The Language of Science as Creole: A Case Against Incommensurability

by

Jean-Jacques Rousseau

A thesis submitted in conformity with the requirements
for the degree of Doctor of Philosophy

Institute for the History and Philosophy of Science and Technology
University of Toronto

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Abstract
The incommensurability thesis (IT) is the claim that historically related theories cannot be fully translated into each other. IT shows how a scientific theory can be different from its predecessor in a way deemed revolutionary. This dissertation, in contrast, argues that IT reflects the development of science only from the perspective of the observer looking from afar. IT does not engage with science as a participant-driven activity. Analysts like philosophers can never directly engage science but can approximate the scientist perspective by peering over the shoulders of participants through detailed historical case studies. Zooming in this way, one can see IT dissolve. The discontinuity IT
modeled is now a consequence of rather than a break from the continuity of scientific work. The dissertation introduces the notion of the language of science as creole to reconcile the observer and participant perspectives. This results in a need to abandon the influential historiography of Thomas Kuhn and begin the search for an alternative. The dissertation also considers implications outside of science studies and argues that practical endeavours like business innovation studies can no longer take guidance from Kuhn. They should be redirected.

Chapter 1 traces the history of IT, outlining three periods of Kuhn’s thought that saw it move from a consequence of the notion of paradigm to his core concern. Chapter 2 considers reactions to Kuhn’s ideas, both friendly and hostile, that result in two key charges: IT is either self-conflicted or it is irrelevant to how science develops. The chapter also identifies a key gap in the literature, namely that both Kuhn and his interlocutors fail to discover the confusion that IT treats a past stage of one’s language and a foreign language as equivalently different from current speech. Chapter 3 advances the notion of the development of the language of science as creole formation. Creoles show how to reconcile the dialectic of continuity and discontinuity that alone can fuel progress. Chapter 4 shows how the notion science as creole can lead to a more actionable philosophy of science that provides a firmer intellectual basis for business innovation policy and practices.
Dedication

To my late father Jean-Robert Rousseau for infecting me with his love of learning and books, and to my mother Jacqueline Colimon Rousseau who was my first and remains my most demanding teacher.
Acknowledgments

It has been a long and twisted journey to here. I am especially thankful to my thesis supervisor Brian Baigrie for encouraging me to pursue the doctorate and for seeing it through with me … always in good cheer.

I also want to thank the members of my examination committee for their time and for judging the dissertation a useful addition to the long dialogue on science: Hakob Barsegyan, Jagdish Hattiangadi, Paul Thompson and Marat Ressin. The mixture of philosophy, history, linguistics and innovation studies that follows is rather esoteric. It is a testament to the open-mindedness of the committee members and I especially thank them for that.

Finally, long conversations with Dara Byrne about Foucault, Habermas and Hegel are background to much of my concerns and a chance meeting with James Robert Brown nudged me to return to the research just when I was undecided. Et à tous ceux et celles qui me disent qu’il n’y a rien que je ne puisse faire, qui peut être fait.
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Introduction

The Incommensurability Thesis

Incommensurability is the cornerstone of what Michael Friedman calls “our best current historiography of science”.¹ It offers an influential theory of the genesis and development of theories that goes against the grain of an older historiography still found in most scientific textbook accounts. From these we learn that Newtonian mechanics is a special case of the more powerful relativity theory; mechanics is relativity at low velocities compared to the speed of light.

This approach interprets relativity as a series of piecemeal additions and revisions to mechanics that retains empirical adequacy for low velocity situations and then expands to also capture higher velocity situations. Incommensurability proposes a different perspective. Like the scientific textbook approach to scientific change, incommensurability interprets a theory and its successor as a pair that shares some empirical content. However, unlike textbook accounts the incommensurability thesis (IT) emphasizes the untranslatability within the pair. Where scientific textbooks emphasize the continuity in the development of science, IT proposes a wholesale replacement of inter-defined core terms of a

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¹ Michael Friedman, Dynamics of Reason (Stanford, CA: CSLI Publications, 2001), 47.
theory with inter-defined terms of its successor. On this model, the development of science is punctuated by breaks. According to IT co-founder Paul Feyerabend this is because a theory is “a general point of view” that cannot be reduced to its predecessor.² When a successor theory appears to be reduced to its predecessor, in a science textbook for example, it conceals the fact that this is only possible because its core terms are re-interpreted and ascribed meanings foreign to the initial theory. Historian Peter Barker labeled this, “the silent amendment of meaning that makes historical sources hard to understand”.³ On the apparent reduction of special relativity to mechanics, the other IT co-founder Thomas Kuhn thought that this simply shows why ”Newton’s laws ever seemed to work”.⁴ Kuhn further argued that, although relativity statements can be reduced to Newtonian statements by restricting velocity, Newtonian statements


⁵ Ibid.
cannot be derived from relativity. One reason is that, insofar as “Newtonian mass is conserved; Einsteinian mass is convertible with energy”. Kuhn’s articulation emerged as the most influential. As Ian Hacking tells us, “Feyerabend dropped the topic, whereas it preoccupied Kuhn until his last days”.

IT as untranslatability was advanced exactly to incorporate the phenomenon of shifts in the meaning of core terms like ‘mass’ into a more defensible philosophy of science. It took the position that an adequate understanding of science must account for its development, and that this development reveals breaks that result from wholesale changes in the inter-related core terms of a theory. Controversially, IT implies that the transition from mechanics to relativity resulted in scientists living and working in a different world than the one before the transition. This is premised on a distinction between ‘normal’ and ‘revolutionary’ change. Kuhn explains: “In normal change, one simply revises or adds […] In revolutionary change one must either live in

incoherence or else revise a number of interrelated generalizations together”.

The move from Aristotle’s finite spherical universe to Newton’s infinite universe is another example of wholesale change. Under both systems, a stone thrown upwards falls back. They are only different in their explanations for why unsupported stones fall. Aristotelian notions of substance and motion make it quasi-tautological that stones fall. Without a universal center, Newton offers a different explanation related to local centers of attraction. According to IT, wholesale change is an all or nothing affair. The world cannot both have a center and not have one, just like mass cannot be both a constant and velocity dependent. Trying to choose between such alternatives through point-by-point comparison is a dead end. I see a book and my cat, which is longer? I can take a ruler, measure and then compare the results. Theories related through normal or piecemeal change can be compared this way, they are translatable. Theories related by wholesale or revolutionary change cannot be compared this way. They are untranslatable for lack of a ‘ruler’. Critics of IT point out that without this ability to compare, the choice between competing theories cannot be rational and a succession of such choices cannot be called progress.

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A Case Against Incommensurability

Even those who believe IT is part of the best theory of scientific change available see a need for reform. For example Friedman calls for the replacement of “Kuhn’s twofold distinction between normal and revolutionary science with a threefold distinction”. 8 The details of his proposal matter less than the reason for it. Friedman points to the need to address the challenge the Kuhnian theory poses to the rationality of science. This dissertation aims at an understanding of scientific change that also side steps the issues with IT.

Consider a science over time as a continuous process of theory change in a given domain. Suppose we take two snapshots of that process far enough apart in time. Each snapshot is a reconstruction that consists of a set of propositions. Each carve out clusters we can consider a stage in a continuous process. If we take a snapshot of mechanics at Cavendish labs in 1890 and another in 1930 and compare them, Feyerabend and Kuhn are correct that the two are incommensurable. By this is meant that their side-by-side comparison results in fundamental mismatch. At this “bird’s eye” level of analysis, snapshots appear as though endpoints of a process. Clearly, the endpoints can only be

8 Michael Friedman, *Dynamics of Reason*, 44.
arbitrarily chosen, snapshots being related to history in the way that composition has been called frozen improvisation.⁹

It is well understood that photographs and compositions are limited in their abilities to capture a dance or musical performance. Similarly, snapshots from the history of science are limited. We wouldn’t want to confuse snapshots for the lived experience of scientists any more than we would an image for its subject. Limitations are not merely due to gaps in the historical record and to the imprecision of our tools of analysis. The object of our study itself also imposes limits, and these are the ones with which this dissertation will engage. These are flux in key terms and concepts over time and fuzziness in their meanings at a point in time.

In terms of flux, they are the shifts of meaning that are captured in the comparison of snapshots. This was the focus of Feyerabend, Kuhn and other interlocutors in the IT debate. This flux results in gaps that can be narrowed by way of the higher resolution case studies. As the interval between the two snapshots diminishes, the gap between core-term-clusters correspondingly narrows. At the limit, snapshots overlap and incommensurability dissolves.

⁹ The idea of composition as frozen improvisation is attributed to Igor Stravinsky. See Mike Heffley, *Northern Sun, Southern Moon: Europe’s Reinvention of Jazz*, 319 n. 70.
because the well-defined reconstructions that formed endpoints are virtually indistinguishable.

In terms of fuzziness, it is a property of beliefs and practices at a point in time. This is also a limit imposed by the object of our study. The concepts that make up theories are fuzzy objects and the mixed practice of scientists is a constant challenge to frictionless communications. These cause difficulties such as the reproducibility of experiments.

To summarize, snapshots are indispensable tools for the study of science. However, it is important to appreciate their limitations as reconstructions. Flux and fuzziness are two limits imposed by the object of study itself. I note that zooming in to dissolve incommensurability due to flux does not return us to the textbook view of science.

Science as Creole

This dissertation proposes a different understanding of scientific change based on the model of creole formation that births and grows new languages. Of course, science is not a language but a set of practices captured in and guided by (specialist) languages. As observers, philosophers do not do science but study these languages of science. It is these specialist languages that are creole-like in the sense of each being a contact language of the sub-disciplinary
practices of theory, experiment and instrumentation. The model of creoles offer a mechanism for understanding how interactions between groups with different backgrounds, assumptions and interests can produce a language they can all understand and use.

Consider the transformation of the language from the times of Chaucer to today. Telling this as leaps from one linguistic group to another as IT tells the story of physics is only a first approximation. It would clearly explain why today’s speakers require special training to read Chaucer. However, using the IT approach would make a mystery of the claim that it is English that we are trained in when taught to read the Tales. This is analogous to the problems IT presents for rationality and progress. In addition, for the modern speaker, translating from the Tales is hard but very different work compared to translating from modern Mandarin. The story of English is that of an unbroken chain of speakers – including Chaucer – who negotiate internal changes and contact with other languages all at once. Whatever lack of mutual intelligibility may exist between Chaucer and us, it emerged from an unbroken chain of custody. When considered this way, no great catastrophe is upon us. Kuhn did not acknowledge this as a theoretician of science but tacitly subscribed to it as a historian. Despite his incessant promotion of IT, he notoriously failed to make use of this concept in the detailed historical study. The IT does not appear in his two major historical
studies\textsuperscript{10}, including in the one he considered his “best work”.\textsuperscript{11} Under the view advanced in this dissertation, this is because, for an observer peering over the shoulder of participants, IT is virtually non-existent. While Kuhn notes that detailed history reveals revolutions are always “within the reach of an exceptionally capable person”,\textsuperscript{12} he does not appear to fully appreciate the implications of this historian’s view to his philosophical notion of IT.

\textit{Implications of Decision-Making}

In addition to theoretical concerns, there are practical considerations related to the influential nature of the Kuhnian view of science for the funding and commerce of science. It is not sufficient to demonstrate that IT is incoherent or even enough to offer the alternative of the language of science as creole. As it stands, IT as untranslatability is the orthodox view of scientific change outside of philosophy. In a recent paper, management theorists Sarah Kaplan and Keyvan Vakili sift through 20 years of US carbon nanotubes patents looking for

\textsuperscript{10} They are \textit{The Copernican Revolution} (1957) and \textit{The Black-Body Theory} (1978).


\textsuperscript{12} T.S. Kuhn, “Afterword” in \textit{Black-Body Theory}, 354.
“breakthrough novel ideas”. From Kuhn they retain that, “shifts in ideas can be detected in shifts in language”. In the view of this dissertation, they are drawing lessons from observer work to formulate recommendations for participants. Given the incoherence of IT, this is a risky endeavour. It is my strong belief that philosophy of science should not only do justice to its object of study but it should also be actionable in the sense of having practical relevance. Therefore, it is important to consider the implications of science as creole beyond physics and its history. An analysis of science in action rather than as settled work unlocks the fuller potential of historicist motivations that informed the rich IT debate.

The Structure of the Dissertation

The following list summarizes the questions addressed in the dissertation:

- **Chapter 1:** What is the incommensurability thesis? Where does it come from? What was Kuhn’s journey of engagement with these problem(s)?

- **Chapter 2:** Is IT as untranslatability coherent? What did the critics have to say? How can we respond to key criticisms? What remains unresolved?

• **Chapter 3:** What would an alternative to IT look like? What is a creole and how can it provide a better model for the development of science?

• **Chapter 4:** What is the link between IT and business innovation? What practical advice does science as creole have for business innovation?

Grounding real-world decision-making in a more correct understanding of science is essential to the realization of a more rational culture in which progress is encouraged. Surely this is a worthy goal to which philosophy of science should actively contribute.
1.0 The Story of the Incommensurability Thesis

Background

IT as untranslatability is an attempt to get at a real problem faced by historians and philosophers when looking back at past science. In the transition from one theory to the next, “even if the same words are in use, their very meaning has changed”.14 Historically related theories that on the surface model the same phenomena, as the theories of motion of Aristotle, Newton, and Einstein, “constitute implicit definitions of their terms, and so they comprise different languages”.15 The problem, as Philip Kitcher sees it, is “a situation involving a special type of referential change, namely change which culminates in a mutual inability to specify the referents of terms used in presenting the rival position”.16 Peter Galison called this the problem of, “paradigms that pass each other like ships in the night”17, and Friedman identified it as the site of, “methodological


stand-off”\textsuperscript{18}. Kuhn himself held “incommensurability has to be an essential component of any historical, developmental, or evolutionary view of scientific knowledge”.\textsuperscript{19}

Although there are multiple candidates for the origins of IT, there is consensus that the thesis takes off about half-century ago, appearing in print in 1962, independently in Kuhn’s work and that of Feyerabend.\textsuperscript{20} Less well-known is the use of the term that same year in a work on Aristotelian physics by philosopher Wolfgang Wieland (1933 – 2015).\textsuperscript{21} Stefano Gatteei traces the general climate in which IT emerged to the influences of Karl Popper on the theoretical character of observations. He also notes that the later Ludwig Wittgenstein was also influential.\textsuperscript{22} Other thinkers including microbiologist-philosopher Ludwik

\begin{flushright}
\textsuperscript{18} M. Friedman, \textit{Dynamics of Reason}, 89.
\textsuperscript{19} T.S. Kuhn, “The Road Since Structure\textit{”, 91.
\end{flushright}
Fleck (1896-1961), the philosopher-chemist Michael Polanyi (1891-1976) and philosopher Norwood Russell Hanson (1924-1967) were all key contributors to the establishment of the ‘new philosophy of science’.  

Eric Oberheim identifies another tributary line from conversations between Feyerabend and Elizabeth Anscombe on logical positivism around 1950 where the term ‘incommensurable’ was used. Specifically, Oberheim argues that Feyerabend found the idea that “some pairs of successive scientific theories are logically inconsistent” in Popper, who in turn credited Pierre Duhem. To follow a concrete example: Georg W.F. Hegel argued that one could derive Isaac Newton’s planetary laws from Johannes Kepler’s. As Oberheim reports, William Whewell disputed this point by invoking an early version of incommensurability. In his own words, Whewell held that, “though Kepler detected with great acuteness the Numerical Laws of the solar system, he laboured in vain to


conceive the very simplest of the Laws of Motion by which the paths of the planets are governed”. Whewell continues:

The distances and periods of the planets were all so many separate facts; by Kepler’s Third Law they are connected into a single truth […] The planets described ellipses round the sun, in the contemplation of others as well as of Newton; but Newton conceived the deflection from the tangent in these elliptical motions in a new light – as the effect of a Central Force following a certain law; and then it was, that such a force was discovered truly to exist.

If Whewell is correct that the deflection from the tangent is the thing, then Hegel is wrong that Kepler’s equations could produce Newton’s theory. All this allows Oberheim to summarize things thus, “Feyerabend (1962), following Popper (1957), following Duhem (1906) merely [applied] Whewell’s point [1858] to Nagel instead of to Hegel”.

26 William Whewell. *Novum Organon Renovatum* in *Theory of Scientific Method*, 108. This is suspect. Brian Baigrie reminds us that the first edition of *Principia* refers to Kepler’s laws as a “hypothesis”. This is confirmed in a letter to Edmond Halley that refers to them as mere “guesses”. This adds up to Whewell and others making, “a mistake of historic proportions to suppose that [Kepler’s laws] served as the empirical foundation for Newton’s system” (Brian Baigrie, “Philosophy of Science as Normative Sociology”, *Metaphilosophy* 19, no 3 - 4 (July/October 1998): 239.


On pre-1962 uses of the term itself, an early and very relevant use is found in a 1938 translation of a passage from Wolfgang Köhler’s *Gerstalen*:

In order to orient itself in the company of natural sciences, psychology must discover connections wherever it can between its own phenomena and those of other disciplines. If this search fails, then psychology must recognize that its categories and those of natural science are incommensurable\(^29\). [Emphasis added].

Given the centrality of examples from Gestalt psychology to Kuhn’s formulation of IT – duck or rabbit, box punched in or out, etc.\(^30\) – he is likely to have read *Gerstalen*. Indeed, the relevance of Gestalt and its treatment of the relationship of parts and wholes to philosophy found its way in publications a decade before *Structure* in an exchange between philosophers Edward H. Madden and Nicholas Rescher.\(^31\) However, the founding expression of IT is by Feyerabend.


\(^31\) The debate centered on whether the analytical view of science can account for the emergent properties of wholes. See Edward H. Madden, “The Philosophy of Science in Gestalt Theory”, *Philosophy of Science*19, no. 3 (July 1952): 228-238; Nicholas Rescher, “Mr. Madden on Gestalt Theory”, *Philosophy of Science* 20, no. 4 (October 1953): 327-328; and E.H. Madden, “Science, Philosophy and Gestalt Theory”, *Philosophy of Science* 20, no. 4 (October 1953): 329-331.
Feyerabend on IT

Paul Feyerabend offered the first systematic exposition of IT. He held that a theory cannot be reduced to its predecessor without core terms undergoing a change through the reduction. A theory pair must share empirical content like Aristotelian impetus and Newtonian momentum do related to motion. A side-by-side comparison reveals the theories are incommensurable in the sense that one cannot be expressed using the “primitive descriptive terms” of the other. Having \( mv \) as an identical mathematical form does not help, since “whereas impetus is supposed to be something that pushes the body along, the momentum is the result rather than the cause of its motion”.\(^{32}\) While the theories produce the same measurements in their shared domain, they have different empirical content outside this domain. In the transition from \( T_0 \) to \( T_1 \), \( T_1 \) does not incorporate the ontology, formalism, and observational terms of \( T_0 \). Instead, \( T_1 \) represents a wholesale replacement of \( T_0 \). Here is a reconstruction of Feyerabend’s original three-step argument,\(^{33}\) presented in *modus tollens* form:

\(^{32}\) P.K. Feyerabend, “Explanation, Reduction and Empiricism”, 65.

Commensurable theories are related by deduction \((C \rightarrow D)\)

Since \(T_0\) is an empirically adequate theory in a sub-set domain of \(T_1\), it can be obtained as a special case of \(T_1\) via logical deduction; \(T_0\) and \(T_1\) are commensurable. By definition, the logical deduction only holds if the meaning of the descriptive terms in the overlapping domain of \(T_0\) and \(T_1\) remain invariant with respect to the deduction.

A precursor theory cannot be deduced from its successor \((\neg D)\)

However, there is no guarantee that when we carry a deduction from \(T_1\) we end up with \(T_0\) because the meaning of descriptive terms in the overlapping domain of \(T_0\) and \(T_1\) do not remain invariant with respect to the deduction. Feyerabend actually argues that \(T_0\ can\ not\ follow\ from\ T_1\, because\ a\ relation\ of\ logical\ deducibility\ requires\ the\ very
meaning invariance that would prevent $T_1$ from being an interesting and fruitful successor to $T_0$. Instead, the deduction will produce $T'_0$, a theory experimentally indistinguishable from but incommensurable with $T_0$.

$T_1$ and $T_0$ are not commensurable ($\therefore \neg C$)

According to this view, science analysts have been unwise to IT because the historical precursor $T_0$ and the version of it, $T'_0$, reconstructed from its successor $T_1$ cannot be told apart experimentally. Kuhn would later adopt this view, holding that reconstructions may even improve a past theory but don’t help when “applied to the very next problem faced by the discipline”. $^{34}$ In this sense, reconstructions lack the fertility of organically grown theories.

Since the historical $T_0$ and reconstructed $T'_0$ cannot be distinguished by empirical tests they appear to be the same theory when in fact they are not. For Feyerabend, the traditional view of progress as content accumulation, a view encouraged by an interpretation of $T_0$ and $T'_0$ as interchangeable, does not hold. Examples of this sort are everywhere. Feyerabend offers the transition from

$^{34}$ T.S. Kuhn, “Second Thoughts on Paradigms”, in Essential Tension, 303.
Galilean science to the physics of Newton. Galilean free fall is premised on frictionless inclined planes and penduli. Newtonian free fall requires the earth to be vacated of air, to not rotate and for distance from the surface to be much less than the radius of the earth. Put side-by-side, these are incommensurable points of view, even if they measure the same constant acceleration of falling stones.

So defined, the phenomenon of incommensurability presents a challenge to philosophy by blurring the line between observational and theoretical terms. Feyerabend did not intend his discovery to undermine the entire legacy of logical positivism. Instead, he saw ”inner contradictions“ in the collected efforts of the Vienna Circle generation and proposed incommensurability as a means of rescuing empiricism from these. This is at odds with the more common view that Feyerabend is an irrationalist, a view promoted by Hillary Putnam.35 Feyerabend himself fought ”the doctrine that theories are uniquely determined by the facts."36 He believed that they are much broader and what is at stake is no less than the future of science as a discovery machine. By recognizing incommensurable theory pairs, we can stage ”crucial experiments between theories which […] give 


36 P.K. Feyerabend, ”Explanation, Reduction and Empiricism”, 60.
widely different answers in unexplored domains”. Imagine putting impetus and momentum at work on the problem of objects at rest. It comes out as a discovery in the comparison that momentum interprets a state of rest as a special case of constant velocity since neither state requires an applied force. This is how conducting tests with incommensurable pairs can help transcend the horizons of present knowledge.

Before moving away from Feyerabend, there is a story he tells about the origins of IT. In his autobiography, he recalls a seminar where Niels Bohr made a parallel between the extension of the number system to include irrationals, and the development of quantum mechanics. Whereas his 1962 argument was about incommensurability as a discontinuity of reference, this notion of Bohr’s is about lack in common measure. Both of these ideas found their way into the core of Kuhn’s versions of IT, exhibiting mutual reinforcement and tensions.

**The Kuhnian Intervention**

Feyerabend’s was a systematic exposition, grounded in the philosophies of physics and language. However, it was Kuhn’s “simple and compelling

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idea”, introduced in a book written by “a man who was not, at that time, a philosopher” that attracted the most attention. Indeed, Kuhn was under the spell of the big idea of incommensurability his whole professional life. He sometimes expressed his belief in the central role IT must play in any theory of scientific change with the slogan: “shifts happen”. Indeed, IT formed the cornerstone of his very influential views on scientific revolutions as leaps that connect otherwise ‘normal’ periods of scientific developments. In a mid-career reflection, Kuhn provides a very condensed outline of his meta-historical views:

The wrenching experience of entering into an older mode of thought is the source of my references to gestalt switches and revolutions; difficulties in translating the discoverer’s language into our own are what led me to write also of incommensurability; and paradigms were the concrete examples needed – since definition in words was impossible – to acquire the language of the older mode.41


41 T.S. Kuhn, Black Body Theory, 363.
The reference is always to his 1962 *magnum opus* *Structure*, a text “on scientific revolutions itself precipitated an intellectual revolution”.\(^{42}\) The revolution was largely due to the seriousness with which Kuhn recommended IT as the lens through which to understand science as a historical phenomenon. There was early opposition. When addressing IT, Karl Popper was blunt: “two theories are not ‘incommensurable’, to use a now fashionable term, introduced in this context by Thomas Kuhn”.\(^{43}\) As Putnam thought of Feyerabend, Popper thought Kuhn denied the progressive character of science. This is at the level of polemic. In *Structure*, Kuhn was open that a combination of personal and historical accidents is “always a formative ingredient” of science. This is not a surprising viewpoint from a historian. As such, it isn’t that science is irrational; it is just that it is not just rational. Observation and experiment are necessary but do not alone account for the corpus of scientific theories as we find them. The branching structure of this corpus is the result of the added feature “we shall come to call their incommensurable ways of seeing the world and of practicing


Kuhn encouraged the view that “men who hold incommensurable viewpoints be thought of as members of different language communities and that their communication problems be analyzed as problems of translation”.45 This alerts us to a question: what communication problems can result in a situation where “the proponents of competing paradigms practice their trades in different worlds”?46

The impressionistic style of Structure attracted both friendly and hostile attention that snowballed into a controversy about rationality and progress in science.47 He meant his notion to be subtle enough that what “emerges from a scientific revolution is not only incompatible but often actually incommensurable with that which has gone before”.48 Although the term only appears ten times in the first edition of Structure, in later writings incommensurability becomes a central problem for Kuhn. This remained so to the end. At a 1982 symposium he

44 T.S. Kuhn, Structure, 4.

45 Ibid., 175.

46 Ibid., 151.


48 T.S. Kuhn, Structure, 103.
expressed frustration that, “virtually no one has fully faced the difficulties that led Feyerabend and me to speak of incommensurability”. In 1993 he shares that his, “encounter with incommensurability was the first step on the road to Structure”, and the notion remains its “central innovation”.

If the history of science exemplified progress-by-accumulation, the argument goes, then historical sources would not be so hard to understand. Without incommensurability, there should be no fundamental differences between current and past science. Rather we would recognize past theories as less sophisticated versions of our own. This is similar to the way the science we learn in high school gets ‘corrected’ as our education advances. What is most significant from the perspective of this dissertation is that the core motivation for incommensurability is premised on an alleged symmetry between going forward and backwards in time. Kuhn expressed the symmetry like this: “The route I traveled backward with the aid of texts was, I shall simply assert, nearly enough the same one that earlier scientists had traveled forward with no text but nature

49 T.S. Kuhn, “Commensurability, Comparability, Communicability”, 34.

50 T.S. Kuhn, “Afterword” in World Change, 315.

51 This is essentially the point of Structure (see Chapter 1).
to guide them”.52 This is the thesis that has been debated since 1962 and the one Kuhn died defending. It is the history of this Kuhnian of notion that will be the subject the remainder of this chapter.

On Kuhn’s approach, each episode of science is characterized by a practice distinguishable by inter-defined core terms. It is interesting to note that Feyerabend is working with a different unit of analysis, focusing on entire theories rather than term clusters. Kuhn’s inter-defined terms form a bedrock of “presuppositions”53 that delimit islands of meaning with no clear connections between themselves. Using his term ‘paradigm’, rationality is then only meaningful intra-paradigm. However, any credible notion of progress intends a distinctly inter-paradigmatic character. It is worth noting that the Popperian approach to science analysis via problem situations does not face this vexing issue; but by arguing that all failed theories are equally falsified, Popper’s account of scientific development as successive theory refutations fail to describe progress altogether. Therefore, the value of IT is that it does have a story to tell


about progress, but its drawback stems from the fact that it speaks of progress in a way that poses a challenge to rationality.

There are three phases in Kuhn’s thought that impact his views on IT\textsuperscript{54}. The first phase is associated with formulations of gestalt switches contained in *Structure*. At this point IT was a consequence of his views on the emergence and development of paradigms. The second phase is the view of theories as lexicons found in papers written circa 1969. It is at this junction that IT takes a central role in his views of how science develops. The third and last phase is the view of revolutions as speciation events. Here IT is the leading motivation for his work.

*The Structure of Scientific Revolutions*

The key text of this first period of Kuhn’s thought on IT is *Structure*. It launches all the variants of IT that share translation as a core metaphor. It is important to quickly note that while incommensurability would turn out to be important, it was not yet a key idea for Kuhn. It is equally important that *Structure* was written by, “a man who was not, at that time, a philosopher”\textsuperscript{55}. In


\textsuperscript{55} A. Bird, “The Structure of Scientific Revolutions and its Significance”, 860.
Structure IT is characterized by its reliance on Köhler’s Gestalt psychology as mediated by Wittgenstein’s late writings on perception. Briefly, Gestalt promotes perception of wholes (which at the time of Structure Kuhn identified with ‘paradigms’) and Wittgenstein held that, “to imagine a language means to imagine a life-form”\textsuperscript{56}. Specifically, in Philosophical Investigations, Wittgenstein speaks of “two uses of the word ‘see’”\textsuperscript{57}: seeing an object and seeing the likeness between two objects (i.e., he offers the example of faces). Take the duck-rabbit. One sees lines on a page but it is only when we “see it as [as a duck or a rabbit] we interpret it”\textsuperscript{58}. The figure cannot be interpreted as both duck and rabbit at once, but as only one or the other. While Kuhn references Wittgenstein in Structure, it appears that he only included the reference at the suggestion of Stanley Cavell after the manuscript was mostly complete.\textsuperscript{59} Kuhn does credit

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\textsuperscript{57} L. Wittgenstein. Philosophical Investigations, 165.

\textsuperscript{58} Ibid., 165.

\textsuperscript{59} A. Bird, “The Structure of Scientific Revolutions and its Significance”, 869.
\end{flushleft}
Hanson and “other colleagues” for the insight that, “[the] history of science would make better and more coherent sense if one could suppose that scientists occasionally experienced shifts of perception”, like the shift from duck to rabbit. *Structure* also likens the move from one paradigm to another to a religious conversion.

![Wittgenstein’s Drawing of the Duck-Rabbit](image)

Figure 2: Wittgenstein’s Drawing of the Duck-Rabbit

Therefore, a change in paradigm is a change in worldview (*Weltbild*). In *Structure* Kuhn states that when “two scientific schools disagree about what is a problem and what a solution, they will inevitably talk past each other when

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60 Hanson uses the ‘bird-antelope’ figure to make the distinction between an observation statement – about say the number of lines on the page – and the statement that the figure depicts a bird (or antelope). Significantly, the first statement is falsifiable in the Popperian sense. As for the second statement, “Its negation does not represent the same conceptual possibility, for it concerns not an observational detail but the very pattern which makes those details intelligible”. See N.R. Hanson, *Patterns of Discovery* (New York: Cambridge University Press, 1958), 87.

debating the relative merits of their respective paradigms”. It is this shift that Kuhn defines as a scientific revolution. When entering into an older mode of thought, a historian going back in time encounters difficulties that rehearse revolutions for historical actors who moved forward in time. Specifically, scientific development is the succession of competing theories embedded in richer paradigms made up of, “law, theory, application, and instrumentation together”. The view that Newtonian mechanics is a special case of the more powerful theory of relativity is a case in point. Kuhn rejects what was the common view. Instead he maintains that, “Einstein’s theory can be accepted only with the recognition that Newton’s was wrong”. In this sense, Einstein did not generalize Newtonian principles the way d’Alembert, Gauss, or Hamilton did with their reformulations of mechanics according to equilibrium, least force, and least action respectively. Instead, Einstein advanced something new.

62 Ibid., 109.
63 Ibid., 10.
64 Ibid., 98.
In order to understand the notion of incommensurability advanced in *Structure*, we need to consider three key metaphors Kuhn offers for IT: irrational numbers, political revolution, and translation between languages.

The first of these, the mathematical metaphor, was inspired by Bohr from which incommensurability gets its name.

The lengths of the sides of a square and its diagonal are incommensurable. For Kuhn, science in one period perceives science in another period as ‘irrational’ in the same way that, from the perspective of the side of a square of rational length, the non-periodic non-terminating decimal expansion of the length of the diagonal is irrational.65 In this vein, Bertrand Russell warned against “the common idea that each extension of number [integers, fractions, real and complex] included the previous sorts as special cases”.66 The gap

65 T. S Kuhn, “Commensurability, Comparability, Communicability”, 35.


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Figure 3: Diagonal of a Square
between rational and irrational numbers, between arithmetic and geometry, can only be grasped by a logical argument. Greek mathematics offered the Pythagorean *reductio ad absurdum* argument that demonstrates that the length of the diagonal of a square cannot be expressed in terms of the length of its sides. Incidentally, it appears that the Pythagorean interest in numbers was motivated or reinforced by the discovery that musical tones are characterized by ratios of lengths of strings under tension. It is therefore useful to think that incommensurable theories, when placed in proximity, produce a kind of cognitive dissonance.

![Figure 4: Method of Exhaustion](image)

The incommensurability of rational and irrational numbers is the reason one cannot construct a square with an area equal to that of a given circle since that would require finding a rational value for the ratio of a circle’s circumference to its diameter. Therefore, to fully articulate achievements made under a paradigm using the resources of another paradigm is as futile an attempt as that of ‘squaring a circle’. However, it is significant for Kuhn that incommensurable quantities can be approximated to any arbitrary degree of precision. By the
‘method of exhaustion’, as the length of sides approach zero, the area of a polygon approaches that of the circle in which it is inscribed (Figure 4). It is also clear that the perimeter of the polygon with \( n \) sides is incommensurable with the circumference of the circle in the same way that the length of the side of a square is incommensurable with the lengths of its diagonals. Kuhn wants to apply the fact that incommensurable quantities can approximate each other to any degree of precision to science in general.

Consider the example of classical mechanics and relativity theory as incommensurable theories. Feyerabend notes that there are many ways in which "the Newtonian and the relativist can and do converse", so that, "the relativist can say that the classical formulae, properly interpreted (i.e. interpreted in the relativistic manner), are successful, but not as successful as the full relativistic apparatus".\(^{67}\) This is the case, for instance, at low velocities when compared to the speed of light where the theories collapse into each other because the Galilean transformation equations disappear. Kuhn argues that this does not diminish the revolutionary character of Einstein’s achievement but simply shows

\(^{67}\) P.K. Feyerabend, *Farewell to Reason*, 271.
why “Newton’s laws ever seemed to work”.

Indeed, although relativity statements can be reduced to Newtonian statements by restricting to low velocities relative to light, certainly mechanics cannot be derived from relativity. A fundamental difference is that insofar as “Newtonian mass is conserved; Einsteinian mass is convertible with energy”. We are back to Feyerabend’s point about the logical gap between the actual old theory and its reconstruction in light of its successor. Although the theories can be closely approximated, they are without common measure in principle. This is the sense in which the metaphor of irrational numbers supports Kuhn’s view that communication between incommensurable paradigms can never be complete, or that translation between them must remain partial. Newtonian mechanics is incommensurable with Einsteinian physics.

The second key metaphor Kuhn uses for IT is the political one. As he sees it: “In both political and scientific development the sense of malfunction that can lead to crisis is prerequisite to revolution”. This analogy highlights the role of

\[68\] T.S. Kuhn, Structure, 102.

\[69\] Ibid.

\[70\] Ibid., 92.
anomalies in motivating the move from one paradigm to the next. Kuhn believed that just like the failure of the *Ancient Regime* to address the aspirations of the French paved the way for political revolution in 1789, a scientific problem like modeling the spectrum of black body radiation that resists solution erodes the professional confidence in the standard way of doing things, and paves the way for the emergence of another paradigm (i.e., Max Planck’s quantum theory in this case). Initially, practitioners face the challenge using only those theories, instrumental practices, and applications offered by the paradigm since “it is a poor carpenter who blames his tools”.\(^7\) However, the resistance of the problem suggests the need to think out of the box; the community begins to consider that there may be something wrong with their tools. At this stage, scientists begin to lose faith in their tools and the worldview that favoured them. These are extraordinary times when, in a manner not seen or tolerated since the prehistory of the discipline, fundamental tenets are questioned and the value of past achievements re-evaluated. This is the period of crisis or extraordinary science that generates competing paradigms, each fighting for the allegiance of the community. Note that, in these relatively short-lived extraordinary times, IT is

\(^7\) Ibid., 79.
about competition between successive theories and not competition between contemporary theories. It is useful to summarize a Kuhnian crisis as an analogue to a political revolution:

- malaise that eventually leads to retooling;
- usually, a state of affairs lasting a long time;
- usually, scientists are aware of this state of affairs;
- the rejection of the embattled paradigm only occurs when an alternative one is accepted;
- the crisis may be faced by a small community of specialists or by the larger scientific community, usually in that order.

The paradigms on either side of a revolution are incommensurable. According to Kuhn, there is no external place from which to judge what represents better science. If, as Laudan might say, the assessment of the success of scientific practice at any given time depends on the aims of the scientists involved, then a Kuhnian shift is a realignment of the aims themselves. As such, all revolutions contain a certain element of religious purpose – they are never fully justified, never wholly successful and so require faith that the new regime will achieve what the previous one could not. This is the metaphor of political revolution as a choice between different forms of life. As a choice it cannot be
rationally justified. To the extent that deliberation has any part to play in theory choice, it is more akin to persuasion than to logic. This is what Hacking called Kuhn’s ‘New-World’ problem.\textsuperscript{72}

The third key metaphor for incommensurability is translation. Kuhn spent the 1958-59 academic year with Willard V.O. Quine at the Centre for Advanced Study in the Behavioral Sciences at Stanford. At that time, he was working on Structure while Quine completed the “Translation and Meaning” chapter of Word and Object.\textsuperscript{73} Both works challenge positivism by emphasizing indeterminacy of translation. Two decades later, in the Preface to Essential Tension (1977) Kuhn writes:

I am now persuaded, largely by the work of Quine, that the problems of incommensurability and partial communication should be treated in another way. Proponents of different theories (or different paradigms, in the broader sense of the term) speak different languages – languages expressing different cognitive commitments, suitable for different worlds. Their abilities to grasp each other’s viewpoints are

\textsuperscript{72} I. Hacking, “Working in a New World“, 275-310.

therefore inevitably limited by the imperfections of the processes of translation and of reference determination.  

For Kuhn, the question is: how is it that we can speak of revolution and progress in the same breath given that one cannot use the terms ‘\text{mass}_{\text{classical}}’ and ‘\text{mass}_{\text{relativistic}}’ interchangeably and still maintain coherence? More disturbing, perhaps, is the inductive skepticism encouraged by the thought that the great Newton was plain wrong. Kuhn’s response is that revolutionary breaks in science are breaks of a special kind. Fuller calls such a person a, “revolutionary paradigm spanner”. Before relativity, mass is velocity invariant and, after relativity, it is frame dependent. As such, IT is premised on the view that the unit of scientific practice (‘paradigm’ or later ‘disciplinary matrix’) is characterized by central terms, exemplary practices, standard conceptual tools, and evaluation criteria that can and do change. Alexander Bird provides the following summary of IT 

circa Structure:

\begin{footnotesize}
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74 T.S. Kuhn, “Preface”, in Essential Tension, xxii. On whether Kuhn took the right lesson from Quine’s work, see S. Fuller, Thomas Kuhn, 180. For the relevance of Quine on Kuhn’s philosophy, see D.G. Cedarbaum, “Paradigms”, 206.  

75 Steve Fuller, Thomas Kuhn: A Philosophical History for Our Times (Chicago: The University of Chicago Press, 2000), 204.  

76 T.S. Kuhn, Structure, 102.  
\end{footnotesize}
Kuhn is aware that there are differences between different periods in the history of a science that are not captured by saying that at $t$, the community, $C$, believed $p$, whereas at $t_0$, their successors, $C_0$, denied $p$ and held $q$ instead. That is straightforward incompatibility. Incommensurability goes beyond this in that $C$ and $C_0$ fail in an important way to be able to see the world from the other’s point of view.\textsuperscript{77}

This strikes at the heart of contemporary philosophy of science: does science develop by perfecting its core theories or by replacing them? As philosopher of art Arthur Danto states the challenge with this problem:

[…} if philosophers disagree, one must first establish what whole it is to which they are committed, and whether the difference arises within the same whole or between different wholes. This makes philosophical criticism treacherous, since disagreements between those who subscribe to different wholes is not a well-understood matter.\textsuperscript{78}

\textsuperscript{77} A. Bird, “The Structure of Scientific Revolutions and its Significance”, 871.

As it seeks to clarify the matter of ‘inter-paradigmatic’ communications, IT promotes exactly such understanding. It trades on the distinction between the difference within conceptual schemes and the difference between schemes. The contention that such a ‘meta-difference’ exists implies a commitment to the disunity of systems of understanding the world in a way that raises the issue of whether communications between these ways is possible, and if so, then whether they are even productive. It also encourages the question of the number of ways to coherently conceptualizing reality. That is, how many ways are there to conceptualize reality in a way that is compatible with everything known?

The example of geometry comes to mind. For most of its life, Euclidean geometry was seen as the bedrock of science. Some searched for parallel postulate proofs in despair and the great Karl Gauss was made to doubt “the truth of geometry itself”. Newton’s physics simply assumes it. Famously, Immanuel Kant took Euclidean geometry as a pre-condition to perception. Then mathematicians discovered consistent ways to formulate geometries with modified parallel postulates. Polanyi saw in non-Euclidean geometries an

79 Letter from Gauss to Farkas Bolyai, father of Janos Bolyai dated December 17, 1799 and quoted in Richard J. Trudeau, The Non-Euclidean Revolution (Location: Publisher, Date), 127.
advanced stage of a long process of “the separation of reason and experience”\textsuperscript{80} that started with Pythagoras and found expression in the works of Copernicus and Galileo. The impact on Kant’s philosophy was destructive but Newton’s science was left standing until Einstein and George Lemaître developed a cosmological theory with non-zero space-time curvature.

Now we know that conceptualizations of reality that take Euclidean geometry as the only one must be false. Gauss coined “non-Euclidean”\textsuperscript{81} and the broader lesson may be that foundationalist outlooks overcommit. We are reminded of David Hume and older warnings of the Skeptics. The challenge is that our outlooks must not merely be compatible with everything known, but they must be so with everything “knowable”\textsuperscript{82}. This is the incommensurability challenge:

- Are there multiple outlooks compatible with everything known and knowable?


\textsuperscript{82} A. Danto, \textit{Connections to the World}, 12.
• If yes, what is the relationship between the outlooks?
• Is ‘inter-outlook’ communication possible?
• Would such exchanges be productive?

These resemble questions raised by those readers of Structure who had the most influence on Kuhn’s continued thinking.

The Linguistic Turn

In a cluster of papers written in 1969 Kuhn takes a “linguistic turn” to respond to his most ardent critics. This period saw a de-emphasis of paradigms and a focus on incommensurability as the impossibility of full translation. For example, in the Postscript to Structure he states that “men who hold incommensurable viewpoints be thought of as members of different language communities and that their communication problems be analyzed as problems of translation”.

83 The Postscript to the Structure, “Reflection on My Critics” in Criticism and the Growth of Knowledge, and “Second Thoughts on Paradigms” in Essential Tension were all written in 1969.

84 In the “Postscript” page 176, Kuhn now says that, “Scientific communities can and should be isolated without any prior recourse to paradigms; the latter can then be discovered by scrutinizing the behavior of a given community’s members. If this book were being rewritten, it would therefore open with a discussion of the community structure of science”.

85 T.S. Kuhn. Structure, 175.
such, “what participants in a communication breakdown can do is recognize each other as members of different language communities and then become translators”. Ever the historian, Kuhn offers this concrete example: “What was for Einstein an insupportable inconsistency in the old quantum theory, one that rendered the pursuit of normal science impossible, was for Bohr and others a difficulty that could be expected to work itself out by normal means”. The move from incommensurability between paradigms to incommensurability between theories signals a narrowing of scope of IT that some commentators, following Kuhn, have called the shift from global to local incommensurability. Specifically, this second phase of Kuhn’s thought is characterized by a collapsing of IT into semantic incommensurability. To the extent that this is primarily about meaning variance, Kuhn appears to rejoin Feyerabend’s earlier arguments. At this juncture, two theories are incommensurable when there is no “language

86 Ibid., 202.

87 Ibid., 185.

88 See S. Gattee, Thomas Kuhn’s “Linguistic Turn”, 110.

89 T.S. Kuhn, “Second Thoughts on Paradigms”, in Essential Tension, 294.

90 S. Gattee, Thomas Kuhn’s “Linguistic Turn”, 101-111.
into which at least the empirical consequences of both can be translated without loss or change”. 91

The narrowing of incommensurability is widely thought to be a response to an article by Margaret Masterman. 92 In a recent publication, K. Brad Wray writes: “Masterman’s (1970) famous paradigm paper played a crucial role in helping Kuhn clarify his own understanding of paradigms”. 93 Also, in an introduction to the 50th Anniversary edition of Structure, Hacking writes: “In an often-cited but seldom read essay, Masterman found twenty-one distinct ways in which Kuhn used the word paradigm”. 94 In a footnote to his earlier cited comments, Hacking observes that “Masterman listed twenty-one senses of the word paradigm, while Kuhn curiously says twenty-two”. 95 Indeed, in “Second Thoughts on Paradigms” Kuhn wrote that Masterman found, “at least twenty-two

91 T.S. Kuhn, “Reflections on My Critics”, 266.

92 Margaret Masterman, “The Nature of a Paradigm” (Location: Publisher, Date), 59-90. Need to add this to Bibliography.


95 Ibid., xvii n20.
different usages”.

Ironically, this observation makes the point that Hacking may have not re-read the article recently, since Masterman is clear that there are, “not less than twenty-one different senses” of paradigm to be found in Structure.

To set matters straight, here is a brief summary of key points. First, Masterman is rather taken by Kuhn, calling him “one of the outstanding philosophers of science of our time”, and characterized her paper as, “pro-Kuhn aggressiveness”. To those who think Kuhn’s reflections “opaque”, she offered that his thinking “reflects the complexity of its material” – the material being science changing. She accepted Kuhn’s notion of normal science as a “crashingly obvious fact”, and threw caution to the wind when trading Popper for Kuhn by endorsing a new trend “[where] ‘paradigm’ and not ‘hypothesis’ is now the ‘O.K. word’”. The turn to Kuhn is no small thing for someone who claimed

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97 M. Masterman, “The Nature of a Paradigm”, 61

98 Ibid., 59.

99 Ibid., 61.

100 Ibid., 60.

101 Ibid.
to hold that, “Popper first said everything that is true about the philosophy of
science” ¹⁰² (although she thought he did not always do so “efficiently”).
Masterman in fact blames the bulk of the confusion attributed to the multiple
readings of ‘paradigm’ to “the superficial reader”, her confidence in the utility of
Kuhn’s idea unshaken by the, “not less than twenty-one different senses” ¹⁰³ of
paradigm she finds in Structure. Lest one fixate on the number twenty-one, she
takes it to be, “evident that not all these senses of ‘paradigm’ are inconsistent
with one another: some may even be elucidations of others”. ¹⁰⁴ In her actual
enumeration, many of the items indeed overlap. For example, a paradigm is (1)
“a universally recognized scientific achievement”, (6) “a scientific achievement”,
and (11) “a standard illustration”. ¹⁰⁵

Although many interlocutors continue to speak of paradigms, Kuhn
responded to Masterman’s enthusiastic endorsement by turning his back to what

¹⁰² Ibid., 67.
¹⁰³ Ibid., 61
¹⁰⁴ Ibid., 65.
¹⁰⁵ Ibid., 61-63.
he now referred to as “paradigm”. 106 Instead, he bracketed the sociological issue of defining scientific communities (“the practitioners of a scientific specialty“)107 and focused instead on the narrower and “more fundamental”108 sense of paradigm – paradigm2 as exemplar. It is useful to note that paradigm2 is a notion “virtually identical” with the view of the near-forgotten 18th-Century German philosopher Georg Christoph Lichtenberg, who appears to be the first to use this term in relation to science.109 Here is a sample on his thoughts on the matter:

I believe that no heuristic lifting-gear is more useful than what I have called paradig mata. I really don’t see why we should not take Newton’s optics as a model for a theory of the calcination of metals […] even the good mind has to be prodded into seeing something new; indeed, it is almost only by such means that new things can be found in a novel manner. If (as Kastner once conjectured) Newton arrived at the Law of Gravity by way of his interest in light, then that is a paradigm.110


107 Ibid.


110 Quoted in D.G. Cedarbaum, “Paradigms”, 181.
Stephen Toulmin also traces ‘paradigm’ to Lichtenberg. He notes that it was introduced at a time when, “the foundations of modern grammatical analysis were also being laid, and the term found a parallel use in linguistics, to refer to the standard forms for the conjugation of verbs”.¹¹¹ This is precisely how Kuhn first introduces the idea in Structure:

In grammar, for example, ‘amo, amas, amat’ is a paradigm because it displays the pattern to be used in conjugating a large number of other Latin verbs, e.g., in producing ‘laudo, laudas, laudat.’ In this standard application, the paradigm functions by permitting the replication of examples any one of which could in principle serve to replace it. In a science, on the other hand, a paradigm is rarely an object for replication. Instead, like an accepted judicial decision in the common law, it is an object for further articulation and specification under new or more stringent conditions.

As a concrete example, Kuhn makes much of the fact that $F=ma$ takes different forms in the cases of free fall, simple pendulum, and harmonic oscillators.¹¹² He argued that the process of changing patterns of similarity and difference that allow a child to learn to distinguish geese, ducks, and swans is the same process that scientists employ when learning to “solve puzzles by modeling

¹¹¹ S. Toulmin, Human Understanding, Volume 1, 106.

¹¹² T. S. Kuhn, “Second Thoughts on Paradigms”, in Essential Tension, 313.
them on previous puzzle-solutions”\textsuperscript{113} – for example Galileo’s trick of studying the pendulum with incline planes.\textsuperscript{114}

At this stage, Kuhn takes the notion of exemplar as the core of what a paradigm means.\textsuperscript{115} He is now focused on a subset of what a scientific community shares so as to peel off “issues that require reference to community structure alone”\textsuperscript{116} and expose the philosophical core. To the extent that

\begin{itemize}
  \item \textsuperscript{113} T.S. Kuhn, “Postscript” in Structure, 189.
  \item \textsuperscript{114} T. S. Kuhn, “Second Thoughts on Paradigms”, in Essential Tension, 313.
  \item \textsuperscript{115} T.S. Kun, “Postscript” in Structure, 175.
  \item \textsuperscript{116} Ibid.
\end{itemize}
incommensurability emerges in this period as a key idea about the
easpects of epistemology, and as the philosophical core of Kuhn’s thought about the structure of scientific change.

**Post-Darwinian Kantianism**

From the 1980s until his death in 1996, Kuhn returned again and again to IT but with a further twist. The linguistic turn circa 1969 was codified in the language of lexicons and taxonomies, and he now spoke of his position as a post-Darwinian Kantianism where, “the biological parallel to revolutionary change is not mutation [...] but speciation”\(^{118}\). Here is a full exposition of this position in his words:

> By now it may be clear that the position I’m developing is a sort of post-Darwinian Kantianism. Like the Kantian categories, the lexicon supplies preconditions of possible experience. But lexical categories, unlike their Kantian forebears, can and do change, both with time and with the passage from one community to another. None of those changes, of course, is ever vast. Whether the communities in question are displaced in time or in conceptual space, their lexical structures must overlap in major ways, or there could be no bridgeheads permitting a member of one to acquire the lexicon of the other. Nor, in the absence of major overlap, would it be possible for the members


of a single community to evaluate proposed new theories when their acceptance required lexical change.\textsuperscript{119}

This third movement in Kuhn’s approach returns to an earlier insight for inspiration. In \textit{Structure} he held that the constraints supplied by nature are less restrictive than philosophers (specifically positivists at the time) believed. For example, different combinations of wavelengths can be experienced as one same colour and the same drawn lines can be perceived as different objects like duck or rabbit (or a box punched in or popping out, etc.\textsuperscript{120}) No doubt, there are some stable relationships between stimuli and sensations but what those are is difficult to pin down since we rely on sensations to make inferences about stimuli. It is difficult to imagine a natural being that wouldn’t be subject to this constraint. This is precisely the Kantian problem of knowing noumena through phenomena which he understood to be the situation of all “rational beings”. Kuhn saw a solution in a post-Darwinian Kantianism where lexical categories as conditions of possible experience are subject to change as traced by the historical development of science.\textsuperscript{121} A successor theory can be a speciation

\textsuperscript{119} T.S. Kuhn. “The Road Since Structure”, 104.

\textsuperscript{120} T.S. Kuhn, “Second Thoughts on Paradigms” in \textit{The Essential Tension}, 308.

\textsuperscript{121} T.S. Kuhn, “The Road Since Structure”, 104.
from an earlier one when it is both a historical descendent and incommensurable with its predecessor.

![Figure 6: Incommensurability as Speciation](image)

Philosopher Paul Hoyningen-Huene, one of Kuhn’s last collaborators, suggests there are two distinct senses of world/nature in Kuhn’s work: scientist’s world as partly constructed by scientific specialist community agreement and the “hypothetical fixed world” or “that which remains of a phenomenal world when all its subject-sided moments have been removed”. Of course, the allusion is to Kant’s division of reality as phenomena (experience) and noumena (the ding an sich). Like Kant, Kuhn holds we can only have access to the phenomenal world. Unlike Kant, he does not see a one-to-one relationship between the world-in-itself and the phenomenal world but a one-to-many relationship. Whereas there is clearly only one noumenal world, paradigms can be understood

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as plurality in phenomenal worlds only. Scientists can live in separate phenomenal specialist worlds. Clearly, this Kuhnian notion makes no progress on the relationship between what we perceive and the *ding an sich*; this remains the mystery that Kant left us. It is controversial whether this neo-Kantian turn adds any light to Kuhn’s position. For example, try reading this fragment as a post-Darwinian Kantian: “questions about retinal imprints or about the consequences of particular laboratory manipulations presuppose a world already perceptually and conceptually subdivided in a certain way […] they receive different answers as a result of paradigm shift”.123 How does progress result from incompatible phenomenal descriptions of a noumenal and inaccessible reality? At least one commentator doubts whether Kuhn troubled himself with such questions. In his review of the 50th anniversary of *Structure* (2012), Alexander Bird cautions against the Kantian reading: “I suspect that Kuhn was himself influenced by Hoyningen-Huene’s giving his earlier thought a (particular species of) philosophical sophistication that it did not really have”.124


This chapter established IT as untranslatability. It traced the history of the incommensurability thesis. It paid special attention to Feyerabend as IT co-founder and reconstructed his early argument that a theory cannot be reduced to its predecessor without also changing its core descriptive terms. This was followed with an exposition of Kuhn’s views divided into three stages: the formulations in Structure influenced by gestalt psychology, the linguistic turn circa 1969 and then the post-Darwinian Kantianism of the last decade of his life.

Over these phases, IT becomes Kuhn’s central preoccupation and the rest of the dissertation engages with IT through his writings and that of his interlocutors.

2.0 A Case Against the Thesis

Background

In Structure Kuhn claims that scientists who experience a revolution live in a different world then the one where they began. Hacking notes that, “the gestalt switch is something that happens to the individual historian. The science itself is a property of the community”.\textsuperscript{125} This is a fair point that can be further developed by recalling a distinction drawn by Feyerabend.

In Against Method, Feyerabend distinguishes between observer and participant questions: “Observers want to know what is going on, participants what to do”, and elaborated: “the critics of a practice take an observer’s position with respect to it but remain participants of the practice that provides them with their objections”.\textsuperscript{126} This seeds a potentially penetrating analysis of the IT debate. The crucial difference is not between individual and community as Hacking saw it. After all, a community of historians could agree they experience gestalt qua historians. Feyerabend alerts us to a more fundamental difference between observers looking back and participants looking forward. What is more,

\textsuperscript{125} I. Hacking, “Working in a New World” in World Changes, 276.

\textsuperscript{126} P. Feyerabend, Against Method, 220.
the IT debate is complicated by being a second order participant perspective: philosophers observe participants in science but participate in debates about the observations (of these participants). This suggests there may be different evaluative criteria for the practices of observers and that of participants.

Neither Feyerabend, nor the canonical interlocutors on IT made much of the observer/participant distinction. A recent paper by Ipek Demir refers to the distinction but makes no mention of Feyerabend. From the perspective of this dissertation, this distinction matters because it helps clarify why observer-analysts see incommensurability where participant-scientists engaged in the practice do not. In this way Feyerabend acknowledges the temporal characteristic of science but alerts us to the relevance of directionality of perspective as well. He is at odds with Kuhn’s assertion that historians studying old science and scientists facing choices between competing theories are in a symmetric relation. An encounter with IT is only possible precisely when observers leave participants behind and get mired in second-order debates.

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In this chapter we consider the emergence of Kuhn the philosopher as he responds to both friendly and hostile interlocutors. The upshot of these exchanges is a double charge (incommensurability is either self-conflicted or it is irrelevant to how science develops), and the discovery that over 50 years of debate left untouched the key assumption of revolution-as-translation shared by the disputants.

**Internal Tensions**

There are tensions between the metaphors that Kuhn employed to discuss IT. He offered irrational numbers, political revolutions and translation as key metaphors for understanding the consequences of revolution in science. These do not always play well together. For instance, the mathematical metaphor suggests a comparison between schemes of arbitrary precision that the political

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**Figure 8: Feyerabend's Observer/Participant Distinction**

<table>
<thead>
<tr>
<th>Observer</th>
<th>Participant</th>
</tr>
</thead>
<tbody>
<tr>
<td>analyst (philosopher, historian, sociologist, etc.)</td>
<td>scientist</td>
</tr>
<tr>
<td>going backwards in time</td>
<td>going forward in time</td>
</tr>
<tr>
<td>concerned with the meaning of theories</td>
<td>concerned with evaluation criteria to choose between competing theories</td>
</tr>
<tr>
<td>semantic</td>
<td>epistemic</td>
</tr>
<tr>
<td>map</td>
<td>map-making</td>
</tr>
</tbody>
</table>

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metaphor does not support. Also, wholesale overthrow of one approach for another proposed by the political metaphor has no analogue in the context of translation. These metaphors each illuminate some aspect of moving from one developmental stage of science to another but jointly they appear ad hoc. That is, they offer no chance at a unifying perspective. Even looked at separately the metaphors stand in the way. Let us look at each in turn.

Figure 9: Metaphors In Tension

The Mathematical Metaphor – Consider Bohr’s parallel between the extension of the number system to include irrationals and the transition from classical to quantum mechanics. As Peter Barker puts it, “there must be some connection between the two structures. Newtonian astronomy is incommensurable with Ptolemaic astronomy, but not with Galenic medicine.
They are just two different fields”. One can identify two criteria here: IT applies to incompatible theories that are genetically related. Their incompatibility makes them distinct and their genetic relationship allows the theories to approximate each other, allowing for partial communication. So far, the mathematical metaphor is illuminating.

However, it only feigns rigor. The understanding that the root of \( x^2 - 2 = 0 \) brings to a model of scientific growth is not obvious. It is one thing to demonstrate that the length of the diagonal of a square is incommensurable with a unit length, but quite a different thing to argue that Newtonian mechanics is incommensurable with relativity. As Hacking notes, when philosophers speak of incommensurability “they are thinking of scientific theories, but of course there can be no exact measure for that purpose”. In a similar vein, Sankey and Hoyningen-Huene caution that the “application of the mathematical concept to the case of alternative scientific theories is an extension of the concept that


129 I. Hacking, Representing and Intervening, 67.
leaves considerable scope for alternative interpretations”. To speak of incommensurable theories in these terms is to be in the grip of what mathematician Gian-Carlo Rota calls the “myth of precision”, an instance of the more general problem of “slavish imitation of mathematics” he sees in contemporary philosophy.

The political metaphor – incommensurability as revolution – can also lead astray. The history of political revolutions does not assign the clear role to specific failure that Kuhn requires. Rather than initiating a big change, the French revolution may have simply accelerated changes already underway. In addition to uncertain causes, political revolutions have uncertain effects. The early proponents of the French Revolution could not predict the many false starts of the Republic – there have been five to date. Similarly, proponents of epistemic dislocations in science cannot predict the trajectories that their ideas will follow. Toulmin describes the problem for the proponent of a new position

130 H. Sankey and P. Hoyningen-Huene, Incommensurability and Related Matters, viii.


thus: “At points of transition between self-contained systems, he will be forced to suspend all questions about justification and rationality”.\textsuperscript{133} While rationality is only meaningful \textit{intra}-paradigm, prevailing notions of progress exhibit a distinctly \textit{inter}-paradigmic character. This is the incommensurability challenge to rationality, truth and progress. Spelt out further, the incommensurability challenge is this: without ahistorical standards of justification the choice between competing paradigms cannot be rational, what amounts to random groping can only accidentally lead to truth and a succession of random choices cannot be called progress. So impressed were some with this analysis that they welcomed it as a ”release from pre-Kuhnian orthodoxy”.\textsuperscript{134} Others were not so impressed. Irme Lakatos, for example, famously equated IT with ”the political \textit{credo} of contemporary political maniacs (‘student revolutionaries’)”\textsuperscript{135} – that is, the credo that ”might makes right”. One need not be so accusatory. With no apparent malice, Richard Boyd states that ”Kuhn’s work has made it clear that the establishment of a fundamentally new theoretical perspective is a matter of

\textsuperscript{133} S. Toulmin. \textit{Human Understanding, Volume I}, 83.


persuasion, recruitment, and indoctrination”.\textsuperscript{136} The problem is clear when one considers the natural tensions between logic and rhetoric.\textsuperscript{137} Barry Barnes and David Bloor remark that it is “a convention of academic discourse that might is not right”\textsuperscript{138} and, therefore, Lakatos’ charge amounts to ruling IT in contravention with ‘the rules of the game’, academically speaking anyway.

To say quantum mechanics is a response to a crisis sparked by the late 19\textsuperscript{th} century struggle to reconcile radiation thermodynamics and the atomic hypothesis is just one plausible shorthand narrative. If quantum theory was sparked by a crisis at all, Kuhn’s historical work makes its character nuanced to say the least. Specifically, Planck focused on the problem of black body radiation as the determination of the form of the distribution function in Kirchhoff’s formulation of the intensity of blackbody radiation over all wavelengths at constant temperature. This is the standard story that ends with Planck being pushed to the “desperate” measure to reject the principle of equipartition of

\begin{itemize}
  \item \textsuperscript{137} Thrasymachus on justice as “the advantage of the stronger”, see \textit{Republic} 338d.
\end{itemize}
energy in favour of quantization of energy. However, Kuhn argues that it is Einstein who, in three statistical papers between 1902 and 1904 pursued, “a program so nearly independent of Planck’s that it would almost certainly have led to the black-body law even if Planck had never lived”.\(^{139}\) This challenges Kuhn’s notion of crisis-propelled science and rejoins another narrative that attributes the fulfillment of the promise of Planck’s quantum to Einstein’s photoelectric paper of 1905. Therefore, ideas suggested by the political metaphor such as the re-grounding of legitimacy by the new regime, the compartmentalization of normal and revolutionary times, and the central role of crisis in transitioning from one to the other, all present as potential dis-analogies in the case of science.

*The Translation Metaphor* – As Kuhn puts it: “proponents of different theories are, I have claimed, like native speakers of different languages”.\(^{140}\) As such, comparing theories from different historical periods is like comparing – or translating – texts written in different languages. To the extent that some parts of a text cannot be translated into the other’s language, we have a revolution


moving from the earlier to later theory. This metaphor is at war with itself on several points. First and fundamentally, revolutions are processes over time. While translations strictly speaking happen in time, the comparison of a statement and its translation (i.e., ‘it is raining’ and ‘es regnet’) happens at a point in time. Kuhnian incommensurability as incomplete translation applies a synchronic procedure to a diachronic process. This is allowable from an observer perspective, but this also reveals the limitation of that perspective. It is limited to the treatment of science as a collection of reconstructions of stages of a continuous process as though it were in fact characterized by stable core vocabularies and interpretations. Comparing the reconstructed snapshots suggests the gestalt switches. These are artifacts of the observer perspective rather than resident in the experience of participants being studied.

Let expand on the idea of snapshots. The historian looking at past science is an observer-analyst in the business of taking snapshots of a continuous process. When comparing a pair, she may notice that its components do not align fully in the sense that core terms of one snapshot cannot be put in one-to-one correspondence with core terms of the other. This is due to the meaning variance between descriptive terms Feyerabend thought we should expect when one theory is an interesting and fruitful successor of an earlier one. I will refer to such changes in meaning over time as ‘flux’. It is flux that accounts for the
discontinuities that Kuhn saw as untranslatability. However, there is another source of conceptual diversity in science. At any given point in time, there are competing conceptualizations of the core descriptive terms of a field due to ‘fuzziness’ in the object itself. This conceptual diversity is an indispensable part of scientific practice, placing limits on the analysis of science but also acting as a source of novel ideas as communities wrestle to clarify their practice. The intensive debate on the electrodynamics of moving bodies in the last third of the 19th century is an example of the role fuzziness plays in maintaining conceptual diversity. This is especially evident in the various conceptions of the electron as carrier of electric current, from a singularity in the ether to a particle. The issue of untranslatability goes away when snapshots are taken closer together. As their interval tends to zero, IT in its strong form dissolves. For example, instead of comparing Einstein with Newton, let us compare his with Henri Poincaré. At this interval, special relativity “no longer appears as a singular, isolated attempt. Most components of this theory are found in contemporary literature.”  

Another way in which the metaphor is at war with itself is that, narrowly defined, translation is a process of meaning transfer from a source language to a

target language. The exercise is premised on the expectation that both languages are fully formed and stable enough to carry out the meaning transfer operation. When transposed unto the realm of scientific practice, the implication is that the relevant unit of analysis (inter-defined core terms, theory, paradigm) is also fully formed and stable. However, this cannot be the case due to an asymmetry. While we can translate Chaucer into Elizabethan English, Chaucer could not have done so because Elizabethan English, yet to be formed, was not available to him. Similarly, participant-scientists in the grips of a theory do not have a fully formed successor to carry out the point-by-point comparison that future philosophers may choose to make. Again, only an observer-analyst can carry out the point-by-point comparison procedure so the failure of the procedure is only a problem for her as she attempts the recovery of past practice. Invariably, by virtue of being an observer looking back, she like Kuhn is left comparing the reconstructed theories that never existed in the stable form she requires.

The move from Newtonian to Maxwellian or later to relativistic science is exactly not like translation; it is not a move between two known quantities but
rather a move from the, “familiar to what was initially alien”.\textsuperscript{142} In this way, at least the post-revolutionary practice cannot be a fully formed practice.

In summary, while the irrational number, political revolution and translation metaphors may be mutually reinforcing in suggesting how discontinuity may propel development, it is not clear how they could be reconciled into a coherent model of scientific change. Neither do they, separately or in combination, account for progress. In fact, the metaphors both illuminate and obscure the character of the transition from one historical period of science to another. On the side of illumination, each metaphor speaks to the key role that discontinuity plays in the development of science. However, the tensions between these metaphors also obscure by making incompatible suggestions about the character of revolutions (are they leaps or breaks?).

**Criticisms**

Fuller makes a difficult charge against the historian turned historicist philosopher when he speaks of “the historical incoherence of Kuhn’s account”\textsuperscript{143}. This is an outlier position. It is true that Kuhn’s major historical work *Black-Body Theory*


\textsuperscript{143} S. Fuller, *Thomas Kuhn: A Philosophical History for Our Times*, 210.
and The Quantum Discontinuity, 1894-1912 (1978), the one he saw as his “best work”, disappointed many. However, this wasn’t due to the historical incoherence of his narrative. The disappointment was due to the fact that Kuhn missed an opportunity to model the relevance of his most influential philosophy to his most meticulous history. Trevor Pinch interpreted the missing references to Structure in Black-Body Theory as “the final stage of a process of retraction initiated by Kuhn in response to some of the reactions which Structure produced”.  

There was engagement with what John Norton later called Kuhn’s “disputed claim” about the role of Max Planck in early quantum theory. This is the minority opinion that Planck did not introduce discontinuity to the core of physics in 1900 but Einstein did in 1906 via a correction of Planck’s black body radiation theory. On this point, Abner Shimony notes, “Kuhn neglects a paper by Ehrenfest and Kammerlingh Onnes of 1914 which threw important

144 T. S. Kuhn, Black Body Theory, 349.


retrospective light upon developments within his chosen period”, 148 while Klein remains “not convinced” 149. After qualifications, Shimony concedes that “[Kuhn’s] interpretation as a whole seems to me thoroughly convincing”. 150. At last, on the merit of the history overall, Pinch speaks for all when he concludes: “Kuhn's book is to be strongly recommended [...] His penetrating analysis [...] provides many insights”. 151

The criticisms that really stick are the meta-historical and philosophical ones. Let us start with Peter Galison's influential reading of Kuhnian historiography as a series of “island empires” where theory and experiment “move in lockstep”. 152 His general point was that, while positivists reduced science to observation and required theory to account for them, in talk of theory ladenness Kuhn and the anti-positivists simply inverted the relationship. On this view, both positivists and anti-positivists reduce science to one of its aspects, respectively observation and theory. Galison saw the anti-positivist reduction to

150 A. Shimony, “Paradise Lost, A Review Symposium”, 434.
152 P. Galison, Image and Logic, 787-797.
theory at play in *Black-Body Theory*. For him, Kuhn did not merely give primacy to theory but was also guided by a commitment to a meta-theory in "quest for coherence in the history of science"\(^{153}\). Unlike many readers of the book, Galison thought that Kuhn’s commitment to coherence in science was a direct link to gestalt, paradigm and core notions developed in *Structure*. Galison is explicit:

[...] the search for coherence in Planck’s thought is intimately tied to the view of scientific change portrayed in Kuhn’s philosophical work.

The difficulties that are attached to such a view seem to be shared by both the historical and the philosophical Kuhn\(^{154}\).

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\(^{154}\) Ibid.
wider physics community”.\textsuperscript{155} This involved drilling to the sub-disciplinary level to expose the dynamics between theory, experiment and instrument making. To the degree that they happen, discontinuities are limited to the level of sub-disciplines and ”it is a matter of historical investigation to determine if [the discontinuities] do line up” and a given point in time.\textsuperscript{156} For example, a break in instrumentation like the introduction of the Geiger counter presents new experimental opportunities against a background of continuity of theory. If and when discontinuities line up, then a Kuhnian revolution is afoot. However, Galison did not expect that this would often if ever be so as each sub-discipline has its own logic.

The upshot of all this is that, for Galison, there are no global breaks in science. The only breaks are local and at the level of the sub-discipline. If this is correct, Kuhn’s entire historiography and the general philosophical positions it supports would collapse. Also, if Galison is correct that Kuhn’s historical practice is guided by a search for coherence grounded in paradigm and shifts, Kuhn’s legacy as a historian would also be require re-examination.

\textsuperscript{155} P. Galison, \textit{Image & Logic}, 783.

\textsuperscript{156} Ibid.
There are several qualifications to make in considering the effectiveness of Galison’s critique. First, Galison tends to conflate his critique of Kuhn with a critique of texts that Kuhn did not author but inspired.\textsuperscript{157} Also, Galison does not always appreciate that paradigm\textsubscript{1} – which Kuhn defined as “law, theory, application, and instrumentation together”\textsuperscript{158} – seems indistinguishable from his own view of disciplines. In both versions, disciplines are made up of parts, and they are the same parts. However, Galison does provide the observer-analyst with new considerations. The disaggregation of disciplines into theory, experimentation and instrument making, with each following its own logic and timing, is suggestive of higher resolution analysis that stays closer to its object. While Kuhn does the disaggregation work in his move to paradigm\textsubscript{2}, he does so only to isolate the notion of practice as exemplar. Paradigm as exemplar remains a bundle of practices. Therefore, Galison does well to criticize Kuhn for failing to see the opportunity disaggregation presents for a higher resolution analysis with the potential to reconcile continuity and discontinuity. The reconciliation happens by layering breaks and continuities as one might with a rope. This


\textsuperscript{158} T.S. Kuhn, \textit{Structure}, 10.
opens the way for Galison to consider varieties of continuity (pedagogical, technical and epistemic) against a backdrop of staggered change.\textsuperscript{159}

Since the history of science does not support breaks in the whole of science, the criticism that Kuhn has radicalized what are simple translation issues, that are themselves only faced by the observer, sticks. Perhaps, it is said, IT was initially overstated. Indeed, the historical record, suggests an image of science as a collection of overlapping specialized languages where revolutions, if they happen at all, are local affairs. Kuhn himself sometimes spoke of changes, 
"revolutionary only to those whose paradigms are affected by them".\textsuperscript{160}
However, this still suggests a break in an entire area of science. Again, Galison reminds us that even these smaller revolutions show evidence of more continuities than one might first expect.

Even if one sets aside Galison’s sub-disciplinary analysis, local revolutions still create problems for rational choice and progress. Bas Van Fraassen provides the following argument from experience:

[...] we take ourselves to have knowledge and to know what it is to be rational. Yet we also look back and see that in our past our presumed

\textsuperscript{159} P. Galison, \textit{Image and Logic}, 21-22.

\textsuperscript{160} T.S. Kuhn, \textit{Structure}, 92.
knowledge went into crisis, and the crisis was resolved in ways that burst the very categories of our then-putative knowledge and reason [...] what if we acknowledge that we could be in that position again?\(^{161}\)

However, there is a problem. Pessimistic induction is reasonable but potentially corrosive. Kuhn associated the view with Hilary Putnam and derives from it the position (based on an implied frequentist interpretation) that “the probability that any currently proposed belief will fare better [than earlier ones now shown to be false] must be close to zero”\(^{162}\). If this were correct and taken as so by scientists, there would be no reason to even try. A genius on the verge of solving the puzzle of unifying relativity and quantum mechanics in black holes could assimilate this philosopher’s argument and give up physics for more promising pursuits. Even more importantly for the cause of IT, Kuhn claimed that without a fixed ruler, “to measure the distance between current belief and true belief”, it is not even clear, “what the phrase ‘closer to truth’ can mean”\(^{163}\). Ultimately, Van Fraassen’s observation points to a problem for science but it is not specifically our problem of evaluating IT.


162 T.S. Kuhn, “The Trouble with the Historical Philosophy of Science”, 115.

163 Ibid.
At base, the criticism of Kuhnian incommensurability reduces to two charges: that IT is self-refuting (Putnam) or that it is a “pedestrian and familiar fact” of translation (Donald Davidson), that is, mundane. Since it is in language $L$ that one attempts to show that a term in $L^*$ is unintelligible, either:

(a) The exercise is futile because, in principle, terms in $L^*$ cannot in be described in $L$ – so IT is self-refuting; or

(b) The exercise is successful because, fundamentally, $L$ and $L^*$ are not so distant after all, but the same – merely words apart.

Therefore, the basic error of proponents of IT is to take a position but act in a way that is inconsistent with that position – an instance of what, following Habermas, one may call a *performative contradiction*\(^{164}\). Here is Putnam’s charge that IT is self-refuting:

… if this thesis were really true then we could not translate other languages – not even past stages of our own language – at all. And if we cannot interpret organisms’ noises at all, then we have no grounds for regarding them as *thinkers, speakers, or even persons*. In short, if

\(^{164}\) Habermas uses the term in a slightly different way from the one introduced here. As an example, the utterance of the phrase ‘Using lies, I finally convinced H that p’ is a performative contradiction because “in the final analysis, convictions rest on a consensus that has been attained discursively” (Jürgen Habermas, “Discourse Ethics: Notes from a Program of Philosophical Justification” in Moral Consciousness and Communicative Action, 90). In light of this view, Kuhn might respond to Putnam and Davidson that to speak of science in absence of a paradigm is a performative contradiction.
Feyerabend (and Kuhn at his most incommensurable) were right, then members of other cultures, including seventeenth-century scientists, would be conceptualizable by us only as animals producing responses to stimuli (including noises that curiously resemble English or Italian). To tell us that Galileo had ‘incommensurable’ notions and then to go on to describe them at length is totally incoherent.\footnote{165 H. Putnam, \textit{Reason, Truth and History}, 114.}

Kuhn’s response to Putnam is similar to the one he gave Masterman. Just as her analysis of ‘paradigm’ led him to the narrower paradigm\textsuperscript{2}, Putnam’s argument is met with talk of “local incommensurability”. Kuhn’s specific response to Putnam is that one can speak of Galilean science as incommensurable to our own and all the while describe it. This is possible because “only for a small subgroup of (usually interdefined) terms and the sentences containing them do problems of translatability arise”.\footnote{166 T.S. Kuhn, “Commensurability, Comparability, Communicability”, 36.} The languages of science and of talk about science are nested in more general languages that allow for communications. Kuhn is serious enough about local incommensurability to make this very seemingly non-Kuhnian statement:
The terms that preserve their meanings across a theory change provide a sufficient basis for the discussion of differences and for comparisons relevant to theory choice\textsuperscript{167}.

This is the more startling when coupled with Kuhn’s later claim that the local version is the original one. It is very difficult to reconcile this position with the depictions in Structure of revolutions as shifts in paradigm\textsuperscript{1}. In any case, making IT local trades one problem for another. Now, the problem isn’t that breaks happen in the whole of science but how the breaks can be contained in a local region\textsuperscript{168}. There is no straightforward answer to this problem.

Before considering the charge of IT as mundane, there are four other responses Kuhn might have had for Putnam. First, translating into other languages or past stages of our own language are not at all equivalent activities – in fact, studying scientific revolutions is more like doing the second than the first. This is not a point Kuhn himself appreciated but it is central to this dissertation. Virtually all the interlocutors on incommensurability held to the dogma of other languages and past stages of our own are equivalently different

\textsuperscript{167} Ibid.

\textsuperscript{168} H. Sankey, “Kuhn’s Changing Concept of Incommensurability”, 772.
to current speech. This may be the result of the common context in which IT arose. As Gürol Irzik and Teo Grünberg note, “Kuhnian normal science corresponds to activity within Carnapian theory”.\textsuperscript{169} Indeed, the identification of a scientific theory with a linguistic framework $L$ and its division as part observation language and part theoretical language is the background of the IT debate. Rudolf Carnap took from his teacher Gottlob Frege that a formal language, like the mathematics Frege sought to model, is atemporal. Just as ‘$5 + 7 = 12$’ cannot become or pass away but stands outside of time, so do statements in $L$. Being uninterested in temporality, the implications of history are missed. Carnap also distinguished questions internal and external to a framework, and understood revolution as framework shift or as changes in the theoretical language, neither of which is accounted for in $L$.\textsuperscript{170} All this reveals a blind spot in the literature. The assumed equivalence of translating from past stages of our language or other languages is a false symmetry that arises from the failure of Fregean logic to account for diachronic processes like language change. The discovery of this fallacy is a key motivation for this dissertation.

\textsuperscript{169} G. Irzik and T. Grünberg, “Carnap and Kuhn: Arch Enemies or Close Allies?”. \textit{British Journal for the Philosophy of Science} 46 (3): 298.

\textsuperscript{170} Ibid., 296.
The second additional response to Putnam is related to how observer-analysts stand in relation to the two groups of participant-scientists respectively associated with $T_0$ and $T_1$. We can translate Chaucer into Elizabethan English but Chaucer could not have done so because that later language variety was not available to him. This is always the position of the observer-analyst: raised in $T_m$ and trying to understand the movement from $T_0$ to $T_1$. This is why the observer can only recover the reconstructed $T'_0$. To even speak of $T_0$ and $T_1$ is to take snapshots of a continuous process. The fact that $T_0$ and $T_1$ are external the practice that observers participate in, and that they are historically related to each other and us, takes the sting from Putnam’s charge of incoherence. Also, both theories are reconstructed by abstracting from the lived experience of historical participant-scientists to support a side-by-side comparison procedure. This is why the observer-analyst can see continuities from $T_0$ to $T'_1$.

Feyerabend provides the third and fourth responses to Putnam. We can become bilingual (“learn a language or culture from scratch”)\(^\text{171}\) and translation changes the target language. The first point is fairly straightforward. Feyerabend rejected Kuhn’s position (seemingly endorsed by Putnam) that it is

\(^{171}\) P. Feyerabend, *Farewell to Reason*, 265-266.
“impossible for an individual to hold both theories in mind together and compare them point by point”. Instead, Feyerabend argues that the only way to engage with “a new and initially unfamiliar semantic landscape”, like the arcane notion of impetus, is to learn the physics, metaphysics, technology, theology, etc. in which to “locate” the notion. As a trilingual person, I can attest to the phenomenon of different and competing languages in one head.

Feyerabend second point, and the fourth bonus response to Putnam, has to do with transformations in the target language that come as a result of translation. When one considers translating from a foreign language to current speech, “the English with which we start is not the English with which we conclude our explanation”. This is a pregnant point. Our everyday language is regularly transformed by attempts to understand foreign tongues. Consider the New Testament notion of αγάπη. The term was traditionally rendered as ‘charity’ in English. The influential Twentieth Century New Testament (1904)

173 P. Feyerabend, Farewell to Reason, 266.
174 Ibid., 268.
175 “Greater love (ἀγάπη) has no one than this, that a person lays down his life for his friends”, John 15:13.
translation rendered the term as ‘love’.

Today, the Hymn of Love found in 1 Corinthians 13 is a staple reading at Christian weddings, a phenomenon ‘charity’ did not support.

Now we turn to Davidson’s charge that IT is mundane:
So what sounded at first like a thrilling discovery – that truth is relative to a conceptual scheme – has not so far shown to be anything more than the pedestrian and familiar fact that the truth of a sentence is relative to (among other things) the language to which it belongs. Instead of living in different worlds, Kuhn’s scientists may, like those who need Webster’s dictionary, be only words apart.

Kuhn’s response to Davidson is the very disarming response: “to the extent that I’m concerned with language and with meanings at all [...] it is with the meanings of a restricted class of terms”. So Kuhn agrees that scientists can only be words apart, but would point Davidson to those cases where these words are kind or taxonomic terms. This is what makes all the difference. Since “a lexical taxonomy of some sort must be in place before description of the world can begin”, being apart by kind terms is equivalent to carving joints for

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179 Ibid.
nature.\textsuperscript{180} According to Howard Sankey, this is, “the most significant refinement”\textsuperscript{181} of Kuhnian incommensurability. It seems to me that this is not an adequate solution. As seen in the discussion on local incommensurability above, an argument is required to explain how kind terms can be tightly compartmentalized so as not to infect the wider language in which they find themselves.

Hacking offers a simpler response: “incommensurability as enunciated may have deserved Davidson’s strictures” but now that Kuhn speaks of local incommensurability, “Davidson’s objections do not apply”.\textsuperscript{182} Again, I have reservations that local kind terms can be held to being local. Kuhn intends inter-defined kind terms to be persuasive enough to support a distinct address to nature. Even with the conceptual diversity brought about by fuzziness (which Kuhn does not emphasize), it is difficult to accept that the disuse of kind terms from the Aristotelian theory of nature only affected the speech of committed Aristotelians.

\textsuperscript{180} This is a variation on Plato’s metaphor of carving nature at the joints.

\textsuperscript{181} H. Sankey, “Kuhn’s Changing Concept of Incommensurability”, 772.

\textsuperscript{182} I. Hacking, “Working in a New World”, in \textit{World Changes}, 298.
Taken together, one can see how the Putnam-Davidson line of argument uncritically accepts the equivalence of interpretation and translation “traceable at least to Quine’s *Word and Object*”.¹⁸³ This assumption is at odds with the simple fact that translation produces a residue:

Glosses and translators’ prefaces are not part of the translation, and a perfect translation would have no need for them. If they are nonetheless required, we shall need to ask why.¹⁸⁴

Although Kuhn does not seem aware of it, his could be a species of what epistemologists have baptized the “preface paradox”. Imagine a competent non-fiction writer confident in each statement of her latest book yet still – perhaps motivated by a form of pessimistic induction – apologizing in advance to the reader for mistakes the book may contain. Therefore, our writer holds both that the assertions in her book are error-free and that the book contains errors; she holds inconsistent beliefs that violate the requirement that rational belief should be constrained by the logical requirements of consistency and deductive


¹⁸⁴ Ibid.
closure. This may be based on an error in modeling our epistemic practices.

Kuhn’s may be called the ‘translator’s preface paradox’.

Philip Kitcher provides another line of attack. It is neither self-conflicted nor mundane, it is a special case of “the general problem of the under-determination of theory by evidence”\(^\text{185}\) and therefore “a threat to orthodox empiricism”\(^\text{186}\). He notes that this is not the problem faced by observers where \(T_0\) and the reconstructed \(T'_0\) cannot be told apart experimentally:

[…] the typical scientific situation is not one in which we are asked to judge the merits of rival doctrines, both of which fit all the available evidence, but one in which we are asked to appraise two theories each of which accords only imperfectly with the data. The task for the proponent of the idea that revolutionary debates can be rationally resolved thus reduces to that of articulating a theory of the reckoning of costs and benefits, of potential problems and exciting prospects. This is no small task, but it is simpler than the talk of observational and conceptual incommensurability might have led us to expect\(^\text{187}\).

Of course, Kuhn would not accept Kitcher’s premise that there is such a thing as “the data”. The problem of planetary motion addressed by Ptolemaic and Copernican astronomies was not merely about the best approach at

\(^{185}\) P. Kitcher, “Implications”, 690.

\(^{186}\) Ibid., 691.

\(^{187}\) Ibid., 690.
accounting for the data. What counts as data is itself theory-dependent as demonstrated by the different uses of the word ‘planet’ made by these groups. Most importantly for this dissertation, Kitcher’s conclusions that the rational resolution of revolutionary debate reduces to cost/benefit analysis is untenable on account that pre-revolutionary science is fully-formed whereas post-revolutionary science is not. It is surprising that the diachronic character of science is neglected in these arguments. Again, only the treatment of incommensurability as a problem of translation can make Kitcher’s criticism credible and this is limited to the observer experience.

**Unresolved Issues**

Sankey reminds us that the IT controversy is about rationality and progress. If the theories held by different speech communities cannot be directly compared, then theory choice is not a wholly rational affair. Further, if inter-defined kind terms change over time, that means past theories and their historically related successors do not refer to the same parts of nature and therefore progress is put in question. Spelt out further, the incommensurability challenge is this: without ahistorical standards of justification, the choice between

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competing paradigms cannot be rational, what amounts to random groping can only accidentally lead to truth and a succession of random choices cannot be called progress.

Mario Biagioli offers an excursion into Galilean science to elucidate the idea of scientific change as “paradigm speciation”, using a “diachronic approach”:

According to this Darwinian metaphor, incommensurability would be necessarily related to the conceptual speciation of a new paradigm”\(^{189}\). This is consistent with later Kuhn: “The biological parallel to revolutionary change is not mutation, as I thought for many years, but speciation.”\(^{190}\)

At this point, it is useful to consider a basic point Baigrie makes about any Darwinian approach: “theories must be produced randomly if the comparison with Darwinian natural selection is to succeed”\(^{191}\). Barbara Renzi presses the point: “Kuhn’s account of progress in evolutionary biology is different from the


\(^{190}\) T.S. Kuhn, “The Road Since Structure”, 98.

\(^{191}\) B. Baigrie, “Why Evolutionary Epistemology is an Endangered Theory”, 359.
account that most biologists would provide, which I accept.” She is referring to two macro trends in evolution, the move from simple to complex life forms and the increased adaptation to the environment. Renzi calls these trends global and local progress, respectively, and suggests Kuhn confuses these to the detriment of this theory. To the extent that the evolutionary approach is the one favoured by the later Kuhn and by this author, we need to make these distinctions and find a place for randomness in disciplinary development.

Alternatively, we can follow Nicolas Rescher who distinguishes between biological evolution (via natural selection) and cognitive evolution (via rational choice). The choice must be made in support of formulating a version of incommensurability that retains the core insight of change in science while engaging with the asymmetry of participant-scientists moving forward and observer-participants looking back. At base, it is this asymmetry that demands an alternative to IT as untranslatability.

This chapter considered the joint coherence of Kuhn’s presentation of IT and criticism from interlocutors in the IT debate. The views of Galison, Putnam


and Davidson were given special consideration. A discovery was that the IT
debate wrongly assumed that a different language and a past stage our
language are equivalently different from current speech. Despite shortcomings,
IT is an insight consistent with observer level work. It is its relevance to
participants that needs to be rejected. However, the asymmetries between the
observer and participant perspectives that force the search for an alternative to
IT.
3.0 The Language of Science as Creole

Background

Kuhn, philosopher of revolution, provides the following response to those who expected categories from *Structure* to be put to work in *Black-Body Theory*:

As almost always happens in historical reinterpretation, the new narrative is more nearly continuous than they seemed before, but also individually smaller, more fully prepared, more obviously within the reach of an exceptionally capable person. Consider the successive scholarly retellings of the story of Newton, Galileo, Darwin or Freud.¹⁹⁴

What happens at the limit is IT due to insufficient historical detail? This dissertation suggests that, while the observers of science experience gestalt and IT, the object of their study feature no such breaks.

This is consistent with a higher resolution analysis of science that stays as close to its subject as possible by peering over the shoulder of participants. New insights can come from such detailed work. For example, Galison points out that the rise of quantum theory is only a canonical example of scientific revolution as an artifact of the way history is done. He specifies, “In 1900-1, the question of the continuum vs. discreteness as such, which for us is of such overwhelming

interest, was entirely peripheral to Planck’s other concerns”.¹⁹⁵ A defensible philosophy of science should be based on insights from higher resolution analysis. It should also account for significant aspects of scientific practice in ways that are not ad hoc but native or intrinsic to the theory under consideration. Competition, meaning variance and progress are significant aspects that need to be taken into account in any theory of scientific change. IT squarely engages with these but IT fails to deliver a coherent model that assimilates the difference between the observer and participant perspectives; many influential readers (friendly and hostile) also doubt whether Kuhn can account for progress. In contrast, positivists and Popperians speak to progress but ignore one or the other significant fact. The first group ignores competition by depicting theory change as content accumulation and the other ignores meaning variance by depicting theory change as successive theory. With an eye to history, neither positivism nor Popperianism stands as a defensible philosophy of science. These alternatives describe science as a finished product but do not capture science in action well.

¹⁹⁵ P. Galison, “Kuhn and the Quantum Controversy”, 83.
This is the justification for continued interest in IT. It takes meaning variance and competition as starting points. However, progress continues to be a problem. Even the narrower paradigm (paradigm as exemplar) makes it ungrammatical to speak in inter-paradigm change as progress. How can the move from a system of inter-defined terms and evaluation criteria from one period of science improve upon a system with different inter-defined terms and different criteria from another period?

The Ptolemaic universe cannot suffer the Earth to orbit the Sun for the simple reason that the definitions of the terms ‘Earth’, ‘Sun’ and such do not allow it. Speaking as an Aristotelian, it is both inconsistent and incoherent to think of Earth, the center of the universe, as orbiting off-center objects like stars. This may be the reason the Forward to the De Revolutionibus entreats readers not be offended but allow astronomers to discharge their duty to account for celestial motion by any means necessary. This means making room for the ”novel hypothesis” that the earth moves whereas the sun is at rest “in the center of the universe”. This is the sense in which Kuhn saw De Revolutionibus as, “the source of a new tradition that ultimately destroys its parent”.196

In transforming the technical Copernican account into a substantive position about the visible universe, Galileo challenged both previous science and the socio-professional identities premised on the old basic taxonomy. The problem with analyzing this episode using the Kuhnian machinery is that it defines paradigms as closed systems where rationality is only meaningful intra-paradigm. Any credible notion of progress intends a distinctly inter-paradigmic movement. Putting paradigms side-by-side amounts to comparing the discourses of Galileo and that of the Aristotelian Lodovico Delle Colombe are independent of each other. But the point of progress is that such boundary discourse involves languages that are intimately related. We want to say Galileo did not only advance a new position when he turned his telescope to the heavens, but that his was an improvement on the alternatives available. Again, when Galileo and his contemporaries were engaged in controversies on buoyancy, the structure of the solar system, the role of mathematics and instrumentation in discovering truth, etc., they where engaged in real debates about nature. What stands in the way of incommensurability accounting for such talk is the form that its commitment to science as epistemic practice takes. Kuhn

197 M. Biagioli, “The Anthropology of Incommensurability”, 185-188.
("a positivist malgré lui")\textsuperscript{198} never transcended the view of his teachers on what theories are: sets of sentences that form closed deductive systems. His suggestion that theories have inter-defined terms at their core simply adapts the Euclidean method to science studies. On this view, Newtonian mechanics is reducible to axioms and definitions of position, mass, force, and trajectory. Once the system is set up, all that is left is to apply inference rules to arrive at statements that can be compared to nature. To the extent this comparison results in mismatch, then axioms and definitions are adjusted and the deductive process repeated. This is the famed hypothetico-deductive method as an upwards spiral with truth as its regulative end. However, statements can only be compared to statements; they cannot be compared to things.

A new mechanism is needed to replace the one-sided IT to account for the mix of logical, causal, and material links between participants that shapes a progressive science. To the question of whether revolutions are leaps or breaks, the answer is both but not by compartmentalizing normal and revolutionary science as Kuhn does. Language formation is an alternative model for revolutions that shows how to reconcile continuities and breaks within one

\textsuperscript{198} J. Agassi, "Kuhn’s Way", 397.
innovative mechanism. Consider Galileo as part of a tradition. He and Delle Colombe were born a year apart and advanced competing arguments based on incompatible taxonomies of nature. The arguments were competing in the sense that no one seems to have defended both positions at once. Yet, the heliocentric view emerged from engagement with the same problems known to Aristotelian opponents. This is an instance of what Kuhn called ‘revolutionary science’. The question is whether this phase of fundamental disagreement freezes as improvisation can into composition to become consensus-powered science. Reacting to a manuscript of Structure, Feyerabend rejected the singular character of paradigms calling the approach, “ideology covered up as history”.199 He goes on to also reject Kuhn’s motivating axiom that a scientist cannot hold mutually inconsistent hypothesis at the same time:

Faraday did so, if I am correctly informed, and so did the Presocratics, and so did Einstein (partly classical, partly non-classical considerations). Also I think I have shown in my own essay that considering a set of mutually inconsistent but factually adequate theories increases the empirical content of any element of the set and this for the simple reason that many tests presuppose the existence of an alternative! (they are crucial tests).200

199 P. Hoyningen-Huene, “Two Letters of Paul Feyerabend to Thomas S. Kuhn”, 355.

200 Ibid., 356.
In later years, Kuhn appears to have softened his position sufficiently to hold that, “though individuals may belong to several interrelated communities (thus, be multilinguals), they experience aspects of the world differently as they move from one to the next”.\textsuperscript{201} Nevertheless, the translation metaphor at the core of IT encourages the view Feyerabend attacks.

The core analogy is of a language variety emerging out of existing varieties the way English did from West Germanic, Latin and French. Now, English is clearly a language and yet it is diverse. It is the result of language speciation via competition that retains the mark of its history. While there are very few Celtic loanwords in English, the Celtic influence on English syntax\textsuperscript{202} allowed word order to do the work of the case system in Old English. This makes suggestions to the observer-analyst. New theories and disciples as speciation events open the way for different expectations. As the case of English above, we are now alert to influences from both past stages of our language and external sources. Revolution as the major conversion of participant-scientists into a new

\textsuperscript{201} T.S. Kuhn, “The Road Since Structure” in The Road Since Structure, 101.

pre-formed worldview in the manner of gestalt switches is no longer to be expected. Also, the stark issue of theory choice is made less so when considering the diachronic character of science. Just as the move from Old English to current speech is a gradual affair that results in a break only when comparing stages with large intervals, the emergence of the heliocentric view was not sudden.

Ptolemy contributed to two approaches to astronomy: physical and mathematical. The mathematical approach was subordinated to the physical (philosophical) – hence the apologetic Forward to De Revolutionibus. Galileo challenged his adversaries to reverse the priority, arguing that mathematical considerations should lead the way instead. Biagioli suggests such shifts provide insights into historical episodes of incommensurability. In the case of Galileo and the Aristotelians, the reversal is an instance of:

[…] the mathematicians’ invasion of the philosophers’ domain and of the attempt to upset received disciplinary hierarchies. As such, the debates they triggered were characterized by comparable types of non-communicative behaviours, a priori dismissals of the other’s

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203 Apart from the Almagest that established Ptolemy’s reputation as a mathematical astronomer, he also composed Hypotheses of the Planets in which he advanced, “a rather unsatisfactory physical mechanism for epicyclic motions”. See T.S. Kuhn, The Copernican Revolution, 105.
positions, and attempts to enforce or change the received rules of the
game rather than to engage in a constructive dialogue.\textsuperscript{204}

A similar story can be told about current debates about string theory.
Here again, there hotly debated issues boil down to incompatible views on what
counts as doing science responsibly or at all. Without empirical predictions to
test, string theorists are charged with failing to meet basic requirements of
hypothesis testing. They respond that theirs is the only route to a unified
fundamental theory.\textsuperscript{205} They also emphasize their ability for retrodiction of well-
established theories like general relativity and to point to technical problems
solved as installment on unifying gravity and quantum mechanics.\textsuperscript{206} This poses a
clear challenge to science as normally understood. As active participant,
physicist Lee Smolin, laments accusations of incompetence and bad ethics from

\begin{itemize}
\item \textsuperscript{204} M. Biagioli, “Anthropology of Incommensurability”, 191.
\item \textsuperscript{205} Richard Dawid, “Theory Assessment and Final Theory Claims in String Theory”, 
\item \textsuperscript{206} S. Ritson and K. Camilleri, “Contested Boundaries”, \textit{Perspectives on Science} 23, no. 2 
(Summer 2015): 205-206.
\end{itemize}
both sides and admits, “it is real work maintaining friendships across the divide”. 207

The debates are not made easier by the fact that Galileo, like researchers today, was not doing textbook science but was engaged in science in action. His positions changed to accommodate the moves of his interlocutors. For example, he initially held that the buoyancy of an object was a measure of its specific weight. However, after an experiment by Delle Colombe that showed ebony of one shape sink but of another float, Galileo turned from speaking about objects on water to objects in water. 208 The fact that both sides were ignorant of the phenomenon of surface tension led the debate into blind alleys, and then a “deadlock” where the parties could not even agree on an experimental setting to resolve their dispute. Again, their incompatible commitments scientific and socio-professional were obstacles to full communication. This is typical of incommensurability: it emerges as a node of discourses diverging over time. In hindsight, it presents as a speciation event with new theories arising in the way new language varieties do.


Friedman shares this insight and anticipates the central thrust of this dissertation when he writes:

Practitioners of succeeding paradigms are not helpfully viewed as members of radically disconnected speech communities [...] On the contrary, successive paradigms emerge precisely from one another, as succeeding stages in a common tradition of cultural change. In this sense, they are better viewed as different evolutionary stages of a single language rather than as entirely separate and disconnected languages.209

However, this is change in only one dimension. This internalist approach is continuous with Kuhn’s approach and consistent with the IT literature. For example, Biagioli explains change that results in epistemic dislocation as follows:

In the same way that the barrier of sterility is an anti-swamping device which prevents the characters of the new species from being absorbed back into the old one, incommensurability could be seen as a form of intellectual sterility – as the impossibility of breeding intellectually.210

Here incommensurability emerges as a break in backwards compatibility where the new species is prevented from being “absorbed back into the old one”. However, the character and timing of the change requires context along at least two dimensions: internal debates in the field and external factors. The

209 M. Friedman, Dynamics of Reason, 60.

history of astronomy provides a good example of the relationship between internal crisis and the demands of wider culture. It is well known that apart from whatever issues with the Ptolemaic model, demands for calendar reform put great pressure on Ptolemaic astronomy. Copernicus, who held the degree of Doctor of Canon Law, was called before the Lateran Ócumenic Council to give his opinion on calendar reform.\textsuperscript{211} He declined the invitation because he appears to have understood that reform of the calendar necessitated reform in astronomy. Copernicus believed he achieved this reform in his \textit{De Revolutionibus} (1543). Some agreed since the Gregorian calendar adopted in 1582 was based on computations made using his work.\textsuperscript{212} However, the calendar was not the only external influence on astronomy. Other factors which contributed to the timing and shape of the Copernican Revolution included the recovery of Ptolemy’s writings in the original Greek, the predominance of Aristotle in the university system of the time, the humanistic opposition to Aristotle and the university system, Neo-Platonism and its obscure connection to sun worship, and other


\textsuperscript{212} T.S. Kuhn, \textit{Copernican Revolution}, 126.
factors besides. The connection to sun worship is particularly interesting as one moves to Kepler. Kuhn had all these in mind when he wrote:

But technical breakdown would still remain the core of the crisis. In a mature science ... external factors ... are principally significant in determining the timing of breakdown, the ease with which it can be recognized, and the area in which, because it is given particular attention, the breakdown first occurs. Though immensely important, issues of that sort are out of bounds for this essay.²¹³

The ”immensely important issues” related to external factors may have been out of bounds for Structure but they cannot be ignored for its subject matter. Kuhn’s facile gesture appears due to his commitment to the view that theories as closed deductive systems, again under the spell of the ”cult of axiomatics” that extends the Euclidean style of geometry to science in general.²¹⁴

It is curious that Kuhn was so narrowly focused given it is precisely the problem of conceptual change to which he contributed so much that suggests that the logical structure of theories is too poor a model for science in action. Specifically, Toulmin follows Collinwood in proposing that conceptual changes must be explained in causal rather than merely rational terms. It is easy to accept that people from different places and times may possess incompatible

²¹³ T.S. Kuhn, Structure, 69.
²¹⁴ S. Toulmin, Return to Reason, 5.
worldviews; Kuhn’s *Structure* is credited for supplying the vocabulary for making this obvious to many. However after decades of thinking about conceptual change the following question is raised: "Is it even clear that people from the same culture at the same time are capable of reaching intellectual consensus?" This takes us back to Feyerabend’s early reaction to *Structure* and strikes at the core of this thesis.

It is the two-dimensional context of change that shows how novelty comes out of everyday interactions. We must account for both the language undergoing change (internal ecology) and the sociolinguistic context in which that change occurs (external ecology). A single component does not alone account for the resultant force. It is similar for science in action. A language is made up of components including vocabulary, syntax and semantics that require multi-dimensional analysis to account for its emergence from pre-existing varieties. Multi-dimensional analysis is key to pushing Galison’s metaphor of science as creole to yield a model. This is exactly where an emphasis on practice makes the difference. A philosophy of science must be sensitive to the specifics of the context in which innovation occurs. Thematic factors like mathematization,

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unification and reduction provide a background but do not of themselves account for the emergence and development of real-life disciplines like string theory. Indeed, the idiosyncrasies of persons and times and places also play their part. Historians of science regularly appeal to the influence of Ernst Mach and to the character of ‘German science’ in explaining the rise of modern physics. Linguists speak of a ‘founder effect’ where, “the linguistic founding population of an area has a built-in advantage when it comes to the continuing influence and survival of their speech forms, as opposed to those of later arrivals”. As we are almost never in a tabula rasa situation, something similar to the founding effect must be at work in the development scientific discipline. Trading zones like the Cavendish are exactly instances of this mix of internal and external factors that propel change.

In order to lay the ground for the notion of science as creole, let us consider three “tentative axioms” of linguistic creolization proposed by linguist Robert Chaudenson:

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1. Creolization is defined as evolution that leads speakers to determine what is allowed based in their own local norms ("autonomization");

2. Characterized by sociolinguistic changes rather than linear and continuous evolution ("linguistic mutation"); and,

3. Creolization must be combined with internal evolution to explain creole languages (sociolinguistic conditions of interactions + internal evolution).

The fallacy of revolution as incomplete translation allowed observer-analysts to systematically overlook "the fine structure of conceptual change"218 or what the later Kuhn called the ‘microprocesses’ that characterize the development of scientific practice. This systematic oversight resembles the misapplication of a principle linguists call rule generalization where, “later generations misinterpret a series of overlapping events as one single sweeping catastrophe”219. The notion of science as creole provides a framework for

\[ \text{218 Harold I. Brown, “Incommensurability and Reality”, 129.} \]

\[ \text{219 Jean Aitchison, Language Change: Progress or Decay? (New York, NY: Cambridge University Press, 2001), 95.} \]
understanding how a continuous process traces a trajectory that can be observed as moving from one endpoint to another.

If scientific revolution is a speciation event, then like evolution it, “occurs through the accumulation of routine events”\textsuperscript{220}, with associated breakthroughs always, “within the reach of an exceptionally capable person”\textsuperscript{221}. This has important implications for post-Kuhnian philosophy of science in action:

i. \textit{Paradigms are inherently unstable and heterogeneous}. Historians should expect to find fluidity in the core concepts of a discipline. Thus, Kuhn’s view that theories are the logical consequences of a core of inter-defined terms needs to be relaxed.

ii. \textit{Paradigms are not uniformly understood nor are they universally endorsed}. Fluidity in core concepts is expressed by key disciplinary practitioners holding distinct views about the nature of their work while still jointly making lasting contributions to the discipline; or sometimes the same person holding incompatible views at different times. To the extent that the individual practitioner is the


\textsuperscript{221} T.S. Kuhn, “Afterword” in \textit{Black-Body Theory}, 354.
agent of change, the communal practice of science is characterized by internal variability at the individual and inter-person levels.

iii. **Anomalies have no special status as agents of revolutionary change.** As such, innovation and disciplinary developments are mostly fueled by the diversity of individual views rather than some clearly defined anomalies facing the community.

From the perspective of Kuhn’s model, these implications can be summarized as blurring the distinction between normal and revolutionary science. It turns out that at each level of analysis – communities, disciplines, theories or individual heads and hands – the processes that produce normal science are the same that yield revolutionary science. Again, revolution happens through an accumulation of routine events. The dependence of IT on the normal/revolutionary distinction calls for significant recasting. Also, rejecting the view of paradigms as monolithic means the central place Kuhn gave anomalies is unnecessary. This is a good thing in light of Baigrie’s reminder that, “Newton did not produce the Principia […] as a cure for the ills of Cartesian science”. In fact, by changing our expectations with counter-intuitive offerings like it first law
of motion, *Principia* actually helped to create crises. As such, as it concerns the role of anomalies, Kuhn, “seems to have put the cart before the horse”.222

**Science as Creoles**

Hacking admonished, “If we want a comprehensive account of scientific life, we should, in exact opposition to Quine, drop the talk of observation sentences and speak instead of observation”.223 I could not agree more. However, we would still “speak” about science and that because observer-analysts like philosophers are not interested in doing science. Instead, we want to extract from its practice knowledge about a world in which knowledge of this kind is produced. Science as creole aims at this epistemological goal. Again, the assumed equivalence of translating from past stages of our language or other languages is a false symmetry that scrambles synchronic and diachronic processes. The discovery of this fallacy calls for the rejection of IT. However, it seems necessary to outline of how one might, as Hacking encourages us to do, “speak” of observation and related components of science. Kuhn himself recognized the value of approaching scientific development, “as a process of


language change”, 224 but focused on Quine rather than linguists for inspiration and guidance. Science as creole provides a framework to take the IT debate from diminishing returns back to fertility.

We start with a more systematic look at creoles. In her book entitled *Creole Genesis and the Acquisition of Grammar: The Case of Haitian Creole*, Claire Lefebvre reports the following empirical findings “any theory of Creole genesis must account for”: 225

i. Creoles develop in multilingual communities where the languages are of unequal prestige and speakers have limited contact with speakers of other languages;

ii. Creoles are ‘mixed’ in that they display elements of grammar and vocabulary derived from the superstratum and various or all the substratum languages;

iii. The superstratum language plays the role of a *lingua franca* for the privileged few and speakers of substratum languages have


restricted access to speakers of the *lingua franca*, otherwise the community could be bilingual;

iv. The resemblance between the superstratum language and the resultant creole will vary proportionately with the frequency with which speakers of the substratum languages had access the superstratum language during the period of creole formation;

v. This mixing is not arbitrary but exhibits a pattern such that lexical elements derive from the superstratum language and syntactic and semantic elements derive from the substrate languages;

vi. Compared to ‘normal’ language evolution, creoles develop at an accelerated pace (presumably due to the pressing need for communication in a multi-lingual environment); and

vii. Creoles tend to be isolating languages.

This is the popular theory of creole genesis built on the relexification hypothesis that creoles are a result of substrate speakers systematically failing to acquire target languages. It is popular but controversial. Indeed, in a review linguist Silvia Kouwenberg found the book made, “strong claims” that attracted little support the creolist community except for the valuable contribution of
showing, “the falsifiability of the relexification hypothesis”. The relevant issue for our topic is the choice of what constitutes the appropriate level of analysis. Lefebvre sees Haitian Creole as the result of Fongbe syntax with French vocabulary. This can only be a first approximation. The slaves and colonists were each made up of speakers of mutually unintelligible but related languages. Slaves sold at port were sourced from different regions in central Africa, bringing their many mutually unintelligible Niger-Congo languages with them. Fongbe was the variety “with cultural rather than numerical dominance" for the relevant period of history. This is similar to the situation where the dialect of Paris was the language of administration for the colonists but not their daily speech. Neither group was linguistically uniform; this opens the door for analysis at higher resolution. There are also the anomalies. Some features of Haitian do not have precursors in Fongbe. Where do they come from?

Kouwenberg reminds us that “relexification requires some semantic overlap


227 Fongbe is a Niger-Congo language spoken in the West African Republic of Benin.


229 R. Chaudenson, Creolization, 151.
between the substratum lexical entry and a superstratum form.”  

This is reminiscent of Kuhn’s point that, in the move from one theory to another, “lexical structures must overlap in major ways, or there could be no bridgeheads permitting a member of one to acquire the lexicon of the other”.  

Since no overlap exists between Fongbe and French, we are led to look elsewhere.  

Consider the non-Fon reflexive use of body part expressions found in expressions like Tèt mwen fè’m mal (I have a headache).  

Lefebvre herself points locates this feature in Kwa.  

The substratum languages of Haitian are neither homogenous nor all mutually intelligible.  

These are facts relevant to the development of the language.  

The case is similar for the colonists who mostly spoke “nonstandard French, marked by langue d’oïl dialectical features”.  

Similar to the influence of Celtic on English syntax, archaic nonstandard French left syntactic traces in current speech.  

For example, in my native Montreal we say

\[ \text{230 S. Kouwenberg, “Review of Creole Genesis and the Acquisition of Grammar”, 132.} \]

\[ \text{231 T.S. Kuhn, “The Road Since Structure” in The Road Since Structure, 104.} \]

\[ \text{232 Kwa is a Niger-Congo language spoken in Ivory Coast, Ghana and Togo.} \]

\[ \text{233 C. Lefebvre, Creole Genesis and the Acquisition of Grammar, 58-61.} \]

\[ \text{234 R. Chaudenson, Creolization, 152.} \]
y en a for ‘there are’, very close to Mauritian Creole that uses éna. This structure comes from colonists who spoke non-standard varieties of French and went to both places. The standard French expression is il y a. The superstratum of Haitian are also plural, also neither homogenous nor all mutually intelligible.

At the unit of analysis of creole, substratum and superstratum, a key point to retain is that theories of creole genesis and development are premised a three-way comparison. Observer-analysts of science also conduct similar comparisons. However, rather than the usual three-way comparison of a pair of competing theories and nature, the creole model suggests that the discipline is the proper unit of analysis here. A theory of disciplinary genesis and development should then compare the sub-disciplines of theory, experiment and instrument-making and different points in time to trace trajectories.

The issue of the appropriate unit of analysis of scientific change is only implicitly addressed in the literature. The idea of low and higher resolution models aims to draw attention to this issue. It is too crude to study languages, “consider languages as objects, without taking into account the speakers and

\[\text{\footnotesize{Ibid., 150.}}\]
their situations”.\textsuperscript{236} A credible account of how dialects actually evolve requires higher resolution models. The cross-Atlantic voyage is a key fact about speakers and their situations with significant implications for creole. The voyages lasted from 6 to 12 months, providing slaves and colonists a linguistic pressure cooker that needs acknowledgement. As a lower resolution model, Lefebvre’s scheme also brackets the complications associated with a salient feature of creoles: their link to a plantation economy where contact between linguistic groups is neither free nor close. To the extent that Galison imagines the subcultures doing science in a trading zone as equal participants, \textit{scientific disciplines are certainly not like creoles}. Lefebvre also misses the emerging consensus that “there are truly no structural features which define creoles independently of the sociohistorical circumstances of their of their genesis”.\textsuperscript{237} Therefore, the differentials in power and prestige, for example, are not only true for creoles but generally for language formation (as we saw with Celtic versus West German in the development of English). A fuller account of how dialects actually evolve,

\textsuperscript{236} Louis-Jean Calvet, \textit{Towards an Ecology of World Languages} (Malden, MA: Polity Press, 2006), 177.

\textsuperscript{237} R. Chaudenson, \textit{Creolization of Language and Culture}, 145.
one that can guide efforts at understanding discipline formation, requires a model at higher resolution.

Haitian-born MIT linguist Michel DeGraff suggests one approach. He argues the relexification hypothesis is internally inconsistent on the grounds of being making faulty linguistic predictions and getting the sociohistorical details wrong. DeGraff points out that language appropriation happens differently for adults and locally born children, the latter being responsible for the relatively stable new language. While Lefebvre emphasizes second-language acquisition (or the lack thereof) in creole genesis, DeGraff argues that it is the role of first language acquisition that needs more attention. This is analogous to considering the generation who, like Einstein, faced the challenges of relativity and quantum theory as native Newtonians versus later generations raised with these in place. Recall that the conservative Planck accepted special relativity theory as early as 1906 (in fact, he named it) and as late as 1909 Poincaré could lecture on relativity without referencing ‘Einstein’. The choice of both the unit and scope of analysis makes a difference in its outcome.

Another significant fact is that, contra to common belief perpetuated by Lefebvre, slaves, French servants and colonists had total interaction with each other, including linguistic and sexual. Mufwene offers that different linguistic outcomes can result from, “variation in the ecological conditions affecting the same language restructuring equation”. This total interaction is material to the assessment of serial and parallel competition of language structures that feed into the ‘restructuring equation’. The equation is analogous to genetic recombination or the blending of inheritance from evolution theory. Given the stability of the blending mechanism, different outputs result from different inputs. It is the ecologies in which blending happens that make the difference. Therefore, it is the unique combination of ecological conditions like the structure of the language undergoing change and the socio-economic context in which that change occurs that determines the kind of language variety that will emerge. This is reminiscent of scientific and socio-professional factors that combine to yield new theories and disciplines.


At higher resolution, communal language emerges out of interacting languages of individual speakers. In this sense, communal speech is an abstraction; only the individual’s language systems (idiolects) exist. In normal circumstances, it is distinct but close enough to that of some others to allow for mutual accommodation that supports easy communication most of the time. The group of mutually intelligible idiolects constitutes the communal dialect or language variant. This analysis applied to science is at a more granular or higher resolution than pondered by Kuhn and his interlocutors including Galison, with the exception of Polanyi who emphasized tacit or personal knowing. However, that proposal was insufficiently developed to account for the stability of community-level beliefs and practices.

Of course, there are no clear boundaries between individual and communal speech. As one linguist put it, “we have to recognize that,
paradoxically enough, a ‘language’ is not a particularly linguistic notion at all”.  

It is the mechanism of mutual accommodation between idiolects is of great interest here. Mufwene argues it is achieved in accordance with a ‘principle of least effort’ (PLE), a notion analogous to similarly labeled principles of variational physics. The ecological factors of language contact and sociolinguistics play important roles, but the PLE is best demonstrated in the realm of internal ecology where speakers of a substrate language adopt familiar materials from the lexifier. Mufwene explains:

So, the fact that the lexifier of a creole, or any language undergoing change, was heterogeneous before the change is an important ecological factor that bears on its restructuring, which often results in the reallocation of expressive functions among units already in the system. For instance, on the plantations where English or French creoles developed, their lexifiers were typically incipient koines from diverse dialects imported from the European metropole and from second-language varieties spoken by European indentured servants from other countries. Those who developed the relevant creoles were often exposed to more than one way of pronouncing the words this and think in colonial English or the word trois “three” in colonial French. 


In the case of English, the reference is the uneven use of the interdental fricative /θ (the ‘th’ sound that distinguishes thigh and tie). Diversity or internal variability is found in all subsystems of a language; not only phonology but also syntax and morphology. Focusing on meanings, one can see that this diversity is a matter of principle when one considers the impossibility of consistent semantic closure. Furthermore, it is clear that the mutual accommodation that supports communication between speakers is only a partial analogy to the situation for science. Scientists are concerned only about accommodation to each other but also to “accommodation of [their] language to the causal structure of the world”.\textsuperscript{243} The central role played by variability in the internal ecology of a creole applies as well to dialect formation in general. To this end, Mufwene remarks that “… the better we understand [creoles], the more we should be prompted to re-examine a number of things we thought we understood well about Language”.\textsuperscript{244} For example, linguist Peter Trudgill makes much of the fact that American English is characterized by the contact between different British


\textsuperscript{244} S. Mufwene, The Ecology of Language Evolution. xi.
dialects that would not have had much mutual contact ‘back home’. In the colonial situation, the degree of variability within a speech community depends on the number of places from which people arrived to the overseas centre and the number of generations since the initial settlement. Trudgill found that the speech of the first generation of New Zealand-born English speakers displayed the widest range of pronunciations. Taking this as the lexifier of a subsequent creole highlights the importance of internal ecology.

We can expect that the situation is similar for the development of scientific disciplines. Intersections of practices result in innovations. Special relativity emerged against a background of an electromagnetic challenge to the mechanistic worldview and a mathematician’s obsession with the relativity postulate – out of a tension between Wien’s electromagnetic worldview and Poincaré’s commitment to the relativity postulate. Innovation can also be reframed as the result of the clash of what Boyd labeled “theory-constitutive” metaphors. The example he offers is the prevalence of the computer metaphor in modern theoretical psychology (the brain is a computer, thought is information


246 Ibid., 14.
processing, etc.). There are no alternative literal paraphrases available with which to discuss mental phenomena.

The upshot of the creole model is that scientific change is best understood against the background of the problems that they address, and that these problems are ‘situated’ in conceptual, historical and sociological spaces. Scientific change as creole formation reveals greater diversity than normally accounted for in standard accounts. All science should be treated as creole en voie de development. The higher resolution analysis of converging snapshots is less vulnerable to charges of top-down analysis that is “simply an application of a general rule that is arrived at by some other means”. For example, string theory is a case of in action. For this reason, philosophers tend to ignore it because “there is, in a sense, no theory for the philosopher to analyse”.

Generally, one should speak of string theories to indicate the family of mathematical structures that attempt to unify general relativity and quantum mechanics via the hypothesis that everything reduces to extended structures.


The phenomenology of particle physics is recovered by postulating divers vibration modes that, at lower energy levels, are experienced as the particles and forces of more established theories living under the rubric of ‘the Standard Model’. Maxwell’s theory offers the analogy of varying oscillations of the electromagnetic field (in a narrow range) experienced as different colours.

String theories have predecessors in hadronic physics that date back to the 1970s but have been self-consciously pursuing the key to quantum gravity since at least 1984. This is the year of the string revolution ushered by Edward Witten. Unlike familiar accounts in the history of physics, the story of how string theories emerged and developed just now being told, its main characters have not yet been divided into heroes and villains, as the end game is still very much uncertain. For all these reasons, a philosophy of string theories is risky. However, the creole model of scientific change can be a good guide for such a project.

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249 Edward Witten’s paper “Some Properties of O(32) Superstrings” in Physics Letters (Volume 149B, number 4,5) published in December 1984 is widely taken as the start of the string revolution.

250 A good recent work is by Dean Rickles, A Brief history of String Theory: From Dual Models to M-Theory (New York: Springer, 2014).
One way into the story of string theory is to consider the motivation for extending the Standard Model of particle physics. This theory contains constants that specify the properties of the twelve elementary particles and four fundamental forces. The problem is that these constants are put in ‘by hand’, a situation that suggests the Standard Model is not a fundamental theory. The aim of string theorists is to develop a fundamental theory that would predict the observed values of the constants of the Standard Model in the same way Maxwell’s Equations predicted the observed value of the speed of light.

A second entry into the story is to follow the long-standing project of unification in the history of physics. Indeed, the history of modern physics can be credibly told as a series of unifications. It is interesting to note that string theories are speculative in the sense that they do not allow for experimental verification at this time. The Large Hadron Collider is thought to have the potential to reach energy levels capable of testing some string hypotheses such as its call for additional dimensions. However, at this point the philosopher is justified in asking whether they are scientific theories at all; a position that Popper might have supported. This places the observer-analyst in the uncomfortable position of musing on whether some of our brightest scientists are doing science at all.
A third approach goes back to an old idea introduced by Planck but put to work by de Broglie: through dimensional analysis, one can combine the physical constants of our most basic theories (general relativity and quantum mechanics) to produce a natural scale for length \([l]\), mass \([m]\) and time \([t]\). Specifically, Newton’s Gravitational constant \((G)\), the velocity of light \((c)\) and Planck’s quantum of action \((h)\) can be manipulated to yield the following units with their approximate values:

\[
\begin{align*}
  l & = \sqrt[3]{\frac{\hbar G}{c^3}} = 10^{-35} m; \\
  t & = \sqrt[5]{\frac{\hbar G}{c^5}} = 10^{-43} s; \\
  m & = \sqrt{\frac{\hbar c}{G}} = 10^{-8} kg.
\end{align*}
\]

The challenges associated with doing physics at the Planck scale, about twenty powers of 10 smaller than the atom, is easy to appreciate when one considers the inverse relationship between the size of an object under study \((\Delta x)\) and the energy required to probe it \((E)\):

\[
E = \frac{\hbar c}{\Delta x}
\]

While a smallest size \(l\) avoids the problem of requiring arbitrarily high levels of energy, as \(\Delta x \to l\), \(E\) approaches a large amount of energy beyond current technology. The goal is then to circumvent this high-energy challenge to allow probing deep into the Planck scale. The creole model suggests each of these approaches stand against a background of competition within its scientific
subcultures and the institutional and macro context in which the work is done.

As such, vertical and horizontal practices can interact over time to yield something genuinely new with a character that emerges from the personal knowing of individual participant-scientists.

With a viable alternative mechanism in hand, we can side step the false choice between meaning variance and progress. Simply put, science develops in ways analogous to how creoles develop. The force of this argument resides in the underlying determinism that guides new dialect formation. If this thesis is correct that the analogy holds, then it provides a new research agenda for the historiography of science. This is taking Galison’s creole metaphor more seriously than he did by raising it to the status of a model for scientific practice.

Obviously, some key features of science are not at all like creoles. For example, the science as creole view hides the fact that scientific discourse is always embedded in a broader system, this is not the case for creoles. While Feynman diagrams are no use in the grocery store, Haitian is as useful as English in any pursuit. In fact, this embeddedness of scientific discourse in natural languages is an essential feature of science. Science as creole also hides the disunity of science. Unlike the unity displayed by dialects in their support of communications via mutual accommodation, scientific disciplines are special-purpose tools. Toulmin offered the image of science as a population of
concepts and theories, where “there are – at most – localized pockets of logical systematicity”. In contrast, syntax is much more evenly applied in the case of natural languages, at least when one focuses on community-level speech. The Fregean view of concepts, which has cast a long shadow over the IT debate, is reminiscent of the Parmenidean argument against becoming. If a thing changes into a different thing, how do we know that it is the same thing that changed rather the relation between two unrelated things? Meir Buzaglo offers a useful and very Aristotelian escape: “a concept includes all the stages of its development”.  

The inquiry on the possibility of revolution as progress in science goes down a well-beaten path, but one that has not yet touched bottom. The above contributes to this debate by advancing a new central metaphor, that of language formation, as a model for the development of science. In this way, the Incommensurability Thesis is given new life. The necessity for this rehabilitation is clear: to the extent that rationality is intra-paradigmic and progress inter-paradigmic, we are potentially left with either an irrational (accidental) progress

251 S. Toulmin, Human Understanding, 128.

252 M. Buzaglo, The Logic of Concept Expansion, 73.
or an unchanging rationality (no progress at all). Both options are at odds with the historical record and analytic considerations. Science as creole moves beyond positivism versus anti-positivism, all the while preserving gains from each. It reconciles continuity and discontinuity by rejecting the observer-sided metaphor of translation focused on vocabularies and approximate discourse.

This chapter presented the notion of science as creole as an alternative to the view of incommensurability as untranslatability. The deep dive into creole linguistics suggested the credibility of a mechanism that can account for observer-level breaks when a continuous process is analyzed with sufficient distance.
4.0 Implications for Decision-Making

Background

Even if the previous chapters successfully establishes that IT as untranslatability is bankrupt, requiring the notion of change it promotes to be replaced, and that science as creole is the suitable replacement, one would need to tell the others. The Kuhnian language of paradigms, paradigm shifts, and revolutions is the general possession of wider culture. The challenge this poses to philosophy was noted by Hacking in a paper written on the occasion of a “Kuhnfest”. What philosophers find most controversial about Kuhn is exactly what “most attracts the casual reader” of Structure. There are many such readers. By the time of its fourth edition in 2012, Structure was rumored to have sold over 1.4 million copies. The impact of these sales is talk of revolutions due to paradigm shifts in business, policy and popular culture. In business, a recent revolution turns

253 In May 1990 a conference was held at the Massachusetts Institute of Technology in Kuhn’s honour. Hempel called it a “Kuhnfest”. C.G. Hempel, “Thomas Kuhn, Colleague and Friend”, 8. This needs to be included in Bibliography.


about “big data”. In seemingly unrelated policy matters, Kuhn is also felt. Here is how former US vice president Richard B. “Dick” Cheney defended a controversial policy:

Homeland security is not a temporary measure just to meet one crisis. Many of the steps we have now been forced to take will become permanent in American life. They represent an understanding of the world as it is, and dangers we must guard against perhaps for decades to come. I think of it as the new normalcy.

Lastly, popular culture finds inspiration in Kuhn as well. In May 2015, Facebook CEO Mark Zuckerberg added Structure to his book club list. He invited readers to consider “whether science and technology make consistent forward progress or whether progress comes in bursts related to other social forces”.


In the case where even specialists limit their analysis to “the ideas set out in *Structure* and in Kuhn’s subsequent writings”, it may be too much to ask for non-philosophical references to worry about qualifications. Yet, as hinted above, sloppy Kuhnian ideas have real world impact. What should be the response of philosophy? Hacking faced a similar choice in relation to talk of social construction in the 1990s. He first thought that “the best way to contribute was to stay silent” but reconsidered and settled on the view that philosophers “should analyze, not exclude”. Taking his example, this last chapter adopts the activist attitude. It considers the real-world impact of Kuhnian incommensurability on business innovation studies and aims to make an installment on an actionable philosophy of science that shares concerns recognizably continuous with such practical concerns.

**Kuhn and Business Innovation**

Business innovation is not a distinct field in the sense that it does not have standard textbooks, journals and replicating institutions like training labs that

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makes participle physics a discipline. However, since the 1970s the term
‘business innovation’ is gaining in use.\textsuperscript{262} Also, governments have entire
departments focused on the development and implementation of innovation
policy to promote science-based business creation and expansion.\textsuperscript{263} The
arguments for these investments are twofold.\textsuperscript{264} First, public institutions generate
great quantities of research with potential for increased wellbeing and
prosperity. Taxpayers have already paid for the science. Advocates argue that
incremental investments in commercialization would unlock this value. This is the
driving force behind the rise in technology transfer offices and entrepreneurship
programs on campuses.\textsuperscript{265} The second argument for government spending on
innovation is to attract business investment in high-growth sectors. As lower-skill

\textsuperscript{262} Using Google N-Gram Viewer, the use of the term in their corpus of books more that

\textsuperscript{263} The Province of Ontario has a Ministry of Research and Innovation since 2003 and the
defederal government elected in October 2015 created a new Department of Innovation,
Science and Economic Development.

\textsuperscript{264} I led life sciences commercialization policy and programs for Ontario between 2008
and 2012. I also contributed to the development of the Ontario Innovation Agenda
[accessed September 12, 2015]. Available from:

\textsuperscript{265} The Engineering Faculty at the University of Toronto launched the Hatchery
entrepreneurship space for its students. I held a 2015 Hatchery Fellowship.
jobs give way to automation and offshoring, advanced economies are challenged to maintain historical employment levels. Traditionally, youth and immigrants count the most on lower-skilled work to gain work experience and therefore are the most affected by these trends. Here, advocates argue for a combination of investment attraction techniques to showcase their jurisdictions to international business. Currently, advanced economies are in fierce competition for employers from the biotech, information technology & communications, and specialized manufacturing sectors.

Despite large investments and the consequential problems they aim to address, there is no consensus on what is meant by business innovation. I define business innovation studies as thinking about practices that aim to foster and


267 As investment attraction lead for Ontario, I led a team that raised over $400 million in new investments into the province from global life sciences companies between 2008 and 2012. This success is a small share of what competing jurisdictions like Boston and San Francisco Bay Area attracted in the same period.
replicate novel ideas with the potential for sustainable above-average returns. I locate this thinking at the intersection of technology, entrepreneurship and ideas. By technology, I mean the consistent search for algorithmic formulations of problems. Trivially, no business is deemed innovative without a website and the computerized automation of at least some of its processes. More substantively, at its core an innovative business has a science-based offering like a patented drug or computer program. Recent talk of the ‘sharing economy’ is signaling the rise of a sub-category of innovative businesses that apply web-based social networking solutions to places where none was evident – such as Uber in the taxi business. Entrepreneurship is the second intersecting area with the startup as the emerging standard form of organizing an innovative business. This is true by definition for new entrants but established companies also utilize the startup model to feed their product pipeline or play in spaces outside of their historical expertise. For example Janssen Inc., a pharmaceutical division of global consumer product conglomerate Johnson & Johnson, has a number of

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268 The standard definition of above-average returns is returns that exceed what equity markets return in a given period, adjusted for risk.
corporate incubators around the world to simulate the startup environment.\textsuperscript{269} Peter Thiel, co-founder of the PayPal online payment system with a market valuation in excess of $US50 billion, recently defined a startup as “the largest group of people you can convince of a plan to build a different future”.\textsuperscript{270} This has resonances with tightknit groups of researchers in the sciences. Ideas are the third intersecting area that contributes to business innovation studies. Of course technology and entrepreneurship are the subject of active scholarly study. By ideas is meant the general premises and background assumptions that inform and delimit talk of business innovation. The Kuhnian approach to scientific change is such a general premise. In this case, the affinities between business innovation as breakthrough-driven and scientific development that is punctuated with revolutions create an opportunity for mutual illumination.

What exactly do business innovation experts do with this thinking at the intersection of tech, entrepreneurship and ideas? They generally give advice to policy makers and business. To government, they offer guidance on what makes


a business climate favourable to novel ideas with commercial potential. A leading example of this is the consulting work of Michael E. Porter based on his theory of competitive advantage developed in his book *Competitive Advantage*. For business, innovation experts distill best practices for taking novel ideas from assets for above-average economic returns. A leading example is the work of Clayton M. Christensen based on his case studies of the differential impact of “disruptive innovation” (he coined the term)\(^{271}\) on established and newer firms. The key text of Christensen’s practice is his book *The Innovator’s Dilemma*. In both the cases of advice to government and to business, the aim is “to build a bridge between strategy and implementation”.\(^ {272}\) The key relevance of the Kuhnian project to the economic and social projects referenced above is the intellectual grounding that business innovation studies seeks from the historiography of science.

A recent article by management theorists Sarah Kaplan and Keyvan Vakili makes a more standard use of Kuhn to apply new computerized techniques to


discovery of latent topics in interrelated texts.\textsuperscript{273} Their project was to sift through patents in search of breakthroughs that combined cognitive novelty and economic value.\textsuperscript{274} As an alternative to the traditional metric of an innovation (number of patents cited), they fed the patents into a supervised machine-learning algorithm to break down the corpus into topics.\textsuperscript{275} The program generates a vector for each topic for each patent so patents can be categorized by their ties (topics in common) and by the strength of these ties (vector weights). This provides a measure of the semantic proximity of texts without the noise associated with misattributed or missing citations.

The authors explicitly cite Structure as inspiration for their method based on the view that “shifts in ideas can be detected in shifts in language”.\textsuperscript{276} They also rely on Kuhn’s idea that “paradigm shifts are triggered by the accumulation of anomalies”.\textsuperscript{277} Paradigms, shifts in language, and anomalies are each heavily

\textsuperscript{273} S. Kaplan and K. Vakili, “The Double-Edged Sword”, 1435–1457.

\textsuperscript{274} The sample was 2,826 carbon nanotubes patents filed in the US over 20 years to 2008.

\textsuperscript{275} The Bayesian algorithm used is the latent Dirichlet allocation (LDA).

\textsuperscript{276} S. Kaplan and K. Vakili, “The Double-Edged Sword”, 1436.

\textsuperscript{277} Ibid., 1440.
qualified parts of Kuhn’s legacy, and therefore not the robust parts to build upon. Kuhn himself abandoned ‘paradigm’ due to its plasticity. However, the use of this notion by the authors may have had minimal impact on the outcomes of their study. This is because what counts as a carbon nanotube patent could only be determined by external factors anyway (the US Patent Office in this case).

As for shifts in language, Kuhn and his interlocutors were agreed that scientific revolutions, if/when they happened, are not always signaled by a change in terms but sometimes by a change in their use. On this point, Kaplan and Vakili may have missed some topic changes in their sample. Thirdly, the hypothesis that anomalies force change in science has been falsified. On a more general note, the study draws lessons from observer work about the relationship between innovative texts to formulate recommendations for participants on how to generate and recognize innovative ideas. It is hard to see how the first task, indeed very valuable for the analyst, could provide sufficient guidance for the entrepreneur. Business innovation scholarship based on the observer-based approach of IT run the risk of irrelevance to practices business innovation theorists want to converse with. This seems to be Feyerabend’s point that
“incommensurability is a difficulty for philosophers, not for scientists”.278

An area of opportunity for mutual illumination between philosophic discourse on scientific change and business innovation is the irreversibility of the type of changes being discussed. In the case of science, what the observer experiences as untranslatability the participant experiences as a fuzzy present where the may be the subject of some agreement but the future is an open question. In the context of business innovation, the phenomenon is framed as the problem of backwards incompatibility. As an example, a recent textbook characterizes the development of the web programming language XHTML 2.0 as “a revolutionary change to the language, in the sense that it broke backwards-compatibility”.279 Both web developers and Internet users were left frustrated. For developers, breaking backwards compatibility meant their hard-earned skills no longer resulted in functional websites and new skills had to be acquired. For Internet users, the result was the unpleasant experience of websites having a different appearance and functionality depending on the web browser being used. This is a case where business innovation illuminates science studies. It is

278 P. Feyerabend, Farewell to Reason, 272.

easy to underestimate the cost a change like the establishment of a new scientific theory like relativity had on individual careers. Of course, the change did not happen all at once. However, once relativity settled as a part of fundamental physics, those who grew up under the old system needed to get supplemental training or be left behind.

Another example of innovation that results in backwards incompatibility is provided by Christensen case study of developments in computer removable storage technology. While the 5.25-inch ‘floppy’ disk was still improving its performance (by reducing its cost per megabyte), the competing 3.5-inch disk emerged with less storage at a higher cost. There are a number of interesting historical details that with potential analogues in the development of mechanics from Newton to Einstein. Seagate Technology started selling 5.25-inch disks in 1980. Conner Peripherals, a spinoff of Seagate, started shipping the competing 3.5-inch format in 1987. The following year, Seagate shipped its own 3.5-inch disks but its customers were not interested. In the meantime, Conner entered into a partnership with laptop maker Compaq and directly targeted the

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281 C.M. Christensen, *The Innovator’s Dilemma*, 22.
emerging portable computer market. These customers appreciated the smaller format and greater robustness of the 3.5-inch disk and paid the premium. By 1991, the market for 3.5-inch disks had grown to $US750 million with Seagate virtually shut out.

Christensen does not bring out this point but it is clear from his reporting that, in 1988 for example, Seagate and Conner were not competing since they did not target the same customers. Although Conner started with the smaller customer base, it had the faster rising segment. When the performance of the 3.5-inch disk caught up to that of the 5.25-inch disk, and the portable segment grew in size, then the companies competed resulting in a winner and a loser. This is analogous to the situation of the work of German Maxwellians who worked on the electrodynamics of moving bodies for years before Einstein’s formulation of the problem eventually attracted enough attention to bring Einstein into the center of discussions.

Christensen introduces the sustaining/disruptive innovation distinction to explain the dynamics at play. These have the same role in his framework as the normal/revolutionary distinction under Kuhn’s model. Sustainable innovation

\[282\] Ibid.,10-25.
follows well-established performance improvement trajectories whereas disruptive innovations initially target a different problem and only in time intersect the performance trajectory established by the incumbent technology. On the disks example, in 1987, 5.25-inch disks were undergoing sustaining innovation with a predictable trajectory of yearly decrease in cost per megabyte. That year, the 3.5-inch disk was a disruptive innovation, only of interest to an emerging but marginal market. Only when the performance of the disk formats converged did the segments also merge, setting the stage for the head-on competition that resulted in the demise of the 5.25-inch parent.

There is a key disanalogy between the way philosophers study science and the relationship between business innovation studies and its object. Since John Locke described himself as “employed as an under-labourer in clearing the ground a little, and removing some of the rubbish that lies in the way to knowledge”, it has been out of fashion for philosophy to presume to guide science. Of course, there are scientist-philosophers like Poincaré, Einstein and Bohr but members of philosophy department are generally not invited to scientific research discussions. The case for business innovation studies is

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different. Scholars like Michael Porter regularly act as consultants with the aim to influence and guide practice. However, this chapter is an installment on closer engagement between philosophers and social scientists and decision-makers that base ground their practice in ideas.

**Science as Creole and Business Innovation**

How does science as creole point the way for business innovation? First, it disaggregates and provides a framework to reconcile internal and external sources of change. A study of innovation in carbon nanotube application must consider more than filed patents to be able to provide advice to participants in the business of innovation. Science as creole also encourages the higher resolution analysis required to model, for example, the genesis of Haitian or the emergence of a theory like relativity. Business innovation needs to reconcile the contributions of individual innovators with firm-level and industry-level considerations to develop insights to support innovation development.

On the other hand, philosophy has much to gain from the innovation studies. Traditionally, philosophers require the history of science as input to their work. While some degree of remove is unavoidable in observer-based

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philosophical work, science as creole proposes historical and socio-linguistics tools that allows for more intimate exposure to participant scientists.

The topic modeling demonstration provided by Kaplan and Vakili also has great potential for philosophy. The application of algorithms to large amounts of texts would be a new source of data for philosophical thinking about science.
Conclusion

When looked at closely, the thesis that successive theories cannot be translated into each other misses the point. Of course, snapshots of science separated in time fail point-by-point comparison due to the incompatibility of core terms. The dissertation showed this is because snapshots are arbitrarily chosen to exaggerate differences. Consider how Maxwell so rarely features in discussions of the relationship between Newton and Einstein. If he did, the incommensurability between the endpoints would not be so clear. Special relativity is a less revolutionary step against the background of Maxwell than against that of Newton. This is why the literature is replete with speculations about the extent to which Henry Poincaré and others may have anticipated Einstein. It is also significant that Einstein’s famous 1905 relativity paper was not engaging with Newton or even Maxwell but with Hendrik A. Lorentz’s theory of a few years earlier. The case is similar for the paradigm case of a scientific revolution: the transition in astronomy from Ptolemy to Copernicus. The new astronomy did not pose the problem of the planets nor did it have a monopoly on such a key feature as a sun-centeredness of our planetary neighbourhood. Even the quintessentially traditional commitment to uniform circular motion is continuous between alleged radically different astronomies. Again, Copernicus
was engaged with contemporaries and not directly with long-dead scholars.

Such theory pairs as mechanics and relativity or Ptolemaic and Copernican astronomies only fail full translation because they are placed wide apart. This is similar to comparing Chaucer’s English and current speech. If the objective is to understand how language changes through use, we must start with the observation of an unbroken chain of custody of speakers. Whatever else can be said about speech practices separated by six centuries, it is clear that it is English that Chaucer and we are in possession of. Zooming in to snapshots closer in time, the language is much more continuous and leaves fewer opportunities for communication breakdown. Similarly, when snapshots of science are taken closer and closer together, IT dissolves.

The kind of change in science discussed until this point has to do with variances in the meaning in core terms over time. There is a second type of change associated with conceptual diversity at a point in time. Throughout the dissertation, the first was referred to as flux and the second as fuzziness. The uses and limitations of IT become clearer when the distinction between flux and fuzziness is superimposed on the distinction between observer and participant perspectives. When observers look back at science and consider successive theories sufficiently separated in time, the two fully formed alternatives cannot be fully translated into the language of the other. IT accounts for such cases. Of
course, they can be approximated as French and English always can but the comparison makes it clear that different languages are at play. On any given day, the scientific community is not engaged in grand conversions but concerned with mutual accommodation of individual beliefs and practices.

Science as creole provides a new understanding of the development of science. It rejects the traditional choice between positivism versus anti-positivism. Rather than forcing a choice between fundamental continuity or fundamental discontinuity, creoles provide shows how a continuous practice can result in breaks over time. Like scientists, speakers of a community never face unintelligibility. Creoles are the mechanism that reconciles the dialectic of continuity and discontinuity that alone can fuel progress.

The dissertation ends with considerations for engagement between philosophy of science and decision-making practices that depend on its findings for their intellectual grounding. Business innovation studies liberally borrowed from the Kuhnian vocabulary on scientific change and revolution. Regrettably, the borrowings have not kept pace with the state of thinking in the core fields of the history and philosophy of science. It is my hope this is dissertation is an installment on a more actionable philosophy that helps ground real-world decision-making in a more correct understanding of science.
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