in the sense that one cannot construct a world-version in an arbitrary way. The subjective elements of a world-version are referred as style, which represents the subjective lens to interpreting the objective world. Different styles in doing science would result in different interpretations of a phenomenon and a theory.

According to Goodman's principle of world-versions, scientific theories could be interpreted in more than one way, dependent on which world-versions one is holding. There would be no definite way to arbitrate between rival theories. Different scientists have different world-versions that would inevitably lead to different judgments on the candidate theories. Notably, Goodman does not suggest that the disagreement among scientists on theory choice could be settled by reference to the reality, for there is no ultimate mind-independent reality. Goodman has used a famous example to illustrate this idea. The assertions that "the earth moves" and "the earth stands still" are both true in different worlds yet incompatible, argued Goodman. The truth value of the assertions is dependent on one's perspective. The non-existence of a single ultimate reality renders theory choice indefinite, in the realist sense that as if there could have only one correct choice corresponding to a single world. However, theory choice is definite in the pluralist sense that there could have one correct choice relative to each of the world-versions. In Goodman's philosophy, theory choice is relative to the pluralistic reality. The recognition of a pluralistic reality is termed as "plurealism" by Scheffler (Scheffler 2000 & Scheffler 2001), which is an approach upheld by Goodman in his theory of world-versions.

As a relativist, Goodman subscribes to the notion of the proliferation of world-version. He holds that "conflicting versions often present good and equal claims to truth" (Goodman 1978, 110). However, the proliferation of world-version is different from Feyerabend's proliferation of theory in terms of motive. For Goodman, proliferation of world-version is a natural corollary of science, as different scientists have different styles and perspectives in doing science. For Feyerabend, proliferation of theory is an unnatural objective, which should be strived for in order to prevent the tyranny of a single dominating theory. Hence, theory choice in Goodman's account is world-version relevant, which could be rationally justified. However, theory choice can hardly be rationally justified in Feyerabend's account because a scientist may choose any theory to counteract the dominance of a true theory.

Goodman's pluralistic notion of world-version invites the problem of theory choice which has also been faced by realist. Though Goodman has rejected a fundamentalist notion of reality, he is by no means an anti-realist. In fact, he can be characterized as a pluralistic realist—perhaps can be called "plurealist", using Scheffler's term. He admits that all world-versions are equally true, which are corresponding to diverse realities that underlie beneath. According to Goodman, there is no absolute truth across the realities. Indeed, what a scientist can pursue is the truth which is relative to each world-version. The problem of theory choice that haunts realist thus comes into this way to Goodman's philosophy: how could the indeterminate observed data determine the theory in each world-version? Crucial experiment is impossible to decisively arbitrate between rival theories in each world-version, because theory is underdetermined by evidence. Scientists are unable to choose decisively among rival theories which entail identical observational consequences. We may assume that Goodman would have suggested that by altering the auxiliary hypotheses the scientists may obtain non-identical observed phenomena. However, Pitts has shown that the empirical equivalence of theories remained stable under the change of auxiliary hypotheses (Pitts 2011). Obviously, Goodman's plurealist account cannot solve the problem of theory choice because theory in each world-version is assumed to correspond to the world-version constrained reality.

Goodman could have avoided the problem of theory choice if he is a modal realist advocating the notion of possible worlds. Unfortunately, he asserts that all worlds are parallel-running actual worlds. A modal realist may at the same time be a relativist. He may say that notwithstanding the fact that theory is underdetermined by data in the actual world, there are possible worlds where observed phenomena are determinate to arbitrate among rival theories. The observed phenomena are, a modal realist may argue, distinct in different possible worlds. Alternatively, a modal realist may state that the empirically equivalent rival theories can co-exist in distinct possible worlds, while the theory choice is made in an actual world. Hence, a modal realist may conclude that theory choice is always in principle possible. If Goodman were a modal realist, he will not face the problem of theory choice in his account of world-versions.

This chapter has elaborated Kuhn's relativism at length due to its significant and lasting influence in the post-positivist philosophy of science. Kuhn's philosophy has received much attention and criticism partly due to the novelty of its notions (e.g. paradigm, normal science), and partly due to the ambiguity of his writings. Notably, Kuhn does not perceive himself a relativist. He has publicly repudiated the label of irrationalist which was conferred upon him.

Kuhn has not professed irrationalism in his thought. Nonetheless, the implication of his philosophy, which was widely read as typical relativism, is often associated by his critics to irrationalism. On the contrary, Kuhn claims that rationality is an indispensable feature of mature science. On his account, rationality is always presupposed in scientific activity.

The criteria of theory choice (e.g. accuracy, coherence, etc) are rational, according to Kuhn. Scientists can and must use a rational criterion to arbitrate between rival theories. Unfortunately, the problem that Kuhn faces is that he fails to account for the prevalence of the irrational factors in the process of theory choice. Although a scientist may make a choice among rival theories with rational criteria, his choice is inevitably influenced by irrational factors such as his academic background, personal belief, and peers' opinion. The close association between socio-psychological factors and scientific practices that has been propounded by Kuhn contributes to the irrationality of theory choice, as accused by scientific realists and antirealists. Hence, they claim that theory choice is to some extent arbitrary in Kuhn's philosophy. Apparently, Kuhn rejects such accusation by holding that his criteria of theory choice are rational and independent from the process of making a theory choice. Kuhn attempts to justify that his criteria of theory choice are normative and independent from the historical context, while the socio-psychological factors that shape an actual decision on a particular theory are contingent and descriptive. Therefore, he stresses that his normative criteria of theory choice is rational and independent from the irrational contingent socio-psychological factors.

The notion of paradigm, which represents one of the most important notions in Kuhn's philosophy, fares no better to get rid of relativism. In fact, it is unclear how a rational replacement of paradigms is possible. According to Kuhn's narration, the nature of paradigm is socio-psychological rather than scientific. Paradigm represents a communal consensus held by the scientists who are working within the same tradition. Consensus can be achieved via persuasion, training, textbooks, communal values and the pressure from peers. The adherents of a paradigm do not always choose to uphold it because of the scientific reason; they may choose to believe and practice in a paradigm merely because they are taught that it is a true paradigm.

Further, shift from one paradigm to another exposes the problem of incommensurability according to which Kuhn has not satisfactorily accounted for the translation failure and referential discontinuity. According to the incommensurability of paradigms, there are no shared empirical consequences between rival theories because scientific terms cannot be translated point-by-point into a neutral language. As a consequence, theory choice inevitably lacks a rational ground.

Feyerabend holds a pluralist view of scientific theory. He has criticized the foundationalism and the rationalism as the tyranny of science. He holds that there should not be a single theory which dominates by ruling out its rivals. According to Feyerabend, the domination of a single theory prevents the growth of other better theories. Contrary to scientific realist's correspondence principle, Feyerabend's principle of proliferation asserts that any theory, regardless of whether it is true or false, should have equal chance to prevail.

There are no rational criteria for theory choice in Feyerabend's relativism. The principle of the proliferation of theory promotes "anything goes" in science. Strictly speaking, any theory could be chosen using any standard. For Feyerabend, it is not a matter of right or wrong. The objective of theory choice is to serve the proliferation of theory. Since

Feyerabend holds that all theory, being true or false, should have the chance to proliferate, theory choice is thus an irrational activity. According to the principle of anything goes, any choice goes.

In short, this chapter has demonstrated that Feyerabend has totally dismissed the thesis of theory choice by his radical relativism, whereas Goodman and Kuhn strive to maintain a rational outlook of their thought on theory choice. Although Goodman and Kuhn have stressed that they are taking a rational stance, their relativism has somehow rendered them an irrationalist image. It is largely because the criteria of theory choice proposed by Goodman and Kuhn are relative to world-versions and paradigms, respectively.