The London School of Economics and Political Science

Michael Polanyi's theory of tacit knowledge: An epistemology of skill in science

Kizito Kiyimba

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Declaration

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work.

I warrant that this authorization does not, to the best of my belief, infringe the rights of any third party.

Abstract

How can we claim to *know* and even tenaciously hold in science what we might possibly *doubt*? Standard methodologies of science have not answered this question persuasively. They either propose an answer that misrepresents science or they propose an irrational approach to science. The reason for these two extreme positions is that the accounts of science in these methodologies are based on a false ideal of objectivism - an assumption that the success of science as a branch of human knowledge is based on it being *objective* in the sense of being *impersonal.*

Michael Polanyi propounds a theory of tacit knowledge, and I claim that this theory provides the best answer to the above question in that it represents scientific activity accurately and rationally. Polanyi rebuttals the false ideal of objectivism/impersonalism in scientific knowledge with a richer account of actual scientific practice. I show that he restores heuristics, and accounts for the role of skill without thereby succumbing to psychologism/subjectivism. I explore Collins and Pinch's claim that controversy is central to scientific progress, and critically examine Mwamba's book length study of Polanyi. I tackle the objections made by the Popperians (notably Alan Musgrave) to Polanyi's theory and the alternative methodology provided by Imre Lakatos/Elie Zahar. I argue that Popperianistic methodologies present incomplete accounts of science. Instead, understanding the nature and functions of tacit knowledge provides a richer epistemology of science.

Further, the theory provides grounds for re-tackling the perennial problem of skepticism. In the theory, every act of knowledge is a skilful act and whenever we can point out *that* we know, we affirm our *ways* of knowing. Thus removed from the false ideal of objectivism, we are closer to resolving skepticism. The thesis is also an introduction to the still nascent philosophy of Michael Polanyi to analytic philosophy. It is akin to but not identical with Thomas Kuhn's philosophy of science.

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0.0 Introduction

How can we claim to *know* and even tenaciously hold in science what we might possibly *doubt*? This is a reversal of the question that Immanuel Kant is dealing with in his *Critique of Pure Reason*, (Kant, 1998/1781) when he seeks to establish the grounds on which we can arrive at abidingly true knowledge by means of fallible experience. And rather than take an abstract or idealistic approach, I intend to try and answer the question by taking into account what is typical in the experience of the scientist. I am not the first to take philosophical interest in how we know in science. On the one hand, enquirers into how we know have had science and all its comparative success at the back of their minds as they propound theories of knowledge or epistemologies. On the other hand, philosophers of science have written elaborate accounts or methodologies of how science functions, among which are such works as *Logik der Forschung* (Popper, 1969/1935), *Structure of Scientific Revolutions* (Kuhn, 1962), etc. Other works have been specifically sociologies of science. Perhaps unintentionally, most of such approaches to knowledge in science (be they philosophical or sociological) have tended to make of science a unique kind of knowledge, separate and above all other forms of knowledge in terms of being objective and making comparative tangible progress in solving the problems it sets itself. I believe that a more complete account of how we know in science can go a long way in complementing foregoing efforts in epistemology, in methodology and in the sociology of science.

What motivates my approach is what I perceive as a stalling of foregoing attempts to provide an answer. Very little is being produced today in terms of explaining how science works and how we know scientifically. And yet there is still no agreement on the nature of scientific knowledge in methodology. This lack of agreement, I opine, is based on the fact that epistemologies have been only partial in providing an understanding of the nature of knowledge as a whole. One way to try and re-ignite interest in this area would perhaps be to begin by overhauling epistemology to come up with a fitting definition of 'knowledge' before we can come up with a well founded methodology.

But this approach would not be practical because it would risk stalling on details of definitions in some midway step of the process. Besides, there is a possibility that epistemology could learn from methodology too since there is no logical priority of epistemology over methodology. The two areas of enquiry could be mutually beneficial. As an alternative to this approach therefore, I suggest that a more accurate account of the experience of a scientist in acquiring scientific knowledge be made, which would be a *descriptive* account. In turn this descriptive account could be extrapolated to cover relevant areas of knowledge in general. This latter part of the approach, I suggest, would then provide us with a *normative* or *prescriptive* account of knowledge.

On the one hand, a purely descriptive account of how science works would be lacking in philosophical interest. It would generate a descriptiveness fallacy that would put too much emphasis on the details that nothing general could be said about science. On the other hand, A purely normative account would most likely be too abstracted from and aprioristic to the actual activity of science to be of any use to scientists. This would be a normativity fallacy. Connected to this normativity fallacy, in my opinion, foregoing standard attempts to answer this and similar questions (to the one I pose at the beginning) have started out with objectivist assumptions (where being 'objective' has the properties of being detached, impersonal and even indubitable) and then sought to describe scientific knowledge in the same objectivist light. In this view, the relative success of scientific knowledge is perceived as accruing from its being detached and impersonal. Not only have there been problems with objectivist theories of knowledge stemming from this normativity fallacy; there have been endless controversies with methodologies (and possibly with sociologies of science).

What my proposed approach could bring about is a bridge, the missing link, an element that weaves through the descriptive and the normative in methodologies of science, which could enlighten the debate in epistemology on the question of knowledge. In my opinion, the bulk of the standard methodologies in the philosophy of science on the question (e.g. verificationism, falsificationism, Kuhnian theory of paradigm shifts in revolutionary science, methodological scientific research programmes, etc.) has been committed to different kinds of methodologies informed by their respective epistemologies and mostly oblivious of *actual* scientific practice. Where the epistemologies have been incomplete they have helped generate incomplete methodologies. A brief outline of the general problems facing these approaches will suffice at this stage.

0.1 Partial epistemologies

0.1.1 Understanding knowledge

The literature on the debate about knowledge and the definition of knowledge in epistemology shows that there is little progress being made towards a conclusive definition of knowledge. In broad terms, on the one hand there is the traditional conception of knowledge defined as 'justified, true belief'. After Gettier's counterexamples to this kind of knowledge, the response has mainly been geared towards buttressing the traditional conception of knowledge against such objections like the Gettier counterexamples. Examples of such responses have been the 'causal theory of knowledge' and a related form of this theory called the 'reliabilist theory of knowledge'[1].

At this stage of my investigation, I think it is a fair observation to make about the debate, that both the traditional understanding of knowledge and the efforts to strengthen this traditional definition take into consideration only one aspect of knowledge, namely 'knowledge that' (MacIntosh, 1979-1980). The motivation for such a ('propositionalist') take on knowledge seems to me to be to explicate propositional knowledge, which would be most characteristic of the examples chosen in the debate. But the disadvantage is that the resultant definition of knowledge is one that puts a lot of emphasis on the one aspect of *belief*.

And yet knowledge has a wider reach than mere 'knowledge that'. An interesting alternative would be 'knowledge how-to'. Such knowledge cannot be defined in terms of belief. I may know how to ride a bicycle, but such knowledge would not be defined in terms of belief. Even if I were to write a manual on how to ride a bicycle, the propositions in the manual would not make me capable of passing on a belief that would make the reader learn how to ride a bicycle just by reading them. An adequate understanding of knowledge should be able to accommodate various kinds of knowledge. In an effort to give a more comprehensive

[1] Both of these theories will be treated in greater depth in the next chapter, defining them here would lead me far afield.

understanding of knowledge, N. Rescher outlines at least four kinds of knowledge broadly understood as having access to correct information. Thus far he points out: a) 'knowledge that', which is a knowledge of facts – that something is the case; b) 'adverbial knowledge', which includes 'knowing what', 'knowing when', 'knowing how', 'knowing why', etc; c) 'knowledge by acquaintance' which associates individuals or things (e.g. I may know Popper because I know the author of a given book by him); and d) 'performatory knowledge', or 'how-to knowledge', which is a knowledge of skills. (Rescher, 2003, pp. xiv-xv).

Now, I do not set myself the task of reaching a conclusive definition of knowledge understood comprehensively, for that would lead me far afield. It is enough to show that there are other kinds of knowledge not accounted for in this traditional definition of knowledge and in its more refined forms, which forms of knowledge could conceivably help make the traditional definition of knowledge less problematic as well as throw more light on how we know in science.[2] Now, it is true that knowledge in science is at least in part composed of 'knowing that', but in addition to this there is substantial 'knowledge how' which goes into making up the whole body of scientific knowledge. And still an objection against the multiplication of 'knowledges' is valid.[3]

The application of Occam's razor in this area would be helpful in coming up with a helpful understanding of knowledge. But Occam's razor becomes necessary only when the various forms of knowledge are either redundant or mutually

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[2] In fact, seen the other way round, some epistemologies have been built around a given conception of scientific knowledge as an example of human knowledge at its best. Kant's *Critique of Pure Reason*, for example, in as far as it is a work in epistemology, takes for granted the success of Newtonian mechanics and the truth of Euclidean geometry to try to explain how we can come up with synthetic *a priori* knowledge.

[3] Hilary Kornblith seems to suggest an application of Occam's Razor when he objects to the view of science as a special kind of knowledge that humans possess for reasons other than mere genetic drift or chance. (1999, p. 337).

inconsistent. The various forms of knowledge ought to be informative of knowledge as a whole and they are to form a consistent and mutually complementing set. On this assumption, it seems like a viable effort to seek an insight in knowledge as a whole by proceeding from an adequate knowledge of a given form of knowledge, in my case, scientific knowledge. And so I hope that if I arrive at a greater clarity of how science works, and how we hold scientific knowledge, I will have hopefully *persuasively* (even if not conclusively) laid the grounds for explicating better the wider concept of knowledge.

0.1.2 Adamant scepticism

One of the most enduring influences on our understanding of knowledge has been René Descartes' reflection on knowledge, given in his First Meditation. (Descartes, 1641/1996) Descartes sets out on a path of methodological doubt with the intention of attaining 'clear and distinct' (indubitable) ideas. But he soon comes up with the idea of a malicious demon or an evil genius, who might be responsible for the ideas in our minds even when those ideas may seem clear and distinct. And so already then, in the reflections of Descartes, the link between the search for indubitable and thus objective knowledge (i.e. objectivity understood as an impersonal detached mental attitude by which indubitable knowledge could be attained) and the enduring presence of scepticism is apparent.

Modern expressions of this 'Cartesian' scepticism continue to use an equivalent to the evil genius, e.g. us being brains in a vat, or being manipulated by a treacherous scientist who has us linked to his computer, etc. The more we want to establish knowledge on sure grounds, the more persistent is the question of how sure the grounds are (i.e. scepticism). The same link has been pointed out, among others, by Thomas Nagel:

> "The two [objectivity and scepticism] are intimately bound together. The search for objective knowledge, because of its commitment to a realistic picture, is inescapably subject to scepticism and cannot refute it but must proceed under its shadow. Scepticism, in turn, is a problem only because of the realist claims of objectivity." (Nagel, 1986, p.71)[4]

Whether scepticism is a problem akin to the realistic approach to the world is open to question, but the discussion of realism is one I would like to avoid given its vastness and complexity. I will make no conscious commitments to realism or anti-realism of any sort. For the moment it is enough that the point has been made, namely that scepticism feeds on the search for indubitable knowledge and thus on objectivism. The search to understand knowledge is as exposed to scepticism as it is attached to an ideal of objectivism. The more we press for absolute, indubitable and objective truth, the more we end up with scepticism. The grander our ideals of objective, indubitable truths, the more entrenched is the attendant scepticism.[5]

The kind of scepticism I refer to here is different from Pyrrhonian scepticism which radically suspends any search for knowledge on the grounds that nothing can be known for certain.[6] This latter form of scepticism is of very little epistemological interest, since in its being so extreme it simply puts an end to epistemological engagement. At the opposite end of the scale, elsewhere, Nagel can be understood as giving a counsel of despair when he suggests that even given the heroic

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[4] This view, that indubitability and scepticism are bound together is also shared by L.S. Carrier. (1974, p. 147)

[5] I mention in passing here and very broadly indeed, that the efforts of Descartes and Kant are commendable as regards finding an answer to scepticism, but they do not rid philosophy of scepticism. Kant proposed a system in which we could have synthetic a priori knowledge, and he held the scientific axioms of his day (Euclidean geometry, Newtonian Mechanics, etc.) as examples of such knowledge. Perhaps the way forward, as we suggest here, is the other way around: to learn from the way science knows and then proceed inductively to the way we ought to know, rather than vice versa, holding a belief about how we ought to know and then seeking to fit scientific knowledge into the epistemological structures resultant from such belief.

[6] There is no rationally permissible opinion, not even this very one. Scepticism purges all forms of opinions and arguments, including itself, like a purgative medicine that clears both the illness and it disappears. Such scepticism urges us to suspend any judgments or knowledge claims. We know nothing for certain. (Sextus Empiricus, 210/1990, Book II, par. 188).

achievements of some ambitions of knowledge, a "pervasive scepticism or at least provisionality of commitment is suitable in light of our evident limitations. (Nagel, 1986, p.69) Nagel downplays scepticism to a mere malaise that we should and could learn to live with while we hold on to our knowledge claims, given that we are limited in the means we have to arrive at indubitable knowledge. But this approach does not take scepticism seriously enough.

In similar vein, a few voices in epistemology have urged a lowering of expectations in the effort to resolve Cartesian scepticism or the scepticism that arises from seeking to achieve Cartesian standards of knowledge about the external world. (Fumerton, 2005) But areas of knowledge like science show us that it is possible to *know* what we can possibly *doubt* without thereby giving up the ideal of finding sure knowledge. A look at the history of science shows that it is normal in science to hold beliefs that can possibly be doubted. And so a closer look at scientific knowledge could be a way to include the *hazardous character of knowledge* in the very conditions of knowledge without thereby being paralyzed by scepticism. In other words, we can proceed to find knowledge even in the face of the possibility that we may arrive at a half-truth or an illusion. There is a fiduciary aspect to scientific knowledge.

This is not to ignore scepticism; rather it is to *choose* not to be motivated by it. Scientific knowledge promises to provide a model for such a view of knowledge. In other words, for the moment I agree with A.J. Holland when he holds that scepticism is built on an assumption – the assumption that knowledge requires "some special kind of assurance or ability to tell that what one professes to know is indeed the case". (Holland, 1977, p.555) And so, one way out of this predicament is to drop the assumption. We are as exposed to scepticism as we are to a false ideal of objectivism. Dropping the false ideal, not just out of exasperation, but because we can show that science (held out as the model of knowledge) does not proceed according to this false ideal, is a plausible way out of scepticism.

Now, this is admittedly not a knock-out response to scepticism. The sceptic may point out that the evil genius could still be manipulating us to think that science has arrived at some knowledge and that scientists have had to proceed in a fiduciary manner, suspending doubt, until they arrived at this putative knowledge. A preliminary response to this sceptic would be to show that the kind of scepticism which points out that the evil genius is not only manipulating a few minds but all minds uniformly is a kind of meta-scepticism which is not harmful to the activity of science. As long as scepticism is kept at that abstracted level, it ceases to be a philosophical concern. But this response will have to be fleshed out by the way I proceed to show how we know in science what we could possibly doubt.

Concluding this section, I propose that the success of a theory of knowledge in providing a way out of perennial scepticism is a measure of the success of the theory itself. The efforts at defining 'knowledge' given at the height of the debate on the definition of knowledge have not been very fruitful in dislodging scepticism or even providing a viable way out of it. In addition, the philosophy of language has tried to find an answer to the perennial problem of scepticism (e.g. Putnam, 1981) In the linguistic approach, the problem is reduced to one of appropriateness of reference (in meaning) between propositions and what they refer to in the world etc, e.g. between 'bottles' and actual bottles. Responses to this approach have been given e.g. in Barry Stroud (1984). Similar or related theories suggested against scepticism would include the principle of charity, theories of verifiability, the causal theory of reference on which I hint, etc. (Nagel, 1986) But I make a pragmatic decision to steer clear of the linguistic and other related approaches while duly acknowledging their seriousness and complexity. The problem of scepticism seems to remain adamantly at the end of all these efforts to dispel it.

0.2 Problematic methodologies

0.2.1 The story of recent standard methodology and objectivism

Given the partial epistemologies on which standard methodologies of science were developed, it is no surprise that neither of them considered on its own gives a complete picture of the way we know in science. Many of them have been proposed as a remedy to perceived weaknesses of foregoing ones. In very broad strokes, without pretending to present a strict chronological account, and without committing myself to mentioning all the particular philosophers,[7] I give an outline of the history of the debate in order to emphasize the point that the reason for these successive failures is that each of the standard methodologies has taken a wrong starting point, principally the one of assuming that scientific knowledge is objective in being impersonal and detached from the scientist.

Thus far, falsificationism comes to correct the errors (mainly logical) of verificationism and its attendant logical positivism. Among the claims that falsificationism makes is that verificationism is not the logic followed by scientists. But falsificationism on its part comes with its set of attendant problems. In reality, scientists are not known to abandon their bold conjectures as soon as a counterexample has been found, and yet falsificationism left on its own cannot account for this apparent discrepancy.

[7] I choose to keep the report very general at this stage for pragmatic reasons. Mentioning names would call upon a little more detail in looking at all sides of the position of a given philosopher.

Thomas Kuhn (1970) suggests a remedy according to which scientific knowledge grows by way of revolutions. Scientists holding on to older theories or positions only change their view in a revolutionary context. Between the revolutions there is a period of normal science and sooner or later, upcoming scientists find problems with the normal science and a revolution is fomented. The shift from one paradigm to a succeeding one is revolutionary, the two paradigms being incommensurable. The virtue of this approach by Kuhn is that it seemed to take into due consideration the actual history of science. However a general criticism levelled at it is that it places an irrational element right at the heart of progress in scientific knowledge. In other words, Kuhn's methodology of science seems to explain progress in this apparently most successful area of human knowledge, using an irrational element. There is a quasi-religious conversion from one paradigm to another.

The question to be addressed then is: what then motivates this change between paradigms and thus accounts for apparent progress in science? There is an attempt to rehabilitate falsificationism by suggesting sophisticated falsificationism or a theory of methodological scientific research programmes. According to this proposed theory, scientists work inside a research programme and they work to protect a hard core using changeable theories, models, formulae, etc. around the unchanging hard core. What happens to effect the (rare) change from one research programme to another is that scientists find that an existing programme does not propose new problems and it solves existing problems only in an *ad hoc* fashion. Scientists then abandon the older programme for one that proposes new problems and solves existing ones without resorting to *ad hoc* methods.

But here too, the question of why scientists choose one programme over another is not satisfactorily solved, at least not according to the standards of objectivity and rationality set by the standard methodologists themselves. It boils down to a whim, the way scientists decide that a given research programme is no longer coming up with new problems and solving old ones. The criteria are not clear on how much time is allowed before the decision is made, for some problems may take longer to solve than others, but they could still get solved within the same research programme.

In response to this problem and in view of foregoing failed attempts to propose a workable methodology of science, Dadaism is suggested according to which anything goes – there is no methodology for science. In other words, science progresses precisely by not having a clearly outlined methodology. What the scientist is interested in is to arrive at the truth about the world, regardless of the method she uses. This latter too falls short of giving a useful account of how we know in science, for it blurs the distinction between, for example, science and magic. Anything goes, according to Dadaism.

There could be an element of the truth about scientific knowledge in most of the methodologies proposed. I argue that each of them does not stand to criticism because each is based on a partial epistemology that emphasizes 'knowledge-that' at the expense of other forms of knowledge on the one hand, as well as elevating objectivism to unsustainable levels on the other hand. On the matter of objectivism, H. Prosch roughly summarizes the objectivist position in methodologies of science thus:

> "Scientific objectivism, in its most simple and straightforward sense, holds (1) that there are objective states of affairs, that is, that something is the case

> 'independently of our minds,' which it is our business to come to know; and (2) that the method of careful and accurate observation of immediately given sense data, without reference to our personal participation, our wishes, wants, values, hopes, fears, or expectations, is of the utmost importance; and (3) that the final arbiter of scientific theory is a crucial experiment with all factors carefully controlled, an experiment that can subject our theory to an acid test because it results in the observations or lack of observation of sense data predicted on the basis of the theory." (Prosch, 1986, p. 29)

A more detailed treatment of the accounts of the methodologies outlined above would show that they share some or all of these elements given by Prosch. Now the same objectivists many of whom form the protagonists of the standard methodologies hinted upon above would admit that in fact science does not follow these steps exactly. But at the same time they would find a way of insisting that logic had to remain logic and any so-called scientific activity that strayed from this norm had to be explained outside the philosophy of science – in another branch of enquiry (e.g. psychology). Thus objectivity, even if it were not exactly followed in the actual practice of science, has to be pursued as an ideal (Musgrave, 1968), hence the description they have earned themselves of being objectivists.

Overall, a way forward towards giving a fuller account of scientific knowledge could be found in paying closer attention to actual scientific practice considered more globally than from an objectivist perspective. Some of the methodologies proposed give a more or less coherent story, but they lack an account of what scientists do and so they end up being *aprioristic* in approach. Some have addressed the matter of the fallibility of scientific knowledge and instead consigned this ideal of objectivity to a 'third realm'. Briefly, this is a realm proposed by Frege and which is revived by the critical rationalists, but which comes with a problem of describing the metaphysical nature of this realm and our relation to it.

0.2.2 Heuristics omitted

Standard methodologies have been concerned about letting the psychology of discovery into accounts of how science functions. Part of this concern is founded, in the sense that by making psychological elements (e.g. personal tendencies or even personal value systems) a part of the methodology of science, it becomes difficult to weed out idiosyncrasies and the claims of charlatans from good science. Further, a well formalized logic of scientific discovery, construed in an objectivist approach could hardly account for such personal elements. And yet, judging from foregoing attempts in standard methodologies, what we are left with when we leave out all that these methodologies labelled 'psychological elements' is hardly satisfactory either.

Perhaps then a decision has to be made to include forms of criteria of the acceptability of knowledge claims that are not exactly formal or formalisable. It does not mean that existing logics of scientific discovery have to be entirely abandoned. There is a place for an enthymeme in logic. When other forms of criteria are allowed into the existing logics, then we could have a more complete picture, one that helps us get rid of the weaknesses of many standard epistemologies. And so the current formalizations could be rescued if they are considered enthymematic in character. And yet there would still be a problem with the ideal of formalization.

Even given the possibility that all the unformalised parts of the logical syllogism that accounts for scientific discovery were to be included in some syllogism, there would still be a problem namely that there is a discrepancy in conceiving of a deductive *logic* of *discovery*. If this were possible, then there would be no real discovery to speak of. Putative new knowledge would simply be a deducible (not discoverable) from the premises. And so I would argue for caution in taking a thoroughly logical-deductive approach.

0.3 Conclusion and a summary of what is to come

Alvin Goldman has claimed that the role of epistemology is to elucidate what he calls our 'epistemic folkways'. (Goldman, 1993) The wisdom in this approach is that it helps us avoid the trap of *apriorism*. It may not be fruitful to approach areas of knowledge (e.g. science) with an already made standard of objective knowledge against which we measure the given areas of knowledge. Rather, we may have to proceed inductively from epistemic activities (actual beliefs, specific knowledge, etc.) towards an understanding of epistemic concepts. I propose that we look first and as accurately as possible at an area of knowledge that has registered some success and from the way that area of knowledge functions we can say something more general about knowledge.

In the beginning this approach would be descriptive in presenting an accurate account of how we come to scientific knowledge, for example. But there could be a valid objection to the effect that a philosophical treatment of knowledge should not be tied to the merely descriptive. It cannot be imprisoned by the actual causal structure of the world. Philosophers ought to be able to propound a theory according to which knowledge *ought* to function – a prescriptive theory. (Kornblith, 1999, p. 338) Even then, the prescriptive enterprise of philosophy would be meaningless if it went against the basic causal structure of the world. Certainly, reference to that causal structure in order to remedy the excesses of a mainly prescriptive approach is in order. In my approach, I explore the descriptive approach in order to come closer to a more exact and more useful understanding both of scientific knowledge and of knowledge as a whole. In this latter sense, my approach is prescriptive. In other words, I narrow the gap between the descriptive and the prescriptive. Thus far, I would suggest that we situate ourselves primarily in the descriptive approach of science in the hope of emerging with a normative view that corrects traditional methodologies as well as propose a wider view to correct areas of traditional epistemology.[8]

The usual focus of the philosophy of science is to present a normative/methodological, or historical/descriptive account of the workings of science. I claim that mainline accounts have ended up with an unrepresentative account. The portrait of science in many of the mainline accounts is either incomplete, or false. As a result, general philosophy itself misses the opportunity of learning from science. I attempt to turn that situation in the philosophy of science around and suggest that philosophy take a lesson from *actual* science. If science has succeeded very well in solving the problems it sets itself, it is not so much because of the special or objectivistic nature of scientific knowledge as because science has taken seriously and systematically our way of knowing – skilfully – has paid attention to skilful knowledge.

Basing myself on Michael Polanyi's theory of tacit knowledge, I investigate the question: How or why do scientists hold on to knowledge that they know could later be changed or discarded? I come to the conclusion that scientists hold on to such knowledge based on *tacit knowledge*. In other words, to know in science is a *skilful* act. My investigation further shows that scientific knowing is continuous

[8] Alvin Goldman claims that epistemology needs the features and findings of cognitive science. The product is what he calls scientific epistemology. Within cognitive science, he distinguishes between descriptive and normative scientific epistemology. (Goldman, 1993, pp. 272-273) I do not go into the analysis of his delimitations here, but I can already point out that the descriptive and normative branches of epistemology are not mutually exclusive.

with other kinds of knowing in the sense that all knowing is based on skill. There is a pervasive tacit element in all knowing, and not just in scientific knowing. And so I suggest that the lesson that philosophy can draw from engaging with science in the philosophy of science is a revision of our understanding of knowledge in order to include the key aspect of *skilful knowing.* It is part of *knowledge* to appraise a problem that can be solved. And a problem which ends up solved attests to the skill of the appraiser. The appraiser has a skill in bridging the logical gap between the problem and the solution. This account of knowledge includes heuristics and originality.

Accordingly, this work situates itself neither entirely in the methodology of science, nor totally in the description of science. The theory of tacit knowledge as propounded by Michael Polanyi bridges the gap between the two areas. It corrects and fills in the lapses of both the methodological and the descriptive approach to the philosophy of science. Polanyi has diagnosed that the most important error of mainline methodological and descriptive accounts of science is the undue emphasis they place on objectivity understood as impersonal detachedness in scientific processes. I treat that as *objectivism.* And so the theory of tacit knowledge helps rid accounts of the workings of science, of objectivism. Now, to argue for the theory of tacit knowledge is not to introduce an obscure term that explains everything, itself remaining inexplicable. It is not a primitive term. While the work to explicate it is nascent, the argument for it is persuasive.

Mainline definitions of and approaches to knowledge have their limited success or strength in as far as they designate 'knowledge that'. But scientific knowledge goes beyond the mere statement of 'the facts' to include the way the facts have been arrived at and to include even the controversies, discrepancies, peculiarities, revolutions, etc. through which or by which the facts are arrived at. My argument, by which I have arrived at the above positions is spread over seven chapters grouped under four parts. In Part One, Chapter Zero, I set the stage upon which major epistemological questions are expressed and the urgent need for them to be answered is emphasized. I point out in this part and chapter that mainline epistemologies are incomplete. On the one hand, as a consequence, the age-old problem of skepticism is left unanswered. On the other hand, a false objectivism that goes hand in hand with such skepticism is promoted. Even more significant and specific for an account of the workings of science, heuristics are not philosophically accounted for.

In Part Two, I discursively elaborate the theory of tacit knowledge. In Chapter One, I present the theory of tacit knowledge as providing grounds for the epistemology of skill, a notion, I argue, that mainline epistemologies have overlooked. Relying on some experiments in animal psychology (e.g. Polanyi's accounts of the way animals learn new tricks and which ways are similar to human learning processes), I set out to expand the notion of knowledge beyond human knowledge in order to show the tacit foundations of all knowledge, including articulate or formal knowledge. There is continuity between such animal knowledge of the most rudimentary type to the most formal human knowledge because of the persistent tacit roots of knowledge.

And thus any history of human formal knowledge that would overlook these tacit foundations would end up being merely anecdotal and erratic. Polanyi has borrowed from Gestalt theory in order to propose an active theory of perception. It is an organizational process. Data on the senses is organized to make sense of the animal's surroundings. More skilful animals are rewarded with more understanding of the surroundings. There is an interest in surviving within those surroundings. The organization is a tacit process. It engages the individual animal and collectively it could have benefits on the species. It is useful in showing the constant reliance of articulate or formal knowledge on tacit or inarticulate foundations, itself being knowledge, to fill in gaps and make perceptions *meaningful* to the perceiver both human and non-human.

In Chapter Two, I discuss Polanyi's example of how tacit knowledge functions. The way tacit knowledge functions, is such that it underlies skill, setting the standards of skilful practice and approving the results of the skill. To make an affirmation, e.g. a knowledge affirmation, is to appraise the act of affirming. There is a personal element as opposed to an objectivistic, impersonal or detached approach to making knowledge assertions or scientific assertions. Using the example of how we learn language (including our mother tongues and symbols e.g. in mathematical language) and eventually learn to use such language expertly. Language is such that to use it skilfully, there must be room for re-applicability of words in comparable situations. And this points to the discretion or judgment of the user. This works within the constraints of a fixed grammar upon which we agree. There is a gradual differentiation of the articulate from the inarticulate.

This differentiation necessarily remains incomplete in the end. But in language, the skilful user must know how to cooperate with the operational principles even at the risk of error. This is when language can open the skilful user to unforeseen knowledge. Examples are the speculations with initially vague terms in mathematics, like the irrational numbers, or negative numbers, etc. The skilful user of the language cooperates with the operational principles but there remains a risk of utter nonsense or sophistry. Judgment (i.e. personal and tacit) then comes to play a key role in deciding on how to adapt and what to adopt or discard. Thus far, I argue in line with Polanyi's position, the highest level of articulation or formalization is never completely detached from its inarticulate, tacit or informal foundations.

This discursive investigation leads me to conclude in this chapter that there is a need to look again at the mainline understanding of epistemological terms like objectivity, rationality, truth and the inclusion of role of the knower in the activity of knowledge in an account of knowledge. The resulting understanding of knowledge would include heuristics. The role that tacit knowledge plays is the recognition of beauty and elegance. A further and more sociological but no less relevant role that tacit knowledge plays is to help in the organization of science. The scientific community is a community built around tacit knowledge within a given area of science. When a discovery is made, or an innovation arrived at, heuristic passion is useful in helping bring others on board.

Such heuristic passion is accounted for in the theory of tacit knowledge. Thus far, in science, 'knowing that' (i.e. propositional knowledge) and 'know how' (i.e. skilful knowledge) come very close to each other but without being logically reducible to each other. The way a scientific fact is established (through controversy, etc.) is crucial for it to be recognized as a fact or not. Scientific standards are set by scientists, and then they are vindicated or discarded in time, again by the scientists.

Part Three is made of three chapters: Chapters Three to Five. The part is a discursive examination of the reception that Polanyi's theory of tacit knowledge has received in the philosophy of science. Some of the reception has been positive. I discuss an example of such positive reception by looking at Tchafu Mwamba's position on the theory. I argue to tighten loose ends and to correct positions of Mwamba that make the theory of tacit knowledge vulnerable to attack within analytic philosophy. In Chapter Four, I answer to the criticisms that Alan Musgrave makes towards the theory of tacit knowledge. I maintain that Musgrave represents a big section of the critical rationalists. He raises the objection of psychologism in the theory of tacit knowledge. I argue that the roots of such an accusation are in false objectivism. And so the critical rationalists as a whole are averse to any psychological explanation of the activity of science.

I argue that the aversion is misconstrued, and when in the end Musgrave realizes the weaknesses offset by the aversion, he only makes an inadequate admission of the weakness of his position. Chapter Five is an examination of the theory of the methodology of scientific research programmes as propounded by Lakatos and Zahar. This is the last sustained effort in methodology of this kind that was dominated by discussions on Popperian, Kuhnian Lakatosian and Feyerabendian methodologies. I argue that the theory of tacit knowledge supersedes even the Lakatosian/Zaharian approach.

These latter are still preoccupied by demarcation and they have an aversion towards a form of psychologism. Their account of the driving force in the progress of science remains objectivist and it further suggests to historians of science to re-write the history of science in order to point out novel facts. Such novel facts would account for the phenomenon of discovery and scientific revolution where there is revolution. I argue that the weaknesses of the Lakatosian/Zaharian approach lie precisely in an unfounded aversion to the psychological and an overemphasis of objectivity/rationality understood as a deductive system. The Lakatosian/Zaharian approach does not succeed in holding both deductivism and accounting for creativity in science. The theory of tacit knowledge fills in the gaps left by their approach because it accommodates psychological elements.

Critical rationalism of the Lakatosian brand proposes a rational reconstruction of history. What the theory of tacit knowledge proposes is a rational reconstruction of methodology and the historical *description* of science. The reconstruction is based in the actual practice of science rather than on aprioristic principles of rationality. The role played by the scientist as a rational agent is restored. The skilled judgment of the scientist is showed as built into the rationality of science. And so the reconstruction that the theory of tacit knowledge suggests frees up space within methodology in the way that creativity, heuristics, and the indeterminate nature of science can now consistently be seen as part of scientific knowledge. Because knowing in science is a skill, more is known about science as science is actually done. And thus an attempt to draw general conclusions about how science works based on hitherto successful science is bound to be partial and even misrepresentative. An investigation of how science works that is willing to take seriously the role of tacit knowledge points towards a new epistemology – an epistemology of skill.

In Part Four, Chapter Six, I make an evaluation of the theory of tacit knowledge, outlining its true virtues both for the philosophy of science and for epistemology in general. I argue that the theory of tacit knowledge does more justice to actual scientific practice for it is not aprioristic and it makes room for the indeterminate nature of scientific knowledge. I argue further that a study of the theory will open up more fertile fields for both methodology and epistemology of science and for epistemology in general. It is in this latter area that the theory of tacit knowledge could pave a new way to try and answer the age-old question of skepticism.

In order to better take on this task of opening up new possibilities for methodology and epistemology, the theory of tacit knowledge would have to work further on precision and definition of terms. Further, interconnections and relationships need to be studied between the theory and mainline analytic philosophy. Added to this, there could be an objection to the theory of tacit knowledge to the effect that it ushers in an *autobiographical* turn in methodology, i.e. too much is conceded to personal talent and the personal account of the individual scientist.

But this objection would be akin to the accusation of psychologism treated above. While it is true that the theory of tacit knowledge is the closest we come to an autobiographical turn in the methodology of science, the theory does not impose autobiographical limits on methodology. Instead, it is wider than autobiography in the sense that it suggests an element that is general or can be generalized about scientific autobiography. The scientist is restored as an important agent in the activity of science is restored to her position. She is active in making judgments and drawing up sketches about reality that remain open to further investigation, adaptation or outright discarding. Further, neither should the theory of tacit knowledge be understood as opposed to or replacing logical approaches. These too have a role to play. The theory of tacit knowledge fills in the gaps. The case for tacit knowledge as it stands so far is *persuasive* in the main, and that is a good starting point.

Part Two: Elaboration of the theory of tacit knowledge

Chapter One: Tacit knowledge – towards an epistemology of skill

1.0 Introduction

The debate on the definitions and analysis of the philosophical concept of knowledge has slowed down before a conclusive argument for any of the contending positions has been reached. The literature shows that most of this debate raged around knowledge understood as justified true belief and Gettier's counterexamples to this position. Standard definitions of 'knowledge' (e.g. knowledge as justified true belief, or a causal theory of knowledge, or even the reliabilist theory of knowledge) are not useful for understanding how we know in science because they only give a partial account of knowledge (i.e. propositional knowledge) leaving out a key element of *how we know* (i.e. tacit or implicit knowledge). Putting back tacit knowledge in the picture will go a long way in providing a more robust theory of how we know in science in that this more robust theory will eliminate the difficulties in standard methodologies.

1.1 Towards a more accurate theory of knowledge in science

1.1.1 Knowledge as justified true belief and Gettier's intervention

Edmund Gettier intervenes to show that the conditions that must hold for it to be said of someone that he or she knows are neither sufficient nor necessary. Before Edmund Gettier's counterexamples, the traditional theory of knowledge held that *S* knows that *p* if and only if

(i) *p* is true,

(ii) *S* believes that *p*, and

(iii)*S* is justified in believing that *p*

In this tradition, Ayer holds in conclusion to his discussion on the theory of knowledge that "the necessary and sufficient conditions for knowing that something is the case are first that what one is said to know be true, secondly that one be sure of it, and thirdly that one should have the right to be sure." (Ayer, 1956, p. 35) And in the same way, Chisholm, as quoted by Gettier (1963, p. 121) holds that S knows that p if and only if

(i) *S* accepts *p*,

(ii) *S* has adequate evidence for *p*, and

(iii) *p* is true.

Without going into the details of Gettier's intervention here, Gettier sets about to show that the conditions stated are not sufficient for the truth of the proposition: *S* knows that *p*. And it is in response to Gettier's objection that the causal theory of knowledge is propounded, and after this, the reliabilist theory of knowledge. Now, there could be two ways of interpreting Gettier's critique of the definition of knowledge that was current at his time.

One way of interpreting this critique is the exclusivist approach in which Gettier can be understood as trying to exclude certain epistemic states from knowledge. In this approach, he is looking for a strict interpretation of knowledge. Most of the known responses to his critique that followed immediately on his critique (e.g. Goldman, 1993) may be understood to try to re-define knowledge in a strict sense so as to exclude epistemic states that may pose as knowledge. The other less usual interpretation of Gettier's intervention could be that he is looking to widen the understanding of knowledge in such a way as to include a few more epistemic states in the realm of knowledge.

Whichever of the two ways we may choose to interpret Gettier's intervention, what is of value is that he made it possible to take a fresh look at our definition(s) of knowledge. Such a fresh look may help us expand our definition of knowledge to kinds of knowledge that are not propositional.

1.1.2 A causal theory of knowing

As a response to Gettier's counterexamples, a causal theory of knowledge is propounded according to which the justification of knowledge could be traced along a causal chain linking the proposition that is known to the knower. But as long as the causal theory of knowledge insists on a reconstruction of the causal chain that leads to some knowledge, the theory is too strong in the sense that should the knower falter in the reconstruction and seem to merely guess the elements of the causal chain, there is no knowledge. (Goldman A. , 1967, p. 363) Here I suppose that the knower needs to be able to retrace the causal chain of her knowledge, otherwise we would have cases in which people would not know that they knew, a case that Gettier brings up in one of his counterexamples.

But not all the links in the causal chain may be specifiable, just as not all knowledge is articulable. As an example, a scientist may discover a solution to a problem that has been nudging her for some significant amount of time. She may be able to declare at the moment of discovery that she *knows* the solution to the problem and yet not be able to justify the stages in her coming to this knowledge. With time, she may be able to 'reconstruct' a way in which such knowledge may in general be arrived at, and from then on, the causal chain is articulable. But this does not mean that knowledge only begins to exist at the moment when the articulation of the causal chain is made. It was no less knowledge at the moment of discovery.

Let us grant for the sake of argument that the knower sets out to give a causal chain for her knowledge. A further problem would still remain: How exhaustively does the knower go about outlining the links in the chain? Can the knower account for all the relevant causal elements? Probably aware of this problem, Goldman lays out his analysis thus:

> "*S knows that p* if and only if
> *the fact that p is causally connected in an 'appropriate' way with S's believing p.*
> "Appropriate," knowledge-producing causal processes include the following:
> (1) perception
> (2) memory
> (3) a causal chain, exemplifying either Pattern 1 or 2, which is correctly reconstructed by inferences, each of which is warranted (background propositions help warrant an inference only if they are true)
> (4) combinations of (1), (2), and (3)" (Goldman, 1967, pp. 369-370)

In these terms, the explication of Goldman's theory of knowledge is weaker and more inclusive of kinds of knowledge that may at the moment be inarticulate or inarticulable. He points out here that this rendering of the theory leaves room for cases of knowledge in which the knower cannot *state* the justification of her belief. The problem with the latter rendering, however, is that it becomes too weak. 'Appropriate' knowledge is explicated by being a result of memory or perception or combinations of the two. It becomes difficult to isolate what claim to knowledge gets left out in such a rendering.

Let us imagine I dream of a unicorn standing in my neighbour's garden. Does that mean that I have knowledge of the unicorn? At least the idea of the unicorn is a result of a combination of memories of various perceptions. It is not a totally original idea in its parts. In this case of the unicorn, the conditions that Goldman sets out for 'appropriate' knowledge seem to be fulfilled, and yet I cannot in my wakeful state claim that I have knowledge of a unicorn. In other words, Goldman's rendering of a causal theory of knowledge does not go far enough in explicating what knowledge is.

Goldman is aware that his theory with the clause of 'appropriate' causal processes opens up to 'some presently controversial causal processes that we may later deem 'appropriate' and therefore knowledge-producing". (Goldman, 1967, p. 371) He cites extrasensory perception as an example. But perhaps the investigation of knowledge does not have to be carried so far to such exotic forms that would be difficult to defend philosophically. An example that would be less problematic to defend philosophically, yet one which brings out the same point would be one of young mathematical prodigies who may habitually arrive at solutions to difficult mathematical problems without thereby being able to articulate the methods by which they arrive at the solutions.

One cannot claim that those children are ignorant of the solution.[9] I believe a more committed enquiry into how we know, grounded in specific areas of knowledge (e.g. scientific knowledge) could help us to learn more about knowledge without having to open up to classifying extrasensory perception as an appropriate causal link in how we come to know. An adequate theory of knowledge should allow for the case where (for example) a body of expert scientists, *basing on their long experience* in choosing between competing theories, *and backed by their track record of successes*, are able to pick out a promising or fruitful theory in a short time or to back a research project that 'seems' more feasible among many that are being

[9]D. S. Mannison cites this example as well. (1976, p. 145)

proposed. The very same scientists may not be able to justify their knowledge rather than insist that they know it from *experience*, which statement in turn they may not be able to explicate. Other scientists may believe in the positions taken by the expert scientists, basing their belief on the foregoing successes of the expert scientists.

There are examples in the history of science to back this position. And so looking both at experience and successes, the scientists and those around them could claim that they have knowledge in a given area even when the experts cannot exhaustively justify how they have come to such knowledge. On a similar point, Mannison argues: "If [ones] *knowing how to Φ* is in that in which [one] *knows that p*, it is not necessary for [one] to know that [one] knows how to Φ for it to be true that he knows that *p*." (Mannison, 1976, p. 146) And still all that is not to deny that there is a causal explanation. Perhaps someday the scientific experts themselves, or others steeped in the methodology of science, can come up with a causal explanation. The point is that such an articulated causal explanation is not necessary for the scientist to be able to claim knowledge.

And so, in order to avoid cases of knowledge like the unicorn above, it is necessary to look more closely at how we make knowledge claims. It is necessary to clarify that not all our knowledge is articulable all the time, and that this is not a weakness. Perception is a conscious effort in which the mind makes sense of what is perceived, sometimes (unconsciously) filling in the gaps from background knowledge. This opens knowledge to so many hazards, but without it the mind would hardly ever be able to come to any knowledge. The sense data would only be patches and noises and sensations patched together with no meaning at all.

1.1.2 Reliabilist theory of knowledge

Another line of response to Gettier's counterexamples is the reliabilist theory of knowledge or reliabilism. Reliabilism attempts to show that the beliefs in Gettier examples are actually not knowledge. (Nozick, 1981) It picks up from the causal theory of knowledge and is an improvement thereof. Nozick, for example, finds that the causal theory of knowledge is an 'inhospitable environment' for mathematical and ethical knowledge. Further, it does clarify the kinds of causal connections it is dealing with. The example he uses is one in which a person floating in a tank has his brain manipulated to make him believe he is floating in a tank. That person would not be in a position to know the cause of her belief even though it is essential to the causal chain that forms her knowledge. (Nozick, 1981, p. 172)

And so, Nozick and others propound a reliabilist theory of knowledge. As defined by Hilary Kornblith, Reliabilism "... is the view that knowledge is reliably produced true belief, and thus that justified belief is reliably produced belief. ... A belief-producing process is reliable just in case it tends to produce true beliefs in actual situations as well as in counterfactual situations that are relevant alternatives to the actual situation. A belief is justified just in case the process responsible for its presence is reliable." (Kornblith, 1980, p. 609)

In this light, Nozick's rendering of reliabilism is a truth tracking account. According to Nozick, "To know is to have a belief that tracks the truth. Knowledge is a particular way of being connected to the world, having a specific real factual connection to the world: tracking it." (Nozick, 1981, p. 178) Reliabilism seems to be most compatible to how we know in science, looking at the way scientific facts are generated, published and established, at least in an objectivist approach. Before the facts are established, it is important to be able to clarify how the claims to the truth were established. In addition to this, there is a community of scientists who approve of the methods used and whose duty it is to welcome new scientific knowledge. But that is in as far as scientific knowledge is concerned.

The claims of reliabilism go beyond mere scientific knowledge. They purport to say something about knowledge in general. And so an enduring weakness of reliabilism is to specify who sets the standards of reliability. There are several possibilities including a community or even some objective standards against which reliability can be measured. And still, at the end of all this process, reliabilism does not offer a sure antidote to scepticism. The objection that Nozick makes against the causal theory of knowledge, in which he uses a version of the Cartesian evil demon, can also be applied to reliabilism.

It is possible (for example) that both the person claiming to know and the community that checks the reliability of the claims to knowledge, are under the spell or control of the Cartesian evil demon. In this case, where there is systematic reason why checking has no connection with improving knowledge claims, neither the person nor the community would ever have *good grounds* for believing the person is tracking, so the idea of tracking becomes otiose. A final and even more important weakness with the reliabilist theory of knowledge is that it too (like foregoing theories) addresses propositional knowledge.

Yet knowledge is not limited to what is phrased in absolutely certain propositions. (Carrier, 1974, p. 148) The reliabilist theory of knowledge does not take into consideration the knowledge of skills. How would we track the truth of the knowledge of skills? A viable theory of knowledge should be able to include all that is called knowledge. As long as such a theory has not yet been established, it remains misleading to base a methodology of science (e.g.) on it. I find that in the tradition of propounding a theory of knowledge in response to Gettier's counterexamples, Keith Lehrer has made the most interesting (because most sophisticated) proposal for my purposes.

1.1.4 Lehrer's sophisticated response to Gettier

Keith Lehrer's is a sophisticated response to Gettier's counterexamples. He holds that a viable theory of knowledge would be such that:

> *S* knows that *p* if and only if:
>
> (i) *p* is true,
>
> (ii) *S* believes *p*,
>
> (iii) *S* is completely justified in believing *p*, and
>
> (iv) If *S* is completely justified in believing any false statement *h* which entails (but is not entailed by) *p*, then *S* would be completely justified in believing *p* even if *S* were to suppose that *h* is false. (Lehrer, 1965)

Lehrer's analysis of knowledge brings out the fact that we do not have to believe what we suppose to be true. Further, our suppositions do not have to be treated as evidence for what we believe in. And yet those suppositions we do not believe can still serve to prevent us from appealing to evidence that we have in justifying our beliefs. (Lehrer, 1965, p. 175) In other words, we can be said to know, even if we are working on suppositions when we arrive at that knowledge provided our suppositions are fully justified.

The reason I find this rendering of a theory of knowledge interesting is that it seems to lay the grounds for a fallibilist approach to knowledge which is in some aspects applicable to scientific knowledge.

Now, it is true that Lehrer is specifically trying to answer to specific objections raised by Gettier. But it is still valid to point out that some problems persist even with Lehrer's analysis of knowledge when we try to apply it to areas of knowledge e.g. science. First of all, it still remains to be specified what is meant by 'truth' in science. The history of science is riddled with examples of notions that were once considered to be true and were later given up (or improved upon) as either false (or only partially true). And so the very first condition given by standard analyses of knowledge and repeated by Lehrer (i.e. '*p* is true') needs to be further explicated. A second problem is linked to the notion of belief that is central to this and similar analyses.

The second condition of the analysis is: *S* believes *p*. A closer look will reveal that the kind of knowledge that is dependent on *belief* is only propositional knowledge. But, as I have already urged, knowledge can be dissociated from belief.[10] Inexplicable knowledge, e.g. knowledge of skills, does not require belief as would propositional knowledge. And so it is clear that this and similar analyses are geared towards understanding only one section of knowledge – propositional knowledge. Little in these analyses would help us understand the knowledge of skills which forms part of scientific knowledge.

In relation to scientific knowledge, such analyses are then only partially useful, for scientific knowledge is broader than the knowledge of scientific propositions. A third problem with this and similar analyses is that they are retrospective. They address knowledge after or according to the fact of knowledge,

[10] D. S. Mannison argues for the same point. (1976, p. 140) J. J. MacIntosh too picks a cue from the fact that we never ask 'why one knows' but rather 'how one knows' and 'why one believes' rather than 'how one believes'. On second thought, this is valid across many languages. He hopes to use this distinction (among others) to dissociate belief from knowledge. (1979-80, pp. 175-176)

i.e. when some subject has been said to *know*.[11] This is not of itself a problem until the analyses are applied to scientific knowledge in the effort to come to a methodology or epistemology of science. In retrospect, methodologists of science can afford to pick and choose among the theories of science that have endured (i.e. successful science) and use these to propose a methodology of science. In other words, Lehrer's and the standard analysis of knowledge can be prefaced with the phrase: "When it is said of a subject *S* that she knows *p*..."

In a context of scientific knowledge, a retrospective analysis of this kind is often not fully descriptive. Informed by such an approach to knowledge, resulting methodologies can afford to place an emphasis on the normative aspect of methodology at the expense of a full description of how science works. Methodology has thus often ended up with well argued accounts that fail to represent the full picture of how science works. Examples of such methodologies are verificationism, falsificationism, and methodological scientific research programmes.

1.1.5 The possibility of knowledge in non-human animals – a broader perspective

Hilary Kornblith suggests that epistemology should not be limited to *our* (human) concept of knowledge but should be widened to investigate the concept of knowledge itself whether human or animal. He suggests that it should be investigated in much the same way as we investigate other natural phenomena. He goes on to suggest that knowledge is a 'natural kind'. I do not go into the implications of treating knowledge as a natural kind. Nevertheless, I agree with

[11] Richard Robinson claims that knowledge is a state and not an act, event, a kind of thinking, process, capacity nor disposition. Without going into the discussion of the various consequences of considering knowledge as any of the other attributes, we shall for now adopt Robinson's view. (1971, pp. 17-18)

Kornblith as long as this widening of the concept of knowledge may lead to a better understanding of the narrower concept of human knowledge. In other words, understanding how animals know may enlighten the understanding of how humans in particular know. But at the same time, Kornblith may be motivated by an objectivist view of science in this proposition to view knowledge as a natural kind. If the latter is true, then Kornblith's approach leads to an objectivist understanding of knowledge similar to the standard one. As I argue in this entire research, science should not be held up in such objectivist light as to overshadow the tacit, implicit or skilful element of scientific knowledge.

Nevertheless, the rest of Kornblith's argument is useful for my purposes of dislodging the objectivist approach to knowledge. He points out that many others including philosophers and cognitive ethologists have attributed intentional states and beliefs to non-human animals. He takes the extra step from attributing beliefs to attributing knowledge to non-human animals, encouraged by the effortless use of the word 'knowledge' by cognitive ethologists, who are more rigorous in using the word 'belief' of the same animals. (Kornblith, 1999, p. 329) As an illustration to Kornblith's argument for attributing knowledge to animals, of animals tending towards knowledge, Kornblith cites the work of Louis Herman and Palmer Morrel-Samuels as they address the issue of knowledge attribution to animals directly. According to him:

> "Receptive competencies support knowledge acquisition, the basic building block of an intelligent system. In turn, knowledge and knowledge-acquiring abilities contribute vitally to the success of the individual in its natural world, especially if that world is socially and ecologically complex, as is the case for the bottle-nosed dolphin ... Among the basic knowledge requisites for the adult dolphin are the geographic characteristics and physiographic characteristics of

its home range; the relationships among these physical features and seasonal migratory pathways; the biota present in the environment and their relevance as prey, predator, or neutral target; the identification and integration of information received by its various senses, including that between an ensonified target and its visual representation; strategies for foraging and prey capture, both individually and in social units; the affiliative and hierarchical relationships among members of its herd; identification of individual herd members by their unique vocalization and appearance; and the interpretation of particular behaviours of herd members ... This is undoubtedly an incomplete listing and is in part hypothetical, but is illustrative of the breadth and diversity of the knowledge base necessary to support the daily life of the individual dolphin. Similar analyses could be made of knowledge requirements of apes or other animal species, but the underlying message is the same: extensive knowledge of the world may be required for effective functioning in that world and much of the requisite knowledge is gained through the exercise of receptive skills." (Kornblith, 1999, p. 330)

And so knowledge acquisition is attributed to these animals on the grounds that the animals cannot survive merely by receiving information passively, but must also have strategies to understand the complex environment. This would point towards a sophisticated processing of information that enables the animal species as a whole to carry out successful behaviour and in turn result in species fitness. But an objection to this apparent suppression of difference between human and animal knowledge would be that it is clear that humans are capable of self-reflection while other animals do not seem capable of it, even granting that they may be capable of beliefs.

The response Kornblith gives to this objection is that indeed there is a difference, but it is based in the fact that the knowledge of other animals, rather than exclude this capacity, does not require it. What matters for the ethologists is that the animal has true beliefs that have been (reliably) acquired. On top of that, many human beliefs are arrived at unreflectively, and so the difference between humans and animals in this aspect loses its significance. The difference that remains is one, not of kind, but of degree of sophistication. (Kornblith, 1999, pp. 335-336) The virtue of Kornblith's position is that it expands knowledge beyond propositional knowledge to include kinds of knowledge that may be inarticulate or tacit. Such knowledge is acquired in the animal's effort to cope with its surroundings – to be fit and survive. It is plausible that at the animal level, humans do share these ways of acquiring tacit or inarticulate knowledge. Kornblith assumes that the reliabilist theory of knowledge is true, and as I have pointed out on treating reliabilism, this approach seems to concur with the way scientific knowledge is established in not being entirely objectivist.

1.2.0 A view of knowledge in science

An objectivist view of knowledge may have contributed to an objectivist view of science. But scientific knowledge is far from being objectivist. In fact, together with the question: What do we know in science, is bound the question: How do we know what we know in science? The question of epistemology is linked to the question of methodology. On its own, this claim does not rid our view of science of an objectivist approach. An objectivist view could still impose itself by insisting on objective methods in order to guarantee true scientific knowledge. I propose that looking at concrete examples of how scientific knowledge is established will help clarify our understanding of scientific knowledge. H. Collins and T. Pinch have done some work (1998) in this area which is adequate to help me appraise the point that the epistemology of science is bound to the methodology of science in a way that is not objectivist.

Using many examples from what they admit is a non-standard and in many ways (admittedly) incomplete history of science, sociologists H. Collins and T.

Pinch point out that controversy in science is interesting and of key importance. They break away from the classical misrepresentation of science as marked by simplicity, objectivity, repeatability of experiments, and success. More often than not, controversy precedes the establishment of scientific knowledge. Published or publishable results of experiments come at the end of long and painstaking efforts in laboratories.

Few significant experiments are simple and straightforward. In order to settle for a given set of results, therefore, statistics are used. And with the interpretation of statistics comes the *skill* of the experimenter. But *skilled practice* in turn tends to be more experimenter-specific, and so different experimenters may tend to get different results of a purportedly similar experiment. This sets into question the whole emphasis on the repeatability of experiments in some accounts of methodology (e.g. verificationism, falsificationism). So-called 'golden-hands' that make a particular experiment work are more frequent than the case is reportedly attested. Collins and Pinch illustrate this with the example of the 'bioassay' in pharmacology. (Collins & Pinch, 1998, p. 9) Typically, a bioassay is a technique that is difficult to transfer from one scientific group to another because it depends to a great extent on skill and practice. How then do scientists in a different group to the one carrying out a bioassay check the results of a bioassay? They may have to rely on the skill of the group that carries out the bioassay. The repeatability of the experiment (at least by another or controlling group of scientists) is no longer of great importance in this case. But there is a possible objection here to the effect that 'skill' can easily be used as an opaque concept and one that blocks a critical approach. In order to dispel the fears of an ad hoc use of 'skill' as a primitive concept, this concept needs to be explicated at least to some extent.

Elaborating more on the nature of controversy in science, Collins and Pinch point out that unlike a logic of experiments which is clear and simple and even at times apparently compelling (cf. attempts by the classical methodologists), controversy drags on for long. At the end, what has been proved remains unclear to the protagonists. On top of that each experiment demands many trials and a statistical analysis. The results are not always definitive. Enduring controversy brings with it an increase in the number of so-called 'important' variables that could affect an experiment. Such variables are termed 'important' as experimenters try to justify the validity of their experiments. All this is for proponents a further emphasis on the need for skill if the experiment is to be repeated successfully. But at the same time, the same argument is nothing but an ad hoc effort in the eyes of the critics of the results.

To the critics, the proponents can be seen as only masking the unrepeatability of their experiments. (Collins & Pinch, 1998, p. 12) In fact in scientific controversy or where findings are unorthodox, the weight and number of experimental replications is not enough to sway the unbelieving critics. The ensuing sets of results can serve to entrench either side of the controversy in its position, believers as well as sceptics. And even at times a single counter example from a renowned and reputable critic (i.e. skilful and experienced) is enough to 'disprove' many results of an unorthodox scientist. The scientific community looks beyond the methods to the skill involved. In yet different conditions, a controversial question in so-called fringe science is given up not because it has been proved fruitless or misleading, but simply because researchers are either tired of it or because proponent experimenters have lost credibility in another related area of research, or even because the question does not interest contemporary imagination.

1.2.1 Two illustrations of arriving at 'scientific knowledge'

The Michelson-Morley experiment was not designed to test relativity but to test the speed at which the earth drifted in ether. Yet after the publication of Einstein's papers, the experiment was retrospectively reconstructed as a proof of relativity – to the effect that it showed that the speed of light is the same in all directions. The original goal of the Michelson-Morley experiments was carried further in spite of the many failures to register a result that showed a difference in the speed of light in different directions. Later, the experiments were carried out by Morley and Dayton Miller, and in 1925, Miller received the 'American Association for the Advancement of Science' award for finding that the earth experiences a shift in ether at about 10 km per second. This was an experiment carried out by a physicist of renown and it was encouraged by Einstein. It disproved the theory of relativity. It could have been held out as a crucial experiment. Instead, Miller's results were ignored as a mere anomaly – the interest of the scientific community was in other areas, namely in relativity.

It was in this vein, that Arthur Eddington undertook to take photographs during a solar eclipse in 1919 in order to compare his findings with the predictions made by Einstein about the deflection of the light from the stars. Two locations were set up, and Eddington went to one of them. In the discussions that followed Eddington's findings, it slowly emerged that Einstein's predictions were confirmed. About this process, Collins and Pinch point out that when Einstein derived the maximum apparent deflection of light rays, it could have been perceived as problematic in the modern understanding of how science works. Those who were less gifted than he was at calculating the answer were confused. It turned out then, as in many delicate experiments, that the calculations, even though they were hardly understood at the time, were only seen as correct *after* the observations had 'verified' the calculations.

This, for Collins and Pinch, goes to show that the claim that science proceeds by having clearly stated predictions which then get falsified or verified is not accurate. In fact, theory and measurement go subtly hand in hand. We accord validity to our theories in as far as we can make measurements. (Collins & Pinch, 1998, p. 44) It happens then that one takes a theory and separates from it the prediction that is derived from it. When the results of the experiment concur with the prediction, not only is the prediction confirmed, but the theory too. Thus Eddington's experimental results confirmed both the prediction and the theory of relativity.

Collins and Pinch report further that Eddington's results were not all of them a perfect fit with Einstein's predictions. Some were even in conflict with each other. So Eddington chose which ones to keep as data and which ones to discard. To do this, he used Einstein's prediction. In their turn, Einstein's predictions were accepted because of Eddington's findings. So, observation and prediction were linked in mutual confirmation and were not distinct stages. Collins and Pinch call this 'an agreement to agree'. (Collins & Pinch, 1998, p. 45)

When Eddington performed the experiment, he did not just look through the telescope. Rather, he made a whole set of assumptions – e.g. how to estimate the known spurious effects (of changed focal length of the telescope, etc.); how error is statistically distributed in the plates, etc. He had to make calculations, interpolations, etc. This happens even with clear and sharp images or photographs. Eddington ended up with inconclusive results and then a controversy ensued about how to interpret the results. To Collins and Pinch, the resolution of the controversy in a meeting chaired by Sir Joseph Thomson the then Astronomer Royal on 6 November 1919 involved recourse to authority. Sir Thomson is reported to have claimed: "It is difficult for the audience to weigh fully the meaning of the figures that have been put before us, but the Astronomer Royal and Professor Eddington have studied the material carefully, and they regard the evidence as decisively in favour of the larger value for the displacement". (Collins & Pinch, 1998, pp. 50-51) Now the larger value was nearer to Einstein's predictions. The observations could have justified a Newtonian displacement (lower value) or an Einsteinian displacement (almost double that of Newton). Nothing was inevitable about them. But Eddington chose those that justified the latter. It was a *decision* or choice. Looking back, the decision or choice seems to have been a correct one. Reducing this to 'authority' is not helpful for understanding how science works. What is the nature of the authority that Sir Thomson points to?

At least two elements are involved here which, I claim, are related. On the one hand, the view of a quasi-logical deduction that goes from prediction to a clear observational test is not accurate. Instead, theory and experiment are intertwined, licensing and being a consequence of each other simultaneously. Observations are carried out because of a given theoretical outlook, and the theory is corroborated when the observations match the expectations. (Collins & Pinch, 1998, p. 52) There is always a *choice* involved in choosing to see data as providing evidence that backs a given theory. On the other hand, the resolution of the controversy is based on a choice of whose 'evidence' to take as more convincing or persuasive. The Astronomer Royal appeals to more than just the authority of Eddington. He appeals to a profundity, an elegance or beauty in Eddington's side of the story. The choice made by the Astronomer Royal is a corroboration of the judgment of Eddington as this latter makes choices between which data to keep, while conducting the experiment as well as in the rhetoric he uses in persuading the scientific society of the 'truth' of his position. (Collins & Pinch, 1998, p. 97) Crucial experiments and an inexorable logic play a very minimal role if any in coming up with what science considers today to be an accurate description of the world – relativity. Both elements are related by the activity of choice or judgment.

This last point about judgment is illustrated in another case that Collins and Pinch bring forward: the case of the non-detection of gravitational radiation by Joseph Weber from 1969 to 1975. Weber claimed to have evidence of big amounts of gravitational radiation. This claim sparked off controversy and many experiments were conducted to find out whether his claims could be confirmed. The results of the counter-experiments carried out to test Weber's claims were themselves inconclusive. And so the central question was: What is the correct outcome?

Collins and Pinch suggest that in fact knowledge of the correct outcome may not be helpful in resolving the controversy. In the case of the gravity waves, it is not clear whether the correct outcome is the detection or the non-detection of gravity waves, for the existence of the waves is itself the issue at stake. They note, succinctly: the putative 'correct outcome' depends on the actual existence of gravity waves that can be detected. In order to establish this latter fact, there is a need for a good detector. A *good* detector in turn depends on whether it establishes the *right* outcome. This process goes on into an infinite regress. What may even complicate the matter more is the fact that even so-called 'standard techniques' for a given experiment might have to be done in a special or highly specific way to achieve useful outcomes of the experiment. This reality undermines 'identical experiment' argument i.e. the insistence that experiments must be repeatable in order to ensure objective results.

And so there are at least two ways to break the impending experimenter's regress. First, the scientists must have a clue of the quantitative range of the outcome. This would then act as a universally accepted experimental quality control. Otherwise, another criterion, independent of the outcome of the experiment must be sought. This opens the debate to a whole range of strategies, some of them more rationally acceptable than others. Most of those evaluating the experimenters will judge them on their prior reputation, connectedness with other researchers, knowledge therefore, skill, personal dedication to the experiment and the apparatus, and qualification to deal with the apparatus and experience.

All the while, the process of deciding who is a good experimenter and what is a good experiment, is part and parcel of the whole process of determining the *correct* results of the experiment in question. A good experimenter is one who, using a good experiment, can arrive at good results e.g. detect gravity waves when they exist, or not detect them if they do not exist. To Collins and Pinch, the way to avoid the experimenter's regress is to view together the scientific and the social aspects of the process of establishing scientific facts. (Collins & Pinch, 1998, p. 101) And so it happens that in the example of gravitational radiation, as in a few other cases of controversial science, experimenter's regress is at times resolved by an avalanche of negative reports against a said experimental result. There is a 'critical mass' of such negative reports which ensures that the experimental report is stopped.

In the case of gravity waves, that critical mass or avalanche was triggered by Garwin's clearly negative and outwardly unhesitating report. Garwin had started out by not believing in Weber's report. He went on to express his disbelief very clearly and ingenuously in his own report. But generally, scientific controversies focus attention on the competence of the protagonists. The questions at stake are inextricably bound to the competence of the individuals involved. And as effort is made to describe the details of the situation in which a scientific position is taken, science comes across as similar to other day to day activities that require skill. Controversy brings out the usually hidden processes. In the end, the facts are usually inseparable from the skill used to produce them. (Collins & Pinch, 1998, p. 116) In the end, what imposes a restraint on scientists not to think the 'unthinkable' is more the scientific culture than Nature itself. We seem to make our science as we like to. (Collins & Pinch, 1998, p. 138)

It is apparent that in order to get a fuller understanding of how science works, we need to understand both the science that succeeds and the science that fails. Some standard methodologies have tended to rely on the science that succeeds to make a universal conclusion about how science works. These methodologies do acknowledge that the transition from one general scientific position (be it a bold conjecture or a paradigm or even a methodological research programme) to the next is often turbulent. What the methodologies do not acknowledge is that what actually goes on in the details and the dynamics of the turbulent transition is essential to understanding what goes on in the science that succeeds and to understanding how science works. Such an approach is more helpful than trying to come up with a logic of scientific discovery, for example. A more global approach that includes an account of the dynamics of controversy shows that science is more about expertise than certain knowledge. This more global approach shows why scientific knowledge *is* contestable. For a given position to be adopted as the acceptable scientific view, experts have to agree.

1.2.2 The anecdotal nature of histories of science in a methodological context

Now, a few contrasting histories of relativity have been written, and this position by Collins and Pinch is certainly not immune to the criticisms levelled at any history of science in a context of the methodology of science. One such criticism is that histories like these tend to pick and choose among events in order to underline or highlight a given position. In fact Collins and Pinch have dealt with criticism i.e. that they misrepresent science, e.g. in the case on the relativity debate. And yet Collins and Pinch respond by emphasizing that their work is not a work of the history of science in the standard sense of the term. They assert that the only claim they have to writing history is in contributing to what they call 'interpretative history of science", which according to them depicts the complexities with which the scientist grapples in her search and establishment of facts, complexities that depict the contentious nature of science rather than present science as marked by simplicity and success. And so they do not pretend to give a complete history.

Even the interpretation of the historical facts, they admit, is affected by the way history unfolds. The success of a theory can influence the way histories of that theory are written. To them, the history of science consists in at least five other kinds of history, besides 'interpretive history'. First is textbook history – which is science presented in historical style; second is official history – written long after the events and to suit the victor's story; third is reviewers' history of science – which contributes to science by sorting out the chaos; fourth is reflective history – which aims to improve science by building collective wisdom; fifth is analytic history of science – which is the one written by social scientists in their attempt to understand the workings of science, and which philosophers use to make 'rational reconstructions' in which history is aligned to scientific ideas.

To Collin and Pinch, controversial science and its treatment in interpretive science has its undeniable value. In the event of scientific controversy, the potential to reinterpret the meaning of an experiment is realized. Consensus is lost and experiments are not accepted uncritically, even though experiments continue to be an essential part of doing science. (Collins & Pinch, 1998, pp. 175-176) Appeal is then made to the judgment of the scientists involved in the controversy and to their audience in agreeing on the way forward. This judgment is in turn rooted in expertise and experience. This is what saves science from some kind of 'experimenter's regress', in which experiments would be contrived to test results of earlier experiments ad infinitum.

Concluding from this survey, 'to know' has both a propositional and a procedural sense. We can 'know that' and we can also 'know how'. The way the distinction or the relationship is drawn between these two aspects of knowledge has been at the core of the controversies in methodology and epistemology of science. In science, these two aspects of knowledge come very close to each other. The way a scientific fact is established is important in determining whether or not it is a fact. Science is that branch of knowledge where the standards are set by scientists and the product of the process is a vindication (or condemnation) of the procedures or the standards. The two elements are mutually important. A more complete account of how science works cannot emphasize one of the elements at the expense of the other. And due acknowledgment of the tacit process at work in both elements points us to the same tacit process in animal knowledge and learning as a whole. This will become clearer soon.

1.3.0 Clues of tacit knowledge from animal learning

Relying on the published results of contemporary experiments in human and animal psychology at his time, Polanyi holds that there is a striking parallelism or similarity between the early intellectual development of a child and a chimpanzee. In the experiments, a series of intelligence tests is passed by both the child and the chimpanzee. A difference begins to occur when the child, responding to adults who talk, begins to understand speech and to speak. The child then gains capacity for sustained thought and inherits the cultural heritage of human society. Polanyi reaches three conclusions from these experiments. First of all, the use of language is a major factor in the human intellectual superiority over animals; secondly, linked to the first observation, but even this distinguishing mark between people and other higher primates, Polanyi argues, is founded in inarticulate capacities that people share with animals, for language itself is based in inarticulate powers of the intellect. And thirdly, short of the linguistic clues, people are not a lot better than animals at solving the problems we set for animals.

Polanyi relied on reports in animal behaviour by Köhler (1925). The results were corroborated by later experimenters like J. Doehl (1966). Captive chimpanzees were reported to have 'solved problems' like accessing food from behind wire netting or hanging out of reach by the use of sticks as tools or by standing on boxes they piled up. Robert Hinde reports that the previous experience and familiarity of the chimpanzees with the objects they used as tools must have been of help. Also the repertoire of movement patterns that the chimpanzees possessed, including those they may have imitated from humans, did play a role.

The results of the experiments are now open to testing against results of chimpanzees studied in a free environment. (Hinde, 1966, p. 450)

Köhler had not been isolated in his research on animal intelligence. Before him, Leonard Hobhouse had carried out studies (albeit in general inconclusively) about the intelligence of animals. This latter began the movement to show that animals did not act out of sheer habit, but were capable of some kind of purposive behaviour or practical judgement. Around the time of Köhler, Robert Yerkes (in the United States) carried out experiments with apes, and so did Ladygin Kohts (in Moscow). For a number of reasons, Köhler's results were not positively received by the mainstream psychologists especially in the United States. Robert Boakes points out at least three. First of all, Köhler had chosen to interpret the results of his experiments (mainly simply carried out) in a subjective way, meaning that he ascribed a purpose to the behaviour of the animals rather than interpret the same behaviour as meaningless. This, for his critics, meant that Köhler was ascribing some kind of non-material mind to the subjects of his experiments. Köhler tried unsuccessfully to explain the behaviour using field theories.

The second reason Boakes points out is that Köhler's experiments brought about an unfamiliar theory through a methodology that was not objective according to his critics. In other words, it was not clear what empirical questions he sought to answer and could answer within the framework he set himself. His critics could not see how Köhler's results about problem-solving could help further research in a productive way. Later on, Köhler is reported to have admitted that the same questions could have been investigated through a more orthodox experimentation. A third reason that Boakes gives is that the critics of Köhler saw in his use of Gestalt theory to explain the purposefulness of ape behaviour a promotion of hereditarianism. Thus far, the apes were being described as superior in intelligence to other species because they had it in their genes. The critics transferred this possibility unreflectively to the human sphere and out of the fear of the prospects, chose to reject Köhler's views. (Boakes, 1984, p. 203)

Meredith P. Crawford has also carried out experiments to test the cooperative ability among captive chimpanzees to solve problems set for them. His aim is to distinguish between human language and animal communication. The problems are set in such a way that their solution requires cooperation among the captive chimpanzees. Some kind of communication that involved solicitation, directing the counterpart to a goal (by a cry or a push), etc. was observed between the animals. But no evocative behaviour (of a speaker-hearer relation type) was observed, what could be termed as language. Neither did the animals have the capacity to point out an object to another nor resort to a symbolic or representational use of language. (Crawford, 1969)[12]

Looking more closely at animal psychology current at his time, on the manifestations of the animals' inarticulate intelligence, Polanyi makes a difference between instinctive actions and actions which do not belong to the native repertoire of animals. He calls the latter learning, and learning is a sign of intelligence compared to instinctive actions which he calls sub-intelligent. He borrows three classes of learning in animals from animal psychology. The three classes that Polanyi borrows are: A: trick learning (e.g. when a mouse learns to obtain a food pellet by depressing a lever), B: sign-learning (e.g. when a dog learns to expect food at the gong of a bell), and C: when an animal "reorganizes its behaviour to serve a

[12] A more detailed version of the report of the experiment is given in Meredith P. Crawford, (1941).

purpose by exploiting a particular means-end relationship" (Polanyi, 1958, p. 73) or a sign-event relationship. The first two classes are rooted in the motility and sentience of animals while the third includes these two functions in an implicit operation of intelligence. In the first two classes, the animal sets up a time sequence either contrived or observed. In class C, the animal achieves a "*true understanding of a situation which had been open to inspection almost entirely from the start.*" (Polanyi, 1958, p. 74)[13] The animal in class C has learnt something that it can show in various and unpredictable ways. This has been called "latent learning" by the same psychologists. Thus the:

> "... capacity of deriving from a latent knowledge of a situation a variety of appropriate routes or alternative modes of behaviour amounts to a rudimentary logical operation. It prefigures the use of an articulate interpretative framework on which we rely as a representation of a complex situation, drawing from it ever new inferences regarding further aspects of that situation. Latent learning is transformed into pure problem-solving when the situation confronting the subject can be taken in by it from the start, at a glance. This reduces exploration to a minimum and shifts the task altogether to the subsequent process of inference. Learning becomes then an act of 'insight', preceded by a period of quiet deliberation ...." (Polanyi, 1958, p. 74)

In the process by which latent learning shifts to being a guide to problem-solving, it reaches beyond itself as an ingenious process of inference that is relying on (sometimes) faulty assumptions. The process is as precarious as it is important. This evolution of inferential powers is always at the risk of inferential error. The same risk can also sometimes be seen in the transposition of practical problems into verbal terms. Thus far, we can see inarticulate behaviour gradually developing and approximating and finally achieving an articulate form. Piaget has studied this development in children and is reported by Polanyi to have inaccurately labelled this

[13] Emphasis in the original.

a development of the intelligence in children. But for Polanyi, this is rather an increased mental discipline which is attained through the establishment of a fixed interpretative framework of growing complexity. This distinction is important in order to keep the flow from inarticulate behaviour to articulate behaviour smooth and continuous. It is already at this stage an antidote to objectivism about science in the sense that what we achieve in science is rooted in the same tacit procedures as what we achieve in many other areas of human culture without the necessity of a break that introduces an objectivist view that is held by some methodologists about science.

Already in the learning process displayed here by the animals, the distinction becomes clear between the heuristic part of the learning process and the formal part of the same process. What is heuristic is learnt once and for all and cannot be repeated, while the formal can be retraced to its origins and repeated – it is reversible. An objection to this position may point out that in standard propositional logic, if I infer *p*, we cannot work back to the premises exactly. It could for example be as a result of a conditional sentence in which the antecedent is affirmed; or it could be the result of a conjunction, or even the result of a disjunction in which the consequent is denied (*modus tollens*). But the answer to this objection would be that it is important that the list of possible premises to which we try to retrace the conclusion is not infinite or unidentifiable. We may not be sure of the exact single one, but at least there is a finite set of identifiable possible premises. Compared to the heuristic part of the learning process, it is difficult to retrace the conclusion (i.e. the fact that has been learnt) back to a set of premises. As Polanyi notes, what is learnt heuristically may be as a result of a systematic exploration with an interpretative framework being gradually built; or it may be a result of puzzled contemplation of the situation in question before arriving at a solution by way of a sudden insight, etc. (Polanyi, 1958, pp. 75-76)

Thus far, the actual learning takes place in the heuristic, irreversible part of the process. The second part of the process which the animal can repeat is routine in character. Polanyi points out that the different kinds of learning above show different heuristic acts. The heuristics in A are contriving; the heuristics in B are observing and the heuristics in C are the act of understanding. Likewise, each of the types has a routine part. Thus the routine in A is the repeating of the trick; the routine in B is the continued response to a sign, and the routine in C is the solving of a routine problem.

For Polanyi, this points to two kinds of intelligence. The one intelligence achieves innovations, irreversibly. The second intelligence operates a fixed framework of knowledge, reversibly. And these operations at the inarticulate level of intellectual life prefigure what happens at the articulate level of intellectual life. Further, these three kinds of learning in animals have counterparts at a more highly developed intellectual level in humans. Type A learning in animals (trick-learning) corresponds with invention in humans; type B learning (i.e. sign-learning) corresponds with observation; and type C learning in animals (i.e. latent learning) corresponds with interpretation.

A general criticism about Polanyi's use of the findings of psychology is that like all nascent sciences (nascent at his time), the results of animal and child psychology should not be taken in uncritically, and especially they should not be used to draw such far reaching conclusions. And yet the response to this criticism is conceivable. First of all, Polanyi does make up his mind about the results of experimental psychology. He is not uncritical in adopting the findings of this nascent science. Rather, what he does is to rely on the results of these nascent sciences hypothetically while he seeks to make sense of the activities of science in general. This approach is consistent with what Polanyi suggests should be the approach to science (i.e. not objectivist). On this count, Polanyi is consistent in his approach. He does not have to commit himself to the results of this nascent science; rather he is willing to use them as a crutch to reach even more profound conclusions.

These more profound conclusions are a more robust methodology and a better epistemology. The more robust methodology will better fill in the gaps left by standard methodologies. The better epistemology may provide a more sustainable response to scepticism. Now, the consistency of Polanyi's approach lies in the fact that even though the results of this science could legitimately still be under investigation, yet they help explain other areas of knowledge. In the pragmatism of Polanyi's approach, this could be seen as an indicator that these results are fecund (revealing more than they are initially intended to), and thus with a link to the truth as defined by Polanyi, namely that which has the capacity to reveal more about the world than what is seen at the moment of its propounding.

1.3.1 Introducing Polanyi's view of Gestalt Theory on perception

Polanyi decidedly embraces Gestalt theory in the hope that it could help explain how we come to scientific knowledge in a non-objectivist way. According to Gestalt theory, there are three qualities relevant to perception by adults. Where there is a perception, the first thing to be perceived is the 'figure' as distinct from the 'ground' (i.e. a given object in relation to its background). The perceiver focuses on an object and uses the background within which the object stands in order to perceive the object. But the two, (i.e. figure and ground) are interchangeable depending on what the perceiver focuses on. The perceiver switches from one to the other depending on which contours she chooses to observe. Without this stage, there can be no perception. And so, before a form can be perceived, this stage of distinguishing the figure from the ground takes place.

Secondly, there is a tendency in perception to organize what is being perceived into configurations in such a way the perception may at times not be an accurate representation of the external stimulus. A certain kind of organization takes place according to which the configuration is as much as possible clear, impressive and stable. This could be achieved by simplification, which in turn could be achieved through an increase or filling in of symmetry and regularity, closure, continuity, inclusiveness and good articulation all geared to making the stimulus into a unified whole. At times proximity and similarity are used to associate parts of the resulting whole. (Vernon, 1970, pp. 34-39) And so, rather than analyse the stimuli or the responses of perception into their constituent elements, Gestalt theory takes a holistic approach to perception.

One may ask whether Gestalt theory offers just one of many possible interpretations of perception. What are the virtues of Gestalt theory over an analytical approach to perception? The immediate answer to this objection is in the end result of any given perception. It must be pointed out first of all that the understanding of perception based on Gestalt theory is not proposing an automation of perception. Two different observers of the same stimuli do not have to end up with the same results of perception. The perceivers are actively involved in shaping the end result of a perception. Their past experiences of how certain ways of perceiving similar stimuli has impacted on their lives matters in their choice of which contours to concentrate on as forming the figure and which as forming the ground. It matters that the study of Gestalt theory is carried out among adults, because these latter have learnt over a more protracted period than infants, either personally or from the experience of others, how to choose between figures and grounds. In the end, because experiences differ, there is room for difference in perception. And yet because experiences are comparable within given contexts, the perceptions may be similar or comparable.

In comparison, the analytical approach to perception produces as an end result patches of perception counter-imposed on one another. The role of the perceiver to organize and integrate her perception is not accounted for. A perception according to the analytical approach would thus be a juxtaposition of different colours, sounds, temperatures, tastes, smells etc or any combination of these. The question would remain unanswered: What gives meaning to all these sensations? The analytical approach to perception locates the meaning entirely in the stimuli and in the shared view of a given community that responds to the stimuli. What the Gestalt approach adds is the personal participation of the perceiver in according meaning to the stimuli. There is a personal contribution. But this contribution is not entirely a product of deliberation on the part of the perceiver. It is tacitly undertaken. It is driven by the desire to make meaning of the surroundings of the perceiver. It can be well informed, partially informed or even misinformed in which case the perceiver makes totally false interpretation of the surroundings.

Seen in this light, this tacit element which is responsible for organizing data is shared between humans and animals. It is the guiding element in the exploratory movements and appetitive drives of all animals. In all animals, it can be detected as a self-moving and self-satisfying impulse that enables learning. In humans, it is more highly organised into intellectual powers, articulate knowledge and science, perhaps with the aid of language and culture. Overall, therefore, perception can be understood as an activity that sets its own standards, actively goes about organizing the data, giving its assent when the standards are met. The muscles of the eye, for example, adjust the thickness of the lens with a goal of producing the sharpest possible image on the retina and thus the eye presents to the perceiver a 'correct' picture of the stimulus object.

In the same way, claims Polanyi, we strive to understand data and to satisfy our desire for understanding by framing conceptions of the greatest possible clarity. (Polanyi, 1958, p. 96) Polanyi gives an example from experimental psychology in which when a ball is shown to us, whose size is being inflated, we see the ball as changing its distance from us – drawing closer to us. In our development from childhood, we have learnt to assume that objects all around us retain their sizes. We have learnt to accept this as the normal thing, the correct way to see, the coherent thing about the world around us, more coherent than balls inflating themselves. And even though the closing of distance by the ball would normally make us see double, our perceptual apparatus is ready to make this adjustment of disobeying the evidence of our retinal images. We do intervene actively by visual accommodation. What matters in this intervention is to establish a coherence over all the clues of visual perception in such a way that our subsidiary awareness shall confirm us as having understood the things seen.[14]

An objection to this view by Polanyi would be that it situates a tacit element of volition in the senses. Alternatively, this view seems to attribute to the will of the

[14] In footnote 1, Polanyi talks of a case in which we could be given spectacles that invert images we see so that they are upside down. We see them as such for some time, but after some time – a few days – we learn to see them right way up. And when we remove the spectacles, the images we see are upside down again. And again, after a few days we learn to see them right way up.(1958, p.97)

perceiver what is carried out perhaps spontaneously in the sense organs. But in fact Polanyi's claim is not situated at the level of the sensation. This latter would be a collection of colours, sounds, etc. Rather, Polanyi's claim is situated at the level of perception – what is made of the data that the senses gather. If perception were conflated with sensation, it would be difficult to explain the possibility of error in perception where the sense organs have correctly gathered the data. The perceiver does make a contribution to the perception. Concerning perception in animals, the contribution of the perceiver is evident here too. Polanyi observes that the way to engender the intellectual interest of an animal, e.g. in a sign-learning experiment, is by making the animal aware of a problem that can be solved by straining its powers of observation. After that, the animal's intelligence is spontaneously alive to the problem of making sense of its surroundings, even when the mechanisms to get the animal interested (e.g. through reward) have been withdrawn.

One may object about the latter illustration that in an experiment to teach an animal, what is termed engendering the intellectual interest of an animal, is a subjective interpretation. Humans (i.e. experimenters) intervene in the animal's process of perception, directing the animal towards a given goal, and the results are interpreted according to human modes and frameworks. Polanyi's claim could survive this criticism if it provides a persuasive answer to the question of how individual animals (rather than whole species) survive through changes in their surroundings. There must be a coping mechanism that works. Sensory actions are indeed strivings on which we rely and which we share (humans and animals). In humans, they are geared to helping us make sense of our world according to standards of rationality we set ourselves. They contribute to the tacit components of articulate knowledge.

This point can be conceded without making commitments about the proper understanding of 'rationality' among non-human animals. It is enough to observe that perception for humans and non-human animals guides the efforts to satisfy a drive and when the drive is satisfied, something has been known and some kind of a 'fact' has been added to a conception of the world. And in all these efforts, the goal is self-satisfaction. The individual animal (human or not) itself tacitly sets the standards for success and tacitly accredits the actions taken to achieve them. (Polanyi, 1958, p. 100)

Chapter Two: How tacit knowledge functions (A Polanyian perspective)

2.0 Introduction

Describing how tacit knowledge functions, is a sure way to come closer to defining and structuring it. I examine Polanyi's description of the function of tacit knowledge, its role in heuristics and in the organisation of scientific communities. On the whole, tacit knowledge is inarticulate knowledge, but parts of it can be rendered articulate. A privileged area in which to examine the function of tacit knowledge is in language – how we learn to use language skilfully. By 'language', Polanyi designates "… all forms of symbolic representation …" ranging from natural languages (e.g. Kiswahili and English) and including writing, mathematics, graphs and maps (Polanyi, 1958, p. 77) Language construed in this way is a privileged area because it shows how an inarticulate hunch, thought or idea can eventually be made articulate and even formalized. All the while, there is a tacit coefficient that manages the process. At another dimension, the tacit coefficient governs the modifications we make about language in order to express ourselves. The same inarticulate coefficient or capacity manages heuristics – by recognising beauty and elegance and directing intellectual passion. Ultimately, the same tacit powers are at work in the way scientific communities are organised to ensure that science is done.

2.1 Insights from learning the skilful use of language

The way we learn and skilfully use a language is a privileged process for showing the function of tacit knowledge. A lot has been written in the area of the philosophy of language, but no clear theory emerges. Language can be understood as being representative, expressive evocative, etc. The field is still turbulent. Like Polanyi, I steer clear of (mainly objectivistic) philosophy of language debates and pick those areas of language that reveal the function of tacit knowledge. As exemplified in Polanyi's treatment of language, I claim that science is one such area where scientific knowledge includes an inarticulate or tacit element (especially in the heuristic phase of developing scientific knowledge) which element evolves into articulate and formal knowledge as well as enable and regulate this evolution to take place. Of course, science takes place at a more specialised and formalised plane than most of language (e.g. ordinary language), but the mechanisms by which the inarticulate in science is made articulate are similar to those that take place in language. In fact the same mechanisms are at work in animal learning and animal knowledge.

At this point it is clear that in treating language, we are considering specifically human activity as distinguished from animal behaviour. Experiments in animal psychology (cited above) have come to the conclusion that while non-human animals may exhibit behaviour in which they communicate, there is still no proof of language comparable to ordinary human language. Animals do not seem to agree on meanings and a grammar of their modes of communication among themselves. In that light, it is fitting to refer to the kind of tacit element at work in language as specifically personal in nature, where 'personal' represents the human. On the same note, Polanyi claims that "… language is primarily and always interpersonal and in some degree impassioned; exclusively so in emotional expression (passionate communication) and imperative speech (action by speech), while even in declaratory statements of fact there is some purpose (to communicate) and passion (to express belief)." (Polanyi, 1958, p. 77) Humans differ from non-human animals in that humans have got language, but we share with non-human animals those underlying tacit or skilful mechanisms which in humans enable us to use language. The way these tacit mechanisms function according to Polanyi is such that:

> "If, as it would seem, the meaning of all our utterances is determined to an important extent by a skilful act of our own – the act of knowing – then the acceptance of any of our own utterances as true involves our approval of our own skill. To affirm anything implies, then, to this extent an appraisal of our own art of knowing, and the establishment of truth becomes decisively dependent on a set of personal criteria of our own which cannot be formally defined. If everywhere it is the inarticulate which has the last word, unspoken and yet decisive, then a corresponding abridgement of the status of spoken truth itself is inevitable. The ideal of an impersonally detached truth would have to be reinterpreted, to allow for the inherently personal character of the act by which truth is declared." (Polanyi, 1958, pp. 70-71)

Here, the obvious problem with Polanyi's approach at this stage is that he seems to take too narrow an approach to utterances, including only true utterances or at most only those made with the purpose of eliciting belief. Yet there can be utterances, even in the area of science, made with a deliberate goal to distort the truth. Such deceitful utterances in the area of science could be motivated by vain glory, for example. Besides, there are also utterances made in a doubting state of mind. These two cases are no less utterances. But this problem is not a devastating one for Polanyi's position because even if a deceitful utterance were made within the context of science, no science would be done for in actual fact no description of the world is being made. Likewise for the doubting mind, if the doubting holder of an utterance goes out to make it public in the context of a publication or a conference, what she is passing on as science is not the doubtfulness but the utterance in the hope that it describes the world and can be believed. In each case, the person of the one who makes a putative scientific statement is linked to the 'truth' they utter. This takes place in the process of judgment involved in language even in scientific language.

No amount of articulation or formalization destroys the inarticulate element that accompanies the process of articulation. An account of scientific knowledge that skirts the inarticulate (or personal) element that governs the skill of knowing in science is a merely partial or incomplete representation of scientific knowledge. Such a partial account has so far been given by standard methodologies of science that put an emphasis on objectivity understood as detachedness at the expense of this very implicit, tacit or personal element in scientific and all forms of knowledge. Looking more closely at how this tacit element operates in language will provide the needed insight into the function of tacit knowledge in general.

2.1.1 The operational principles of language: Language-learning and language-use

What then are the operational principles of language that *enable* articulate expression to emerge from inarticulate efforts at expression? Language and articulation in general has got its roots in the inarticulate. How does the transition occur, for example in indicative forms of speech as used in the statement of fact, e.g. scientific fact? While an answer to these questions is being thought out or surmised, we need to remain open to the possibility that articulation itself in the sense of dealing with symbols chosen for expression seems, in its own right, to confer intellectual powers to the users of the language. This could help in explaining the intellectual superiority of humans over non-human animals. Taking the cue from Polanyi, the operational principles of language can on purely commonsensical grounds be classified into those that constrain the process of linguistic *representation*, i.e. seeking to say something about the world, and those that constrain the very *operation* of symbols to help in the process of thought. The illustration I suggest here is the one of mathematics in which we would talk of applied mathematics in the first instance (i.e. representation) and pure mathematics in the second instance (i.e. operation). We can arrive at the constraints needed in each of the two principles by stretching the advantages of each to its absurd limits.

2.1.2 Constraints on 'representation' in language

Language strives in some measure to communicate and thereby to represent[15], i.e. to declare something about the world (real or imagined as in mathematics) in the measure that this is possible. The more about the world an ordinary language can represent, the richer is the language said to be. Let us then say that we want to increase the wealth of a given language, L *indefinitely*. We could do so, as Polanyi suggests by representing each single sentence by a unique word that would act as a verb. There would be unique and unrepeated words to cover each sentence made about the world. Yet in this very process, the language would be destroyed. Not only would nobody be able to learn all the words, there being too many of them, but also the words would become meaningless. The meaning of a word is formed or agreed upon and manifested by its repeated usage of the word.

The structuralist school in the philosophy of language would object to this position and assert instead that the position of a given word in the overall structure of a given language is what gives a word its meaning, rather than the usage of the

[15] I choose not to go into the controversy on whether language is representational, because such controversy would lead us adrift from the simple point being made here. The details of the debate spearheaded by Donald Davidson and others do not leave us with a conclusive position. Polanyi avoids a similar controversy on the matter of language and what he says is valid for this controversy: "The present argument ... and its restriction to the representative function is not meant to endorse, e.g. a 'Representative' as against an 'Expressive' or 'Evocative' theory. I am engaged here not in constructing still another theory of the origin of language, but in an epistemological reflection on the relation of language to its inarticulate roots." (1958, fn.1, p. 77)

word. As indicated earlier, I steer clear of this debate in the philosophy of language and support Polanyi in making this claim on purely commonsensical grounds.

Surely, if for each sentence there was a new word, and if it is assumed that sentences declare something about the world, then for each new situation in the world there would be a new word. Our words would be repeated once or only too rarely to establish a definite meaning.[16] Polanyi concludes: "It follows that a language must be poor enough to allow the same words to be used a sufficient number of times. We may call this the Law of Poverty." (Polanyi, 1958, p. 78) Grammatical order and more or less fixed meanings then come to the rescue of a limited vocabulary to enable a language to report the vastness of experience. In other words, besides the need for the Law of Poverty, there is a need for a Law of Grammar according to which specific combinations of words would be permissible to convey a particular meaning.

Yet these two laws, of poverty and of grammar are not sufficient to provide the operational principles of language. Both of these refer only to words, and words are only words when they are both identifiably repeated and consistently used. (Polanyi, 1958, p. 79) Being identifiable distinguishes words from shapeless (or meaningless) utterances like groans. Consistent use distinguishes them from other repeatable utterances. It is in the combination between being repeated and being used consistently that meaning (or denotative power) comes to words and to language. Even then, the element of 'consistency' remains vague. For there to be consistency, there must be an identification of obviously different situations in relation to a given feature. Here again, there is a need for personal judgment

[16] Locke uses a similar argument to come to the usefulness of general terms and the uselessness of a name for every particular thing. (1690/1978, Book III Chapter 3, sections 2-4)

according to Polanyi. (Polanyi, 1958, p. 80) But the personal judgment called upon here is not as extreme as proposing a private language.[17]

On the contrary, what is meant by personal judgment here is the ability of a user of the language to draw from experience both of the language and of the context to come to a meaning of a word. There is some creativity involved, and a kind of heuristics on the part of the listeners or readers of the word in question. The personal judgments come about in e.g. choosing the relevant variations, choosing what real variations are as opposed to changes that completely destroy a feature as a persistent experience. We form frameworks or theories of the universe within which we hope to refer to reality in the future. Having figured out a meaning in a particular context, the user becomes more capable of recognising the same or a similar meaning in other contexts in the future.

What results is an interesting dynamics. The user of the language discovers how the language classifies objects and situations in the world adequately. She then remains satisfied that the language is right in the sense that it is used well. She accepts a so-called 'theory of the universe' or world view (given that each language makes its own description of the world) implied in the language as true. And by and large the art of speaking precisely, by applying the vocabulary of the language is comparable to the delicate discrimination as practiced by an expert taxonomist. Seen in this light, we have a vivid example in which a formal structure (in this case a language) is applied to experience. The element of *indeterminacy* and *judgment* comes out: the meaning of a word is expertly found out or applied by the expert user of the language. Judgment is playing a role. The process is inarticulate. Seen in

[17] Wittgenstein would object to the use of personal judgment to fix meaning in his argument on private language. (1953, paragraphs 243, 256ff)

this light, denotation is understood as an art. What we say about objects and situations in the world assumes our own endorsement of our own skills in practicing the art of denotation. (Polanyi, 1958, pp. 80-81)

2.1.3 Constraints on operation in language

Next we look at the operational principles that constrain the very operation of language, where language is understood as a set of symbols. As introduced above, the very manipulation of the language as a symbol may confer intellectual powers to the users of the language which powers help the user to declare something about the world. The symbols can be compared to a map in its relation to the world. Yet if (as in the case of representation above) we seek to perfect the virtues of the symbols in an exaggerated way, we come to the constraints on the operation of the same symbols.

We imagine with Polanyi that a given geographical map were so perfect in representing a part of the world, that its scale approached unity. Yet, if it were to attain unity, the 'map' would be useless for it would be as difficult to locate places on it as it would be to find one's way on the ground. Replacing this illustration back in the context of language understood as a symbol (or a map) for a language to be useful, linguistic symbols must be of reasonable size in comparison to the phenomena they symbolise. In other words, they must consist of manageable objects. The symbols that make language can assist thought only to the extent to which the symbols can be reproduced, stored up, transported, re-arranged, and thus more easily pondered, than the things which they denote. This is the Law of Manageability. (Polanyi, 1958, p. 81)

When a language is thus easily manipulable or manageable, it is helpful in enlarging the human intellectual capacities. Managing here consists in representing an experience in a way that with the help of the creativity of a user of the language reveals novel aspects of it. The representation may be by writing, printing, drawing or speech. The managing may follow prescribed formal rules or it could be totally spontaneous or informal. There are basically three stages involved: primary denotation; reorganization, and the reading of the result. Sometimes the reorganization takes place mentally.

Managing may involve various levels of ingenuity, including genius. The intellectual superiority of humans over animals (cf. Type C learning above) seems to be based in this capacity to represent experience in manageable symbols that can be reorganized to give new information. The whole seems to rest on the tacit powers of speech. Thus far, to speak knowledgeably or skilfully in a given language means having the capacity of contriving signs, observing their fitness and interpreting their alternative relations. (Polanyi, 1958, p. 82) All these are tacit capacities. Non-human animals happen not to combine all these capacities to come up with language even if they possibly possessed them. But as for humans, there is a whole range of articulation from the less articulate through speech or a text to the very formal and to the making of conceptual decisions.

2.1.4 The range of articulation

2.1.4.1 The ineffable domain – where the inarticulate dominates

One area of language in which articulation gradually differentiates itself from the inarticulate is the area of the relationship between thought, idea or hunch, and speech. Any articulation always leaves something unsaid about a matter we are addressing. Before we manage to express a thought in speech, the thought may reside in a domain we can refer to as the *ineffable domain.* In this domain, the tacit or inarticulate component predominates, making articulation impossible at the moment. We can indeed talk of ineffable *knowledge* without thereby sounding logically vacuous. To take an extreme example, how would I speak of or communicate knowledge of how to ride a bicycle in a way that is exhaustive?

Yet I cannot say that I do not know how to ride a bicycle. There is knowledge we possess, which we cannot articulate fully. Such knowledge is most eminent in the knowledge of skills. It is instrumental in the sense that we *use* it as an instrument to focus on some other area of knowledge. In knowing how to ride a bicycle, so many other subsidiary or instrumental areas of knowledge go into this knowledge. And should I focus on these other areas, I lose my focus of the main skill and may make mistakes in its performance. It is not possible to pass on the knowledge of a skill by merely passing on a text of the list of the subsidiary knowledge elements that contribute to the skill. This is because the list is inexhaustible, because the subsidiary knowledge itself relies on other subsidiary elements etc. Such knowledge is passed on through practical example and not by precepts.

An objection to this position may be that in finite time, a complete list of the subsidiary elements could be arranged and the knowledge passed on by precepts. But Polanyi gives an example of topographic anatomy in support of his position. In this branch of knowledge, all the particulars are explicitly specifiable in terms of detailed diagrams. And yet the skill lies in the personal insight that helps in integrating the parts – i.e. how the particulars interrelate. The detailed diagrams only offer clues for understanding a part of the body. Much is demanded of the intelligence of the student of topographic anatomy. She makes an effort to integrate the parts and understand how they work together. She makes up her mind by suggesting an *interpretative framework*.

Thus far, nothing distinguishes the student's efforts from those of an infant or a non-human animal: all are engaged in forming an *interpretative framework.* This is a step towards articulation, a process that moves from the inarticulate to speech relying on a capacity. And so it becomes clear from this example that articulation is appraised by the subject in order to come to precision. In humans, the articulation comes to the use of words in speech etc. But the words are not private on the one hand, while our capacity is not infallible on the other hand. We are either competent or incompetent to use them. (Polanyi, 1958, p. 91) We must rely on the use of our ineffable capacities at the risk of error, if we are to speak at all.

2.1.4.2 The domain of the co-extension of the articulate and the inarticulate

Another domain in which the articulate (or text) and the tacit are interrelated is one in which they are coextensive. It is possible to imagine that on receiving post in different languages, one may remember the message in a letter and yet not remember the language in which the letter was. Or again, it is imaginable that the context of a text is learnt faster than the words of the text. In these instances, there is tacit knowledge even where the source of the knowledge is verbal. There is instrumental knowledge of the language in the first instance, and instrumental knowledge of the words of the text in the second instance. For a word to have a meaning in a given text, it must have acquired the meaning before, through its use, a meaning that may be modified by the current use. At the same time, our understanding of the things signified by the word will have been acquired before, in experience. This is an example of the domain in which the tacit is co-extensive with the text of which it carries the meaning. Yet that is similar to what takes place in the way animals get to 'understand' their surroundings. It is through repeated experience of trial and error.

In this way, when I understand the message of a given word, I am focally aware of the message and subsidiarily aware of the text and of the many foregoing experiences that have been designated by the word - usages of the word and things that have given the word its meaning. When I focus on my knowledge of a given concept (i.e. when I know a concept focally), I am only instrumentally or subsidiarily aware of all the instances, experiences, words, meanings, etc. that go into building this concept.

There is a problem with this Polanyian position of co-extension between thought etc. and text. In the latter expression that relates all instances, experiences etc to the meaning of a text, there is an implicit suggestion that the more the experience the less the likelihood of erring in the use of a text. This strategy may serve Polanyi's goal well, of explicating the centrality of apprenticeship in passing on the knowledge of skills. But left as it is, this position promotes dogmatism. There are certainly instances in which less experienced users of the words and younger scientists have arrived at novel or original meanings and discoveries. The remedy to this weakness would be to qualify the position by attributing to the knower the choice of those experiences, meanings, instances that are actually relevant to support a given interpretation. Yet even with this correction, the skill of choosing the relevant experiences, instances, etc. is attributed to the knower and it is inarticulate or unspecifiable.

2.1.4.3 The domain where sophistication begins

A third domain in which the formal and the tacit interrelate in language is what Polanyi calls the domain of sophistication. In this domain, the tacit and the formal fall apart either because the speaker has not yet grasped the speech (i.e. is fumbling and requires a correction later by tacit understanding), or because the symbols have superseded our understanding and gone as far as anticipate novel modes of thought (i.e. the speaker is making some pioneering steps which are to be followed up later by tacit understanding). Sometimes there is a discrepancy between our tacit thoughts and our symbolic apparatus. We then face the choice, which of the two to rely on more and which to correct in view of the other. To give an example, children might have come to a way of solving their practical problems before they learn a language. But when they are learning the language, they feel encumbered rather than aided by the symbolic apparatus related to solving the same problems.

As we learn a new interpretative framework, there is always risk of error – error due to false interpretations of elaborate systems. New systems bring with them new kinds of errors. There are kinds of errors that animals are free from because they do not have this elaborate system of language. Together with the benefits that come with adopting a language or a system of formalization are the uncertainties and possibilities of error. The attempt by linguistic philosophy, to eliminate uncertainties in language by imposing precision on language, was bound to fail. The benefits of language come only when the language is allowed to function according to its own agreed operational principles with which the user must cooperate even at the risk of error.

In fact there have been times when the novel expressions in an established language have been declared vague, and when they have been fully appreciated, the novel expressions have helped to widen the scope of language. An example of this is the invention of irrational, negative, imaginary, and transfinite numbers in mathematics. They ended up depicting a mathematical reality and are now well established in the formal language of mathematics. And so, speculative, unconvenanted uses may open doors to fruitful uses of a language or a system of formalization. In line with Gödel (1931), Polanyi points out that within a deductive system like arithmetic, we can in some cases prove that a system is inconsistent. But we cannot prove that a system is consistent if it is sufficiently expressive. We end up committing ourselves to a risk of talking total nonsense if we are to say anything at all in the system. (Polanyi, 1958, p. 94) Ordinary language behaves in a comparable way. When we look at terms describing an experience, their descriptive function is conditioned on some recurrent nature of the experience they describe. In other words, we cannot *describe* an experience using totally unfamiliar terms and hope to communicate. We take recourse to the repertoire of existing terms that describe familiar experiences. And yet our terms remain open to a new view of the experience we describe.

Now a generalization over the recurrent features we seek to describe provides the rudiments of a theory or a world view. Grammatical rules help to amplify these theories or world views and we may end up with a system that anticipates much more knowledge than was surmised by the originators of the system. The myriads of arrangements of verbs, adjectives, adverbs, nouns, etc. cannot be wholly controlled in advance. But the verbal speculations that usher in novelty can either bring about new true knowledge or mere sophistry.

Faced with this dilemma in each case, we need to decide by using our judgment to adjust a given text to the conception it suggests and to the experience this may affect. At times we may need to adapt the use of language to include a new meaning, or we may decide to continue holding our meanings based on former experiences and instead adjust or correct the text, or else we may even dismiss the text as meaningless. All in all, language requires of us to constantly make judgments in the use of the formalism it suggests – either to maintain the formalism or to reconsider it basing on experience. Just as our knowledge always bears a tacit element that does not allow us to make explicit *all* what we know, so too with meaning, we are never fully aware of the *full* implications of what we say. (Polanyi, 1958, p. 95)

2.1.4.4 Formalization

As we enter into the realm of articulation and increased formalization, we find that even as this realm expands and increases our human mental capacities, it is itself a continuum that remains throughout linked to the inarticulate at one end. As examples, when data is transferred into a picture form, e.g. a graph, or onto a grid to form a map, etc., characteristics of the data which were at first not visible may be easily read off and even interpreted. Comparatively much more information about a situation that is being studied is gained through this transference of data or suitable symbolization. But in general, the mere manipulation of the symbols does not on its own produce any new information. Rather, it is effective because it assists the inarticulate mental powers used in reading off the results of the manipulations. And this applies even in the manipulation that uses mathematical computation. There is controlling intelligence – a tacit intelligence – throughout the use of mathematical symbolization.

In the natural sciences, the data that has been gathered and rendered in numerical denotation may be computed to yield new information. Such data can be greatly expanded into the logical apparatus of the exact sciences by throwing in a formula representing a law of nature. On their part, the descriptive natural sciences (zoology, botany, etc.) rely a lot on the systematic accumulation of recorded knowledge which knowledge is then looked at from different points of view.

Representing these 'findings' symbolically in print and multiplying and storing this print where it is easily accessible (i.e. managing in libraries, etc.) aids human mnemonic powers greatly. Thus accessible, these records are an aid to the speculative imagination of the inventors. This is how articulation (and formalization) can help enhance the intellectual powers of human knowers. By it, the essentials are presented on a reduced scale that is easier to manipulate creatively than the mass of data. (Polanyi, 1958, p. 85)

In the same way, the invention of appropriate symbols that can be manipulated according to specific rules and without reference to measured entities (experience) has enhanced human intellect. There are examples in logic and pure mathematics. In the pure mathematics, the invention of the decimal and of zero has made the whole group of symbols more powerful for measuring and counting. Likewise, formal logic has gained from the development of symbolic notation. The symbols help us reduce complex sentences to manageable symbols and also to make deductive inferences where they would be much more complex. The sequence, from one to the next, of descriptive sciences, exact sciences and deductive sciences shows increasing use of formalization and symbolic manoeuvring and decreasing contact with experience. There are more impersonal statements, more reversibility, and more precision.

Such tacit intelligence is used in understanding the situation, i.e. the problem involved, in choosing the appropriate symbolic representation and in the correct performance of the operations, and finally in the correct interpretation of the result. (Polanyi, 1958, p. 83) In this way it becomes clear that the capacity to denote furnishes humans with a huge range of mental powers on the one hand, but on the other hand, even with their powers of thought being enhanced by the use of symbols, humans still think within the frameworks of unformalized intelligence. Such unformalized intelligence may not be different from the intelligence of the non-human animals. Something comparable to this process takes place in ordinary language. The language that describes experience as fully as possible is often very imprecise and poetic. (Polanyi, 1958, pp. 86-87) Such language seems to allow room for the tacit coefficient or inarticulate judgment to fill in the gaps of imprecise and indeterminate speech. We participate personally in denoting, in saying something about experience.

A question about Polanyi's rendering of the relationship between language and experience arises. Does he not concede too much to the mere manipulation of language in such a way that we should be able to retrace our steps from the results of such manipulations to concrete experience? It seems as if Polanyi suggests that we can conjure up reality simply by conjuring up language. It is true that in pure mathematics interesting and novel results can be arrived at by manipulating the formulae etc. But how much of experience is retraceable from the results of such mathematical manipulations? I imagine that Polanyi can give a satisfactory response to this criticism. In the first place, it should be remembered that language is agreed upon to describe experience. It is not a fortuitous effort indulged in for its own sake. It is agreed upon for very pragmatic reasons – to describe and be able to communicate about experience. And so language properly used remains in touch with experience. But the interpretation of the various nuances of the same language reveals different sides of experience to different users of it at different times. The wealth of the language that is not seen by a given user at one time may be brought about by another user because of her different experience.

That is one way to explicate the novelty that arises with the manipulation of language. Another way to explicate such novelty, which does not rule out the first way, is by means of the tacit component. Because of the poverty of language, there are gaps in its description of experience. We have already discussed above to show that the poverty of a language is not necessarily a negative for the language. The gaps left by the poverty of language are filled in by the skilled user of the language, basing on her judgment and ingenuity. These two reside in the tacit component of intelligence. And so, the cases of more profound novelty that occur in a language can be accounted for using this tacit component. It is important to point out that the tacit component is not totally independent of skill in the use of the language, and neither is it independent of the experience of the skilled user of the language. The tacit component underlies both the experience and the use of the language, giving sense to both. It is in this light of the tacit component being active in both language and experience that conceptual decisions are to be understood

2.1.4.5 Conceptual Decisions

A further area in which the articulate differentiates itself from the inarticulate is in the domain of conceptual decision-making. The way the tacit faculty functions to increase our knowledge achieved by articulation is such that this faculty provides us with the power to understand both the text and the things to which the text refers within a conception which is the meaning of the text. Our senses are already imbued with the urge to pick up perceptions and make some kind of sense of them. In the same way, we are imbued with the urge to understand experience and the language referring to the experience. There is an impulse to shape our conceptions, moving away from obscurity to clarity, from incoherence to comprehension.

This impulse is due to a discomfort with things being unclear as is seen in our senses (e.g. when the eye is impelled to make clear what we see, even if that entails filling in or reshaping what it sees). In this way the paradox is enlightened: why articulation plays such a key role in our intellect even though the focus of our articulation is actually conceptual with language only playing a subsidiary role. For when language is used well, we end up with a conception which tells us both about how our language refers and about its contents (the things to which it refers). We only learn to speak by learning to know what is meant by speech. (Polanyi, 1958, p. 101) We are aware of language in every thought, even when we think of things and not of language. Likewise, we never understand language unless we understand the things to which we attend in thought.

As an example, the student of radiology starts out by seeing patches and webs in the X-ray picture of an infected chest. The language is technical and the things that the experts say it refers to seem to be merely imagined. Yet she gradually begins to *see* what is referred to and to *understand* what she sees, with more experience in seeing. Likewise for speech, there is a joint understanding of the words and the things. Once this knowledge has been acquired, it acquires a latent, inarticulate or tacit character. It is no longer tied to the words in which it was learnt nor to the natural language in which it was received. We can manifest this knowledge in an indefinite number of ways. For this flexibility of expression to be possible, this knowledge must continue to exist in an inarticulate or tacit way. The consideration of the range of articulation throws light on how the tacit capacity functions. And aided with this knowledge of how the tacit functions, we can arrive at a series of consequences, adjustments to the standard or received view on a number of epistemological areas as related to language.

2.1.5 Consequences

2.1.5.1 Consequence 1: Reinterpreting objectivity, rationality and reality

In contrast to animals, humans are able to combine the practical, observational and interpretative in the area of language setting the operational principles of language (as discussed above on treatment of speech, linguistic symbols, print etc.) in motion, and perhaps even making some new discoveries. Thus far, to be educated is to enter upon a vast wealth of knowledge which is being managed through access to verbal and other linguistic pointers. These help us to keep track of a vast amount of experience. We have a sense we are masters, and this sense is an inarticulate form of knowledge. Polanyi specifies: "Education is *latent* knowledge, of which we are aware subsidiarily in our sense of intellectual power based on this knowledge." (Polanyi, 1958, p. 103) In other words, our concepts are powerful in terms of being fecund. They are powerful in always remaining ever open to new aspects of things that we know. It is akin to the perceptive and the appetitive apparatuses, of always looking out, anticipating and being able to recognize something new. To be intelligent is to a great extent visible in the capacity to continually enrich and enliven one's conceptual frameworks. We open up to experiencing new conceptual frameworks, and we trust ourselves to be able to interpret them successfully.

Thus far, our thoughts can be deeper than we know, and they might disclose novelties to later generations in the way that Copernicus anticipated Kepler and Newton. This is a token of objectivity. Our conceptions can make sense beyond specifiable expectations in unprecedented situations. We entrust the guidance of our thoughts to our conceptions because we believe that our conceptions can only have been manifestly rational if they were in touch with aspects of domains of reality.

We give our conceptions (which are in fact our own creations) a sway over us in as much as we believe they are intimations of reality – we make contact with reality through them. They will enable us to see deeper meanings of reality in the future. Yet we are ready to modify and update these conceptions judging from our contact with reality. We have self-set standards, but we trust that we can subjectively recognize objective reality.

This view of objectivity, rationality and reality is bold and novel, and must face up to some scrutiny. To recap, what Polanyi seems to argue is that faced with reality in terms of the world we experience (i.e. as capable of revealing itself more deeply in the future), we try to make sense of it in the way that our senses make sense of perceptions. We create a language or kinds of formalisms to describe our experience of the world. We entrust ourselves to these languages and we continue to vet and adjust them in as far as they continue to describe our experience in ways that make sense to us. We are rational in remaining open to what our experience will reveal to us and what we may arrive at by paying closer attention to what our languages will reveal about the world. In the same way, objectivity is both an attitude of a knower (i.e. tacit) and an attribute of propositions held by the knower. It is an attitude in the sense that the knower remains open to what the experience of the world will reveal. It is an attribute of the propositions in that with time they are either corroborated, or modified or rejected depending on what the experience of the world reveals. The problem with this rendering of objectivity, rationality and reality is that it seems sanction relativism – reality is as I create it for myself. And if I find that my language, my methods, etc are working, then I am in touch with reality.

In a criticism to the whole notion of personal knowledge, Alan Musgrave raises the point of subjectivism/relativism that underlies this notion. A more systematic answer is given to this objection when I treat answers to criticisms. But already here a brief response is that what saves this Polanyian position from outright relativism are two elements. First of all, the search for knowledge right from the senses is aimed at making sense of the surroundings of the animal (human or non-human) it is a matter of survival for the animal, what sense it makes of its experience. There is a possibility of illusion even with the best of intentions, it is true. On the other hand, what the individual human animal has come up with as knowledge is stored and communicated in a common language agreed upon by many. Her propositions about her experience of the world are open to being tested by the experience of others immediately and in the future.

2.1.5.2 Consequence 2: Modification of language

Verbal clues play an important role in the way the educated mind acquires and develops knowledge. Whenever language is used to refer to experience in a changing world, language itself is changed and so is our conceptual framework. Language is being re-interpreted as it is being used. According to Polanyi, when we reformulate idiom we reformulate at the same time the frame of reference within which we are to understand our experience. (Polanyi, 1958, pp. 104-105) As it were, we reformulate ourselves. We deliberately *choose* new premises, not following strict rules of argument. We make a decision based on our personal judgment to modify our idiom and ourselves in order to arrive at a more satisfactory intellectual existence. We are satisfying ourselves, but not in an egocentric effort. Rather, self-satisfaction is here a token of that which should be universally satisfying. We have modified our intellectual identity because we hope to draw closer to our experience of reality. We may end up being wrong, because the effort is conjectural in nature, but that does not make our efforts mere guesswork. "The capacity of making discoveries is not a kind of gambler's luck. It depends on natural ability, fostered by training and guided by intellectual effort." (Polanyi, 1958, p. 106)

An obvious question to this position would be: What then motivates our desire to modify language? Is it our encounter with our environment? Is it our minds detached from experience? Is it a conflict that we experience between these two? Polanyi would respond in the line that like artistic achievement, this effort though unspecifiable, is not arbitrary or accidental. In modifying a language, we are involved in a tacit activity, a heuristic feat, driven on by a desire to come into closer contact with what is true and right.

The modification or re-interpretation of language takes place at three levels: The first level is the *receptive level.* This is when e.g. a child, is learning to speak a language. At this level, the child's language may appear foolish or conjectural, yet the conjectural character of language here remains with us in the use of language throughout. We doubt and fumble when we come across unusual words. We may associate meanings of two or more words because the words sound similar, etc. Scholars may conjecture at the precise meanings of technical terms (e.g. *arête*, etc.)

The second level is the *innovative level* where poets and scientists propose new uses and even teach others to apply them. This is the case e.g. in the sciences where confusion in the use of a term and the understanding of experience may persist for years before an intervention is made by a scientist in a way that clarifies terms and introduces a new framework of reference that may aid the understanding of experience. An example that Polanyi gives is the confusion that reigns upon John Dalton's general introduction of atomic theory, before Stanislao Cannizzaro proposes a precise distinction between three closely related conceptions of atomic weight, molecular weight, and equivalent weight. Here it is clear that what guides the betterment of language or of conceptual discovery is the better *understanding* of experience. Again, there is a decision based on judgment – a heuristic act – when choosing to clarify or dismiss a position.

The third level of modification is what Polanyi calls the *intermediate level*, where language is modified imperceptibly without any conscious effort at innovation. This kind of re-interpretation of language takes place continuously on a daily basis. The world keeps changing and our anticipatory powers have to adapt to suit the unprecedented situations.

An example of imperceptible modification of language in science that Polanyi (Polanyi, 1958, p. 111) gives concerns the use of the word 'isotope'. When heavy hydrogen was discovered by Harold C. Urey in 1932, he chose to call it an isotope of hydrogen. Protests from Frederic Soddy in 1934, to the effect that the word was being wrongly applied, since isotopes were not meant to be chemically distinguishable as heavy hydrogen was distinguishable from hydrogen, were ignored. The word 'isotope' gained a new meaning that way, i.e. a linguistic reform. But at the same time, there was a new form of chemical separability launched, between two elements with the same nuclear charge.

When we encounter new experiences which we can identify as variations of our earlier familiar experiences, we adapt both our conceptions and our use of language in order to give meaning to the novelty. The groping for meaning while adjusting both conceptions and language is done subsidiarily, in the same way that we try to find clear and coherent perceptions without knowing focally how we carry this out. Our speech changes meaning without our being focally aware of it. While we grope for meaning, words are invested with a wealth of unspecifiable connotations.

2.1.5.3 Consequence 3: The contextual or cultural truth of language

Polanyi makes a link between 'truth' and 'rationality'. When a given culture uses a language confidently, they are affirming the interpretative framework of foregoing generations in the same culture. The current use of the words can be relied on as meaningful and true because down the generations, the meanings that these words have gained through the gropings about in the dark by the culture have been reliable. According to him, a child learns a language by accepting the interpretative frameworks of the culture in which it grows up. And thereafter, every effort made by the educated mind (i.e. skilful user of the language) is made in this framework. If this interpretative framework were wholly false, then the rationality of the said educated mind would be wholly lost. The said educated mind "… is rational only to the extent to which the conceptions to which [it] is committed are true." (Polanyi, 1958 p. 112)

Now the link made between rationality and truth is interesting. It is interesting because it leaves room for error – it is fallibilist. The scientist (for example) can still change her mind and admit to the falsity of her conceptions. Yet the same understanding of commitment to the rationality of a whole culture of truth and rationality helps to explain why the scientist may be committed to a particular interpretative framework. The reason why a given interpretative framework seems to survive could be (in the mind of the scientist) that the framework has latched onto a truth about the world – a reliable way of describing the experience of the culture within which the scientist is working.

This link between rationality and truth is nevertheless problematic. The problem is that this view can allow for a relativist conception of truth. Concepts are true, if they are rational and vice versa. But the rationality of an interpretative framework in this view is based on its having persisted over a long period. This view cannot explain why some interpretative frameworks survive over long periods before they are rejected as false or incomplete. An example of such a persistent view was the Ptolemaic view of the universe. And so it is not enough that a given interpretative framework corroborates the experience of a given culture.

This objection picks out a real weakness in the Polanyian view of rationality and truth. But I will postpone the defence of it to the next two parts that treat intellectual passion and intellectual culture. For now it is enough to point out that Polanyi's view remains open to an external criterion. The positions that result from a given interpretative framework are open to the scrutiny of later discoveries. These later discoveries and modifications can reject a view if it is false or modify it if there is an element of truth in it, i.e. if it reveals more about our experience of the world than was originally supposed.

2.1.5.4 Consequence 4: A personal approach to how words 'refer'

Polanyi proposes an interesting understanding of the use of words to refer to objects in the world. (Polanyi, 1958, pp. 113ff) In a given language, the choice of words is done carefully, and not merely as a matter of convenience. They are meant to express a matter of truth and error, of right and wrong. The use of words goes further than mere language games and further than nominalism (which he seems to understand as a treatment of words detached from the reality). Polanyi seems to opt for a more robust metaphysics in which words are bound to reality depending on the fitness of the user of the language in which the words occur to *judge* and to vet the use of the language.

Thus far, words do not merely mean anything. They have particular meanings that will be decided upon or vetted by the expert in the language. The expert user of the language stands between the words and the reality they are used to refer to. Experience of the reality often influences the use of words, but there may be instances in which words revealed something more about the experience they are meant to refer to than originally known. The link there is between words and reality (or our experience of reality) is borne out when there is a disagreement about the nature of objects in the world. A mere analysis of the *words* would not resolve the problem. Rather, a resolution is to be sought when words are understood as really referring to the world, to the things about which people disagree. As the expert uses the language, our human capacity to recognize real entities and to designate them to form a rational vocabulary is acknowledged and confirmed.

The process of logical intensionality by which we recognize, identify and classify objects in the world and experiences takes place at three levels, according to Polanyi. First of all there are the readily specifiable properties of objects that make classification of objects with similar properties possible. Second are known but not readily specifiable properties. Words are designated to describe such properties even though the properties are only subsidiarily known. The chosen words then bear subsidiarily known connotations that may be rendered explicit on deeper reflection on the words and by a study of the use of the words in various times and contexts. These come to be known through investigation, e.g. Socratic enquiry as applied to such terms as ‘justice’, ‘truth’, etc. Definitions are then arrived at.

Philosophy comes in handy at this point in arriving at true and new discoveries by an analytical investigation and definition. But even then, the reality being investigated and defined guides the process of definition. The way we arrive at the proper use of the word then is by focusing, not on the word but on the reality to which it refers. We thus uncover more meanings of the word. A third level of intensionality is one in which we designate a word and are not yet aware of all the indeterminate meanings it might convey, but we anticipate that it will reveal much more than what we mean right now in a way that confirms the truth of the term. *The reason why we believe that the term can reveal itself in more and unexpected ways is that we believe in turn that it is true or in contact with reality.* (Polanyi, 1958, p. 116) This is the way in which formal speculation upon the words chosen and expertly used could raise new problems and lead to new discoveries.

Two related objections can be raised against this position – one is about the metaphysical assumptions involved and the other on the level of epistemology. The objection about the metaphysical assumptions involved is itself twofold. In the first place, Polanyi's position seems to have a background in naive realism. He assumes that the world out there exists as we sense it, and that is why our words and our language can refer to it to some reliable degree of accuracy. The objection then is that Polanyi does not support his realism with convincing arguments. Rather, he assumes that realism is an accepted metaphysical outlook. But the debate about realism has been raging for long, and the objections raised by the anti-realists (of all kinds) could be applied to Polanyi's position. But so would the pro-realist arguments be raised in answer to the objections by the anti-realists.

This objection of Polanyi not declaring himself explicitly on the realism debate would be an easy objection to respond to. In the first place, Polanyi could have been aware of the realism/anti-realism debate and still chosen to ignore it because it was no longer a fecund debate. On the other hand, Polanyi can be defended by pointing out that his views on realism are not central to his position here. He can maintain his position without committing himself to realism as it is currently construed. One way to do this is by referring to our experience of the world rather than to objects in the world. And so the words and the language referred to in this section could be understood as referring to our experience of the world in Kantian terms – a phenomenal description – rather than a noumenal description which would be aimed at the reality (or non-reality) of objects out there in the world.

A more serious objection about the metaphysical assumptions that Polanyi makes on this point is that he seems to assign some kind of metaphysical link between words and objects in the world to such an extent that words and objects are causally linked. Seen in this light, words cause objects, and objects cause the words that the expert user of a language chooses to describe them. There seems to be a list of the uniquely *correct* words to use to describe a given set of objects. This objection is justified because Polanyi points out that in the description of an experience of the world, words are chosen for being correct and for describing reality in a truthful way. The objection is all the more difficult to deal with, especially if Polanyi claims that a formal analysis (e.g. of a language) could lead to discoveries about objects in the world. In order to answer to this objection, Polanyi needs to emphasize that his analysis does not take place at a meta-level of language. He is arguing within a given interpretative framework of a given natural language. This framework has been handed on in history, tested for its adequacy, and its rules and assumptions are considered as clear and agreed upon by all expert users.

Within such a context, the use of the words can be seen as appropriate or not, correct or incorrect by the expert users of the language. There is a certain amount of flexibility within which the judgment of the expert user is free to apply a given word to a given specific context. This is because the expert user bases herself on subsidiary knowledge of the object or experience that needs to be described. This room that allows for flexibility and the judgment used by the expert user combine to allow for various connotations of the word depending on the exact circumstances within which the word is applied. And so the implied connotations may come out at a later period as the word continues to be used to describe an object or an experience. In this way, words do not cause objects or experiences. Rather with expert use of words, the connotations that were not expressed before could come out at a later stage.

The emphasis placed on expert use is the basis of a second possible objection – the objection on an epistemological level. Polanyi places such emphasis on the *expertise* in the use of words in a language that in the end he seems to be promoting a kind of elitism (Lakatos, 1978). The experts must always be there to judge and vet the novel uses of a word in a language. He seems to assign special and privileged knowledge to the expert users of the language. This is a justified objection, but again the position of Polanyi can be defended against it. It must be pointed out, first of all, that the experts do not 'know' all the connotations of the word in a given language beforehand. Rather, they work out the suitability of novel connotations by and large, relying on other experiences of adjusting the meaning of the word in various circumstances and focusing on the object or the experience that the word is meant to describe.

Further, the experts do not form an exclusive club. Any user of the language can accede to its expert use simply by adopting the interpretative framework handed down within the language and by exercising the use of the language with the guidance of the already existing experts – i.e. by apprenticeship. In the end Polanyi has proposed a personal as opposed to an objectivist theory of how we use language to refer to objects and experiences in the world. His theory is consistent with his overall tacit or personal (as opposed to objectivist) theory of knowledge. Overall, therefore, we are self-reliant in probing into reality, setting our own standards for correctness and truth, and believing that we shall have the capacity to recognize our efforts as leading us to know reality as it really reveals itself in deeper ways in the future.

2.2 The role of the tacit in deductive processes and heuristics

2.2.1 The role of the tacit in deductive processes

2.2.1.1 Mathematical Proofs

A deductive process, e.g. a proof or the solution of a mathematics problem, can be compared to using a map. We become subsidiarily familiar with the map and by it we go about the landmarks around us. Using the map, we form a conception which we can reorganize to find out the particular itineraries we are really interested in. Likewise in a deductive process, we are involved in a conceptual decision based on what we know already but one that furnishes us with a new interest. The conceptual decision is itself informal even though it is based on articulation i.e. the manipulation of the formal language of mathematics etc. There might be mental effort required, and indeed a problem is solved in the end. This, according to Polanyi (Polanyi, 1958, pp. 117-118), is a deductive transformation in which one set of symbols is transformed into another set. The second set of symbols is implied in the first set. The transformation is akin to a denotation in which, e.g. a descriptive word evokes the reality that it describes. Likened to a denotation, the deductive process can be seen as conveying both our understanding of the formal manipulations (of the relevant language), and our acceptance of the process as right.

Thus far, there is much of the tacit and of the person involved in being convinced even by a mathematical proof. No one would be convinced by a mathematical proof that they do not understand. The minimum required to understand a mathematical proof is to understand the logical sequence involved as a *purposeful* procedure. Poincaré describes this required understanding as that (something) which constitutes the unity of demonstration. (1935, p. 26, pp. 20-34) He argues for this 'something' in the context of arguing for the role of intuition in mathematics and creative or inventive science besides pure logic. When that 'something' is grasped, even when the details of the steps of a proof are forgotten, the mathematician can still be said to *understand* a proof. And so, the actual *meaning* of a formalism is to be found in a subsidiary awareness of the formalism itself and a conceptual focus on the whole or the goal that is sustained by this formalism. That same *meaning* is necessarily absent where focus is shifted to an impersonal treatment of the symbols of the formalism. (Polanyi, 1958, p. 119)

Likewise when mathematical formalism is over emphasized with a goal of reducing ambiguity as much as possible, general clarity, and with it intelligibility suffers. Yet proofs are in fact ingenious contrivances, purposive actions aiming to establish an implication and to compel acceptance of it. In this way, proofs do not just denote their subject matter. They bring it about. Proofs contrive what they eventually convey. The tacit operations at work in mathematical proofs (and in deductive processes in general) are similar to those at work in perception and in the learning and application of a language.

2.2.1.2 Problem-solving

All animals are possessed of an awareness, an 'awakeness', i.e. a readiness to perceive and to act in order to make intelligent and practical sense of their surroundings and to adapt. What happens in problem-solving is that a hidden aspect of a situation is appraised. To this aspect, the animal assumes to have a solution. The solution is provided through the clues to which the animal has access through perception. *Already at this stage of appraising a problem, something is being added to knowledge*. An objection to the use of the word 'knowledge' is in place at this point. This must be a very qualified use of the term, for Polanyi admits that in choosing a problem, the investigator makes a decision that is fraught with risk.

But the use of the word 'knowledge' here must be detached from the objectivist approach that seeks to define knowledge entirely on the grounds of propositional knowledge. A fuller definition of knowledge must be able to account for inarticulate knowledge, e.g. skilful knowledge. Being able to single out a problem involves a skill. In this sense of knowledge that permits of inarticulate knowledge, the appraisal of a problem, especially if the appraised problem culminates in a solution is knowledge.

And on this point of finding a solution to an appraised problem, an objection may be raised. There is a possible situation in which an investigator singles out a problem that is insoluble or too difficult to solve. How useful is an appraisal of an insoluble problem? In the day to day functioning of science, the balance must be struck between playing safe to the extent of stunting research and problems that can be solved within finite time and at not too great a cost (in terms of talent, labour and money). In the end, the process of choosing a problem is an assessment of the very skill of the investigator and her collaborators. It is not guaranteed that their intuition about a soluble problem is right, and yet whenever it succeeds, it is a confirmation of the investigator's skill and the dependability of the process of appraising problems. As Polanyi points out: "To form such estimates of the approximate feasibility of yet unknown prospective procedures, leading to unknown prospective results, is the day-to-day responsibility of anyone undertaking independent scientific or technical research." (Polanyi, 1958, p. 124) The whole is a tacit process comparable to perception and to the learning and application of a language.

Thus far, to see a problem is to add to knowledge and to find a problem that can be solved is a discovery. Now a distinction should be made between discovery which comes as a result of accident and discovery which comes as a result of intelligence. Both manners of discovery are part of heuristics. What is philosophically interesting and what concurs with Polanyi's treatment of tacit knowledge, is not the first kind of heuristics. In that kind of heuristics, intelligence plays a negligible role. Polanyi deals rather with heuristics or discovery in which the discoverer is engaged in an intellectual effort. Borrowing from Poincaré again, Polanyi considers that the second type of discovery follows a series of identifiable stages after a solvable problem has been discovered.

Briefly, these stages are: 1)*Preparation* – in which the discoverer takes time to think about the problem and take in the whole context with the clues that might lead to its solution; 2)*Incubation* – during which there is a heuristic tension within the mind, or at the back of the mind of the discoverer as she struggles to find a solution even as she continues with other tasks; 3)*Illumination* – in which the discoverer suddenly comes to a tentative solution in her mind; and 4)*Verification* – in which the discoverer tests the tentative solution in practical life.

I find the four stages uncontroversial for the purposes of explicating the nature and functions of tacit knowledge, and I avoid going into the discussion of finding out whether they are necessary and sufficient. What is of interest as far as tacit knowledge is concerned, however, is what Polanyi underlines as the personal or emotional involvement of the discoverer, in the moment of pensive preoccupation before the tentative solution is arrived at, and the joy at the discovery of a solution.

Without such a personal or emotional engagement, the problem is really not a problem to the discoverer. It is either too easy and can be solved effortlessly, or it is too difficult and thus does not puzzle her. A problem is not a problem in itself, except in relation to identifiable persons. And once the problem has been solved at least once, the discoverer can get back to the solution of similar problems in a routine manner without heuristic tension and thus without discovery. That explains the irreversibility of heuristic progress – we cannot restore the heuristic tension for each of the similar problems.

And given the possible fact[18] that deductively logical conclusions can be traced back to a finite possibility of their premises, this understanding of heuristics precludes them from being strictly logical. Instead, where discoveries are to be made, there is a 'logical gap' in the sense of deductive logic. And crossing that gap is the function of originality, i.e. ways of solving a problem without using intelligent conclusions from already existing knowledge. In this light, originality is a natural talent that alone enables the discoverer to start an important innovation and genius

[18] I call it a possible fact basing on my argument for it in dealing with learning and reversibility.

singles out problems and proposes solutions in ways beyond general anticipation. Genius can then be understood as extensive originality, a token of the originality in biological life.

2.2.1.3 Mathematical heuristics – an illustration of tacit knowledge in heuristics

The function of heuristics in mathematics can be used as an illustration of how heuristics work in discoveries in natural science and technology. The question here is: how do anticipatory powers lead to discovery? Mainline philosophy of science has concentrated on defining and justifying the process of empirical induction with little attention being paid to heuristics. Some have consigned heuristics to psychology (later Lakatos, etc.). When we are open to the tacit aspect of knowledge, heuristics become important.

Now, practice plays a major role, even in a formalized branch of knowledge like mathematics. In other words, acquiring an art is important for this formal discipline. What goes on in the practice is the transformation of a language thus far passively acquired into an effective tool for solving new problems. Now, to solve a mathematical problem, as discussed in the last section above, is to use clues to arrive at something totally new to us. Moving towards a solution involves both deliberate and passive moments. Some things, like happening on a solution, may just happen to the investigator in the moment of incubation, which is akin to one of the four major stages in the solution of a problem pointed out above. In that sense, happening on a solution is a comparatively passive moment in contrast to verification. The verification of the solution (another stage in the solution of a problem pointed out above) is a deliberate act.

Now, in the case of students of mathematics, it matters that the problems set for them are believed to have answers, even though the students might never have come across the answers before. There is an expectation of how the answer will eventually present itself or look like. The answer to a mathematical problem is similar to the satisfaction of a desire – the problem. Inventiveness is geared up as the students get engrossed in satisfying the desire. Their thoughts are then reorganized continuously in the search of suggestive aspects. They focus on something they do not know, yet one about which they have formed a notion. They also focus on the data that they already have, not in themselves, but as clues towards the unknown. They try to see how the data all fit together, convinced that they actually do belong together to form a whole. Memory may help in the comparison of a particular problem with earlier solved problems. The students rely on their capacity to recognize a yet unrevealed relationship between the clues and the solution. And as they get nearer to the solution, they need to know that they are getting nearer, otherwise their efforts will be characterised by random conjectures and remain fruitless.

All these are tacit capacities. The students have the capacity to sense a hidden inference from given premises and the capacity to keep tweaking the premises until they strike in the direction of the inference. The probability of hitting the right answer grows above zero. The mind may continue to work in this manner, narrowing the logical gap to the solution, and reducing the effort needed to finally arrive at the solution. The students finally come to the solution and it comes stamped already with its own accreditation of being true, given that the students were searching something about which they had formed a notion. Even the most outstanding originality is perceived by the investigator as more of a revelation of what has always been there than an initiation of something completely new. Such investigators are usually rewarded when their discovery reveals more aspects than formerly foreseen. This is a clue that the investigators may be in touch with reality understood as that which reveals itself in unforeseen ways.

2.2.2 The heuristic recognition of value, elegance and beauty

2.2.2.1 Scientific value

Values are considered as outside the ambit of scientific knowledge in an objectivist approach. The objectivist approach can itself be traced back to a position of Laplace who holds in the introduction to his work on probability that:

> "We should therefore look at the present state of the universe as the effect of its anterior state and as the cause of its subsequent state. An intelligence which for a given instant would know all the forces that are active in nature and the respective situation of the beings composing nature, and if moreover this intelligence were vast enough to subject this information to analysis, such an intelligence would embrace in the same formula the movements of the biggest bodies of the universe and those of the lightest atom: nothing would be uncertain for it, and the future, like the past would be present to its eyes. The human mind offers a weak sketch of this intelligence, in the perfection that it has attained in astronomy. Its discoveries in mechanics and geometry, combined with the discovery of universal gravitation, have brought this intelligence at the beginning of understanding in the same analytical expressions the past and future states of the world system. By applying the same method to a few other objects of knowledge, the intelligence could relate observed phenomena to general laws and could foresee those that given circumstances must bring about."[19] (Laplace, 1886, pp. vi-vii)

[19] A translation of « Nous devons donc envisager l'état présent de l'univers comme l'effet de son état antérieur et comme la cause de celui qui va suivre. Une intelligence qui, pour un instant donné, connaitrait toutes les forces dont la nature est animée et la situation respective des êtres qui la composent, si d'ailleurs elle était assez vaste pour soumettre ces données à l'Analyse, embrasserait dans la même formule les mouvements des plus grands corps de l'univers et ceux du plus léger atome : rien ne serait incertain pour elle, et l'avenir, comme le passe, serait présent à ses yeux. L'esprit humain offre, dans la perfection qu'il a su donner à l'Astronomie, une faible esquisse de cette intelligence. Ses découvertes en Mécanique et en Géométrie, jointes a celles de la pesanteur universelle, l'ont mis a portée de comprendre dans les mêmes expressions analytiques les états passes et futurs du Système du monde. En appliquant la même méthode à quelques autres objets de ses connaissances, il est parvenu à ramener à des lois générales les phénomènes observés et à prévoir ceux que des circonstances données doivent faire éclore. » (Laplace, 1886, pp. vi-vii)

This Laplacean hope is interpreted by Polanyi as an attempt to pursue science with the ideal of absolute detachment. The world is represented in terms of exactly determined particulars and a universal mechanics governs the motions in the universe. All that is needed in such a view of science are the impersonal facts, the *data*. But this is a partial representation of science, for science is a human activity and values have got a role to play. Polanyi holds that an affirmation is deemed *valuable* for science, i.e. acceptable as part of science, on the foundation of three points. 1) The affirmation must possess certainty/accuracy; 2) It must have systematic relevance/profundity; and 3) It must have intrinsic interest. (Polanyi, 1958, p. 136) The three criteria apply jointly, and they compensate one another, i.e. excellence in the other two makes up for the weakness in one of the areas.

It is clear in this outline already that scientific value is one of those things that even though they are not precisely definable, and are therefore inarticulate, it can be reliably assessed. Each contribution in a journal of science is judged on its scientific interest. Research grants are given where referees judge that the research is worthwhile. What is judged trivial or false is denied publication or the funds. Evidence that appears incompatible with an already established system of knowledge is usually initially ignored by the body of scientists in the hope that it will soon be proved to be irrelevant or false. At times useful evidence is thus ignored, but it is difficult to see how else science would survive being caught up in chasing blind alleys. The rules for avoiding this complication are tacit or inarticulate because scientific value is itself tacit or inarticulate. The function of scientific value is only vaguely describable. The way that science seeks value is similar to human perception in the sense that it is done skilfully and deliberately. Polanyi maintains that:

> "Just as the eye sees details that are not there if they fit in with the sense of the picture, or overlooks them if they make no sense, so also very little inherent certainty will suffice to secure the highest scientific value to an alleged fact, if only it fits in with a great scientific generalization, while the most stubborn facts will be set aside if there is no place for them in the established framework of science." (Polanyi, 1958, p. 138)

Some of this approach has already been presented in the treatment of the Michelson-Morley experiments earlier. It should be pointed out that the impersonal element in scientific knowledge is not being denied. But an excessive emphasis of this impersonal knowledge would leave us with a partial and therefore inaccurate representation of knowledge. Values do have a role to play. But the claim for the role of values in science needs more elaboration. What guides value towards scientific truth? Polanyi suggests an emotional element. Great scientific theories operate like works of art – there is an emotional force in both of them. This emotional force can be evoked in an articulate culture. When a scientist comes across a great scientific theory, this experience is accompanied by an expression of delight. For Polanyi, the theory "... has an inarticulate component acclaiming its beauty, and this is essential to the belief that the theory is true." (Polanyi, 1958, p. 133)

We have seen earlier that the treatment of language (e.g. the formal aspects that form pure mathematics) expands the range of our thought, so does it expand the range of our system of emotional responses by which we can appreciate scientific value and ingenuity. The beauty of a theory calls attention to itself and by the beauty, the theory makes a claim on empirical reality in the same way that a work of art by its artistic beauty makes a claim to artistic reality. Science appreciates its own beauty through a kind of passion: *intellectual passion.*

Yet the analogy that Polanyi draws between artistic beauty and the reality to which scientific theories relate is not without problems. It is for example possible to think of the reality to which scientific theories relate as one that reveals itself gradually and cumulatively, governing and directing the scientific theories. Artistic reality, on the other hand, is difficult to perceive as being cumulative towards a greater clarification. The various masterpieces of art are difficult to unify and coordinate or perceive as contributing to one wider reality – an artistic reality that would correspond with the scientific reality. A further problem with this comparison between art and science is that while scientific reality can plausibly make 'truth claims', it is difficult to think of artistic 'truth claims', or even to conceive of what nature such claims may have.

A response to both objections would be to assert that what Polanyi is engaged in is the *mechanism* by which scientists come to hold on to and assert scientific theories. The *contents* of the theories have to be tested against reality. But what has been argued all along is that the *mechanism* is an important part of the contents. An attempt to depict scientific knowledge as absolutely detached and impersonal leaves us with an incomplete account of how science works.

A complete account of science should be able to explain the passionate origins of science. Scientific passions are not just a psychological by-product of science and shying away from them is not helpful to explicating how science works. Rather, the passions perform a logical function because they respond to an important element in a scientific claim and the claim may be said by the scientist to be right or wrong depending on the presence or absence of that element. (Polanyi, 1958, p. 134) But Polanyi must have a unique and specialised sense of emotions or passions, if his position is not to be obviously controversial. As a descriptive definition, intellectual passions are appreciative expressions about theories, discoveries, etc. which theories etc. are considered as having intellectual value that is precious to science. It is not about every whim and caprice, therefore. He is aware of the controversial status of his claim and so he goes on to explain:

> "The function which I attribute here to scientific passion is that of distinguishing between demonstrable facts which are of scientific interest, and those which are not. Only a tiny fraction of all knowable facts are of interest to scientists, and scientific passion serves also as a guide in the assessment of what is of higher and what of lesser interest; what is great in science, and what is relatively slight. I want to show that this appreciation depends ultimately on a sense of intellectual beauty; that it is an emotional response which can never be dispassionately defined, any more than we can dispassionately define the beauty of a work of art or the excellence of a noble action." (Polanyi, 1958, p. 135)

Thus far, according to Polanyi, we possess a vision of reality which accompanies scientific knowledge. This vision is not really knowledge, for it is a guess. But at the same time, the same vision is in fact more than knowledge because it relates to things not yet known and presently inconceivable, but which things may later reveal themselves as true in experience. To Polanyi, it is by this vision of reality that we are able to weather the storm of crises when a given scientific position is shaken by contradicting evidence. By the same vision we are able to ignore apparently founded positions in the hope that further evidence will help disprove the positions.

This seems to me to be the furthest we can go in explaining the way emotions are involved in the appreciation of scientific theories without thereby succumbing to an irrational explanation. It is important to first of all get rid of objectivism or the view of science as a purely detached and impersonal engagement. The alternative left, if this role of emotions is not acknowledged, does not leave an account of science more rationally robust in the objectivist sense. It is my view that the attempts at a 'logic of science' or any similar account of science as seen in standard methodologies can be improved by improving the way they handle this emotional element in science. Thus far, falsificationism could better explain why scientists do not immediately abandon a bold conjecture once it has been falsified in a crucial experiment. Also, it can be seen why there is a quasi-religious conversion on the part of scientists who abandon one paradigm for a new one. (Kuhn, 1962) The same can be said about what motivates the members of a given methodological research programme to shift to new programme once it is detected that the older programme can no longer generate new answers to new problems. (Lakatos, 1983)

Polanyi asserts further that we need and do have a general vision which acts as a general guide for the interpretation of experience. This vision is in turn guided by intellectual passions otherwise it would spread out in all directions, in pursuit of various trivialities. The intellectual passions propose a number of things: what is scientifically valuable, beautiful, and worthwhile to pursue; the kind of conceptions and empirical relations that are plausible; the empirical relations to reject as specious even in spite of evidence that supports them, etc. (Polanyi, 1958, p. 135) In the meantime, our vision relies on the evidence of our senses.

Obviously, the way in which beauty or elegance is being used about scientific theories as perceived by a scientist needs to be explained further. Polanyi is aware of a major objection to his claim pointed out above in the treatment of value, that the intellectual beauty of a theory is a token of its being in touch with reality. He himself points out that there have been discoveries made from purely manipulating the formal symbols of earlier theories. Examples are when Adams and

Leverrier calculated the position of another planet – Neptune by the use of Newtonian mechanics. Likewise, van't Hoff arrived at the laws of chemical equilibrium by manipulating the Second Law of Thermodynamics. It is therefore plausible, as Polanyi admits in reference to the position of Mach, that the true value of a theory is in its being an economical account of observed facts.

Polanyi points out that a similar debate had raged in the time immediately after Copernicus, between the Copernicans (Kepler, Giordano Bruno, Galileo, etc.) and those who held on to the Aristotelian world view (Osiander, theologians, etc.). For the latter, the Copernican system was not true; it was merely an economical hypothesis. (Polanyi, 1958, p. 146) According to Polanyi, both sides agreed on the meaning of 'true', i.e. contact with reality, one that is "destined to reveal itself further by an indefinite range of yet unforeseen consequences." (Polanyi, 1958, p. 147) 'Fruitfulness' could be used to designate this idea of 'truth', yet it falls short.

In the example of the dawn of the Copernican system, the Ptolemaic system had been a fruitful source for a thousand years. But it was a source of error. The Copernican system was a source of *truth* by contrast. Still as Polanyi notices, a false theory can be a fruitful source of truth, at least by accident. But the difference with the Copernican system is that it did not accidentally lead to Kepler's and Newton's discoveries. Rather, it led to them because it was true. Polanyi has a unique view of truth of scientific theories – "the indeterminate veridical quality …" of a theory. (Polanyi, 1958, p. 147) The Copernicans saw this quality in the Copernican system long before Newton published his *Principia.* They saw the veridical quality not subsequent to the fruitfulness of the system. Rather, they *intimated* this fruitfulness.

And now, a further objection that Polanyi foresees is that still the formal reformulations of a theory may help lead to new discoveries. This seems to have been the case with the reformulation of Newtonian mechanics by d'Alembert, Maupertuis, Lagrange, and Hamilton. But Polanyi points out that the discovery character of these contributions is in as far as they made huge strides in mathematics. They are in fact formally elegant and do not bear on intellectual beauty, which beauty makes contact with reality.

Again Polanyi foresees two important objections to his approach so far. One could wonder what use experiments are in such a framework. Secondly, what distinguishes science from astrology in this same framework? Polanyi's answer to the first question is that experience is necessary as a clue to understanding nature, but it does not determine that understanding. The understanding is a kind of groping around in the dark or unknown, with the meaning of the *facts*. The experience dispels the notion of mere formal elegance or formal advance, and reaches further to a new insight in the nature of reality. The indeterminacy persists, we are thinking on our feet. We do not yet know what we mean, or whether we mean anything. And when at last we decide to take up the theory as valuable to science, the indeterminacy is merely limited, not eliminated. Yet this indeterminacy must remain if our newly adopted theory is to have a bearing on reality – only in its being indeterminate can it reveal itself ever more deeply in the future. The whole process can be compared to the learning and the use of a language discussed earlier.

On this point of beauty and elegance, Polanyi's position is not distinguishable from astrology, for example. Even the astrologers can claim to find beauty and elegance in their explanation of the world. This looks like a serious objection on the lines of the discussion about the demarcation between science and other branches of knowledge. Polanyi needs to find a way of marking the difference between science and astrology without succumbing to objectivism. Tentatively, Polanyi answers to this objection by pointing out that the intellectual beauty of a putative scientific theory points towards the veridical powers of a theory. With more research along the lines of the theory, the theory reveals itself ever more deeply, bringing to light unforeseen knowledge about the world. True to fact that astrologers will claim to see a beauty in their astrological claims and endeavours, but these will rarely reveal themselves ever more deeply in the future and will require constant ad hoc re-interpretations to fit the theories with the events.

A further objection in this line would be that this criterion proposed by Polanyi is not useful in helping to choose immediately between two competing theories or world views. By pointing to the future indefinitely, the criterion only postpones the problem. How long must researchers wait before they can approve of a theory as true or jettison it outright as false? The criterion becomes less useful when the two theories are closely comparable, e.g. if both share a scientific approach to the world (e.g. a Ptolemaic versus a Copernican world view).

To answer to this objection, it needs to be pointed out that Polanyi's criterion like his whole theory of tacit knowledge, is *not a neat criterion.* The criterion ought to be seen as forming part of a wider process or system. The criterion comes towards the end of an interpretative framework that has been tested over the years and is being adapted to the times. The intellectual passion of the individual scientist or of a group of scientists researching the same question should also be taken into account. And finally, the community of scientists also plays a role in vetting theories. These latter two elements will now be further elaborated.

2.2.3 Heuristic guidance of intellectual passion

Intellectual passions have been introduced earlier (in the section discussing beauty and elegance) and they are described as appreciative expressions about theories, discoveries, etc. They can be further analyzed into heuristic and persuasive intellectual passions. Both of these perform the function of bridging a logical gap between what is known and accepted and what is about to be discovered or introduced as a theory. We look first at heuristic passion.

For Polanyi, intellectual or scientific passions play a further role besides affirming harmonies that open the scientist up to an indeterminate range of future discoveries. These passions guide the scientist to specific discoveries as well as sustain years of scientific labour towards the discoveries that they foreshadow. He is of the opinion that "The appreciation of scientific value merges here into the capacity for discovering it; even as the artist's sensibility merges into his creative powers. Such is the *heuristic* function of scientific passion." (Polanyi, 1958, p. 143)

That is to say that a heuristic passion, an unspecifiable impulse, sustains and guides creative scientists in their lifelong effort to guess right. And remaining within his wider framework of how discoveries are made, Polanyi holds that we need originality to cross the logical gap between what we already know and what we are yet to discover. A new framework of interpretation is launched which changes our way of seeing the world. Originality demands intention, it demands passion in order to be accomplished.

As an example, Einstein was guided by an aspiration stimulated by Mach, to free himself from the mistaken assumptions about space and time and "to replace these by a frankly artificial framework in which the assumption of absolute rest was replaced by that of an absolutely constant velocity of light." (Polanyi, 1958, p. 144)

He ended up with a vision in which the electro-dynamics of bodies in motion were set beautifully free from the traditional constraints of space and time. According to the interpretation of this event by Polanyi, "[a]ccepting this intellectual beauty as a token of reality, Einstein went on to generalize his vision further and to derive from it a series of new and surprising consequences .... The new beauty inaugurated the modern view of a mathematically defined reality." (Polanyi, 1958, p. 144)

Thus far, intellectual passions and beliefs attendant to them are held personally by the creative scientist, yet she holds the conviction that they are universally valid. The possibility still remains, however, that the passions and the beliefs may be mistaken. To this objection, Polanyi may answer that there is no way around this limitation rather than the *judgment* of the scientific community within which the scientist works, and the deepening of the putative theory with the passage of time, showing the theory to reveal more to those who use it than originally foreseen by those who propounded it.

The second role of intellectual passion is persuasive. The thrust of heuristic passion does not end when at last a discovery is made. Together with the said discovery there comes a claim to universal validity, for the discoverer would normally make a claim that what she has discovered is universally valid. And so, there follows the struggle to make it accepted by other scientists. This is a persuasive passion which, like heuristic passion, meets with and tries to cross a logical gap in the following way.

The innovative scientist has separated herself from the rest of the wider scientific community in terms of having a different interpretative framework brought about by her new discovery. Initially the struggle is to make the others believe that the new framework means anything before they can even give the innovative scientist an audience. Winning intellectual sympathy for a theory that they have not yet understood is crucial. Conversion, persuasion, etc. are the way forward because the formal argument that the outsiders subscribe to belongs to the older framework. There is no logical transition from the one to the other rather than conversion and persuasion, etc.

As an example, for over 148 years from the publishing of Copernicus' *De Revolutionibus* to the publishing of Newton's *Principia*, the Copernicans and the Ptolemeians disagreed among themselves and on top of each of the systems having explanations of phenomena, neither of the two was totally spared from internal problems.

An empirical approach may play a key role in the persuasion, but again tacit or personal knowledge, rather than a set of rules plays the key role. In other words, while empiricism as an approach is valid, it is only a maxim the application of which is only part of the *art* of knowing. Not every theory that is backed up with empirical evidence is accepted as knowledge in the scientific community. It is usually the case that the mainstream scientists bluntly refuse to discuss the views of their opponents in details, contenting themselves with a caricatured designation of them.

Neither is a theory dismissed merely on the grounds that it is not backed up with empirical evidence. In the case where the proposed theories are seen as mere guesses, the practice of science is such that it would be wiser to remain by the guesses of the scientist who guesses within a conceivable scientific system, and is thus making a competent guess. Guesses based entirely on speculation without any empirical backing, are incompetent guesses. Thus in summary, intellectual passions serve to affirm the scientific value of given propositions while denying it in others. They thus play a *selective role*. The same passions play a *heuristic role* in linking the appreciation of value to a vision of reality. Heuristic passion both sustains and produces originality and consequently propels the innovative scientist to persuade others, thus evolving into persuasive passion.

2.3 The tacit in the organization of science

The role of the scientific community and the whole culture of doing science has been hinted upon above as regulating passion. Polanyi holds that the articulate systems (e.g. elaborate cultures) which both foster and adjudicate over intellectual passions need a society to support them and to form of them a part of wider culture. To him, the tacit coefficients by which such articulate systems are understood and accredited are also part of the cultural life of the community. Wherever there is articulate communication, there is underlying it tacit knowledge understood as a set of assumptions agreed upon by all involved in and profiting from the communication. A sharing of this tacit knowledge, in tacit interactions, is the foundation on which cultural interchange and life are built. Our attachment to the truth may then mean our attachment to a community of people whom we trust to respect the truth. (Polanyi, 1958, p. 203)

A ready example is the use of a natural language, e.g. Kiswahili. All parties in a communication in Kiswahili agree tacitly that it is a language with a small and manageable set of symbols, with grammatical rules and a vocabulary. They all assume that their correspondents intend to communicate sense and those communicating assume that the content of their communication is accessible by their counterparts, etc. The idiom used, the examples given, in brief the experience referred to tacitly is all tacitly agreed upon.

And so when we communicate what we consider to be facts, thus wishing to communicate knowledge, we call our interlocutors' attention to the message of our communication and to us. We are using more than the mere descriptive powers of language. There is still a possibility of misunderstanding in communication. Even though people may misunderstand one another on particular words, they still do manage to convey information on the whole by speech. Their tacit judgments on denotation coincide in some way. And in the end, different people can use the same set of symbols skilfully to reorganize their knowledge. Polanyi gives an example from animal intelligence. Animals imitate their more intelligent counterparts in a process by which there is a genuine transmission of an intellectual performance – a communication of knowledge in an inarticulate way. (Polanyi, 1958, p. 205)

This is also the way in which arts are learnt – by imitation of the practices of those trusted because they are better initiated, through tacit judgments of unspecifiable skills. Whenever we communicate, say by telling somebody something, we make an effort to find the best fitting words to express what we mean. Having said it, we endorse our words as being the best expression of what we mean. We then hope that our words will be received as we meant them, by our interlocutor. We do make tacit endorsements of our words in such cases. We may be mistaken, but we have to accept the risk in order to say anything at all.

Yet for novices of any articulate system of cultural lore to assimilate the lore, they need to have previously been affiliated in an act of apprenticeship to a community considered to be the masters of this lore. The novices appreciate the values of the lore and endeavour to live by them. Confidence in the intellectual leadership of the masters of the lore is cultivated. The learner believes that what she is learning makes sense, before she knows that it does make sense. She takes this on authority. This may sound medieval, but the learning of a natural language, especially of a mother tongue, is an example of this process. But also the learning of science involves passing on a whole interpretative framework from the experienced and skilful scientist to the novice or apprenticeship. Science is not just passed on when the propositions that form the theories are passed on by rote. The budding scientist adjusts to a whole worldview.

An objection to this position may point out that the independence of the mind of a scientist may be eroded in this approach to science. But Polanyi makes a fitting response that merits being quoted at length. He holds:

> "Every time we use a word in speaking and writing we both comply with usage and at the same time somewhat modify the existing usage… even when I make my purchase at current prices I slightly modify the whole price system. Indeed, whenever I submit to a current consensus, I inevitably modify its teaching; for I submit to what I myself think it teaches and by joining the consensus on these terms I affect its content." (Polanyi, 1958, p. 208)

This response by Polanyi advocates more for a piecemeal evolution of science rather than by revolutions. It brings together both the discoveries in science and the influence of the scientific community on the work of the individual scientist. In this context, it makes sense why the individual scientist may hold on firmly to what she may believe to be *true* even though she knows that it may conceivably be *false*. It so happens that in the actual practice of science the body of scientific knowledge is so vast that any scientist can judge at first hand only a hundredth of the total current output of science. Still the community of scientists jointly administers the advancement and dissemination of science (through a control of academic appointments, research grants, scientific journals, degrees, etc.) In so doing, the community establishes the current meaning of the terms 'science' and 'scientist'. (Polanyi, 1958, pp. 216-217) The rest of society takes the meaning and truth of these terms on trust in the established scientific community.

Where consensus exists in the scientific community, it is usually held that by this consensus is meant the 'fact' that observations in science can be repeated and confirmed. Yet in reality this 'fact' is a different way of showing adherence to the consensus. No significant part of the observations of science is in reality ever repeated. If an attempt to confirm scientific observations fails, the blame is usually laid on the incompetence of the scientist who is seeking to confirm the observations. And if the observations are reliably repeated, this would still not be enough to justify the scientist's acceptance of the generalizations that science sees as accruing from these observations. Neither would the choice of these particular observations as subjects of scientific observation be justified.

And finally, the truth of a generalization is not enough to make it part of science. Other coefficients must be present before a statement can be said to have scientific value, namely the statement must have reliability, systematic interest, and intrinsic interest. And so consensus in this sense is not principally about a common experience. It is rather about an appraisal of a common domain where those consenting are experts only of a narrow section of the whole.

But how would this process qualify to be called a consensus? According to Polanyi:

> "Each scientist watches over an area comprising his own field and some adjoining strips of territory, over which neighbouring specialists can also form reliable first-hand judgments. Suppose now that work done on the speciality of B can be reliably judged by A and C; that of C by B and D; and that of D by C and E; and so on. If then each of these groups of neighbours agrees in respect to their standards, then the standards on which A, B and C agree will be the same on which B, C and D agree, and on which C, D and E agree and so on, throughout the whole realm of science. This mutual adjustment of standards occurs of course along a whole network of lines which offers a multitude of cross-checks for the adjustments made along each separate line; and the system is amply supplemented also by somewhat less certain judgments made by scientists directly on professionally more distant achievements of exceptional merit. Yet its operation continues to be based essentially on the 'transitiveness' of neighbouring appraisals ...." (1958, p. 217)

A valid objection to this presentation is that it makes science look as if there is neither dissent nor room for dissent. In fact there is dissent e.g. in modern day physics. Polanyi's suggestion then should be understood as an idealization on this point of how to resolve the differences there are in views. In this way the consensus has two roles. First, by it there is a continuous network of critics who maintain the same minimum level of scientific value in all accredited publications of science.

Secondly, the same network, with members relying on the expertise of neighbouring members of the network, is able to judge outstanding distinction using the same standards throughout the network. Appropriate decisions about the distribution of human and material resources as well as nodding to or halting novel departures in science can thus be made. Mistakes have been made and there have been delays in recognizing novelties. Further, dissent has at times dragged on for long within the scientific community. Nevertheless, "... we should acknowledge that we can speak of 'science' as a definite and on the whole authoritative body of systematic knowledge only to the extent to which we believe that [such] decisions are predominantly correct". (Polanyi, 1958, pp. 217-218) In this last quotation, Polanyi is speaking at a meta-level. At this level, the hazardous nature of human knowledge[20] (Polanyi, 1958, p. 245) in general and of scientific knowledge in particular, together with the imperfections of science, can be acknowledged. On the other hand, the efforts by scientists to arrive at an ideal of true knowledge about the world can be evaluated.

2.4 Conclusion: The tacit premises of science

At the end of this long and complicated argument, the question is relevant: What then are the premises or the presuppositions of science? Are the rules for correct procedure specifiable? Is substantial belief in the scientific propositions justifiable? And the answer is not direct. Polanyi holds on this point that the rules of correct scientific procedure and the beliefs and valuations we hold in science determine each other mutually. We posit what we expect to be the case and then we proceed accordingly, shaping our anticipations according to the success that our procedures have accorded us.

The beliefs are used to project the standards of evaluation and the evaluations are about the beliefs. (Polanyi, 1958, p. 161) There is a continuous adjustment of both poles as the scientific enterprise develops. The way we establish what end up being scientific *facts* is important in determining whether they are *facts*. Thus far, establishing the premises of science and likewise establishing facts is comparable to skills. Skills, according to Polanyi, are practiced without any focal awareness of their operational principles or premises. If anything, those premises are discovered in the practice of the skill. They can then be known focally and be passed on to the

[20] On these grounds, Polanyi hopes to give a response to skepticism.

one who wishes to learn the skill by re-embedding them into the practice of the skill as maxims. That way, the skill is improved.[21]

The body of science is so vast that there is no single scientist who knows an aspect of it in its entirety, enough to judge the validity and value of propositions made within it at first hand. The rest of the judgments made are accepted on trust of authority of other members of the scientific community. These members of the community have been accredited as scientists by yet other scientists. This happens in a complex chain. Each scientist can only accredit a small number within her area of research. She is accredited in turn by that small number she is accrediting and by others. There is then a series of second hand recognitions that spreads throughout the community and backwards in time. The whole community has the same masters. There is in that way a general consensus on what science is, what is scientific and what does not qualify to be called science.

But the commitment to authority and tradition in this community is not absolute or unconditional. The existing opinion is accepted as a competent authority, but not as a supreme one in identifying the subject matter called 'science'. This latter must remain open-ended to an extent since science defines itself by and large in the very process of doing science. There is room for conflicting views within science itself. There is also the possibility to change beliefs and values considered hitherto fundamental. In that light, the premises of science are ever

[21] On this point it should be pointed out that Polanyi is not oblivious of the fact that tacit knowledge can be made explicit in some form and to some extent for the purpose of passing it on. It is important to separate the imparting of this knowledge from the performance of the skill, or else hitherto subsidiary awareness of the skill will take the place of focal knowledge needed in performing the skill. We can also safely conclude in these circumstances that Polanyi does not think that the teaching of science can be limited to passing on of a list of propositions. The skilful part needs to be passed on in the context of apprenticeship.

changing as science keeps changing. These premises play a guiding role as the innovative scientist goes about her work of discovery and verification. Science keeps emerging from vagueness to ever deeper clarity. And so, Polanyi seems to have tapped into an original insight in the way philosophers of science up to and shortly after his time (verificationists, falsificationists, holders of the methodology of scientific research programmes, etc.) described science in their methodologies. Such methodologies, when they are justifiable, are only useful for scientific theories that are considered hitherto *established.* (Polanyi, 1958, p. 170) The formal criteria suggested by the mainline philosophers of science at the time of Polanyi can at best function as *maxims* or idealized and abstracted guidelines of backward looking scientific value and procedure. But methods keep changing and so should maxims of procedure.

The purpose of coming up with logical antecedents of science as furnished by some of the philosophies of science (e.g. Popper) should not be understood as aimed at arriving at forward looking axiomatic presuppositions of empirical inference. This way of understanding logical antecedents of science obscures the very purpose of coming up with premisses of science. In fact, the reason why such premisses have been upraised in the history of science is in order to help resolve conflicts and upheavals in science. In themselves, detached from their historical contexts, they are neither comprehensible nor convincing. Such postulates are a 'highly attenuated summary' of a body of knowledge which is itself a result of passionately sustained efforts of generations of people society has recognized as scientists.

The reason we hold on to the postulates reflects back and should lie in the body of knowledge they are meant to summarise. Science can then be seen as a vast system of beliefs handed down in history and organized by a specialized section of society. (Polanyi, 1958, p. 171) The element of our commitment to given scientific positions is then accounted for more accurately. Science is a system of beliefs to which we are *committed* and which cannot be represented in detached, impersonal and non-committal terms in the defence of objectivism. This is a closer description of science than one which tends to depict science as the acceptance of a formula. And given the passionate involvement in the establishment of scientific 'facts', rather than emphasize the infallibility of scientific positions, an accurate account represents their *competence.*

Intellectual passions in humans differ from similar passions in animals in the way that the human intellectual passions are attached to an articulate system – language. Thus, when the passions have been gratified in discovery etc, they can be passed on to a cultural system and thus become a heritage. In intellectual passion, we wish to gratify ourselves, not frivolously, but respectfully. We yield to a desire for self-education to the measure that our passions set for us. These passions differ from other passions in seeking to fulfil universal obligations rather than private satisfaction. Thus far, intellectual passions are both private and public at the same time.

Part Three: Responding to the reception of the theory of tacit knowledge

Chapter Three: A critique of Tchafu Mwamba's sympathetic appreciation of Polanyi's theory of tacit knowledge

3.0 Introduction

This chapter is a critical examination of the interpretation of Polanyi's theory of tacit knowledge by a recent sympathetic adherent to his philosophy of science – Tchafu Mwamba. I assume that I have made a defensible and faithful discussion of the theory up to now. Now I test my interpretation of the theory against the interpretation of Mwamba. How does Mwamba and other similar adherents agree or differ with my rendering? A few more aspects of the theory become clear in this examination.

Tchafu Mwamba has published his doctoral thesis on the thought of Michael Polanyi. (Mwamba, 2001) In a chapter, he discusses Polanyi's theory of tacit knowledge.[22] A critical examination of this chapter will reveal that Mwamba does only partial justice to the general notion of tacit knowledge. From the outset, I agree with Mwamba on two important ideas about tacit knowledge. First of all, I agree with him when he holds in general agreement with Polanyi that reports about scientific theory have been incomplete in having only given an account of what is clearly observed or expressed in logical terms. Thus far, he accurately remarks that "Polanyi's aim is to describe a strategy for inquiry, more in keeping with the strategies of working scientists rather than the descriptions advocated by the logical positivists, or even by the 'standard' view." (Mwamba, 2001, p. 9)

Secondly, I agree with him on the point that Polanyi's most important and most original contribution to epistemology and to the philosophy of science lies in

[22] I limit my analysis to this chapter on tacit knowledge, but Mwamba goes much further to engage in the contribution that Polanyi has made to rationality and methodology.

having insisted that personal/tacit knowledge not be assigned simply to the realm of the context of discovery. (Mwamba, 2001, p. 4)

Nevertheless, there are areas that I consider important to the whole theory of tacit knowledge, which need fine-tuning if the full potential of tacit knowledge as an important epistemological notion is to be gained.

3.1 Objections to Mwamba's rendering of the discovery/justification or psychological/logical distinctions

Mwamba does not give due emphasis to the fact that Polanyi steers clear of the debate on the separation between the psychological and the logical (or the contexts of discovery and justification) by showing that tacit knowledge is pervasive in all areas of knowledge including the logical. For Polanyi, the learning process (which we share with non-human animals) is governed by tacit capacities. It consists in the learner abiding by self-set standards and evaluating progress either through experience or from the feedback of the community of more experienced or more skilful counterparts in the learning process. Heuristics are not only at work where big discoveries are made. Rather, they are an attitude of the learner towards the environment in which the learner actively makes sense of the environment. The tacit capacities that enable the learner to proceed in this way are in turn guided by intellectual passion.

In the context of justification too, tacit capacities are at work. In the particular area of making a contribution to science, the scientist operates within a culture that defines science in a particular way, besides other specific views and practices. The justification of a novel scientific position pays subsidiary attention to all these – it assumes them. An attempt to justify each of these views and practices would stall the process of justification. In addition, personally, the scientist is guided by a commitment to her (novel) position on which she seeks to convince or persuade other scientists. Even when she makes an effort to remain as objective as she can, she is not thereby detached and impersonal about her proposed novel position. And so the separation between the context of discovery and the context of justification, or the one between the psychological and the logical in that order, is for Polanyi not enlightening. Mwamba does not emphasize this point enough and this could be a source of criticism from proponents of the separation.

Mwamba does point out nevertheless, that Polanyi's goal is not to describe a theory of the psychology or the sociology of scientific knowledge, nor of mapping the unconscious process of cognition nor the social determinants of science. (Mwamba, 2001, p. 5) But left as it is, this position can only invite criticism for not offering an alternative to the claim that Reichenbach, Popper and the critical rationalists have made that the psychological should be separated from the logical in epistemology. Polanyi's point of departure is rather that tacit knowledge is pervasive – it bridges the two sides of the separation. The divisions of 'sociology of science', 'psychology of discovery', etc. are of secondary epistemological importance therefore. And yet there are more parts of his analysis in which Mwamba seems to have subscribed to the separation between discovery and justification. He claims:

> "My concern, and of deeper significance to the methodology of science, is with Polanyi's theory that understanding is a process we know in as far as we exercise it, but find difficult to talk about and to describe explicitly. This is because we attend to the content of understanding and not to the process itself. We attend either to actual scientific discoveries or to methodological principles but not simultaneously to both." (Mwamba, 2001, p. 5)

Here, Mwamba is extrapolating or borrowing from the notion of focal and subsidiary awareness in Polanyi, according to which we pay attention to some areas of knowledge only as clues while we focus on other areas. Thus we have subsidiary awareness of the first clue-like elements, and focal awareness of the objects of our investigation or observation. The subsidiary/focal distinction is a Polanyian context. But to apply it in the context of discoveries and the context of methodology would be to succumb to the very separation that Polanyi is seeking to undermine in his theory of tacit knowledge. There is a continuum from the simplest and inarticulate learning process (e.g. one carried out by a non-human animal) and the most abstracted and formalised scientific discovery in the sense that tacit knowledge operates in these two extremes and anywhere in the learning processes placed on a scale between these two extreme ends. Tacit knowledge pervades all of learning and all of discovery.

3.2 Evaluative observations on Mwamba's 'central theses' of Polanyi's theory of tacit knowledge

According to Mwamba, the central theses of Polanyi's theory of tacit knowledge are four:

> (1) "True discovery, exemplified particularly by the progress of science, cannot be adequately explained in terms of a set of wholly explicit rules or algorithms, used either to find or to test theories
>
> (2) Though public and social, knowledge is in varying degrees, and most importantly, inherently personal
>
> (3) Not only is it impossible to make all knowledge explicit, since "we can know more than we can tell" but the knowledge that cannot (always) be specified is more fundamental than explicit knowledge ...
>
> (4) Tacit and explicit knowledge are neither empirically nor theoretically completely separate nor independent processes. They interact in a kind of synergistic relation." (Mwamba, 2001, p. 18)

First of all the effort is commendable: that an understanding of Polanyi's theory of tacit knowledge is rendered into a few clear theses. One of the criticisms levelled at Polanyi's theory of tacit knowledge mainly from the quarters of analytic philosophy (Musgrave, 1968, Lakatos, 1978) is that it is not outlined in clear analysable theses. And so Mwamba is to be commended on initiating the effort to fill in this gap. However, it should be pointed out that it is neither by omission nor in a bid to remain vague, that Polanyi does not give such a concise summary of 'central theses' of his theory of tacit knowledge. I suggest two reasons why Polanyi avoids such a summary. First of all, tacit knowledge does not lend itself readily to a summary of 'central theses' just in the same way that it defies a concise definition. This in turn is because tacit knowledge pervades all knowing in the sense that it is what is presupposed for any knowledge claim to be made. In other words, whenever we make a knowledge claim, there is a lot else towards which we only pay subsidiary attention in order to pay focal attention to what we claim to know. To give an example, the knowledge claim: 'I know that this paper is white' is in turn based on my currently tacit knowledge of paper and of white etc. And if I tried to make explicit all the knowledge on which I rely to make this knowledge claim, I would end up with an infinite regress of background knowledge.

The second reason why Polanyi does not give a precise set of 'central theses' of the theory of tacit knowledge is that such an effort would go against the very objective he sets himself – to work against an objectivist approach to knowledge. As already pointed out, the objectivist approach to knowledge treats only explicit knowledge. Explicit knowledge can conceivably be summed up in a set of theses.

But knowledge is more than just explicit knowledge. Mwamba himself subscribes to the distinction there is between explicit and tacit knowledge in the description he gives of both. For Mwamba, the characteristics of explicit knowledge are: "precise analysis, verbal articulation, descriptive identification, observational objectivity and a clear distinction between the knower and the known". (2001, p. 29) On the other hand, the characteristics of tacit knowledge according to Mwamba are: "intuitive discovery, bodily expression, holistic recognition, embodied subjectivity, and a contextual distinction between the knower and the known ...". (2001, p. 29) Mwamba makes a clear distinction between the two kinds of knowledge as he goes about to give a summary of the central theses.

Now, just because tacit knowledge defies summary or definition does not mean we should abandon and discourage every effort to summarise it into central theses. Caution must be exercised in choosing which of the many features of tacit knowledge to be the central one(s). It is therefore important to look at the theses that Mwamba has proposed as the central theses. My goal is not to replace Mwamba's theses with other theses I consider as being more central, for I do not think that such central theses are crucial for the theory of tacit knowledge to be tenable.

The first thesis holds that "True discovery, exemplified particularly by the progress of science, cannot be adequately explained in terms of a set of wholly explicit rules or algorithms, used either to find or to test theories". (2001, p. 18) I claim that the phrase 'true discovery' is misleading and it is based on a misunderstanding of Polanyi's theory of tacit knowledge. For Polanyi, there is no 'true discovery' as opposed to 'untrue discovery'. He addresses discovery in general. His treatment of learning and heuristics as these are performed in science is different in degree and not in kind from what is practiced at other levels of learning. What distinguishes human learning and heuristics from non-human animal efforts at learning and solving problems is the use of language, Polanyi argues.

A further problem with this first thesis is the phrase 'adequately explained'. I claim that in terms of explaining, when we look back at advancements made in the area of science, there is a possibility of *explaining* them *adequately* because tacit knowledge can be rendered explicit. There will always be a tacit component to explicit knowledge, and therefore not *all* knowledge about a given scientific advancement or discovery can be rendered explicit at the same time. But at the moment that we choose to focus explicitly on what has up to now been tacit, we thereby in turn pay implicit attention to other areas.

The second thesis holds: "Though public and social, knowledge is in varying degrees, and most importantly, inherently personal". (2001, p. 18) Mwamba is touching on an important thesis in the whole theory of tacit knowledge. Polanyi does address the personal nature of scientific knowledge. However, Mwamba puts the emphases in the wrong places. Polanyi's theory of tacit knowledge underlines *personal* knowledge, but not in contrast to public and social knowledge. In fact it can be argued that organisations, cultures, scientific communities, etc. can have tacit knowledge. Rather, Polanyi underlines the personal nature of knowledge in opposition to the objectivist and impersonal tendency in accounts of how science works. In his elaboration of how we learn language and by extension how we arrive at scientific knowledge, Polanyi explains that the society within which we operate plays a key role. The knower tests her knowledge of the language against the approval of the community. Still, the learner of the language can come up with a neologism or a new use of a word (as in poetic licence). But in order to do this, the user of the language needs to have learnt how to come up with new words or new uses, and this skill is sanctioned by the community within which she learns and uses the language. The same can be extended to science in terms of making new discoveries. The community of scientists continues to play a key role even in what may end up being a revolutionary discovery or theory because the scientist who comes up with the novelty learns the skill of arriving at novelties within the scientific community. And so Mwamba should not oppose the 'personal' to the 'public and social' in the theory of tacit knowledge.

The third thesis states: "Not only is it impossible to make all knowledge explicit, since "we can know more than we can tell" but the knowledge that cannot (always) be specified is more fundamental than explicit knowledge ...". (2001, p. 18) As I have noted in my response to the first thesis, what is tacit knowledge at some point can be made explicit at some later point. What happens is that should we succeed in making erstwhile tacit knowledge in some area of investigation or engagement explicit, we rely in turn on implicit knowledge of some other area of the matter under investigation etc. An example is in the learning of a language. A language, e.g. a mother tongue can be passed on from parents to children just by allowing the child to grow up in the environment where the language is used. But a language can also be broken up into explicit grammar, idiom etc. and passed on to a learner. What went as tacit knowledge in the first instance has now been mostly made explicit in the second instance where there is a written grammar. And yet when a learner relies on the written grammar and idiom to learn the language, she must still rely on these tacitly in order to use the language. She cannot be thinking of the correctness of the grammar all the time if she is to become fluent in the language.

This example can be applied to other forms of language learning. And so Mwamba's third thesis needs to be qualified by including that it is impossible to make all knowledge about a given aspect (e.g. of a scientific investigation) explicit *at the same time and in the same aspect.* Elsewhere, Mwamba admits of this element of tacit knowledge when he points out: "That one knows these tacit factors can be made clear by asking a person to focus on them, whereupon we may become quite articulate about the movement of our head and hands, and about the rational steps necessary to identify an object or an action. But then, some other factors will be supplying the tacit context within which this new focusing is taking place." (2001, p. 29) And finally is the fourth thesis, i.e. "Tacit and explicit knowledge are neither empirically nor theoretically completely separate nor independent processes. They interact in a kind of synergistic relation." (2001, p. 18) I think that this thesis is unproblematic and it agrees with my objections I raise about the other three theses.

Besides the objections about Mwamba's proposed theses, there are some important omissions and ambiguities that should be pointed out. First of all, in what he proposes as the central theses, it is not made clear that tacit knowledge is *skilful* knowledge. In other words, similar to the perceiver in the act of perception, the knower participates in the act of knowing, filling in missing gaps and ignoring so-called 'interfering' or 'irrelevant' details in order to come up with a view that makes sense. Tacit knowledge is creative in this way. This is how the theory of tacit knowledge is better poised to explain heuristics than other objectivist theories of knowledge.

A second omission by Mwamba is the central role played by intellectual passion and commitment in tacit knowledge. The knower is motivated in her search for knowledge by the inarticulate or tacit belief that the knowledge is attainable. She is committed to what she is yet to discover and by the same tacit capacities she is able to recognise it when at last she attains it. The details of this aspect of tacit knowledge have been discussed earlier.

One ambiguity that needs clarification is the undue emphasis that Mwamba puts on the way Polanyi advocates for the inarticulate or the unproven. Mwamba holds that in his theory of tacit knowledge, Polanyi hopes "...to replace the self-evident foundational truths of critical thought with the knower's pre-reflective context. This context is historically and culturally conditioned. Polanyi's post-critical thought holds beliefs that are both non-explicit and 'unproven'." (2001, p. 15) But represented in this way, Polanyi may seem to be choosing the inarticulate and unproven over and above the articulate and proven. And yet his goal is to show both the inarticulate foundations of what is articulate and the unavoidability of the inarticulate. His approach is more inclusive than Mwamba is giving him merit for. Polanyi allows room for the inarticulate and the unproven – the tacit – in such formalized rational processes like scientific reasoning. The pre-reflective, non-explicit and 'unproven' characteristics of tacit knowledge pointed out by Mwamba are only part of a wider category of knowledge.

In conclusion to these observations, Mwamba's effort to give a summary of central thesis looks like a step in the right direction. Some theses in it need to be revised to show a better understanding of his theory of tacit knowledge. A few more theses should be included before it is complete. But the task remains a daunting task for the theory resists being condensed into a few theses.

3.3 Mwamba on Polanyi's solution to the *Meno* paradox

In Plato's dialogue entitled *Meno*, Socrates is engaged with Meno in a discussion about virtue. Socrates challenges Meno into accepting that he, Meno does not *know* what 'virtue' is. But Socrates offers to help Meno in searching for what 'virtue' might be. Meno objects to this offer by Socrates, pointing out: "But how will you look for something when you don't in the least know what it is? How on earth are you going to set up something you don't know as the object of your search?" To this Socrates replies by showing that this is a problem that faces all learning.

On the one hand, how would we find out what we do not know? On the other hand, why should we set out to search for what we already know? Socrates resolves this paradox by showing Meno, with the help of one of the servants answering questions without help, that learning is in fact recollection. The background of his solution is that we know with the help of our soul and the soul, according to what priests and poets teach, is immortal. Life and death are different phases that the immortal soul takes on at various times. And so what appears as learning is in fact a recollection of what the soul knew from an earlier existence in life. All nature is interrelated, and every memory can lead eventually to every other memory.

The paradox cannot be swept aside as trivial. Plato's Socrates gives a solution to the paradox that is consistent with other areas of the platonic world view. What is surprising is, as Polanyi points out (Polanyi, 1967, p. 22) that even though the paradox (which he reduces to a dilemma, i.e. a: either we know what we are looking for, and then there is no problem, or b: we do not know what we are looking for, and thus we cannot recognize it when we find it) may be acknowledged, Plato's suggested solution has not been overlooked yet unaccepted for so long. Problems have been solved over time and yet there is hardly an effort to resolve the paradox. Polanyi makes the bold step of suggesting that the theory of tacit knowledge may provide a solution to the paradox. The sum total of clues that the researcher uses both to single out a possible problem and to recognize the solution when she arrives at one, is part of tacit knowledge.

Polanyi's bold suggestion is met with opposition from Michael Bradie and Herbert Simon. (Bradie, 1974, Simon, 1976) Using examples from mathematics, both Bradie and Simon claim that one of the horns of the dilemma proposed by Polanyi is false and consequently the paradox collapses. They argue that the false horn of the dilemma is the claim that knowing what we are looking for means there is no problem. The example that Bradie gives (and Simon takes up together with other examples of his own) is that a mathematician may seek to refute Goldbach's conjecture that every prime number can be represented as the sum of two prime numbers. Such a mathematician, they maintain, knows what she is looking for – namely a counterexample – an even number that cannot be represented as the sum of two prime numbers. Yet the problem of finding such a number remains. And so there can still be a problem even when we know what we are looking for.

Mwamba intervenes by pointing out that for Polanyi, 'knowing what we are looking for' begins already in the capacity to find a good problem with which to launch the inquiry. It is not merely limited to the solution or the answer to the problem. Taken in the context of the objection by Bradie and Simon to Polanyi's proposed solution, Polanyi's theory of tacit knowledge accounts for how the mathematician in question comes up with the idea of finding a counterexample to Goldbach's conjecture – why she thinks this is a fruitful problem. The mathematician could have scattered her search in innumerable other problems. It is not random that she chooses this particular one. The choice of a good problem is guided by the same tacit faculties as the recognition of the solution to the problem when at last it is arrived at. In fact the same tacit powers govern the actual process of solving the problem. Heuristics are being applied from the moment of the choice of a problem, through working out a solution to arriving at and recognizing the solution. As Polanyi holds, as the researcher seeks a solution to an identified problem, "Things are not labelled evidence in nature, but are evidence only to the extent to which they are accepted as such by us as observers." (Polanyi, 1958, p. 30)

Bradie and Simon may have a strong point in pointing out that there may exist a formula, a way to solve a whole class of problems. But this would apply only for routine problems. At times of great scientific advancement, what is needed is a creative new problem. The counter-example that Bradie and Simon are proposing does not suffice when the problem is not a standard problem. Science allows for such moments when great originality is needed both in finding a totally new problem and an equally new solution to it. In Mwamba's interpretation of Polanyi's theory of tacit knowledge, Polanyi is dealing with such unique moments of science. (Mwamba, 2001, pp. 49-50) I disagree with him in this interpretation, as I have argued in my critique of the central theses proposed by Mwamba. Heuristics, I argue, pervade the whole learning process. And in relation to Plato's dialogue, the paradox pointed out concerns the whole learning process.

The problem with Bradie and Simon is even deeper. When they point out that the mathematician seeking a counterexample to Goldbach's conjecture "... knows that *what* he is looking for is a number which is *not* the sum of two primes ..." (Bradie, 1974)[23], they make an ambiguous claim. Bradie and Simon commit the fallacy of amphibology on the term 'know'. One way to expose the fallacy is to ask the researcher *what* the number is. It turns out that the researcher knows the method

[23] Emphasis in the original.

by which to find the number, but does not know the actual number. And in fact, even when the researcher knows the method, she remains open to clues and heuristics that she cannot make fully explicit in that very method. There is a kind of knowledge she has which she cannot make explicit – a tacit kind of knowledge.

Denying that there is a paradox by denying one of the lemmas does not help resolve the valid question posed by the *Meno* paradox. For Mwamba, Polanyi's notion of tacit knowledge is borne out both logically and in experience. Logically, it is founded in the fact that logical arguments always have unstated or silent/tacit premises to them – the so-called enthymeme. In experience we are always 'empathic' in our observations, and it is through such 'empathy' that we join the dots in our stimulus. (Mwamba, 2001, pp. 52-53)

3.4 Mwamba's answer to the 'vagueness' objection

Polanyi's theory of tacit knowledge is easily criticised as being vague. In answer to this criticism, Mwamba has the following answer in line with D. Scott. By its very nature, tacit knowledge defies definition, because it is part of the very foundation on which the awareness of definition is made possible. Mwamba points out that all attempts to express Polanyi's thought in analytical style are bound to be faced with difficulties. In addition, and quite accurately:

> "... critics might argue that merely explaining that the tacit is not limited to the psychological aspect of knowledge does not clear up the confusion in Polanyi's use of the term, especially since Polanyi seems to be applying the tacit interchangeably to three quite different components: to awareness, to mental acts, and to the subsidiary. He seems to treat these as if they were primitive terms needing no further clarifications notwithstanding that they make up the feature of knowing that analytic philosophers, if they mark it at all, consider anomalous, lucky, or anarchistic." (2001, p. 56)

In other words, Mwamba points out that there is an ambiguity almost inherent to Polanyi's notion of tacit knowledge. He agrees with R. Gelwick (1977) whom he quotes as holding: "the objective ideal of knowledge omits two major areas of knowing: our subsidiary reliance upon clues and our integrative powers as persons." (2001, p. 56) For Mwamba, Polanyi's position suggests that even at the end of a rigorous analysis of our perception, there remains an irreducibly tacit dimension. "Indeed we never encounter anything or anyone absolutely directly and immediately in brute form. We never simply attend to something, but rather, of necessity, always attend from something to something." (2001, p. 57)

For the simplest instances of knowledge, there is always an infinity of particulars relied upon and tacitly implicated. We cannot specify all the subsidiary factors or clues relied upon. This is not just because the content of such clues cannot be made explicit. Rather, it is because of the nature of knowledge. Once we turn our gaze to such clues, they lose their character or function as clues in the overall search for knowledge. They gain new meanings and play a new role in the whole search for knowledge. They are no longer clues. We no longer indwell them the way we indwell our clues in order to focus on what the clues point out to us.

Mwamba's defence of Polanyi's theory of tacit knowledge against the objection of vagueness fits in with the whole theory, but it leaves the theory exposed to an imprecise use of the tacit. Mwamba has pointed out at least three areas where Polanyi uses the term. The question remains – what binds all these uses together consistently? My response is in two instances. First of all, it needs to be emphasised that our understanding of tacit knowledge must resist the urge towards objectivism. As I argued in the treatment of knowledge in general, the analytic tradition of epistemology seeks to understand and define knowledge in objectivist terms. But not all knowledge meets the objectivist standards. Not all knowledge can be made explicit. In that light then, the effort to designate adequate definitions for some of this tacit knowledge is understandably difficult though not fully impossible.

Secondly, I pointed out earlier in my discussion of the central theses of the theory of tacit knowledge as proposed by Mwamba, that heuristics and learning are central to the theory. In answer to this question of three varying uses of the term 'tacit', a view of heuristics and learning is helpful. What is common in each of the three varying areas that Mwamba points out (i.e. awareness, mental acts and the subsidiary) is the urge to make sense of reality by the knower. Learning and heuristics – both of them operating tacitly – are the way the knower makes an effort to make sense of reality. In brief, I propose that the objection of vagueness be redirected to epistemology. If epistemology drops the objectivist assumptions (that I argue it makes in my discussion of knowledge above), and there is room for inarticulate or tacit knowledge, epistemology would stand to gain because the inarticulate is ubiquitous even in formalized and objective knowledge.

3.5 Mwamba's answer to the 'subjectivism' objection

Mwamba examines the objection of subjectivism levelled at Polanyi's theory of tacit knowledge. It is claimed by some (Musgrave, 1968, Lakatos, 1983, Popper, 1969) that Polanyi's theory of tacit knowledge endorses subjectivism. In response, Mwamba points out quite accurately that Polanyi's notion of the personal is not synonymous with the subjective. He notes that the personal in us, should be distinguished from the subjective. The personal actively enters into our commitments, while in our subjective states we merely endure our feelings. The personal is neither subjective nor objective in the objectivist sense of being impersonal and detached. For Mwamba in his interpretation of Polanyi, "The personal is a quasi-normative concept while the subjective is merely descriptive, non-normative, non-committal." (2001, p. 61) In order to know the external world, we rely on the way we are in the world. It is always a personal experience. Even in scientific practice which relies on a prescribed and rigorous procedure, we need our personal discretion and judgment.

Mwamba goes into some detail in showing that Polanyi's position that points out the limits of formalism indeed has similarities with the Gödelian position to the same effect. Thus:

> "Gödel's theorems are actually special, self-referential consequences of the requirement of consistency: in a consistent system, something must remain unprovable. One unprovable statement is the statement of that very fact, namely the statement that says of itself that it is unprovable (first theorem): you cannot prove a statement that says that it can't be proved (and remain consistent). Another unprovable statement in a consistent system is the statement of consistency itself (second theorem). In addition, if the formal system has a certain stronger form of consistency, the sentence which asserts its own unprovability, called the Gödel sentence, is also not refutable in the system." (2001, p. 65)

Thus far, to Mwamba's interpretation, the limitations in formalization that Gödel's theorems demonstrate are seen by Polanyi as an indicator that formal languages are similar to natural languages. Further, it becomes clear from the comparison with Gödel's theorems, formal systems function in virtue of informal or unformalizable ingredients in them. (Mwamba, 2001, p. 67) Tacit knowledge, as well as personal judgment, always comes in to help sustain a formal system. And so, Mwamba holds that Polanyi's theory of tacit knowledge is essential in the understanding and growth of science. Mwamba regrets that it is rare to find it taught in colleges today, that the purpose of science is to discover the hidden reality underlying the facts of nature. He points out that the modern ideal of science is to establish a precise mathematical relationship between the data without acknowledging that if such relationships are of interest to science, it is because they tell us that we have hit upon a feature of reality. Citing Polanyi, Mwamba remarks that Polanyi's aim is to restore the idea of reality at the centre of a theory of scientific inquiry. (Mwamba, 2001, p. 74, Polanyi, 1967)

My estimation of this position by Mwamba is that it leads Polanyi's tacit knowledge far out into the minefield of the realism debate. There is an anachronistic element in Mwamba's interpretation of Polanyi when Mwamba applies the term 'realism' to Polanyi in a later period when the realism debate is advanced. I argue that there is room for adhering to Polanyi's theory of tacit knowledge (even as far as it claims to be realistic in some sense) while at the same time remaining agnostic about realism or anti-realism. There is a way of adhering to the theory of tacit knowledge as operating within a descriptive or phenomenological context without making substantial metaphysical claims about the true nature of reality. Mwamba himself holds that Polanyi presents seeing as a process that involves the construction of objects seen, besides engaging the seer in a process of learning to see – learning to see wholes. (Mwamba, 2001, p. 79) And so when our tacit capacities lead us to an aspect of nature, this aspect could be a confirmation about nature as it 'really' is or it could be a confirmation of our methods of learning and our heuristic strategies for learning about nature. I deliberately avoid the realism debate because I think it has no direct relevance to the theory of tacit knowledge.

There is an élan in Polanyi's theory to shift (unexaminedly, according to Harré, as cited in Mwamba, 2001, p. 82)[24] from relations and processes regarding perception to conceptual and propositional relations. But this can be stopped at discoveries without going further into explicating the nature (or ontology) of what is discovered. The question is whether Polanyi is careful enough to stop at that level. But Mwamba himself acknowledges that this latter is an additional step: "It is one thing to recognize rain as rain. It is quite another thing to understand why it rains and what rain really is. This understanding is not given in the perceptual forms themselves. It results from an intelligent insight into the connections between phenomena. It is literally added to the data of perceptual forms and events." (2001, p. 83) And I argue that we do not have to go as far as that in order to find that Polanyi's theory is defensible.

3.6 Conclusion

Mwamba interprets Polanyi's theory of tacit knowledge sympathetically.[25] But his interpretation still needs to be fine-tuned. He touches on areas of the theory that are central, but at the same time he omits some that would have helped the theory to be better understood and perhaps better received. The effort of Mwamba could have been to understand Polanyi's theory of tacit knowledge from the point of view of an analytic philosophy of science. The problem with this approach is that analytic philosophy of science has its own conceptual baggage to deal with, questions in epistemology, philosophy of language, etc. that have not been resolved

[24] Mwamba citing from Harré. (Harré, 1982)

[25] A few other sympathetic interpreters of Polanyi's theory of tacit knowledge include: Stefania Ruzsits Jha, (2002) and H. Prosch, (1986). They too have their strong points and areas where I would argue that they have misinterpreted Polanyi's theory of tacit knowledge. But to include this treatment at this point would lead me far afield.

and have almost stalled. It would not serve the purpose of understanding the theory of tacit knowledge adequately if this baggage is brought along to an interpretation of the theory. And if at the end of the effort it is found out that the theory of tacit knowledge sits uncomfortably with a few areas of analytic philosophy of science, as I argue throughout that it does, then the theory needs to be taken on its own merits. I foresee a possibility of breaking new ground and finding solutions to age-old questions in analytic philosophy of science if a sympathetic and correct interpretation of Polanyi's theory of tacit knowledge is made.

Chapter Four: A response to Alan Musgrave on Polanyi's theory of tacit knowledge

4.0 Introduction

In accounts on the methodology of science, Polanyi's theory of tacit knowledge receives scant, oblique, and often dismissive consideration. Many methodologists of his time simply ignore him. Some, mainly of the Popperian streak, address him often in the ways I mention above. It is therefore a privilege to find a direct, sustained and systematic response by a Popperian – Alan Musgrave – to Polanyi's theory of tacit knowledge. Revealingly, the contrast there is in titles between Alan Musgrave's (unpublished) doctoral thesis, "Impersonal Knowledge: A Criticism of Subjectivism in Epistemology" (1968), and Michael Polanyi's main work, *Personal Knowledge, Towards a Post-Critical Philosophy* (1958), is not coincidental.

Alan Musgrave sets out, in the main, to critique what he presents as a subjective element in Michael Polanyi's epistemology of science. On his part, Musgrave belongs decidedly to the school of the Popperians – disciples of Popper who subscribed to the latter's idea of falsificationism and by extension, of critical rationalism (henceforth CR, others include David Miller, etc.). CR is broadly speaking falsificationism applied to the whole of rationality beyond merely scientific rationality. Falsificationism is the position that theories in science are bold conjectures that are either corroborated or falsified depending on how they fare when faced with evidence in crucial experiments. When this view expands into critical rationalism, it puts a certain understanding of logic – deductive logic understood as a method for 'probing rather than proving' (Miller, 1994, p. ix) at the centre of reasoning.

In his description of CR, David Miller (1994, pp. 66-67) holds that criticism is naturally central to CR. The way to critically examine any hypothesis that is presented as a candidate for truth is not on the grounds of whether or not it is *actually* true, but whether, if false it can be overthrown and rejected. Thus logic comes to play a crucial role in CR. In science, conjectures or hypotheses are proposed and from these, further conjectures are derived. The conjectures can be tested, e.g. by experience.

When the consequences of the derived conjectures are found to be false, they are to be rejected together with the original conjectures. This can be done because valid derivations (i.e. deductions) transmit truth and retransmit falsity backwards from the conclusion to at least one of the premises. And so logic (also called reason) is given a central role in CR in criticizing the proposed hypotheses. (Miller, 1994, pp. 66-67) Deductive logic understood in this light as the core of reason (Miller, 1994, p. ix), is seen as providing an impersonal and objective method for being rational. In that vein, it becomes understandable why proponents of CR are intolerant to whatever they perceive as subjectivism, for to them subjectivism leads to relativism. In contrast, objectivity is the ideal and standard that governs the probation of scientific hypotheses.

At some points of his work, Polanyi addresses the work of Karl Popper directly or in general on the logic or methodology of science (Polanyi, 1958, p. 167). Polanyi is opposed to the Popperian misrepresentation of science which depicted science as 'objective' in the sense of being detached and detachable from the scientist and thus impersonal. In particular, he finds fault with falsificationism and its attendant CR. He thus advocates for "personal knowledge" based on his theory of tacit knowledge as a more accurate representation of how science works. I make the conscious assumption that Musgrave's doctoral thesis written at the suggestion and the guidance of Karl Popper himself suffices as a representative Popperian response[26] to the Polanyian core position of tacit and personal knowledge and thus of the theory of tacit knowledge. The response is a refutation of Polanyi's tacit and personal knowledge. And so the question is: How adequate is this Musgravean response and by extension the Popperian or CR-ist response to Polanyi's theory of tacit knowledge and personal knowledge? A further preliminary assumption I make is that any methodology of science will be judged upon how closely it comes to balancing both a normative account of science and an informative account of how science actually works.

4.1 Musgrave's global understanding of Polanyi

Musgrave describes Polanyi's view of the epistemology of science as a "subjectivist theory of scientific objectivity". (Musgrave, 1968, p. 129, and Chapter VIII) Instead, he argues for impersonal knowledge. Impersonal knowledge, according to him, is objective, public or interpersonal knowledge. It is the more fruitful of only two possible methodological approaches to knowledge[27], the other

[26] The assumption is based on the Acknowledgements by Alan Musgrave in his Doctoral Thesis, where he expresses his double indebtedness to Sir Karl Popper, both as the one who suggested the topic to him and being the source of the views defended in the Thesis (!). That is in addition to much advice, encouragement, helpful criticisms and suggestions throughout the work. This should suffice as a basis for the claim that Musgrave's thesis is in a true Popperian voice.

[27] It is worth noting that Musgrave argues in terms of epistemology as a whole. Even with this holistic view of epistemology, Musgrave *et al.* include the epistemology of science as a first among equals, as epistemology at its best – epistemology to emulate. The question is: Is the epistemology of science distinguishable from general epistemology? There are indications that in mainline traditional approaches to epistemology, the two are *distinguishable*. The epistemology of science seems to be able to proceed with a working definition instead of the definitive truth. General epistemology, on the other hand, can afford to occupy itself with ideals of truth and also to pick and choose between

approach being the personal, psychological or subjective approach. Impersonal knowledge, as Musgrave presents it, is directly opposed to personal knowledge in that the latter is subjective. In his words:

> "An epistemologist can approach human knowledge from one of two very different points of view. He can approach it from the personal or psychological or subjective side, and consider knowledge as systems of ideas or beliefs in the minds of human beings. I shall call this the *subjective approach to knowledge*, or *subjective approach in epistemology*, and shall sum it up, very crudely, in the slogan 'Knowledge is something in people's minds'. One can also approach knowledge from the interpersonal, or public, or objective, side, (*sic.*) and consider it as systems of statements, or propositions, or theories. I shall call this the *objective approach to knowledge*, or the *objective approach in epistemology*, and sum it up, again very crudely, in the slogan 'Knowledge is something in books'." (1968, p. 8)[28]

To be fair to Musgrave, he does admit that he is not seeking to discuss which of the two approaches he cites captures the fundamental nature or essence of knowledge. He admits that knowledge is both subjective (personal) and objective (interpersonal). Rather, he hopes to show that objective, impersonal knowledge is a more fruitful and accurate way to describe the way science works. (1968, p. 8) That is a weaker position than to attempt to identify the fundamental nature of human knowledge in science. And this weaker position could be a way to find common ground between Musgrave and Polanyi. But again, as a general remark, Musgrave misunderstands 'personal' in Polanyi's notion of tacit and personal knowledge.

First of all, Musgrave conflates *subjective* knowledge with *personal* knowledge. As long as Musgrave considers that Polanyi is a subjectivist, Musgrave misses the point of Polanyi's notion of tacit and personal knowledge. Polanyi would

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plausible and implausible methods of arriving at the desired truth. What is more, while the epistemology of science works close to phenomena, general epistemology can afford to be abstract from any phenomena and rely entirely on some given rules of logic.

[28] Emphasis in the original.

agree on the one hand that subjectivism is counterproductive to science (as Musgrave rightly argues). Polanyi asserts on the other hand that tacit and personal knowledge is at the core of scientific practice properly understood and represented. He holds on this matter that the difference between personal and subjective knowledge is not merely one of degree but one of kind. In subjective knowledge, there is no intellectual commitment to the truth i.e. what is known as being capable of unfolding in yet unpredictable ways in the future. Subjectivism further lacks universal intent – the hopeful drive to discover (passively) and to realize (actively) that what is held by as knowledge by the person will find corroboration in the community of knowledge (e.g. a scientific community) and universally. (Polanyi, 1958, pp. 65, 201)

Musgrave could respond to this criticism by pointing out that even granted the possible misunderstanding within which he may have conflated subjectivism with 'personalism' in knowledge, his criticism goes even deeper. He is opposed to the whole 'psychologistic' approach to knowledge (a criticism that I treat below). Those who subscribe to the 'psychologistic' approach to knowledge focus more on the knower and on the psychological aspects of knowledge than on knowledge itself. He could thus reply that the above criticism does not take his suggestion of objectivism seriously enough. He could maintain that it is possible to think of contents of knowledge (objective facts) without thinking of the knower (subject, person, etc.).

Epistemology, and especially the epistemology of science *should* (and in the CR-ist view *does*) concern itself with these *objective* contents rather than on the processes by which the scientist arrives at those contents. In that sense, theories of science would be evaluated, justified or given up, on the merits of the grounds of their contents rather than on the grounds of the person of those who propound them.

At the level of the justification of scientific theories (which is one of the proper subject of the philosophy of science), a consideration of the persons of those propounding theories would lead to subjectivism and consequently to relativism in the following manner. Relativism results when each individual scientist holds on to her theory as true even if it was contrary to another theory. And so, Musgrave would argue, for the sake of ensuring the conditions of the possibility of science, objectivism is a better option than subjectivism. Science is about the truth or the falsity of theories propounded within it. And in the case of subjectivism and resultant relativism, a theory cannot be tested for truth or falsity.

4.2 Contextualizing Musgrave's accusation of 'psychologism'

This is how Musgrave comes to accuse Polanyi's epistemology of 'psychologism'. Musgrave claims that the epistemologist who is concerned about the *contents* of her mind must end up in methodological solipsism when she considers that all that she knows are the contents of her mind, even when these contents are beliefs about the world *outside* her mind. (1968, p. 20) When the epistemologist has turned her attention to the contents of her mind, she faces the problem of how to catalogue the 'raw materials' of her knowledge and to come to a way in which she can show how the rest of her knowledge is derived from this catalogue. She is concerned, according to Musgrave, with the explaining how the contents of her mind come to be there. This problem, according to Musgrave "... is, properly speaking, a factual problem of psychology. We might call it the *problem of the psychological genesis of beliefs*, or the *problem of the psychology of discovery*," (1968, p. 22)[29] or a "factual problem concerning knowledge as a psychological entity." (1968, p. 48) And so the initially methodological solipsist becomes an actual epistemological solipsist when she cannot really know, by inspecting the contents of her mind, whether anything exists outside the contents of her mind. (1968, p. 26)

The methodological turned epistemological solipsist must deal with an additional problem besides the *genesis* of the contents of her mind, namely how to *justify* her beliefs. To Musgrave, traditional epistemology has often fused and thus confused these two clusters of problems. They should be kept apart. Addressing Polanyi's epistemology, Musgrave declares that the focal point of his own criticism of Polanyi's theory of knowledge (among other 'subjectivist theories of knowledge) is "... the contention that we must distinguish sharply between two sets of problems: first, psychological problems concerning *subjective knowledge* (ideas or beliefs); and second, non-psychological problems concerning *objective knowledge* (the contents of ideas or beliefs)." (1968, p. 47) The problem of the justification of beliefs is non-factual and non-psychological. It deals with truth and falsity and with the relation between a given set of beliefs held by one knower and other beliefs held by other knowers. It is a "problem about knowledge approached objectively." (1968, p. 49) Musgrave thus argues that the answer to the psychologistic tendency is to insist on objectivity.[30]

Musgrave does not come up with a clear definition of 'psychologism. E. Sober is an example of those who make the accusation of psychologism about other epistemologists. Sober defines 'psychologism' as "... a family of views, all tending

[29] Emphasis in the original.
[30] Gregory Currie has critically investigated the objectivist trend in Frege and Popper and he addresses some of the positions of some Popperians including Musgrave. (1978)

to downplay or deny distinctions between epistemology and logic on the one hand and psychology on the other." (Sober, 1978, p. 167) Sober then clarifies that psychologism breaks down into two forms. First there is *epistemological psychologism*, which "... denies that there is any question of justifying a logical rule or epistemological maxim above and beyond the question of whether it is in fact followed by practice". Besides that, there is *metaphysical psychologism* which holds that "... the laws of logic and the characterization of rationality that epistemology seeks to formulate are *about* human mental activity." (Sober, 1978, p. 167)

This definition of psychologism in both of its manifestations fulfils the purpose of this discussion for two reasons. First of all, it shows what the accusers think is a problem with those they accuse of psychologism, namely that these latter do not make the *needed* difference between psychology and logic/epistemology, and secondly because it shows that in his critique of Polanyi's theory of tacit knowing, Musgrave is referring to epistemological psychologism.

4.2.1 Roots of the psychologism accusation in Reichenbach and Popper

Musgrave fits in a wider context of philosophers arguing against 'psychologism'. The accusation of psychologism spans over a long period in the history of philosophy. A few lines would therefore not do enough justice in attempting to address it. But what is within the ambit of this discussion is to outline a brief history or genealogy marking the major positions, with the intention of finding a fitting response using Polanyi's theory of tacit knowledge. Thus far, the direct source of Musgrave's psychologistic accusation in Polanyi's theory of tacit knowledge is Karl Popper. (Popper, 1959, p. 99, 1963) For Musgrave, Popper has picked on a salient point when he points out that both empiricist and intellectualist classical epistemologists have confused two problems: the justification of beliefs on the one hand and the genesis of beliefs on the other hand. According to Musgrave's interpretation of Popper, the classical epistemologists (both empiricist and intellectualist) have tried to answer questions about the two areas in one stroke by providing what Popper calls 'sources of knowledge'. Thus beliefs can be psychologically derived from these 'sources' and the reference to such 'sources' should provide the logical derivation or justification of the beliefs. For Popper, both the empiricists and intellectualists agreed that the two questions could be answered in one stroke, even though they disagreed on the nature of the 'sources'. (Musgrave, 1968, p. 49)

Popper's analysis (Popper, 1959, 1963) and consequent attack on psychologism in turn refers to and shares something with a similar analysis by Reichenbach. Reichenbach separates the field of the physicist from that of the philosopher. He traces the division of labour between the physicist and the philosopher to the limitation of human capacities. In that sense, the goal of the physicist is to discover relations that can be empirically verified. The methods of the philosopher are analytical and critical and not predictive. On the other hand, the physicist may be guided by a certain faith and guesswork, but the philosopher is not interested in the thought process that leads to the discoveries. The philosopher deals with the logical analysis of completed theory and with the relationships that help establish the validity of the completed theory. The physicist deals with the context of discovery, and the philosopher deals with the context of justification. (Reichenbach, 1949, p. 292) This is an application of a more general view by Reichenbach as I expose below.

It must be acknowledged that Popper does not reproduce Reichenbach's position in every aspect. Reichenbach holds that science is in need of a principle of induction, and no serious scientist can contest the need for a principle of induction. Popper disagrees with the need of such a principle and opts for deductivism. He argues that such a principle of induction would never be free from the problem of induction as discussed by Hume. Another point on which Popper disagrees with Reichenbach is the view that epistemology is engaged in a 'rational reconstruction', unless according to Popper this reconstruction is a "logical skeleton of the procedure of testing" a proposed hypothesis. (1959, pp. 31-32)

Nevertheless, what both Reichenbach and Popper share is the view that the epistemology of science should be rid of *psychologism*. Popper proposes a logical approach. While he agrees with Polanyi that there can be no *logic* of discovery, logic referring to deductive logic, the two differ in their responses to this belief. Polanyi is willing to concede that an alternative to 'logic' is not necessarily irrational, while Popper holds that anything outside deductive logic is irrational. And so Popper claims that that discovery contains an element of irrationality – a creative intuition, "an intellectual love of the objects of experience". (1959, p. 32) For Popper, this part of doing science has no place in epistemology because epistemology deals with proposed hypotheses and their logical properties and relations with other similar hypotheses. Polanyi, on his part is willing to rethink the traditional notions of epistemology in order to account for what has for long been downplayed in an objectivist understanding of epistemology.

As indicated above, further backwards in the genealogy of the accusation of psychologism, and more generally on the difference between the context of discovery and the context of justification as applied to the difference between psychology and epistemology, Reichenbach holds an interesting view. He argues that because the structure of knowledge is the system of connections as it is followed by the thinking process, there is a temptation to understand epistemology as a description of the process. Rather, epistemology is about the system of logical connections and not about the actual thinking process. The psychology of the thinking process fluctuates and may skip logical operations needed for a complete exposition. So epistemology should be separated from psychology. Epistemology is therefore normative – constructing a thinking process as it ought to occur linking the starting point to the end result if it is to be a consistent system. Epistemology is therefore a substitute of the real process – a '*rational reconstruction*' – a phrase carried over from Carnap's *rationale Nachkonstruktion.* As a background to the position that Popper and Musgrave take up, Reichenbach continues to claim:

> "Many false objections and misunderstandings of modern epistemology have their source in not separating these two tasks; it will, therefore, never be a permissible objection to an epistemological construction that actual thinking does not conform to it. ... In spite of its being performed on a fictive construction, we must retain the notion of the descriptive task of epistemology. The construction to be given is not arbitrary; it is bound to actual thinking by the postulate of correspondence. It is even, in a certain sense, a better way of thinking than actual thinking. In being set before the rational reconstruction, we have the feeling that only now do we understand what we think; and we admit that the rational reconstruction expresses what we mean, properly speaking. ... If a more convenient determination of this concept of rational reconstruction is wanted, we might say that it corresponds to the form in which thinking processes are communicated to other persons instead of the form in which they are subjectively performed." (Reichenbach, 1938, p. 4)

While this view by Reichenbach is widely held among the CR-ists, they do not analyse or explicate the phrase 'postulate of correspondence'. This is an important term because it is the *link* between epistemological theory as proposed by

Reichenbach and the CR-ists, to actual thinking. It prevents the logic-driven epistemology from becoming totally out of touch with and irrelevant to *actual* thinking. That is why the concept of 'postulate of correspondence' needs to be explained. Does it, for example operate like an analogy between the thinking process and (deductive) logic? Is it a set of highly abstract logical relations that both the reconstruction and the actual process must correspond with? Is it, in the language of the CR-ists a part of the psychology of knowing?

But in the long run again, it is difficult to see the virtues of this concept. It multiplies the number of *logical relations*. Beyond the logical relations between the logic-driven rational reconstruction that is the subject matter of epistemology and the actual thinking process, there are the relations between each of these with the concept itself. In other words, there are three 'entities' to deal with, namely: the actual thinking process, the rational reconstruction, and the postulate of correspondence. It is an unnecessary introduction to which Occam's razor is to be applied. The problem lies in trying to separate the psychological from the epistemological while still admitting that the two are related. The solution should be integrative – in finding a way to see them as mutually beneficial to us in our understanding of how we know. I argue that Polanyi's theory of tacit knowledge provides such an integrative solution.

4.2.2 Psychologism in western philosophy – Martin Kusch's account

Following up further backwards on the genealogy of the anti-psychologism argument, in his turn Reichenbach owes the view of separating psychology from philosophy in general (and the philosophy of science in particular) to an earlier tradition. There is before Reichenbach and Popper an effort to rid philosophy of a 'psychologistic' trend. Kusch has traced the history of psychologism. The position of philosophy relative to psychology as two very distinct (even irreconcilable) areas of inquiry is a certainty only to a few traditional minds. And the developments in cognitive science have further made the certainty less clear. To these traditional minds, Bloor points out in a foreword: "[Philosophy], ... deals with knowledge, with what makes something into knowledge rather than mere belief. Psychology, by contrast, deals with the processes and conditions of coming to know. These are quite different and disjoint concerns. Psychology deals with causes, philosophy with reasons. Philosophy concerns truth; psychology cannot rise above belief and taking-for-true." (Kusch, 1995, p. xi)

As Kusch points out, western philosophy in general has from the end of the 19th Century to the beginning of the 20th Century and until recently been "... hostile to the idea that central epistemological, logical or metaphysical questions could be answered by the natural or social sciences." (1995, p. 1) According to him (1995, pp. 2-3) psychologism first broke into German philosophy around the time of the death of Hegel (1831) when idealistic philosophy and the whole philosophical enterprise fell into disrepute, and was being replaced by natural sciences.

Philosophers then adopted a naturalistic or positivistic approach, namely that philosophy too should be subjected to the ideal of knowledge and the justification of the empirical sciences. It was at this time that Feuerbach, Marx and Engels went as far as developing materialistic philosophies. Thus philosophical problems in epistemology, logic, ethics, etc. would have to be solved by empirical research. Logic was particularly approached as a branch of psychology – as an empirical generalisation of the way human beings reason – by British empiricists like Locke, Hume and Mill and by some German logicians like Erdmann, Lipps and Sigwart.

Frege and Husserl come to rescue philosophy from this erroneous path when they attack this naturalism in philosophy around 1900. Both of them argued that the doctrine of psychologism is self-refuting. According to Frege, logic and arithmetic are not about mental contents. Even if human psychology changed, and even in the absence of thinking animals, the sum of two and three would continue to be five. And so, arithmetical and logical statements do not depend on the psychology of thinkers for their truth value. Logic and arithmetic are not about mental entities for they do not depend on mental entities. (Sober, 1978, p. 167) Kusch reports that Frege was initially ignored by mathematicians and logicians on the continent. His critique of psychologism was adopted by Russell, Moore, Wittgenstein and many of the analytical philosophers from Carnap and Popper, to Sellars. (1995, p. 3)

Yet in fact, Frege and Husserl did not solve the problem of psychologism, for it continued to be a question in the philosophies of Reichenbach, Popper, and Musgraves and the CR-ist school of thought. Neither do the CR-ists, and particularly Musgrave and Popper, give a convincing remedy following this diagnosis. One reason for the continued existence of this 'problem' is, as Kusch and Notturno point out, that the accusation of 'psychologism' is a blanket and systematically vague term with no clear agreed definition that has been used *even* against self-declared anti-psychologists (like Frege and Popper) those who are purportedly fighting it. (Notturno, 1985, p. 9, Kusch, 1995, pp. 4-5)[31] The accusation of 'psychologism' fails the objectivist or precision test set by analytic philosophy. It is not a precise term. It stands for everything that is not objectivist in

[31] Kusch has given a series of varying definitions of 'psychologism'. (1995, pp. 4-6) On top of that, he has provided a table of philosophers accused of psychologism and those who accuse them. According to that list, and cited by Kusch, Frege is accused of psychologism by Kitcher, (1979). More interestingly, Popper is reported to be accused of psychologims both by Pandit (1971, p. 89) and by Willard (1984, p. 200).

the CR-ist sense of objectivism (which in the epistemology of science and in methodology is seen as concerning logical relations between hypotheses by CR-ists after Popper). Given such a wide scope for the application of the term, it denotes subjectivism (Musgrave, 1968), personal and tacit knowledge (as misunderstood by Musgrave), the psychology of science, etc.

4.2.4 Musgrave on psychologism in Polanyi's theory of tacit knowledge

Thus in line with the anti-psychologistic tradition traced above, Musgrave is bent on dissociating the origin of interesting ideas (e.g. scientific discoveries: how they arise in the mind of a scientist) from what he calls the 'objective merits and demerits' of the ideas. He maintains that the origins can shed little light on the merits and demerits. A fitting counter-argument to this objectivist position by both Popper and Musgrave is given by Currie. Currie objects to Popper's and Musgrave's claim that epistemology, even the epistemology of science, is about the logical interrelations e.g. between hypotheses.

Although it would be unreasonable to include it in the results of a test that someone felt a deep conviction about the results, in empirical science it is assumed that the observation has been reliably carried out by somebody reliable. (Currie, 1978, pp. 155ff.) If the rationality of our methods (e.g. of justification) depended entirely on some objective logical relations, then we would not be able to rule out the tossing of a coin to decide between competing theories. The tossing of a coin is objective given that we know it is a fair coin and the method is agreed upon beforehand by all concerned. On the contrary, in the long run it matters that a scientist has carried out the observations and registered the impression the observations make on her and how strong or weak they are. Currie calls this an argument for a 'whiff of subjectivism' – i.e. subjectivism as Popper and Musgrave understand it – observational basic statements made based on perceptual evidence. (Currie, 1978, pp. 155-165) And yet this is not to argue that our senses give us indubitable knowledge. Our senses do mislead us at times, but again they are the only means we have of ever coming up with so-called 'objective' theories about the world.

When Musgrave and the CR-ists limit their epistemology of science to dealing with the logical relations between hypotheses, there is a hidden assumption that science comes up with a set of indubitable truths upon which, then, the epistemologist can apply the logical relations. There must, in the limit be a hierarchy of hypotheses with those that merit being manipulated logically on the one extreme and those that are not worth such manipulation on the other end. Otherwise the epistemologist would have an infinite task to apply her logical toolbox to all the available hypotheses.

Yet as soon as this epistemologist admits to a hierarchy, then the criterion of placing the various hypotheses in their place in the hierarchy points towards some empirical report about how the hypotheses were arrived at or chosen. Yet such a report would fall under psychologism/subjectivism, for it would be introducing the 'sources' of the hypotheses. Currie has argued for a whiff of subjectivism on those lines. In my opinion, Currie calls this a plea for a whiff of subjectivism in order to fit within the mindset of the CR-ists that divides knowledge into objective and subjective. Polanyi's theory of knowledge allows for a third alternative – the personal element in knowledge which as I discussed in an earlier chapter accounts for the commitment of the knower to knowing the truth about reality as well as keep her open to the possibility that she may be wrong.

On the other hand, let us imagine that science arrived at a set of indubitable theories that were proved to be *true*. Still, the objectivist epistemology that Musgrave and the CR-ists are proposing would not be fruitful. Neither the set of theories nor the logical relations between them can be rendered fully objective. As I have discussed earlier in outlining the nature of tacit knowledge, articulate or 'objective' (i.e.in the objectivist view) knowledge relies on inarticulate, tacit elements. There cannot be rendering of knowledge that makes all knowledge objective in terms of being articulate. And so both the set of would-be 'true and indubitable' theories, and the logical relations would rely in turn on inarticulate or tacit elements.

4.3 An examination of proposed non-psychologistic questions

In the CR-ist and anti-psychologistic effort of detaching what he calls psychological aspects from the epistemological ones, Musgrave holds:

> "I ... argue that when we evaluate our knowledge, this has nothing to do with knowledge considered as a subjective or psychological phenomenon. In order to evaluate the content of a belief, or a theory, we ask such questions as "Is the theory true or false?", "Does it explain a lot?", "Does it solve its problem?", "Is it an advance on previous theories?", "Is it compatible or incompatible with previous theories?", "Does it perhaps follow from some other theory?" or "Is it consistent with the available evidence?". Now none of these questions has anything to do with the way in which the inventor of the theory, or anybody else, came to believe the theory, or with the strength of anybody's belief in the theory, or even with whether anybody believes the theory at all. The questions all concern objective properties of theories, which hold independently of anybody's belief in those theories, and even whether anybody is conscious of the theories at all." (1968, p. 65)

This cited paragraph shows a number of problems with the CR-ist approach to the epistemology of science, especially in its critique of psychologism. In the first place, if the so-called objective properties are detachable from the beliefs of the scientist to the extent that the scientist need not believe in her positions, the question is: what use is such objectivity at all? For as Polanyi endeavours to show, the scientist does *believe* in her positions and her *belief* matters to her in the process of coming up with a theory. There is commitment to her theory or hypothesis, which commitment is useful in motivating her research, in arriving at a solution to her question, and in arguing for it and publishing it.

Besides commitment, the scientist engages in evaluation and judgment at various steps of her research. Besides using her evaluative judgment to select the methods of observation, experimentation and calculation to undertake, and the interpretation of the results, she uses it in interpreting the given laws. (Dewey, 1939, p. 174) The evaluative judgment is important in helping the scientist to decide whether or not she has arrived at a close (however temporary) of her inquiry. It is by these evaluative capacities that she can recognize the 'beauty' or 'elegance' of a hypothesis or theory in as far as these refer to a harmonious ordering of various facts. (Dewey, 1939, p. 176) These are used by the scientist as pointers to the 'truth' of the hypothesis.

Now, one of the non-psychologistic questions that a theory of scientific knowledge is supposed to ask, according to the citation above, is whether a given hypothesis is *true* or *false*. The background to this question is that science has got a set of *truths* which are the subject matter of the epistemology of science. But this position would be contradictory to the whole falsificationist programme for in this programme science is only ever approaching the truth asymptotically. It arrives at verisimilitude. Science is a work in progress. For example, such a question can hardly be asked of Newtonian mechanics or even relativity without attaching an empirical report to the answer, e.g. by qualifying them as beliefs or as representing the state of science thus far. But the empirical report would be psychologistic because it would involve the *sources* of the empirical knowledge.

So far, if such commitment and evaluative judgment fall under the category of psychologism as Musgrave and the CR-ist tradition would hold, then we have a kind of psychologism that is useful for epistemology to understand better and explain more fully the workings of science. Instead, the position that Musgrave has just explained in the paragraph cited leaves us with an epistemology of science that is *detached* from what scientists actually do. What matters is the way theories hang together logically. In that case, what is of primary importance are the logical relationships between theories. The substance of the theories is of secondary importance. Of no relevance whatsoever is the process through which the person of the scientist is involved in coming up with the theory.

The question is: What function has such an epistemology of science? It is neither descriptive of the activity of the scientist, nor is it prescriptive/normative of what the scientist ought to do. There may be moments in science when a set of hypotheses in one scientific view cannot be logically reconciled with another set of hypotheses in another view, when scientific views clash. And yet scientists may hold on to their position despite the fact that it does not logically fit with another position. Some scientists may even shift from one view to the opposing one without thereby undermining their credibility as scientists. An example could be the corpuscular versus the wave theory of light. Scientists could shift from one view to the other in order to explain the behaviour of light in different circumstances. When a study of the logical relations between hypotheses is more important for the epistemology of science than the actual process, we end up with an epistemology that is not informative about the activity of science. This is a weakness of Popperian falsificationism understood as a description of the methodology of science.

4.3.1 Examining '*p* is true' in a context of science

Musgrave disagrees with Polanyi on the *sincere* assertion of the statement '*p* is true'. For Polanyi, when this statement is asserted sincerely, it shows the belief of the one who makes the assertion. (Polanyi, 1958, pp. 27-30) For Musgrave, that is an introduction of subjectivism/psychologism into the epistemological notion of truth. Musgrave claims:

> "Clearly, what has misled Polanyi is that if a person says, sincerely, that a theory is true, then he does of course, by using this form of words, express the fact that he believes in that theory. But he is *not only* expressing this psychological fact – he is saying something about the theory, that it has a certain objective property. The making of assertions about the objective properties of theories is, no doubt, correlated with psychological attitudes towards the theories. But to *define* '*p* is true' as 'I sincerely believe *p*' is to abolish an objective property and to substitute for it a psychological attitude." (1968, p. 139)

While it is true that there is an 'objective' property *à la* Musgrave (or in the CR-ist objectivist sense) in the statement '*p* is true', Polanyi's statement needs to be seen in its context. In a scientific context, the statement that '*p* is true' cannot be detached from the belief of the one who asserts (e.g. in a hypothesis) that *p* is true. In the context of science (e.g. at conferences, etc.) it would introduce a lot of distractive complications if sincerity and the belief in one's statements were withdrawn from assertions like '*p* is true'. The statement may end up being rejected as 'false' by the scientific community. But again it will have been held sincerely by the one asserting it. Sincerity goes hand in hand with commitment which as discussed above is needed in scientific inquiry.

Musgrave avers on this point that the single main objection to all subjective theories of truth, including Polanyi's, is that they involve *relativism*. According to Musgrave, if "*p* is true" means simply "I believe *p*", then the same statement can be true for one person and false for another person. We end up losing the objective standards for deciding the truth. (1968, p. 142) Musgrave's objection is warranted in an objectivist approach. But within the epistemology of science, the situation he is describing is not necessarily one of relativism.

Various and even at times contradicting positions can be simultaneously held by the general scientific community with each of them solving various sets of problems. The Ptolemaic position co-existed for some time with the Copernican position and in each of them various problems were solved. One or a few of the competing positions may end up asserting itself. What makes this possible is again the sincerity with which the scientists carry out their inquiry because they are committed to discovering the truth about the world and they remain open to unforeseen manifestation of the more accurate position in the future. This latter attitude is what Polanyi calls universal intent, as explained in the discussion of the nature of tacit knowledge.

4.3.2 Conflation of justifications - of attitude and of theories

A more serious criticism of Polanyi's theory of knowledge (and those classified as subjectivists) by Musgrave is the one of conflating two kinds of justification. Polanyi is said to conflate the justification of attitudes towards theories with justification of theories. This obscures the fact that the justification of attitudes depends upon the circumstances of the person considered while the justification of theories does not. (Musgrave, 1968, p. 301) The criticism is serious because if such a conflation happens, then relativism is truly introduced in the process of justification because personal idiosyncrasies are introduced. However, the picture surrounding justification is itself complex. In as far as justification is understood as a logical process by which scientific knowledge claims are assessed and vetted, this criticism by Musgrave is a valid criticism. The conflation of justifications as described above is a serious flaw. But as long as justification is understood as the acceptance of a position by a significant number of scientist and the dependence on that accepted position for further research, and given positive findings (Collins & Pinch, 1998) then the conflation is not a problem. Sometimes in the actual practice of science, theories have been accepted before the due empirical data has been 'justified'. On the other hand, there have been cases in which data has been overlooked which should have been evidence enough to abandon a given theory.

Left at this, it may appear that science is capricious in the ways in which it chooses to follow strict logical procedures of justification at times and then following some kind of intuition at other times. This would be an irrational representation of the way science functions. I argue that Polanyi's theory of tacit knowledge provides the needed rationality that brings together the two apparently separate methods of justification. By tacit knowledge, scientists will *know* to choose when a proposed theory is acceptable without further rigorous logico-deductive tests, and when a proposed theory must be subjected to further testing before it becomes part of the body of scientific knowledge. And had Polanyi's theory only been brought in at the moment of justification, it would be an ad hoc strategy. But Polanyi's theory shows that tacit knowledge pervades all knowledge. Tacit knowledge plays a role in the choice of research question, helping in choosing an interesting and yet answerable question. It helps in choosing the methods of research, in reading off, evaluating and deciding on data, in drawing conclusions and in the commitment to argue for these conclusions as part of knowledge. In the end it is involved in helping accept or reject conclusions of a given research.

Overall, therefore, there is a change of perspective from one in which the two methods of justification are strictly separated to an approach in which the two (and perhaps more) methods can be seen as mutually complementary or intertwined. Polanyi is not arguing for the replacement of logico-deductive rigour with a court-style consensus among scientists. His approach shows that the reduction of justification to this logico-deductive method is unwarranted. The push for 'objectivism' in this sense is not helpful for understanding how science works.

On this note, Musgrave may still insist on the impersonal nature of the justification of theories thus:

> "... the context of criticism remains 'impersonal' in the sense that *what* is critically discussed is the theory itself and its objective properties, rather than subjective facts about adherents or opponents of the theory. This can be recognised by the participants in a critical debate, who will therefore try to propose objective *arguments*. Rationalists too have inarticulate visions of reality, but they try to transform them into explicit, discussable, theories. Rationalists surely have subjective feelings of conviction or doubt, but they try to transform them into objective arguments." (1968, p. 316)

Two responses can be given to this Musgravean position. First of all, it needs to be emphasized that in the practice of science, the contrary of 'objective' is more nuanced than being merely 'subjective'. Polanyi's theory of knowledge emphasizes the personal role of the scientist. Such a scientist brings her inarticulate knowledge to bear on her scientific work. She is committed to discovering the truth about nature as it unfolds in unexpected ways. This is not an 'objectivist' approach, but neither is it 'subjectivist'.

The second response is that Musgrave himself lays emphasis on such terms as 'argument', 'conviction', etc. This emphasis concurs exactly with what Polanyi is advocating for when he proposes a central role for intellectual passions. What guides the participants in a scientific debate to rephrase their positions into 'explicit, discussable theories' should not be glossed over as being merely subjective or psychological in a bid to arrive at the purely objective and articulate. Certainly, when we are seeking to understand the process of justification, we cannot overlook these efforts. In the above citation, what is logically deductive does not need to be argued in terms of being defended. It is only demonstrated, and the interlocutor can only take it or leave it. But Musgrave himself admits to 'arguments' and 'convictions' in other words intellectual passion in science debates.

And in addition to this, let us say for the sake of argument that the parties in an argument about a given scientific theory managed to outline their argument so deductively, even then there would be the recalcitrant inarticulate that persists in any form of human or animal act of learning or attempt at articulation. This position has been discussed in the treatment of the nature of tacit knowledge.

Musgrave appears to be ready to rebut this position elsewhere by maintaining:

> "We *can* take science seriously if we adopt an objective approach to knowledge; if we begin, not by adopting epistemological solipsism, but from knowledge as a public, intersubjective affair. Here we insist upon the distinction between the objective merits and demerits of the theory considered, and psychological properties of anybody's belief in that theory, or awareness of that theory. We will find irrelevant, in discussing the theory, psychological facts about whether a person finds that for him the theory is dubitable or indubitable, certain or uncertain, self-evident or counter-intuitive. If we are

> interested in the theory, and its objective properties, then these psychological facts become uninteresting." (1968, pp. 83-84)

Here again, it is significant that Musgrave admits of the *intersubjective* nature of scientific knowledge. It would be interesting to find out how he manages to separate this intersubjectivity from what are the *subjects* that are guilty of *subjectivism.* A way out of this Musgravean impasse is to move away from 'subjects' and 'objects' to talk of 'persons', i.e. to approach scientific knowledge not with 'objectivism' as opposing 'subjectivism', but with *personal* and *tacit* knowledge. The *person* is imbued with intellectual passions and committed to universal intent, who is being proposed by Polanyi.

Further, the crucial questions here would be: Why are the so-called psychological facts 'uninteresting', especially given the fact (as discussed in the nature of tacit knowledge) that without them we end up with an incomplete methodology of science and in the light of the view that Polanyi hopes to be able to put these facts to good use in proposing a fuller and more accurate methodology of science?

Secondly, is Musgrave still interested in the scientist and her work? Or is he rather focusing on a version of science (a rational reconstruction) a step away from the scientist, which would be a creation of the philosophers of science? For it is difficult to imagine how a scientist would go on to publish what they actually doubted and still claim to be doing science. The philosophy of science can legitimately detach some aspects of science from the practice of science and propagate positions and theories about how these detached aspects of the practice operate. But what happens then is not a methodology of science provided by the philosophy of science. We end up with such methodologies as proposed by the verificationists, the Popperians, etc.

There are further grounds to consider that Musgrave may make a concession to Polanyi's position. Musgrave holds: "We cannot justify our theories but we may, by considering the present state of the critical debate about them, be able to justify our preference for one theory over some others." (1968, pp. 302-303) This may provide grounds to suppose that Musgrave concedes that at times the justification of the theories is not to be found in the ostensive arguments given, but in the 'state of the critical debate'. This state is not further described by Musgrave, but it may have similarities to what Polanyi describes as the intellectual passions that motivate debate, besides other such inarticulate factors.

On the whole, however, Musgrave attacks psychological attitudes because he contrasts them with 'objective aspects of our knowledge'. The former are thus unimportant in comparison to the latter and the growth of the latter. He does make some concessions, but remains attached to an objectivist understanding of how science works. Accordingly:

> "One must admit, however, that for the individual scientist and his work decisions about which theory to accept may play an important role. Some argue that a scientist cannot articulate and elaborate a theory unless he has a 'deep commitment' to its truth. If a scientist is like this, then for the good of the growth of objective knowledge, he should be left to his deep commitment.... But more important than attitudes are the objective views and arguments which may follow upon the adoption of the varying attitudes; and if *nothing* follows upon the adoption of some attitude, it remains a mere attitude and can, and should be, ignored." (1968, p. 304)

Polanyi's theory of tacit knowledge has two responses to this objection. First of all, personal epistemology is built on *commitment* neither to the personal attitude of the knower nor to self-standing propositions removed from the phenomena they are purported to represent. The commitment is to the phenomena under investigation as these manifest themselves in unforeseen ways in the future. The propositions may fall short and some of the knowledge may be inarticulable. But there is commitment to arriving at an understanding of the phenomena.

The second response is that the knowing subject, the person in Polanyi's theory of tacit knowledge, is *actively* involved in the act of knowing, because, as Polanyi holds and as discussed in the nature of tacit knowledge earlier, knowing is a *skill.* Right from the moment of a heuristics when a knower, for example a scientist, has a notion of the answer to a problem, she is committed or impassioned about moving closer to the solution. Her guide, according to Polanyi, is intellectual beauty. She is able to recognize the 'answer' to her inquiry guided by this notion of elegance (a very personal judgment).

This overall personal participation includes the most abstract areas of scientific knowledge. Even such abstract areas as mathematical physics assimilate experience to beautiful systems of indeterminate bearing even if their application to experience may be strictly predictive within certain not strictly definable conditions. (Polanyi, 1958, p. 320) And at the end of the research process, before the claims of the scientist become knowledge (i.e. through publication in articles and textbooks etc.) they are subjected to personal judgment by a body of other experts in the area of science. Thus both the process and the product of scientific inquiry are personal without thereby being subjective.

4.3.3 Musgrave's Parrot as an Illustration of his Core Position

Further, an illuminating quotation could help clarify Musgrave's position as he tries to keep the psychological separate from the epistemological/logical, and thereby preserve the objectivity of scientific statements. He holds:

> "At the risk of labouring the obvious, suppose a parrot were somehow to make noises which a physicist can *understand* as a statement of Newton's theory of gravitation. This is only possible because the *meaning* or *content* of Newton's theory is not *constituted* by any private goings-on which may, or may not, be correlated with the utterance. We can understand a theory, find it meaningful and interesting, examine it, test it, see if it is compatible with other theories, all without even considering any private goings-on. Newton himself, of course, was not a parrot, and when he uttered and wrote down his theory he was trying to articulate some thought-processes. But the *historian* of science begins with Newton's theory, which he can try to understand independently of Newton's thought-processes (to which he has no direct access). Moreover, it makes perfectly good sense to say of some utterance, that the person did not know what he was saying..." (1968, p. 74)

There is a problem with Musgrave's parable of the parrot uttering statements. The problem lies in the attempt to detach language from the speaker of a language. In attributing meaningful scientific statements to a parrot, Musgrave may seem to have succeeded in detaching language from the persons who use it. But has he? The only reason we can make sense of what the parrot is prattling is because there is the teacher of the parrot and us. On its own, the parrot only prattles. Turning this position around, we do not know whether other bird and animal noises are actually statements of some laws of physics, but they could be. In other words, what gives sense to the prattle of the parrot are the persons around it. The parrot makes noises whose closeness or similarity to our language allows us to attribute *speech* to the parrot in question.

At a deeper level, this Musgravean argument can be held for any form of meaningful phrases (poetry, rhetoric, etc.) that the parrot can be taught to repeat. But when it comes to making scientific statements, the *intent* to be believed cannot be overlooked on grounds of making objective and impersonal statements. They may be made in the most abstract terms, but controversy and argumentation about precision and truth of the statements are an essential part and parcel of the process of coming to scientific truth. The *intention* is crucial to their nature as scientific statements given the fact (discussed in the nature of tacit knowledge) that the scientist is guided by intellectual *passion.* One may argue that this position is about the intentional state and not about the personal component of a scientific statement. But that makes the example of the parrot even less appropriate for making scientific statements. If the teacher of the parrot fed the parrot a nut whenever it repeated the statements of Newton's laws, the 'intention' of the parrot in repeating them at any one time could be – not to make a scientific statement – but to induce the listener to give it a nut.

Musgrave does not succeed in salvaging this impersonalist theory of language by resorting to the analyses of Buehler and Popper on language. Buehler distinguishes three functions of language, namely: expression of a state, stimulation of a response, and description of a state of affairs. To these three, Popper adds a fourth which he describes as being eminently human: argumentation and explanation. Musgrave, and Popper before him (1963, pp 180-181) purports to show that in argumentation and explanation, what is taking place is a mere logical comparison of positions. This understanding of argumentation downplays the personal element. But something motivates the discussants to take their respective positions, even when they cannot articulate their position clearly enough in objective propositions. There is a tacit background even to an articulate discussion on abstract subjects. The intellectual passion that drives the discussion is eminently personal.

4.3.4 Musgrave's understanding of language and logic as an illustration of objectivism

Musgrave holds a naïve objectivism in regards to the way language conveys scientific truth. According to him, "language ... intuitively speaking, *transcends* the personalistic, subjectivist framework. It is through the use of language that subjects communicate theories ... argue ... explain problems ... in short, *share* their knowledge. Language enables subjects to go beyond their own knowledge, a private and personal affair, and to participate in *public* knowledge." (1968, p. 38) In contrast to this impersonal understanding of language, Polanyi proposes that there is a personal element – an element of judgment in the use of language. Whenever a person uses a word in a new context, she *adds* a new meaning to the word provided by her interpretation of the new context, while still remaining connected to the use of the word in the language until then.

Musgrave insists on there being 'fundamental objective properties of the *content* of a belief'. To him, these properties are: "its *truth or falsity* and its *logical relations* with other contents (or theories)." (1968, p. 48) Thus far, according to Musgrave, argumentation and explanation introduce autonomy to language, autonomy from the subject or user of the language. According to Musgrave:

> "For if we use a language to describe and to argue, then we can consider these descriptions and arguments independently of the user of the language, and his state of mind. This is connected with the fact that the fundamental properties of descriptions and arguments (the truth or falsity of a description, the validity or invalidity of an argument) are objective properties which can, and should be discussed *independently* of the psychology of the describer or arguer." (1968, p. 76)

But the same arguments can be used to arrive at the opposite conclusion, namely, that the personal element of language cannot and should not be ignored. Two adjustments need to be made. First of all, it should be pointed out that Musgrave is laying emphasis on the activity of *argument.* Polanyi's position on argumentation (as already discussed) is that something more than just the logical syllogism motivates the argument.

There is an intellectual passion behind the most abstract argumentation – the passion to arrive at the truth and to convince others about the attained truth. The second adjustment to be made to Musgrave's argument is a correction. It should be pointed out that the personal is different from the individual or idiosyncratic. The personal lends itself to the objective while the individual and idiosyncratic is in the domain of the subjective. And so, both Musgrave and Polanyi would agree that language is the vehicle of scientific theory. But while Musgrave holds on to a naïve objectivist understanding of language, Polanyi holds that language is a tool in the hands of the user, and as such there is a demand on the personal skill of the user and an allowance for personal modifications in the use of the language.

On this point, Dewey holds a position that agrees with Polanyi's on language and logic and goes against the position held by Musgrave. Dewey argues that *practice* is inextricable in the articulation of scientific theory, as it is in a richer or more complex treatment of language. Thus for Dewey a logicist/objectivist view of language:

> "...reduces a proposition of practice to a formal combination of a singular and general proposition ... [and] applies only to *ex post facto* linguistic analyses of either an act performed from habit without the intermediation of judgment or

> else of a judgment that has been completed. If deliberation and appraisals involving propositions actually intervene in reaching the decision ... then a judgment of practice is a factor in the ultimate determination of the existential material which the preliminary judgments of appraisal are *about*." (1939, pp. 165-166)

And as I have shown earlier in the discussion of the nature of tacit knowledge, the way scientific positions are arrived at is by means of such appraisal and deliberation. Further, Dewey's view goes directly against Musgrave's position. We can validly exemplify Dewey's 'propositions' with 'scientific theories'. In that light, Dewey observes:

> "When either facts [e.g. scientific theories] or conceptions are taken to be completely assured (whether because of earlier successful use or for any other reason), direct action, not judgment, ensues. It is a matter of great practical convenience that many facts and ideas may be so taken and directly used. But conversion of this practical value into assured logical status is one of the commonest ways of establishing the dogmatism which is the great enemy of free and continued inquiry." (1939, p. 169)

This view by Dewey shows the problem that faces Musgravean objectivism. Dogmatism thrives on such a mistaken assurance of 'objectivity' which does not take into due account the actual, often messy, means by which scientific 'truth' is arrived at.

4.4 Musgrave's tempered objectivism and its persistent inconsistencies

Towards the end of his thesis, Musgrave tempers his objectivism. He points out that when it comes to understanding 'truth', objective, timeless truth is a standard. Individual theories deal with truth as a public, interpersonal reality. On this tempered tack, there are possibilities of reconciling Musgravean and Polanyian epistemologies. But I argue that Musgrave does not go far enough, and even his tempered position contains some inconsistencies. I will look at a number of quotations in which Musgrave both tempers his objectivism to some extent but still incurs a few inconsistencies. To begin with a lengthy quotation that lays the ground for Musgrave's later tempered position is the following:

> "So much for my *first* point, that there is no complete and foolproof criterion of truth, and therefore that the objective theory of truth cannot be criticized because it does not provide one. But if this is so, what is the importance of the objective idea of truth? Do not the really important problems lie, after all, in trying to determine truth and falsity in particular cases? Why bother with truth as an objective notion, if it contributes nothing to a solution of these problems? .... I can sympathize with the attitude behind these questions. And they bring me to my *second* point, the importance of the objective idea of truth. Consider all our techniques for determining truth and falsehood, enshrined in the procedure of the law, the technique of experimental scientists, or of mathematicians, and so on. All these techniques are incomplete, more or less difficult to apply, and fallible, *and are not recognised as such*. This means that all these methods operate with objective truth as a *standard*: they are all used under the understanding that the truth of a statement is something *more* than any property which use of a method allows us to identify in that statement." (1968, pp. 177-178)[32]

Musgrave acknowledges that there is a problem with an objectivist idea of truth. It is removed from the reality of the activity of seeking truth (e.g. in science). The compromise he proposes is to view objective truth as a *standard* while the efforts to arrive at it are fallible and incomplete. Already at this stage, Musgrave's position is problematic. First of all, it is not clear how an individual scientist working on a particular theory is to be guided by this abstract standard of objective truth. How is this standard epistemically or logically related to the theory under investigation? Unless this and similar questions are answered, Musgrave's suggestion of objective truth as a standard remains vague and unhelpful in the search

[32] Emphasis in the original.

for scientific truth. Secondly, and more important given Musgrave's effort to avoid psychologism, the nature and source of this standard is not clear. Is it a private inspiration of the individual scientist? Is it written in some text that is accessible to all scientists? Is it a Platonic recollection from a world of forms?

All these questions can end up suggesting a psychologistic source or nature of the standard of objective truth. I can already point out here that Polanyi has a more economical solution when faced with the same problem of objectivity on the one hand and the findings of an individual scientist on the other. Polanyi admits of the role of tacit knowledge and thus of personal knowledge. The individual scientist working within a social framework is convinced of the 'truth' of her positions, and she remains committed to these positions being corroborated later by yet other findings. That is to say that the scientist is committed to the discovery of the truth by means of a given theory she has arrived at. At the same time, she is open to the truth as generated by this theory unfolding in unforeseen ways in the future or even being replaced by a better theory. This is a more economical approach because it does not necessitate the positing of a standard outside the work of the scientist. Further, it is more representative of the way the scientist works.

Musgrave's further argument on the standard does not make the position less inconsistent. He holds that:

> "Thus, one who believes in rational argument must reject subjectivist theories of truth. Adherence to the objective or absolute notion of truth is part of the faith of the rationalist. According to this notion, truth is above human control, and independent of psychological feelings (of certainty, doubt, commitment, or what you will) possessed by any person whose belief we consider. Truth thus becomes a regulative standard to which we can try to conform, admitting our fallibility, and that we may always fall short of this standard." (1968, p. 181)

This description of how the knower is to relate to truth as a standard is clearly psychologistic in a way that contradicts the efforts of Musgrave to get rid of psychologism in epistemology. He uses terms like 'faith' and the knower's 'trying to conform', which are in the realm of attitudes (similar to the ones he dismisses as being psychological: certainty, doubt, commitment ...). And still to preserve the objectivity of truth as a standard, Musgrave asserts that such truth is of timeless abstract entities while our attitudes towards these entities are transitory. He holds:

> "Now an important point about the objective properties of truth and falsity is that they are *timeless*. If something is true at time *t*, then it is true at all times.... Since truth and falsity are timeless relationships between 'something' and the facts, the 'something' cannot consist merely of psychological entities or inscriptions. For states of mind and inscriptions are transitory things, which can be forgotten or erased, while the truth or falsity of an inscription or a belief is eternal. To do justice to the eternal character of objective truth and falsity, it seems that we are driven to acknowledge timeless abstract entities as that which is, in primary sense, true and false." (1968, pp. 275-276)

This position may seem to resolve the quandary of having fallible means at our disposal while seeking objective and timeless truths. But in order to see the inconsistency in Musgrave's position, we need to remember that he is dealing with *knowledge*. He has insisted on objectivity of knowledge, rid of psychologism. But when he reflects on timeless truth, the objectivity of knowledge seems to pale given our fallible means to arrive at timeless truths.

Intent on preserving the objectivity of truth, Musgrave adopts the Fregean and Popperian concept of the 'third world'. In the same argument, he makes his biggest concession to Polanyian epistemology. Musgrave holds:

> "Once we have recognized the existence of the objective *contents* of our thoughts or beliefs (spoken and written) utterances, then it becomes possible to

regard our knowledge as inhabiting a sort of 'third realm' or 'third world', distinct from both the mental world and the physical world. And human knowledge as a psychological phenomenon consists then in the attempts of human beings to *participate* in this third world of objective knowledge. The objective or third-world properties and relationships of knowledge serve as the *standards* to which human beings try to conform when they participate in the third world. The idea of the third world, metaphysical though it sounds, is basically an attempt to do justice to the objective, inter-personal, and public aspects of human knowledge." (1968, p. 286)[33]

This is a big concession to Polanyian epistemology because Musgrave admits that knowledge *is* after all a psychological phenomenon. It is only in this light that Musgrave also admits that objectivity of knowledge, the inter-*personal* nature of knowledge, and its public aspect are not mutually exclusive. Yet again, the inconsistency persists on the notion of the third world as Musgrave explains it. The question is: How does this third world, made of an accumulation of human and animal experience which has evolved to some kind of autonomy from individual human minds (Musgrave, 1968, pp. 287-288), concur with the timeless entities of truth and falsity? How do the two entities populate the third world? How do empirical, contingent, and fallible methods of seeking truth accede to timeless truths? And in fact, the third world is populated among other things by problems, theories and arguments. Musgrave argues that:

> "To speak of undiscovered problems, theories, and arguments, may be perfectly in order if we wish to emphasise the objective aspects of these things; but it should not be seen as attempting to play down the fact that it is human beings who create knowledge, discover problems, and propose arguments. The 'third world' does not, so to speak, possess a 'life of its own': it does not, for example, *evolve* in some impersonal way independently of the contributions made to its evolution by human beings." (1968, p. 288)

[33] Cf. also K. Popper, "Epistemology without a Knowing Subject", Sections 2-4 (1972, pp. 112-122) and K. Popper, "Of Clouds and Clocks", sections xiv-xvii (1972, pp. 235-241) as well as "A Realist View of Logic, Physics and History" (1972, pp. 285-289) especially the Introduction.

This, I argue, is a concession by Musgrave of the personal nature of knowledge. This personal nature of knowledge persists even in such an abstracted realm as the third world. Here, once again, Musgrave seems to be coming around to a position similar to Polanyi which reinstates the human person at the centre of the whole scientific project, and not subjecting her to a secondary position before detached 'objective' entities like theories etc. He further asserts that: "Human knowledge, then, inhabits the third world, but it remains *human* knowledge, which is created, discussed, and developed by human beings." (1968, p. 288)[34] While Polanyi does not go as far as posit a third world, his position is reconcilable to that of Musgrave. Polanyi's advantage is that he arrives at the same conclusion more economically.

4.5 Conclusion: The resultant nature of scientific objectivity according to Polanyi

In a word, 'scientific objectivity' is for Polanyi of a personal nature. Polanyi sets out to counter both 'objectivism' that misrepresents scientific knowledge, and 'subjectivism' which leads to relativism. He comes with personal knowledge. Musgrave, in his criticism of Polanyi, is dealing with a misrepresentation of Polanyi's notion of personal knowledge. But other philosophers like Dewey have supported a Polanyian view. Dewey holds, for example that the scientist:

> "... has to decide what researches to engage in and how to carry them on – a problem that involves the issue of what observations to undertake, what experiments to carry on, and what lines of reasoning and mathematical calculations to pursue. Moreover, he cannot settle these questions once and for all. He is continually having to judge what it is best to do next in order that his conclusion, no matter how abstract or theoretical it may be as a conclusion,

[34] Other concessions to a Polanyian view that scientific objectivity has got a social, inter-subjective regulating aspect as well as a scientific tradition can be found in Musgrave. (1968, pp. 338-339) Musgrave cites from Popper. (1945, p. 217)

> shall be grounded when it is arrived at. In other words, the conduct of scientific inquiry, whether physical or mathematical, is a mode of *practice*; the working scientist is a practitioner above all else, and is constantly engaged in making practical judgments: decisions as to what to do and what means to employ in doing it." (1939, p. 161)

To this position by Dewey, Polanyi would add that commitment in scientific statements binds the scientist to both their belief and to a reality outside their minds. Polanyi is not subjectivist, he is personal. As long as science continues to examine its claims, none of its statements could qualify to be objective in the sense that Musgrave stipulates. On the other hand, the kind of objectivity proposed by Musgrave is at best merely methodological. "Objectivity" in this sense is a category in hindsight, useless both for the actual doing and describing of science. The personal element advocated by Polanyi ensures the *responsibility* that binds the scientist both to their beliefs and to a universal intent to arrive at the truth of the reality they are researching.

Science then seeks to describe reality. It does so only partially. But the expectations on science are sometimes misplaced or misleading, with the example of the kind of traditional objectivity foisted on it in Musgrave's thesis. Science proceeds rather by trial and error, building up a picture of reality only gradually. On this point, the emphasis by Popper to situate falsification at the heart of the "rational" is a misrepresentation of the way science works. Rationality is actually personal – manifested in commitment to the discovery of the truth, and in the universal intent to remain open to richer expressions of the truth initially only partially arrived at. And hence Polanyi's attempt can be understood as giving the philosophy of science grounds wider than the objective/subjective ones which fail in traditional epistemology by not being able to withstand the onslaughts of scepticism.

The attempts by the CR-ists exemplified in this section by Musgrave are aprioristic, idealistic and a-historic (in the sense that they actually ignore the history of science). This is a weakness that springs from the objectivism that is characteristic of the CR-ist position in the philosophy of science because this latter position does not succeed in furnishing us with an accurate account of how science works. The CR-ists seem to present a workable logical justification of their account of how science works. But this account is at the expense of the accurate story of how science works. On this point, Dewey has made a pertinent plea: "In substance ... logical theory [should] be made to conform with the realities of scientific practice, since in the latter there are no grounded determinations without operations of doing and making." (1939, p. 180) In similar fashion, Feyerabend criticises Popperianism on these grounds. He holds:

> "... Popper and his pupils ... developed and now defend a more technical version [of a methodology or philosophy of science]. By now this technical version has become a veritable malaise. The aim is no longer to understand and, perhaps, to aid the scientists; nor is there any attempt to check the version by a comparison with scientific practice. The aim is to develop a special point of view, to bring this point of view into logically acceptable form (which involves a considerable amount of rather pointless technicalities) and then to discuss everything in its terms... Not the ever-changing demands of scientific research but the rigid requirements of an abstract rationalism decide about the form and the content of the principles accepted." (Feyerabend, 1981, p. 21)

In other words, the CR-ists together with Musgrave have sought to approach scientific activity with a rigid mould to which scientists must conform rather than remain objectively open to the methods, however novel, by which science actually proceeds. On this note, I argue, Polanyi's notion of tacit or personal knowledge is more accurate and more economical. It does not have to deal with the question of how the scientist *adjusts* her personal and committed attitudes to accommodate a version of 'objectivity' (or objectivism) that is imposed from areas of research external to science. In Polanyi's tacit or personal knowledge, commitment to the current position or theory is bound to future revelations of the reality under study. Personal knowledge is wider than subjective knowledge. It cannot be accused of solipsism because it remains open both to richer expressions of the truth and to the scrutiny of the scientific community and its experts.

At the end of his work, Musgrave shows that his goal of objectivity (i.e objectivism) is actually less ambitious and more qualified than it is initially expressed. He points out that "I have certainly not shown that science is nowadays objective in the way I have explained it...What has been shown, I hope, is something more limited; that it is at least *possible* for science to be objective even though scientists are not objective in the traditional sense." (1968, p. 343) This remains a merely logical possibility scant on historical examples, and not showing how science actually works. Its attention to Polanyi's positions is inadequate and misled. However, there is a qualification of the CR-ist position that is more sophisticated than Popper's falsificationism. It is developed by Lakatos and Zahar, and it is the methodology of scientific research programmes (MSRP). Can MSRP be interpreted to represent a more robust critique to Polanyi's epistemology?

Chapter Five: A critical comparison of Lakatos/Zaharian and Polanyian methodologies

5.0 Introduction

Imre Lakatos, in his methodology of scientific research programmes, offers a more sophisticated Popperian position than does Musgrave's objectivism. Lakatos deals with what he calls Popperian problems and how they form a whole research programme. But he points out that he arrives at solutions that may not concur with Popper's solutions. (Lakatos, 1974) Ellie Zahar has helped to give MSRP a more robust grounding. Still, MSRP has itself failed to assert itself as the mainstream view in methodology. Using Lakatosian terms, it seems to have been abandoned as a research programme. I claim that Polanyian methodology supersedes Lakatosian MSRP, and the test case is the treatment the latter gives to our understanding of the Copernican Revolution.

5.1 Drawing comparisons between Lakatosian and Polanyian approaches to methodology

Lakatos' general approach is in at least two phases, and in one phase it is not in direct contradiction to Polanyi's tacit or personal.[35] It is argued in both systems (i.e. the Lakatosian and the Polanyian as in Popperianism) that the logic of scientific discovery e.g. as proposed by logical positivism, specifically in verificationism, is misleading.[36] Ironically, both of them parted ways with naive Popperian

[35] The two philosophers of science share much more than a national background (as Hungarians) and still much more than what they ended up being – Hungarian intellectuals in the Diaspora. Both of them have had firsthand experience of the dire effects that accrue from the interference of ideology with science. Both are witnesses to the serious consequences of curtailing academic freedom. Both of them set out to argue against the logical-positivist approach to science.

[36] In his doctoral thesis, appearing later as *Proofs and Refutations, The Logic of Mathematical Discovery* (1976) Lakatos expresses that: "According to logical positivists, the *exclusive* task of philosophy is to construct 'formalised' languages in which artificially congealed states of science are

falsificationism as a viable alternative, although to differing degrees.[37] Neither of them thinks that there can be deductive rules on heuristics.[38] The interesting paradox is, I argue, that even given such common ground, the two systems do have *crucial* differences that would put the Polanyian system ahead of the Lakatosian (and ahead of the Lakatos/Zaharian system). The two systems begin to show marked difference in their approach to the role of psychology in the methodology of science.

Lakatos, under a strong influence of the Popperian focus on the demarcation problem, underplays the role of psychology (simply understood as psychologism in scientific methodology) to the point of denying that it does contribute to the rationality of science. (1978, pp. 112-115) Polanyi, on the other hand, is not averse to psychology and as is shown already in the discussion of the nature of tacit knowledge, he shows that it is key to understanding the rationality of science. He embraces the results of experiments in psychology contemporary to his time and on investigating the role of psychology in a methodology of science, concludes that psychology does indeed contribute in important ways to the rationality of the scientific method.

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expressed .... Science teaches us not to respect any given conceptual-linguistic framework lest it should turn into a conceptual prison ..." (1976, fn 1, p. 93)

[37] Already in his doctoral thesis, Lakatos argues against the identification of mathematics with formal abstraction which in turn leads to the replacement of the philosophy of mathematics with mathematical logic. This, to Lakatos, is pernicious. It leads to a philosophical neglect of all that is not formalised or formalisable in mathematics. As an example, formalism in mathematics would lead to a rejection of Polya's concept of 'plausible reasoning' in mathematics, a key role in Polya's mathematical heuristics. Such a rejection leads to a marginalization of the study of the development of concepts and the growth of knowledge, according to Lakatos.

[38] It must be noted, however, as Brendan Larvor points out that Lakatos used the term 'heuristic' in different senses at different stages of his philosophical development. His use of the term in his doctoral thesis on the role of heuristics in mathematics is idiosyncratic and different from his later common English use of the same term. In his later development, his use of the term has psychological meaning. (Larvor, 1998).

To follow the apparent mutation of Lakatos' thought from being tolerant and even defensive of psychology, in as far as heuristics is understood as a psychological endeavour in his *Proofs and Refutations,* (1976), to a harsh critic of psychologism in his later philosophy, e.g. in *Mathematics, Science and Epistemology*, (1978), it is informative to distinguish a Lakatos who agrees with Polanyi in the fundamentals from a Lakatos who basically disagrees with Polanyi and only incidentally agrees with Polanyi in the fundamentals. I look only briefly at the early Lakatos who agrees with Polanyi and concentrate on the Lakatos who disagrees with Polanyi.

5.1.1 Lakatos in agreement with Polanyi

Early in his intellectual development, influenced by his work on mathematical heuristics as treated by Polya, Lakatos argues that the way mathematical conjectures are produced by individual mathematicians is rational even though the conjectures are fallible. Polya calls these techniques 'plausible reasoning'. Lakatos takes this position on the rational nature of the production of conjectures in a more general bid to argue *against the separation* between the context of *production* of conjectures, deemed philosophically irrelevant and to be left to psychologists and sociologists, and the context of *evaluation* of the conjectures, which is supposed to be philosophically relevant, because it is a rational process – a subject of epistemology and logic properly understood.

On this point, Lakatos holds that the separation between production and evaluation of conjectures is artificial, impoverished and not helpful. In fact, the two processes interpenetrate. "Even when a conjecture seems to have come from a flash of genius, it is unlikely to be sheer good luck. Even the greatest leaps of insight need some logical scene-setting." (Larvor, 1998, p. 20) Larvor reports that Lakatos' study of the philosophy of mathematics is in the hope of depicting mathematics as a rational, objective process. (1998, p. 21) Unfortunately, even when he rejects the separation of the two contexts, Lakatos does not make a concession to a certain epistemological trend he calls 'psychologism', which he finds irrational and to be excluded from the epistemology of science.

I argue that in the long run, Lakatos' effort to dissociate (or demarcate) science from psychologism/irrationalism leads him into a predicament similar to the one he is seeking to resolve i.e. methodology as viewed by the logical positivists. In this predicament, philosophers of science end up propounding a theory of how science works, which is out of touch with the actual practice of scientists. The logical positivists narrowed themselves to the final products of science in order to avoid psychologism (as explained earlier). In this way, a certain understanding of how science works (i.e. science viewed from its successes) was considered by them to be the *ideal* of inquiry. A certain form of rationality was deemed by them to be *the* rationality. In the case of Lakatos, similarly, science distilled of irrational elements (e.g. psychological processes) is ideal and attainable.

5.1.2 Lakatos in disagreement with Polanyi

5.1.2.1 Correcting psychologistic justificationism

In introductory fashion in a paper drafted by Lakatos, and brought to completion by later editors of his work, Lakatos addresses 'psychologistic justificationism'.[39] He does not give a precise definition of this phrase, but the closest I come to a description is the following: psychologistic justificationism is the epistemological process in which the justification of a scientific statement or position is sought in the state of the mind of its proponent at the moment the

[39] The paper is entitled "Newton's Effect on Scientific Standards". Lakatos himself is reported as having been dissatisfied with the paper and considered that it needed substantial revision. It was edited and completed by J. Worrall and G. Currie. (Lakatos, 1978, pp. 193-222)

statement is made or the position is taken. (1978, pp. 196-197) So, to check a statement would involve finding out the state of mind of the knower – whether the knower was in 'healthy', 'normal', 'right', or 'scientific' state of mind. And so, basic propositions (i.e. "anchors of Truth", something similar to erstwhile Euclidean axioms) are held as proven as long as such a mind holds them to be true. For Lakatos, the concern to find or prepare the right mind preoccupies those that he calls *psychologists* and the concern is not new in epistemology. Thus:

> "There were many different theories of the criterion of the *right mind.* Aristotle – and the Stoics – thought that the right mind is simply the medically healthy mind; according to Descartes the right mind is primarily the one [that has been] steeled in the fire of sceptical doubt and [has] then [found] itself – and God's guiding hand – in the final loneliness of pure thought. According to Baconians, the right mind is the *tabula rasa*, devoid of all content, so that it can receive the imprint of nature without distortion, etc. All schools of dogmatism can then be characterised by a particular *psychotherapy* by which they prepare the mind to receive the grace of proven truth in the course of a mystical communion." (1978, p. 197)

That brings to greater light what Lakatos has in mind when he refers to psychologism. There is a link between psychologism and dogmatism in the sense that the quest to find the right mind leads to dogmatic declarations about what the right mind is supposed to be. And consequent to psychologistic justificationism, are two other problematic epistemological attitudes, namely: apriorism and elitism. (1980, pp. 112-116) According to Lakatos, Polanyi's epistemological approach is an example of psychologistic justificationism and elitism. Lakatos interprets Polanyi's methodology as claiming that only the experts with tacit knowledge have the capacity to vet scientific truths. But this interpretation is not accurate because it does not take into account the main point that Polanyi makes, namely that scientific knowledge is a skill that sets its own standards. Apprentices can have access to it with the guidance of experts, yet the experts themselves, by the very nature of tacit knowledge, remain open to surprising ways in which hitherto upheld 'truths' will unfold to reveal even deeper unknown 'truths'. A closer look at Lakatos's epistemology of science is warranted in order to give a Polanyian response to it.

5.1.2.2 MSRP as the solution to the problem of psychologistic justificationism

As a solution to the problems cited above as brought about by psychologistic justificationism, Lakatos proposes a reconstruction of the history of science in such a way that justification of scientific positions is grounded in the facts independently of minds. The end result should be a viable methodology of science. As a background to the whole effort by Lakatos, I note that in a published conversation, Lakatos reconstructs the history of epistemology. According to Lakatos, classical epistemology (onwards from Descartes, through Kant and Russell to Carnap) with its sceptical criticism fails because it uses a content-reducing heuristic.

No knowledge can withstand the withering justificationist criticism of such scepticism. Unlike the heuristic of classical epistemology, Lakatos proposes a Popperian scientific heuristic which accepts criticism from refutation by *hard facts*. The scientific heuristic is thus content-building: it works towards the growth of knowledge. With the scepticism of classical epistemology comes dogmatism, and the two operate like two poles of the same dialectic. There is then a stalemate in classical epistemology between scepticism and dogmatism. Lakatos thus proposes that learning from the success of science, the sceptico-dogmatic heuristic which ensues from a justificationist epistemology should be replaced by a new dialectic: "the rational and irrational, where 'rationality' would stand for a generalized logic of scientific discovery." (Richard Popkin, 'Scepticism and the Study of History', p.

222, also Larvor, 1998, p. 24) This spells out the goal towards which Lakatos strives when he propounds a methodology of science.

As noted earlier, Lakatos purportedly deviates to some extent from the Popperian programme. But I argue that overall he remains faithful to the core principle of Popperianism. Lakatos holds that the "central problem in philosophy of science is the problem of normative appraisal of scientific theories; and in particular, the problem of stating *universal* conditions under which a theory is scientific". (1978, p. 168) This, according to Lakatos is properly speaking the demarcation problem. And so Lakatos agrees with Popper on the global level of the central problem of the philosophy of science being demarcation.

It follows then, from Lakatos's Popperian demarcationist background, and from the ideal that he sets himself to propound a methodology that will help resolve the sceptic-dogmatic dialectic, that Lakatos is opposed to the Kuhnian idea of normal science being *uncritical.* In the Popperian tradition, criticism, constant and mutual among scientists and communities of scientists, is what ensures the advancement of science. Criticism, as opposed to dogmatism, is the mark of rationality. But for Kuhn (and in some sense to Polanyi), a certain unquestioning or fiduciary stance is necessary for new and upcoming scientists vis-à-vis established positions within their scientific communities.

Lakatos concedes some of this Kuhnian unquestioning stance, given that it was more faithful to the history of science.[40] But still, Lakatos wants to map out a system that remains close or faithful to the history of science and still remains

[40] These comments are deliberately very general at this stage in order to simply indicate possible points of discussion. Engaging in those discussions would draw us away from the point I am arguing on the one hand, and I think I will have argued a way around them by the end of the chapter, on the other hand.

sufficiently *rational* (in the above objectivist sense). (Lakatos, 1974, p. 148) And so Lakatos takes a step away from falsificationism, because this latter has to distort history in order to find 'crucial' experiments whenever a theory is rationally discarded. In this context, Lakatos suggests his own 'methodology of scientific research programmes' (MSRPs).

5.1.2.2.1 Overview of MSRP

In this new view (i.e. MSRP) the unit of appraisal is neither a theory (as in Popperian falsificationism) nor a proposition (as in logical-positivistic verificationism). It is neither a hypothesis nor a conjunction of hypotheses, but a *problemshift* i.e. a developing series of theories or a focus on examining sequences of historically related theories. Once a central idea has been developed, modifications made to it should not lessen its empirical content. Rather than concentrate on purely logical features of scientific points of view, the focus should be on the *manner* in which clusters of thoughts are developed over time. And thus the unit of appraisal is the research programme. The research programme is a sum of various stages through which a leading idea passes. MSRPs have three main features. First is the *hard core*, tenacious and unchanging. The leading idea provides the hard core of the research programme – a set of *commitments*, the abandonment of which would entail the abandonment of the programme. An example of a hard core in the Newtonian Programme is the collection of the three laws of motion plus the law of gravitation.

The second feature of MSRPs is the *heuristic*. This is a set of problem-solving techniques. In the Newtonian programme, it consists in mathematical apparatus e.g. differential calculus.[41] The third feature of MSRP is the *protective belt.* These are auxiliary hypotheses. They protect the hard core from refutations. The hypotheses are constantly modified, increase in number, and are complicated. Questions are directed towards the protective belt rather than directly questioning the hard core. In the case of the Newtonian research programme, the protective belt would be geometrical optics or atmospheric refraction. If predictions are not empirically corroborated by observations, then the problem should be sought in geometrical optics or the interpretation of the data. The protective belt changes, as guided by the heuristics of the programme – a set of problem-solving techniques.

Overall, there are two ways in which a programme can be progressive: a) theoretically, i.e. if "each modification leads to new, and unexpected predictions, and b) empirically, i.e. when at least some of the novel predictions are corroborated. (1978, p. 179) So called "refutations" are always there in each programme. It is enough to have a few dramatic signs of empirical progress for a programme to be considered still viable. A programme is heuristically progressive if successive modifications in the protective belt are within the spirit or tradition of the heuristic. A programme supersedes a rival programme if it has an excess of truth content over its rival, i.e. it predicts what the rival programme predicts and even more. On the other hand, this is the way a programme degenerates. The heuristic is faced with problems that the protective belt cannot keep away. Ad hoc measures are then taken (i.e. ad hoc adjustments to the programme) to protect the hard core, using devices foreign to the programme. The adjustments they make are at best geared to

[41] In a footnote Lakatos remarks: "The demarcation between 'hard core' and 'heuristics' is frequently a matter of convention ..." This is interesting, because it seems to establish conventionalism right at the core of his programme or methodology. (1974, fn. 1, p. 181)

resolving the specific problems without making any predictions. And when the heuristic cannot solve the problems, degeneration sets in.

Thus far, in terms of the wider underlying Popperian demarcationist programme, Lakatos appears to have set a more stringent principle of demarcation. Briefly, he suggests that bad science is synonymous with pseudo-science. In turn, the distinction between science and pseudo-science is that in the ongoing war of attrition between different (and competing) research programmes in both science and pseudo-science, in which some are progressing and some degenerating, the progressive ones should triumph over the degenerating ones in science.[42] That is the mark of scientific discipline and of rationality. Thus far, 'progress' is when change in a programme is a result of the inner logic of the programme, whereas 'degeneration' is when change is brought about within the programme due to criticism from sources external to the programme.

5.1.2.2.2 Crucial similarities between Lakatos and Polanyi

A number of points of convergence between Lakatos and Polanyi are important. First of all, in Lakatos's MSRPs, empirical progress is achieved when new and undreamed of facts are predicted and corroborated. Theoretical progress occurs when modifications lead to unexpected predictions. Polanyi too makes a lot of capital out of this undreamed of or unexpected element in scientific progress. To him, what is objective is what remains open to revealing unexpected truths about a phenomenon under investigation. Through tacit knowledge, the scientist remains attached or committed to the findings of a branch of her research and these may reveal further truths which end up corroborating what the scientist held as 'true' in

[42] He holds: "The term 'demarcationism' stems from the problem of demarcating science from non-science or from pseudoscience. But I use it in a more general sense. A (generalized) demarcation criterion, a methodology or appraisal criterion, demarcates better from worse knowledge, defines progress and degeneration." (1980, p. 108)

the first place. Secondly, it is clear that the heuristic of a programme provides two things: a logic of discovery for the scientists who research within a given research programme, and the standard upon which the programme itself is supposed to be evaluated. Thus far, a research programme which remains *faithful* to its heuristic is judged to be progressive. (Larvor, 1998, p. 55) Polanyi emphasises the need for the scientist to remain faithful to the traditions within which she is researching in her bid to learn the skill of knowing within that tradition.

The application of 'heuristic' in both Lakatos and Polanyi is thus comparable. For both of them, the heuristic is central to their systems. The heuristic, for Polanyi is central to discovery and discovery is the driving force in scientific progress. For Lakatos, the heuristic is central to the research programme. Besides this, in his interpretation of Lakatos' MSRP, Larvor picks out another element which to my understanding is a similarity between Lakatos and Polanyi on the concept of the heuristic.

Larvor points out that the concept of a heuristically driven programme is an explanation both to why some expert and experienced scientists can act in cavalier fashion to anomalies and to the relative autonomy of theory to experimentation e.g. when theory seems to surge ahead of experimentation. (1998, p. 56) However, the difference between the two systems is that while Lakatos hopes to be able to locate the heuristic objectively or externally (in order to avoid what to him is the pitfall psychologistic justificationism), Polanyi sees it as part of the tacit coefficient of knowledge that is personal without thereby being objective in a Popperian objectivist sense. For Polanyi, the skill possessed by the individual scientist and learnt within the tradition, which skill brings with it commitment to the tradition, contributes to the scientist's insistence on a new idea by a so-called cavalier scientist. Polanyi's explanation is thus more economical and because it is richer, it supersedes or is more progressive than the Lakatosian project. Yet the Lakatosian project has more weaknesses, a number of which I now examine below.

5.1.3 The circularity of Lakatos's reconstructivist strategy

Lakatos divides intellectual history into two: internal and external history. Internal history consists in a history of ideas, (i.e. the internal skeleton of rational history) while external history is social history. (1978, pp. 102ff., 190ff.) He goes on to divide historical writing into a normative-rational part and a socio-psychological part. The normative part supplies a rational reconstruction of the growth of objective knowledge. That is, it presents the development of some body of learning in a way which explains why it counts as knowledge. The other kind of historical writing, the socio-psychological part, is there to fill in the gaps. It is the non-rational part of a history-driven account seen as a methodology. And so Lakatos rewrites history in such a way as to bring out the rational nature of the activity of science.

A critical look at Lakatos's effort to reconstruct history, proposed as a solution to what he calls the problem of psychologistic justificationism, reveals two interconnected flaws. First of all, Lakatos fallaciously assumes that the mass of historical facts provides evidence that can sustain a *normative* rational approach to scientific methodology. To begin with, and in line with Polanyi's notion of tacit knowledge, Laudan agrees that we may have pre-analytic intuitions (PIs) about scientific rationality, which intuitions help us to appraise and evaluate various normative models of rationality from given instances. He holds:

> "...[T]here is, I shall claim, a subclass of cases of theory-acceptance and theory-rejection about which most scientifically educated persons have strong

> (and similar) normative intuitions. This class would probably include within it many (perhaps even all) of the following: (1) it was rational to accept Newtonian mechanics and reject Aristotelian mechanics by, say, 1800; (2) it was rational for physicians to reject homeopathy and to accept the tradition of pharmacological medicine by, say, 1900; (3) it was rational by 1890 to reject the view that heat was a fluid; (4) it was irrational after 1920 to believe that the chemical atom had no parts; … . What I shall maintain is that there is a widely held set of normative judgments similar to the ones above. This set constitutes what I shall call *our pre-analytic intuitions about scientific rationality* (or "PI" for short) … *[T]he test of any putative model of rational choice is whether it can explicate the rationality assumed to be inherent in these developments ... [and] the degree of adequacy of any theory of scientific appraisal is proportional to how many of the PIs it can do justice to.*" (Laudan, 1977, pp. 160-161)[43]

Now, Lakatos hopes to give a theory of rationality which would be based in history (albeit reconstructed) in his MSRP. The question is whether his model of rationality does actually explicate the rationality in the instances he reconstructs. To examine this question, let me first note together with Brown (who agrees with Laudan, 1977) that history is important for the philosophy of science, specifically in helping to make explicit the complex relationships between the two. However, Brown notes that "... it is surprising to find that *most of history will be evidentially neutral to normative philosophy of science.*

The reason for this is very simple. The set of historical episodes is large while the set of PIs is rather small. "Since ... it is only the PIs which matter ..., most of history is evidentially irrelevant." (Brown, 1980, p. 240)[44] So, a history that is *specific* to science needs to be written. But even such a history would invoke a methodology of science. And that is the second flaw with Lakatos's strategy – it is circular. The reconstructed history that Lakatos uses presupposes a methodology,

[43] Emphasis in the original.
[44] Emphasis in the original.

rather than the methodology being a consequence of the reconstructed history. And so Brown argues:

> "A *theoretical reconstruction* is a description of some historical episode using the concepts of some methodology. For example, should we describe some episode using Popperian methodological concepts, the account will be in terms of "crucial experiments", "basic statements", "falsified theories", etc. If we were using the concepts of the so-called "methodology of scientific research programmes" then the history would be written in terms of "research programmes", "heuristics", "hard core", etc. These terms ... are theoretical terms. Any written historical episode must be a theoretical reconstruction; that is, it must invoke such concepts, concepts which owe their being to some methodology of science. This is because our histories are explanatory and they appeal to reasons as the causes of events. This requires an account of what good reasons are." (1980, p. 242)

But in the case of Lakatos, the question goes beyond a theoretical reconstruction. Lakatos engages in a reconstruction that is *normative* in the sense that it is meant to point the way for a rational methodology and out of the predicament of psychologistic justificationism. Lakatos is involved in describing how history *ought* to have gone according to his proposed methodology. (Brown, 1980, p. 242) This involves the strategy of Lakatos in a circularity problem as described by Laudan. Laudan holds:

> "If the writing of history of science presupposes a philosophy of science and if philosophy of science is then to be authenticated by its capacity to lay bare the rationality held to be implicit in the history of science, how can we avoid automatic self-authentication, since the history of science we write will presuppose the very philosophy which the written history will allegedly test? (1977, p. 157)

In my view, Lakatos's strategy of resolving what he calls the problem of psychologistic justificationism suffers from this problem of circularity pointed out by Laudan. And linked to the problem of circularity is yet another. The proponents of any given methodology that claims evidence in history may choose only those episodes in history which support their methodology. Now, some philosophers of science focus on controversy, some focus on change. But each of them selects supporting episodes in the same history of science. As a remedy I suggest an openly pluralistic approach in which the particular nature of each candidate epistemology is explicit about the fact that it is only one of many possible methodologies.

But such an approach is not favoured by methodologies influenced by Popperian objectivism. And Lakatos's methodology, with its insistence on a specific type of rationality and normativity is itself objectivist in a Popperian way. Any approach to the history and philosophy of science that is exclusivist rather than pluralist is bound to leave out an aspect of how the complex system of science works, for it would be aprioristic. As argued earlier in the discussion of the nature of tacit knowledge, more about science is being understood as science is done. Marga Vicedo argues that logical positivists have since long left the centre-stage of the discipline of the philosophy of science i.e. their research programme was abandoned, not only because of its inadequacies, but also because their views of the philosophy of science have been found to be too restrictive. (Vicedo, 1992, p. 490) The same evaluation befits this particular form of a Popperian programme when it purports to be too restrictive, excluding those methodologies it finds 'psychologistic' without giving convincing reasons why psychology should be ruled out of the epistemology of science.

In comparison, Polanyi's system offers a way out in two ways. First of all, tacit knowledge avoids the horns of the dilemma in not putting too much (almost exclusive) emphasis on the history of science on the one hand or on objectivist rational methodologies on the other. There is a dilemma here as seen above because too much emphasis on the historical approach would be oblivious of the under-determination of the 'facts' by the chosen methodology. On the other hand, too much insistence on objectivity or objectivism would ignore often contrary evidence that history offers. The middle ground proposed in Polanyi's notion of tacit knowledge is to approach science more from the point of view of the person and the tacit knowledge. Rather than have a normative system (whether historically based or based in objectivism), Polanyi's system builds up an understanding about how science works that is accumulated from the experience of individual scientists and is as a principle open to modification in its understanding of what is true/objective and what is rational in science. Thus far, Polanyi's notion of tacit knowledge includes the actual experience of the scientist both in the process and in the products of science.

The second way Polanyi's approach gives a way out is when he emphasizes that scientists should be left as much autonomy as possible, even if science should be organized. Polanyi is combating the central planning of science. In *Logic of Liberty*, he argues that when science is polycentric, and when the scientists are given as much liberty as possible to pursue their branches of science, there are more chances for it to advance than when it is centrally planned. I extend this central planning here to include a single objectivistic rationality that is centrally proposed at a meta-level of a normative methodology.

And so, Polanyi does make a diagnosis of the problems facing the methodologies of his time similar to the one by Lakatos. He notices that there is a tension between historical accounts and objectivist accounts. Polanyi does resort to history in his construction of the notion of personal and tacit knowledge. However, he does not propose a reconstruction of history, be it theoretical or normative. Rather, he fills in some details of history that are usually left out in standard histories of science that are governed by some objectivist/aprioristic methodology or other. After that, he re-interprets the same history to show the role that personal and tacit knowledge play. A few examples of this have been discussed in the section on elaborating the nature of tacit knowledge. His approach is more representative of the activity of science, and it is more economical and less ambitious than re-writing history. I substantiate my claims by looking critically at the treatment of the Copernican Revolution by Lakatos and Zahar. In this treatment, Lakatos applies his approach of rewriting history in a way that illustrates MSRPs at work.

5.2 Lakatos/Zahar's on the Copernican revolution: MSRP

Lakatos/Zahar observe that theories (e.g. Popperian demarcationism, Kuhnian and Polanyian elitism, as well as Feyerabendian relativism) of how the Copernican Revolution can be rationally explicated have failed. Nevertheless, "the Copernican Revolution can be explained as rational on the basis of the methodology of scientific research programmes" MSRP. (Lakatos, 1978, p. 178) He has two objectives. First of all, he seeks to save the rationality of science within the context of historical fact. Secondly, he hopes to demonstrate the descriptive superiority of MSRP over the descriptive aspect of other methodologies. (Lakatos, 1978, p. 180)

Lakatos/Zahar claim that Copernicus realized the heuristic degeneration of the Platonic programme at the hands of Ptolemy and his successors. Copernicus then levelled three charges of ad hocness against Ptolemy. (Lakatos, 1978, pp. 181-182) First, the introduction of equants is ad hoc because it goes against the heuristics of Ptolemy; second, giving the stellar spheres two distinct rotations went against

Ptolemy's paradigm/hard core, namely that stars being perfect bodies would have at most one uniform motion; and third, that in spite of all these charges, the Platonic geostatic heuristic remained ad hoc. And so, remaining within the Platonic programme, Copernicus sought to revitalize its Aristarchan version.

The hard core of this programme is that stars are the frame of reference for physics.[45] With this (hard core) Copernicus employed a revived Platonic heuristic, which was that stars – being the most perfect bodies – should have the most perfect motion. To Lakatos' interpretation of the Platonic heuristic, uniform circular motion is most perfect because it is akin to rest. In contrast, Ptolemaians had attributed double motion to the stellar spheres – i.e. a daily rotation and another about the axis of the ecliptic, itself irregular due to erroneous astronomical data. (1978, 183) According to Lakatos/Zahar, Copernicus seeks to restore the Platonic heuristic.

But one may ask: If the earth is imperfect and thus perfection must be other than terrestrial, and if perfection must be similar to or approximate the heavens (for that was the prevailing Judaeo-Christian theological position that propounded a fallen world), how come the only perfect body, i.e. the static body in this programme was the earth? How could Ptolemy have gone so decidedly against the very hard core of the programme to which he belonged and still remained Platonic? Do Lakatos/Zahar overlook this important detail in their reconstruction of history because it does not depict the rationality of science? Lakatos/Zahar situate the 'revolution' in the Copernican Revolution other than in an isolated hypothesis. (1978, pp. 169-170) To them, there is no such thing as an isolated hypothesis – it always fits in some kind of context, it brings with it a whole world view.

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[45] It is not trivial to note that in the reconstruction of history, Lakatos overlooks the evolving meaning of 'physics', if this term was used in the said context. The basis of this objection will be treated in the objections to MSRP.

Nevertheless, Lakatos actually points out the Copernican *Revolution*, and it is in one thesis: "Copernicus ... fixed the stars, thus making them genuinely immutable. It is true that he had to transfer their motion to the Earth; but in his system the Earth is a planet and planets are anyway less perfect than stars, if only on account of their multiple epicyclic motion." (Lakatos, 1978, p. 183) Lakatos/Zahar mitigate the importance of this change in which motion is shifted from the stars to the Earth. But it is not clear why such a change is small even when Lakatos/Zahar point out that such multiple epicyclic motions were common currency between the Ptolemaists and the Copernicans.

In my view, such change as distinction between the two systems, is revolutionary enough. The fact that the epicyclic motions were common to both may obscure the fact that the starting points of the two systems are in fact very different. Lakatos/Zahar wish to show that both the Copernican and the Ptolemaic systems were within the same Platonic programme. In that light, all that the Copernican Revolution effects, is to *save* the Platonic system.[46] Pushed to its limits, this position by Lakatos/Zahar claims that the programme is robust enough to contain a revolution. There is enough room within the same programme to accommodate revolutions. Only in this light can Lakatos/Zahar claim the following:

> "But the phases of Venus prediction was not corroborated until 1616. Thus the methodology of scientific research programmes agrees with falsificationism to the extent that Copernicus's system was not *fully* progressive until Galileo, or

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[46] On a similar note of criticism, Thomason points out: "Lakatos and Zahar's history raises two questions: First, when assessing 'the Ptolemaic programme' to whose theory construction should we apply Zahar's criterion: Plato, Eudoxus, Aristotle, Apollonius of Perga, Ptolemy? Second, were Ptolemy or Apollonius of Perga even working in the Pythagorean-Platonic programme at all, if their theories 'ran counter to the Platonic heuristic? ... Was Ptolemy (roughly 100 to 165 AD) working in the 'Pythagorean-Platonic research programme', defined by Lakatos and Zahar in terms of 'uniform circular motion' or a post-Platonic research programme?" (1992, p.170)

> even Newton, when its hard core was incorporated in the completely different Newtonian research programme which was immensely progressive. The Copernican *system* may have constituted heuristic progress within the Platonic *tradition*, it may have been theoretically *progressive* but it had no novel facts to its credit until 1616." (1978, p. 184)

Yet this citation illustrates how Lakatos was not consistent in his use of the terms "system", "tradition", and "programme". It appears that Lakatos/Zahar use these terms interchangeably. But in my view, the terms should be used precisely if the whole MSRP approach is to be useful. The importance of using the terms precisely is borne out, for example in the following citation. Lakatos/Zahar hold "From the point of view of the methodology of scientific research programmes the Copernican programme was not further developed but rather abandoned by Kepler, Galileo and Newton. This is a direct consequence of the shift of emphasis from 'hard core' hypotheses to heuristic." (1978, p. 184)

I find it surprising that Lakatos/Zahar are willing to see more affinity and less change from Ptolemy to Copernicus, than from Copernicus to Kepler, Galileo and Newton. This view is so unique to MSRP that it goes against the very objective of MSRP in replacing elitism. The view leads to the abovementioned counterintuitive positions, which in turn is a direct result of a counterintuitive re-writing of history. This view goes even as far as appearing rather ad hoc in the Lakatos/Zaharian meaning of the term for it is an example of a 'parameter of adjustment' approach to dealing with the problem of explaining the Copernican Revolution within MSRP.

Polanyi did not directly address Lakatos/Zahar on this elaboration of MSRP. But Millman gives due consideration to the Lakatos/Zahar view and suggests a Polanyian alternative, without resorting to rewriting history. He holds that:

> "At the time of its initial introduction and for long after, the Copernican theory as a whole was not superior to the Ptolemaic theory as a whole, considering all criteria of choice. Indeed, it was inferior in that it seemed to be refuted by the absence of stellar parallax and was more fundamentally in conflict with the accepted theory of motion. Nevertheless, the Copernican "harmonies" did provide arguments which gave the early Copernicans good reasons for entertaining the Copernican theory and trying to develop it further." (Millman, 1976, p. 143)

This view by Millman is akin to Polanyi's view in the emphasis it puts on *harmonies*.[47] These harmonies are akin to the element of elegance and beauty that is picked out by tacit knowledge in the Polanyian system. Milliman makes a simple statement of the basic hypotheses in both the Copernican and the Ptolemaic systems as they are generally accepted in standard histories. According to him, the basic Copernican hypothesis is that the earth and the other planets move in orbits around the sun. In contrast, the basic Ptolemaic hypothesis is that the seven Ptolemaic planets (Mercury, Venus, Mars, Jupiter, Saturn, the sun, and the moon) move in orbits around the earth. Each of them, except the sun and the moon, moves on a major epicycle whose centre rotates on another circle (the deferent) whose centre in turn is close to the centre of the earth. And so, while the basic Ptolemaic hypothesis accounts for the existence of retrograde planetary motions, there remained an aspect that demanded explanation.

[47] In a further explanation of the reasons why the Copernicans preferred their system to the Ptolemaic system, Millman proposes three reasons: (a) *The fundamental principles of the new theory provide a simpler explanation of an important qualitative phenomenon than do the fundamental principles of the established theory.* (b) *The fundamental principles of the new theory explain a qualitative phenomenon which the fundamental principles of the established theory cannot explain by themselves* without the aid of further *ad hoc* assumptions. (c) *The fundamental principles of the new theory enable measurements to be made of certain properties or relations which either could not be measured on the basis of the established theory or could be measured only with the aid of additional, arbitrary assumptions.* (1976, pp. 145-146) I object to this explication of 'harmonies' as long as they are a *post factum* interpretation of history and are thus not so helpful in terms of a forward looking methodology. Fortunately, Millman apparently changes his stand further on.

Millman makes a very fitting and Polanyian criticism of Lakatos' approach when he observes that Lakatos's restricted meta-methodology or what Millman calls 'methodology of historiographical research programmes' omits an important sphere of *rational appraisal* of scientific theories. In that sense, he infers, it is inferior to, and must be replaced by a modified methodology which also includes *plausibility appraisals*. (Lakatos, 1978, p. 146)[48] I agree with Millman's conclusion about Lakatos/Zahar's approach when he points out that the discussion of Lakatos/Zahar's treatment of the Copernican revolution "has shown that rational plausibility considerations exist and are appealed to by 'leading scientists' in the course of doing 'great science'. Any methodology which ignores them is incomplete." (1976, p. 147) That is precisely my argument against Lakatos/Zahar's rewriting of the history of the Copernican Revolution. But Lakatos/Zahar have made an attempt to save their position by introducing the notion of 'novel facts'.

5.3 A critical examination of Zahar's criterion of 'novel facts'

An outstanding question that arises from Lakatos/Zahar rewriting of the history of the Copernican Revolution is: Was Copernicanism *actually* progressive? As I have already remarked, MSRP accommodates revolutions within a single given research programme. One way to resolve this conflict is to argue for a synthesis between the change that comes with revolution on the one hand, and the static or stable nature of a research programme on the other hand. What could result is an evolution. Thus far, MSRP could be an argument for evolution in science. To achieve this goal, Lakatos/Zahar introduce an element (originating in Lakatos and modified by Zahar) of 'novel facts'. By this notion, Lakatos/Zahar hope to show

[48] Wesley C. Salmon argues along similar lines when he points out the role of "plausibility judgments" in rationality and objectivity in science. (Salmon, 1998)

that Copernicanism was actually *progressive*. Thus, the redefinition of 'novel facts' by Zahar included 'dramatic' so that facts which may not already be known and therefore not strictly 'novel' as such, can lend new or immediate support to a position – being an unintended, unforeseen by-product of a programme – a 'present'. A novel fact gives unexpected dramatic corroboration to a theory within a programme.

An example of a novel fact given by Zahar is the way the explanation of Mercury's perihelion was not intended by Einstein's theory, but was a *present* from Schwarzchild. In this line, Lakatos goes on to show that there was a series of conclusions – facts predictable before any observation, rooted in the Copernicans' position. (1978, pp. 185-186) In my view, here again is an attempt to patch up an inadequate methodology, namely MSRP, in an ad hoc manner. Novel facts described this way can only be recognized in retrospect. They are not useful in a methodology, especially not in a normative methodology in which we are trying to demarcate between good and bad science.

I point out here that Polanyi's system is a more economical expression of a similar idea. According to Polanyi, the novel ideas of a scientist are proven in their novelty in time, when they are confirmed in ways not thought of before. At the moment of their conception, the only claim they may have to such confirmation is their elegance (in the eyes of the intellectually passionate scientist who is in the process of acquiring the skill of how to know in science) and their openness to universal intent. The novelty is thus sustained all through the Polanyian system, and not only in dramatic appearances of it.

The importance of Zahar's rendering of novel facts to Lakatos is further manifested in the latter's interpretation:

> "Zahar's account then explains Copernicus's achievement as constituting genuine progress compared with Ptolemy. The Copernican Revolution became a great *scientific revolution not because it changed the European Weltanschauung*, not – as Paul Feyerabend would have it – *because* it became also a revolutionary change in man's vision of his place in the Universe, but simply because it was *scientifically* superior. It also shows that there were good objective reasons for Kepler and Galileo to adopt the heliostatic assumption, for already Copernicus's ... *rough model* had excess predictive power over its Ptolemaic rival." (1978, p. 188)

But again, the reason given in conclusion for the Copernican Revolution to be *revolution* is inadequate in two ways: first: Lakatos does not explain what he means when he introduces another term: 'scientific superiority'. It is not clear when and how superiority is established, given a set of competing ideas. On this point, Lakatos seems to be begging the question, for when he sets out to set a more stringent standard of demarcation, he is meant precisely to provide a criterion for such superiority. A second reason why Lakatos's position is inadequate is that now he is forthright in declaring the revolutionary nature of Copernicanism. It was apparent in his earlier position discussed above that Copernicanism was not so much of a revolution and that there were greater revolutions in other periods of history than in Copernicanism. Arguing in this line, Lakatos/Zahar hold a difficult position:

> "It was also shown that Galileo and Kepler rejected the Copernican Programme but accepted its Aristarchian hard core. Rather than *initiating* a revolution, Copernicus acted as a midwife to the birth of a programme of which he never dreamt, namely the anti-Ptolemaic programme, which took astronomy *back* to Aristarchus and at the same time *forward* to a new dynamics." (1978, p. 189)

Now this introduces an epistemological quandary. To claim that the Aristarchan hard core always existed in some kind of epistemological limbo or womb during the time that the Ptolemaic system held sway raises too many questions. Among other questions: Where was this hard core stored or preserved? How was it used? Who used it and to what effect? What is the relationship between this Aristarchan hard core and the Ptolemaic system? What are the epistemological advantages of this view over the view that takes the Copernican position as a revolution? This is a high price to pay just in order to preserve the stability of the Platonic research programme over the ages and to mitigate the revolutionary character of Copernicanism. It is true that ideally no question is closed once and for all in science, but again for the progress of science, scientists are willing to consider certain positions as standard and the competing ones that have gone ahead of them as having been replaced.

Further, let us grant for the sake of argument that Copernicus 'unconsciously' performed the role of midwife, helping the deliverance of the Aristarchan hard core. Then the question remains: Why does the same have to be the case for Galileo and Kepler? Galileo and Kepler may actually have considered themselves (or could have been conscious of) being part of the 'Copernican programme', and this as a revolution (given the opposition they faced and how they stuck to their positions) and not merely working within the Aristarchan hard core. Lakatos/Zahar needs to show where Galileo and Kepler actually reject the Copernican system and prefer instead to work with the Aristarchan hard core.

5.3.1 Thomason's critique of Lakatos/Zahar's notion of 'novel facts'

Thomason argues at length against the position of Lakatos/Zahar on novel facts, namely that these latter in the Copernican system show that it was objectively scientifically superior to the Ptolemaic system. In the first place, Thomason points out that Lakatos/ Zahar apply their new principle of a 'novel fact' to *fictional* history rather than to *actual* history. And linked to this, they compare Copernicus to Eudoxus rather than to Ptolemy, they ignore Tycho Brahe, and do not take into consideration facts that could have been novel within the bounds of geostatic theories. Thomason's criticism on this point merits citing at length. He holds:

> "In brief, on Zahar's criterion, to determine if a fact is a novel fact or a theory, one must look at the origin of that theory. In particular, one must answer a historical question: whether that fact played a role in the scientist's construction of the theory or the problems it was designed to solve? ... So, to be a novel fact for heliostatic theory, a fact must not have played a role in its construction. It must not have been one of the problems Copernicus was trying to solve. To apply Zahar's criterion, one needs a detailed history of the heuristic reasoning Copernicus used in arriving at his theory. ... Unfortunately, we have nothing approaching Copernicus's private correspondence or diaries from the crucial period.... There is certainly no document clearly showing Copernicus's problem situation or his heuristic reasoning. ... Thus, an application of Zahar's criterion must rely on historians' reconstructions of Copernicus's reasoning. If there were a consensus among historians, this would not pose a problem. But, despite considerable and very sophisticated historical research, there is no such consensus." (1992, p. 163)

Secondly, , Thomason points out that Lakatos/Zahar do not cite any historical account according to which the list of novel facts they give, although it did not belong to the problem situation that governed the construction of the Copernican hypothesis, was nevertheless predicted by heliostatic theory. Instead, the two draw the list independently and speculatively. Their response to this objection may be that they are aware of the novelty of their point of view and their objective is to show that science is objectively rational. Yet such an effort would be similar to what MSRP is meant to correct in verificationism and falsificationism, namely the lack of historical content. And in connection with this problem, Thomason raises a third criticism about novel facts. He holds that leaving such fictional history aside and looking at real history, there is a lack of consensus among historians that would make it difficult to determine which facts are 'novel' in Ptolemy, Copernicus, or Brahe. It was even more difficult for earlier historians to determine which of the theories held by these scientists were actually progressive. Those earlier historians would need the private correspondence of the scientists. (1992, p. 167)

Fourthly, and closer to my position of proposing a whole new epistemology along the lines of Polanyi's notion of tacit knowledge, Thomason cites and agrees with Bochner's observation:

> "For my part I would not discard the possibility that Copernicus may well have started nurturing his heliocentric proclivities long before he had the specific knowledge by which to support them. Thus his dissatisfaction with Ptolemaic assertion that the huge sphere of stars performs daily rotations need not have come about at all by the knowledge of specific astronomical detail from specific astronomical works. It may have been simply the expression of a meta-physical malaise." (Thomason, 1992, p. 176)

The appealing point in this observation is that it shows the weakness in Lakatos/Zahar deliberately rewriting history to suit a given idea of rationality which may itself be partial or mistaken. The question is: Why is such a motivation as resolving a metaphysical malaise not part of a full account of how science is rationally carried out? I have discussed this matter earlier in the chapter that deals with the incomplete definition of knowledge. A more complete approach to knowledge and by extension to rationality, should embrace the way of acquiring knowledge that is proposed by Bochner above. That is more economical and less counterintuitive than rewriting history to suit a selected kind of rationality. And linked to this criticism is a sixth one. Thomason argues that historically, scientific communities do not first consider the origins of a theory (i.e. using Zahar's criterion, of the way a theory is built and the problems it is designed to solve) before they accept or reject it. In the case of the Copernican system in the 16th century, the arguments of the scientists were a mixture of all kinds of considerations ranging from explanatory power, to simplicity, to Neoplatonism, mathematical models, biblical quotations, citations of the Church Fathers, contemporary aesthetics, etc. (1992, p.194) In other words, more than use Zahar's criterion, scientists rely on their *considered judgments*.

And finally, Thomason makes a seventh and more global criticism to Lakatos/Zahar's MSRPs. History may furnish us with the facts of who, when and what was done in science. But deciding on the research programme within which the scientist was working, among many contemporary and competing programmes is an arbitrary decision. So is it an arbitrary decision: to distinguish a widely held background belief from a given programme's hard core; to decide even with the help of Zahar's notion of novel facts, where one research programme actually ends and a new one takes over.[49] He asks quite pertinently: "Was Einstein working in Lorentz's programme? In Mach's? In Poincaré's? Hull's history of recent systematic zoology has so many scientists combining, refining, and separating ideas from so many sources that the reader can plausibly conclude that their theories do not have Lakatosian hard cores". (1992, pp. 193-194)

[49] Thomason gives an example: "...How is one to distinguish a new research programme from a variant on an old one? How does one tell if Brahe's geo-heliocentric theory is a different research programme from Martianus Capella's quasi-geo-heliocentric theory? Did Copernicus and Aristarchus work on the same programme? Copernicus and Kepler?" (1992, pp. 193-4)

5.3.2 Attempts to salvage Lakatos/Zahar's notion of 'novel facts'

Thomason points out that there have been attempts to modify and perhaps salvage Zahar's criterion. But most of them cannot stand up to criticism. Gardner (1982, p. 10) suggests that novel facts are only unknown to the scientist who constructs a given theory at the time she constructs it. But the problem is that historically, such ignorance is rare among truly innovative scientists. "Einstein knew about the orbit of Mercury and Copernicus knew that the superior planets retrograde only when in opposition." (Thomason, 1992, p. 196) Another attempt to modify Zahar's criterion is by Frankel (1979, p. 25). For Frankel, a fact is novel "with respect to a given research programme ... if it is not similar to a fact which already has been used by members of the same research programme to support a hypothesis designed to solve the same problems as the hypothesis in question." (1979, p. 25) So Frankel extends the constraint to the whole research programme where Zahar stopped at a particular theory. The problem with this modification is that the task for establishing the novelty of a fact becomes more difficult with Frankel's modification. As Thomason further notes:

> "...Frankel's criterion entails that *every* fact used or explained or predicted by *every* creator of *every* research programme is a novel fact. This is true regardless of however intuitively *ad hoc* the use of such facts may have been. For example, Copernicus's proposal that we do not detect stellar parallax because the fixed stars are so far from the earth would, on Frankel's criterion, count as a novel fact." (1992, p. 197)

Another attempt to rehabilitate or modify Zahar's criterion can be detected in Nunan. (1984, p. 279) Nunan builds on Lakatos' idea of competition between research programmes. He proposes that a fact is novel, "... if it has not already been used in support of, or cannot readily be explained in terms of, a hypothesis entertained in some rival research programme." (1984, p. 279) But this rendering of the criterion excludes the Copernicus's theory from having had novel facts, since the Ptolemaic theory had already explained the phenomena that Copernicus is dealing with. And as Thomason points out, Nunan's criterion does not distinguish between *ad hoc* and genuine explanations. (1992, p. 197)

Other failed attempts to modify Zahar's criterion include one by Musgrave, who finds that a strictly temporal understanding of novelty would be too austere on the one hand, and Zahar's view is too relativistic on the other hand. Yet when he proposes a new criterion, it is problematic. His proposed criterion is that a fact is novel for a given theory if the theory predicts the fact without a background theory being able to predict the same fact. And vice versa, should a fact be confirmed both by a new and an old theory, then it is not novel. The problems with his criterion are: First of all, the radical novelty of novel facts that Musgrave is proposing would narrow such novel facts to a very small number, if any at all. Musgrave's modification of Zahar's criterion restricts scientific progress to radical revolutions in science. I find that this is a very complicated way to point out that novel facts are very rare, and that evolutions rather than revolutions are the norm in scientific progress. There is certainly a less restrictive way than to look out for revolutions. Polanyi's epistemology of science offers that way, in my view.

5.3.3 Lakatos's own concessions

Lakatos/Zahar and more specifically Lakatos may be insistent on MSRP as the solution to the problem he calls psychologistic justificationism. But he is not oblivious of the internal weaknesses therein. On scattered occasions in his writings, he points them out even though he does not give viable solutions to the weaknesses. He admits, first of all, that rival rational reconstructions of history are similar to research programmes, each with a different normative appraisal as hard core and psychological hypotheses as the protective belt. Thus far, such historiographical research programmes are to be appraised or evaluated on how close they remain to the actual historical account. And as such they are appraised like any other MSRP on progress and degeneration. (Lakatos, 1978, pp. 191-192)

Further, Lakatos admits that theories of rationality, just like scientific theories, "*remain for ever submerged in an ocean of anomalies.*" (1978, p. 134)[50] Unfortunately, this admission is short lived as Lakatos quickly resorts to prescribing ever better rational reconstructions or external empirical theories to explain the anomalies. Lakatos is thus caught in a regression to ever more abstracted meta-methodologies, from which only a concession precisely to the rationality of what he dismisses as psychologism and as expressed in Polanyi's approach is the economical solution. Elsewhere I have argued that the Popperian (and in that case Lakatosian) preoccupation with 'psychologism' as being outside the boundaries of rationality is self-defeating and is reminiscent of the exclusivism characteristic of positivism.

And finally, more hope to resolving the problems of Lakatos's MSRPs in the direction of Polanyi's approach is given by an interpretation of Lakatos by Larvor. According to that interpretation, in answering the question why scientists stick to their apparently falsified theories, Lakatos is interpreted as answering:

> "… research programmes supersede each other by attrition, and there is rarely a unique moment at which a programme dies … [A] methodology [e.g. Lakatos'] must agree as far as possible with the settled judgments of the elite as to which episodes in the history of science were rational and which not, without recourse to *ad hoc* devices." (1998, p. 61)

The hope is that the elite being referred to here will include the scientists. As long as the judgment is left to philosophers only, there is bound to be room for error

[50] Emphasis in the original.

in the sense that methodologies are proposed which may not be representative of scientific activity. Such judgment is what Polanyi's notion of tacit knowledge appeals to.

5.4 Conclusion: Why Polanyian tacit knowledge supersedes MSRP

Lakatos's demarcationist bid to depict the rationality of science has been occupied by two notions: rewriting history and changing the unit of appraisal from individual theories to entire research programmes. My investigation and discussion has shown that the emergent picture still leaves out an element of rationality, which element Lakatos forecloses as being part of psychologism/elitism. This element, I suggest, is the Polanyian personal judgment of individual and collective judgment also described as tacit knowledge.

A close and critical look at the attempt by Lakatos/Zahar to change the unit of appraisal from theories and clusters of theories to research programmes shows that what they intend to save is the driving force of scientific *progress*, i.e. discovery. Lakatos/Zahar would like to preserve the rational nature of this driving force without at the same time succumbing to an aprioristic approach (as is seen in verificationism and falsificationism). But in the end, what Lakatos/Zahar achieve is a system that is so stable and so inflexible that it includes revolutions under the same research programme (e.g. the Copernican Revolution within the Aristarchan/ Platonist programme) on the one hand, and cannot account for *progress* on the other hand.

In contrast, Polanyi's notion of tacit knowledge offers the needed resolution to the problem. On the one hand, Polanyi's tacit knowledge remains in touch with history because it gives room to the personal rational experience in an account of how science works. On the other hand, rather than make of the driving force of science (namely discovery) a rarity that occurs only during a revolution, Polanyi's notion of tacit knowledge generalizes this driving element and finds it at work in the day to day scientific solution of problems. There is a smooth continuity between such daily discoveries and revolutions when these occur. Lakatos had set out to tackle psychologistic justificationism and its child, the polarized pair of scepticism and dogmatism. I find that Polanyi's notion of tacit knowledge gets rid of the polarized pair (as I have discussed earlier on in treating the nature of tacit knowledge) in an economical way.

Every application of a formula in the solution of a problem involves the originality of the scientist. She uses the formula as a tool and is able to use it to solve the problem at hand. Thus far, every arrival at a solution is a discovery of some kind. In the meantime, the gap is bridged between a critical attitude on the one hand, and dogmatism on the other. The scientist is open to innovation when she employs the given laboratory practices and methods. She makes them her own within the tradition in which she researches. What drives her activity is a *commitment* to discovering the true nature of the world. Her innovativeness is open to being tested and perhaps proved in time. She has got universal intent in each of the scientific activities she is involved in day to day in the laboratory.

Further, it has been seen how Lakatos abandons Popperian falsificationism because it distorts history when it tries to come up with crucial experiments. He has introduced MSRPs which propound clusters of theories to which a given community of scientists is committed. Thus far, Lakatos admits of the element of commitment. But he makes two errors after that. First of all, he is involved in a tension between holding on to commitment across the clusters of theories (e.g. in defending the hard core, etc.) on the one hand, and maintaining the hope that the commitment is rational because of novel facts that are inherent in the hard core.

The error in this position is that Lakatos depersonalizes commitment (in a bid to de-psychologize methodology) and appeals to a deductive mechanism that underlies the commitment and makes it rational. He is then hard pressed to show why shifts from one programme to the next are *rational* i.e. why commitment could *ever* change from one programme to another. This again springs from a misunderstanding by Lakatos of the nature of personal commitment. The kind of psychologism that Lakatos combats is one in which the personal commitment is to a programme regardless of the possibly contradicting facts. But the personal commitment that Polanyi's notion of tacit knowledge advocates is commitment to reality as it unfolds. Such commitment is aided by a research programme, but it goes beyond the programme and opens up to future corroboration or disconfirmation.

Part Four: Conclusion - an evaluation

Chapter Six: The virtues of the theory of tacit knowledge

6.0 Introduction

An evaluation of the theory of tacit knowledge is due, having propounded it, discussed it, defended it from criticism and compared it to current methodologies of science. I begin by showing the virtues of the theory of tacit knowledge and then I outline the areas where the theory can be further developed – the weaknesses of the theory.

6.1 The virtues of Polanyi's theory of tacit knowledge

6.1.1 A critique of objectivism

Polanyi's theory of tacit knowledge is a constructive criticism to objectivism as promoted by the CR-ists. The theory shows the incompleteness of objectivism and proposes an alternative approach to science which, I have argued, is more plausible. H. Prosch makes a critical study of the theory of tacit knowledge. (1986) According to Prosch, such philosophers of science as Norwood Hanson, Paul Feyerabend, Thomas Kuhn and Stephen Toulmin raised objections to the objectivist drive in the philosophy of science and even proposed radical new ways to look at the methodology of science.

But even these critics seem (to Prosch) to have left the ideal of objectivism intact. Their criticism does not go far enough to dislodge objectivism, in fact they seem to take objectivism in scientific knowledge for granted as a given. Frederick Suppe, according to Prosch, recognizes that he is in a minority when he abandons the *K-K thesis*. According to the *K-K thesis*, knowledge is conditioned on the knowledge by the subject that she knows and that what she knows is correct. In other words, there is only knowledge where the knower can articulate what she knows and when what she knows is objectively correct. The *K-K thesis* is central to objectivism. To Prosch's understanding, Suppe is of the opinion that even such philosophers of science as Kuhn and Feyerabend, contemporaries of Polanyi, either explicitly or tacitly endorse objectivism because they leave the *K-K thesis* intact.

And so Suppe criticises and then dissociates himself with such objectivism. But this attempt by Suppe to dissociate himself from objectivism is not successful for he still seeks as an ideal: "to allow the canons of rationality for assessing knowledge claims to evolve without compromising the objectivity of knowledge." (1977, p. 723, also Prosch, 1986, p. 31) Suppe's goal is to leave objectivism or objectivity as an ideal (in a way similar to Musgrave's approach), safe from the real life contingencies of scientific activity in history. Prosch points out more evidence that shows that Suppe does not really abandon objectivism: "Epistemic relativism ... wherein changes in canons of rationality amount to changes in what counts as knowledge, making knowledge be whatever a science accepts and allows to enter its domain, [must be avoided] ... since it destroys the objectivity of scientific knowledge in precisely the ways that Kuhn's and Feyerabend's accounts do." (1977, p. 724, also Prosch, 1986, p. 31) And so in as far as the methodologies proposed by Kuhn, Feyerabend and Polanyi allow for actual scientific process to influence what qualifies to be knowledge, Suppe departs from their methodologies. He maintains what would qualify as an objectivist view in the understanding of Polanyi and Prosch. Given this position by Suppe, which position is nuanced but still fails to detach itself from the ideal of objectivism in science, Prosch's observation about mainline philosophies of science in general is plausible:

> "The notion that 'what counts as knowledge' must be true and detached objectivity seems to be ... still very much the basic creed of epistemologists and

> philosophers of science, whatever their critiques of each other. Therefore Polanyi's contention that the modern mind is fatefully enamoured with such an ideal is not at all passé. It was not merely the positivists of his day and those who espoused the 'received view' who were so enamoured with detached objectivity. It is still our own contemporaries in the natural sciences, the social sciences, psychology, and philosophy who have these ideas entrenched in their thinking. The philosophers, it is true, are having difficulty showing to each other's complete satisfaction just how objectivity *is* involved in the knowledge claims in science; but the ideas that science is, in fact, a source of objective knowledge and that what 'knowledge' *means* is strict objectivity, they apparently do not dream of giving up." (1986, pp. 31-32)

Polanyi's theory of tacit knowledge is a way out of objectivism. It accounts for the commitment with which a scientist or a group of scientists who make a novel contribution or discovery to science remain open to the possibility of their tenaciously held view being proven wrong in time. Thus far there is a personal aspect to knowledge in science which is not in opposition to an objective pole, this latter being the way currently held 'facts' may reveal themselves in deeper and unforeseen ways in the future. Examples of this phenomenon are the way Kepler's views are deeper manifestations of the Copernican position.

To resolve the persistent objectivist tendency, Polanyi goes back to the very way we perceive, in order to come up with a theory of how we come up with scientific knowledge. The way we perceive, according to him, is *active*. As Prosch points out, in Polanyi's rendering of perception, "... we create a tacit integration of sensations and feelings into a perceived object that then gives meaning to these sensations and feelings which they had not previously possessed". (1986, P. 53) Now, it happens that the activity of perception is to be learnt and it becomes a *skill* – one of knowing how to feed in the apparently missing links in order to complete a pattern (rightly or wrongly). Prosch interprets Polanyi's position to the effect that the ability to *see* objects is a result of learning the skill of attaining meaningful (even when non-explicit) integration of sensory clues. This is by a sustained, conscious effort which is itself aimed at perceiving. It is thus far neither exclusively a subject of formal inference from sense data, nor is it a direct and unmediated perception of the objects. (Prosch, 1986, P. 54) The clues that are spoken of do not all have to be used consciously. The perceiver may not be explicitly conscious of some of the clues, and yet she does use them – she perceives objects *focally* while relying on these clues *subsidiarily*. And for some clues still, even if she tried to know them explicitly, they would forever remain non-explicit.

Added to these clues which are immediate to the act of perception, some of the components of tacit knowledge are memories of 'normal' perceptions – past experiences we have absorbed into the back of our mind as being normal or standard. We bring these too to an act of perception. And further, some of the clues remain in the mechanics of perception, e.g. eye muscles and their adjustments, etc. These too influence our perception in ways we can neither totally ignore nor fully explicate or articulate. All the while, there is a conscious effort to organize the objects of our perception into meaningful wholes. We are trying to make *sense* of what we actively perceive. Some of perception necessarily remains at a subliminal level.

It is thus plausible to hold that we *know* more than we can actually ever explicate or articulate. It is akin to the knowledge of a skill e.g. the skill of riding a bicycle or swimming, which may not be fully articulated, yet it is knowledge. It is thus tenable on strictly epistemological grounds that the various factors that go into the formation of our perceptions have a subsidiary role to play as we make the integrations towards knowledge. They do not automatically, impersonally or 'objectively' cause our knowledge, even when they are completely subliminal. (Prosch, 1986, pp. 58-62)

6.1.2 A promisingly more robust epistemology and more versatile metaphysics

With the theory of tacit knowledge, Polanyi introduces a personal epistemology as opposed to an objectivist epistemology on the one hand and outright skepticism on the other hand. A central feature of this new epistemology is that *all* cognitive judgments are personal. This in turn is based on the fact that there is a tacit or inarticulate element to all knowledge. And the nature of tacit knowledge as discussed earlier is that it involves commitment. Thus far, even abstract proofs are in fact personal, for none of them can be made wholly explicit – there is a tacit dimension to them, e.g. the agreement on the meanings of the symbols used, the idea of proof, etc. Their meaning and acceptance is founded in or depends upon elements known only tacitly by a given mind in the action of judgment.

It is a valid objection to point out that there is an endless regress or reflexivity about personal judgment that seems to recede into infinity. When we focus our attention on the clues that help us to hold a personal judgment, those clues rely on other clues that must remain implicit for us to focus on the first set of clues. This recedes into infinity. I understand Polanyi's theory of tacit knowledge as a way to account for the reflexivity without slipping into skepticism on the one hand, or into dogmatism on the other hand. (Prosch, 1986, p. 7) I have pointed out together with Lakatos that the bipolar pair of skepticism/dogmatism is mutually dependent – where the one thrives, there thrives the other too.

Within the epistemology of science as proposed by Polanyi's theory of tacit knowledge, there is a way to avoid relapsing into dogmatism or scepticism. The scientist has learnt the skill of dwelling in the clues, formulae and theories she is using to access reality the way a blind person dwells in the tip of his cane or the way a researcher dwells in a tip of the probe she uses to access a hidden region. The theories, formulae, etc, the tools she has learnt to use to access reality through experimentation, observation, etc. are always personal because the world is presented to her, not in detached manner, but in the clues in which she dwells to be able to perceive the world.

The scientist dwells in the clues skilfully and personally. The resulting scientific knowledge is therefore also skilful and personal knowledge. On the one hand, she is thus shielded from skepticism for scepticism would be an absurd position to hold in the face of personal knowledge (as opposed to objectivist, detached knowledge). On the other hand, the scientist is constantly open to the possibility of her scientific *knowledge* being either confirmed in unforeseen ways, or even disproved as false altogether. She holds on to what may eventually be proved wrong. She is thus protected from dogmatism, which would be a contradictory position to hold in the context of personal knowledge.

A different metaphysics relates to this kind of epistemology. Without pronouncing himself specifically on the matter, Polanyi comes up with a metaphysics that helps avoid the horns of the realist/antirealist dilemma that grows in importance soon after Polanyi. In various places, Polanyi proposes that an entity is real for science just in case it can be expected to reveal itself in an indeterminate range of future discoveries or manifestations. (Polanyi, 1967, pp. 32-33)[51] And thus, if what we have grasped or what we know about an entity is true, our knowledge will be confirmed in unexpected ways in the future. In this sense, the

[51] Also argued by Polanyi. (1969, pp. 119-120, 135, 168; 1958, p. 147)

'realism' that Polanyi subscribes to is one in which minds, problems and skills are more real than cobblestones even though the latter are more tangible. The significance of an entity is of more importance than its tangibility in this case.

This kind of metaphysics expands 'reality' to entities that are still only clues that could lead to discovery. We can thus *know* such clues, i.e. tacitly. And as discussed earlier, tacit knowledge does not stop when we eventually know the entity more than just tacitly. Further, such a metaphysics is useful in understanding heuristics. Discovery is then a process rather than a jump or broken step from knowing nothing to knowing something suddenly. Rather, as the scientist draws nearer to making a breakthrough, she *knows the entity* towards which she is heuristically moving closer. And finally, such a metaphysics accounts for the epistemology of tacit knowledge in the sense that there is room for the active (and therefore fallible) participation of the perceiver, which is in turn an antidote to objectivism.

Thus far, the ingenuity of this metaphysics is that it avoids being bogged down in the realist/anti-realist debate. Polanyi is thus a realist in a way that is useful for a scientist. When Polanyi describes himself as a realist, he is not taking sides in the realist/anti-realist debate as it rages now. What the scientist is busy discovering is reality, but the search for discovering reality continues in open-ended fashion, guided along by an intellectual passion which is in turn guided by commitment and perception of beauty or elegance. Problems set are solved intermittently while these solutions remain to be tested against future discoveries. The scientist values the fact that she latches onto some aspect of reality, answers a given question, but she does not become dogmatic about the solution. The contentment of the scientist is a straightforward experience that does not necessitate positing or justifying a world external to the experience of the scientist (e.g. Plato's world of forms or ideas[52] or even a 'third realm/world' as Frege does and is followed by Popper and Musgrave). The functions of such a world (if it exists) are accounted for economically or in straightforward manner in the theory of tacit knowledge as part of the active imagination and experience of the scientist.

To illustrate this point on how there is no need for positing or justifying another world (e.g. a world of forms or a third world) I turn to an example given by Prosch. Prosch holds:

> "Present-day scientific taxonomy continues to order species in this manner [of natural classification – as propounded by Linnaeus and resumed fifty years later by A. P. de Candolle for plants and by Lamarck and Cuvier for animals] and is able to fit in new species when they are discovered. The number now so catalogued makes it ... a 'grandiose achievement' [and]... illustrates the most striking powers of tacit knowing. [We can] focus our attention on the joint meaning of particulars, even when the focus upon which we are attending has no tangible centre.... [Thus], through tacit knowing, we are able to know realities that are other than tangible objects. For surely the classes developed by the biologists, although not tangible objects, are as real as rocks, inasmuch as they have manifested themselves in their own future and have proven able to surprise us with even deeper meanings as time has passed. ... Thus... the conception of a real class is built up from our integration of tacitly known instances of this class, and it continues to be built up and modified ... by our continually noticing such instances." (1986, pp. 82-83)

And so past successes confirm the usefulness of the skill and in turn the discoveries made by using the skill show that the entities guessed at or surmised using the skill are actually real. All this happens within the tacit knowledge and the experience of the scientist without there being a necessity to posit an external world

[52] Plato's theory of 'a world of forms or ideas' is scattered over diverse writings including: *Meno* (71-80), *Parmenides* (129-135), *Phaedo* (73-80), *Republic* (i.e. Allegory of the Cave, 500-517), etc. I do not wish to go into details of it here, but would only like to mention that he posits this world to solve epistemological and metaphysical puzzles. (Plato, 1997/347)

of forms or a third world. In sum, the theory of tacit knowledge opens up a new epistemology and with it a more versatile metaphysics.

6.1.3 A more fruitful and more representative methodology of science

Polanyi's theory of tacit knowledge sketches a methodology that complements other methodologies in the sense of providing ways out from their quandaries. On top of that, his theory remains relevant to actual science rather than promote an aprioristic structure (e.g. objectivism). In these two ways, the theory of tacit knowledge keeps the debate on methodology open and fruitful in the sense that we can follow up on the debates on falsificationism, Kuhn's paradigms and revolutions, incommensurability, Lakatos's methodology of scientific research programmes, and Feyerabend's Dadaism.

In my opinion, none of these methodologies (or denials thereof) gives a satisfactory answer to how science works. Polanyi's theory of tacit knowledge shows that the difference between the way science works and the way perception works (and with that the learning of skills, recognition of physiognomies, use of tools, and the use of speech etc.) is one of degree and not in kind. Thus the crucial aspects of scientific knowing (e.g. induction of classes and concepts, etc.) are linked to tacit knowledge as seen in the above areas of knowledge and skills. The crucial aspects of science are only a more complex integration of less familiar clues, but this integration demands the *skills* of performance. The clues are in turn provided both implicitly and explicitly by the scientific community in which the scientist works and learns the tools of her trade. (Prosch, 1986, p. 93)

What many other methodologies had sought to brush aside as belonging to the realm of psychology as opposed to methodology and the philosophy of science, Polanyi brings back in the centre of scientific methodology. As Prosch points out, many other methodologies could not deal with the proper problem of induction, namely the true origin of the hypotheses that the said methodologies (verificationism, falsificationism, etc.) claimed were put to an explicit and objective test. Many of the said methodologies relegated this question to psychology. But Polanyi sought to restore it to the formal structure of science. Prosch observes:

> "Polanyi thought that it was rather such a rational or logical account of the origin of hypotheses which was the crucial problem. For, as he said, hypotheses (particularly those created by good scientists) proved to be on the right track far too often to be the results of mere chance trial and error or the nonheuristically oriented, nonrational psychological or sociological factors." (1986, p. 94)

Thus, for Polanyi, heuristic processes (as discussed above in the treatment of the nature of tacit knowledge) should form the basis of the methodology of science. In other words, methodology of science should engage itself primarily with the process of discovery. Even if the nature of these processes led away from formal to informal methods, philosophy of science should pursue them there too, rather than set out to establish an aprioristic and rigid formal structure (e.g. objectivism). Unlike such aprioristic methodologists of science (e.g. forwarded by objectivists), the theory of tacit knowledge maintains that at the core of scientific knowledge was the knowledge of a good problem. This is as much tacit knowledge as is the knowledge of a face or a taxonomical class. In both of these examples, the knowledge is unspecifiable, yet it exists. The theory of tacit knowledge propounds that such knowledge, rather than a posited world/realm of real ideas is the solution to the paradox of how science actually keeps on track through the surmises of its experts.

As to the *truth* of such knowledge, it depends on whether or not the putative knowledge makes contact with reality (as defined above). This 'knowledge' is false if it does not continue to reveal itself in the same or unforeseen ways that reveal aspects that were not known before, and the contrary holds if this knowledge reveals itself in unforeseen ways. It is not entirely a question of consensus or being backed by others seeing the world in the same way. Nor is it definitively negated when it does not receive backing from other scientists. It is personal, bearing the conviction of a given scientist or scientists, and has an external pole of universal intent. What *we* hold with *universal intent* to be true, we hold to be *knowledge.* (Prosch, 1986, pp. 97-98) Given this structure, there is no longer a dichotomy between objective and subjective knowledge. The personal element bridges the two extremes. The scientist is committed to her view about the world and yet remains open to the confirmation or disconfirmation of her position.

As the scientist comes closer to a discovery, she engages her imagination on the one hand, and her intuition qualified by Polanyi as interpreted by Prosch, not as a "... supreme immediate knowledge à la Leibniz or Spinoza or Husserl ... [but rather] ... it is a skill for guessing with a reasonable chance of guessing right, a skill guided by an innate sensibility to coherence, which can even be improved by schooling." (1986, p. 102) This intuition is set spontaneously in motion on the other hand. The intuition senses the *nearness* of a solution to the set problem. The same intuition launches the imagination and organizes the evidence that may be useful in the integration that leads to a solution. (Prosch, 1986, p. 101) In the end, the intuition points out the *feasible* answer to the problem and puts on hold the process of discovery. Creative imagination works hand in hand with creative intuition. A viable and representative methodology cannot overlook this central activity in science in the name of objectivism or any aprioristic formal account. The theory of tacit knowledge shows that there is a way of accounting for intuition and imagination within the methodology of science. Thus far, as Prosch points out:

> "Our creative imagination is ... 'imbued' with our creative intuition. Imagination stimulates and releases the powers of intuition by imposing upon intuition a feasible task. Still, imagination is guided by our intuition in its intimation of the feasibility of a problem and in its engagement in pursuit. So imagination sets actively before us the focal point to be aimed at, but it is intuition that supplies our imagination with the organization of subsidiary clues to accomplish its focal goal, as well as the initial assessments of the feasibility of this goal. Intuition thus guides our imagination. Sallies of the imagination that have no such guidance are ... idle fancies." (1986, p. 103)

As discussed earlier, intuition plays a further role in guiding the scientist to know when to stop the search and settle on a result, at least for the time being. Thus, shunning a philosophical reflection on the intuition/imagination in methodology would only weaken the accounts of methodology. The argument that this area of the scientist's activity is to be left to psychology is not adequate, as I have argued in my response to Musgrave and those who argue like him above.

An objection to this approach may point out that advocating for such an account disenfranchises the methodologist of science and makes her totally dependent on the biographical accounts of scientists. I will deal with this weakness of tacit knowledge below, but a preliminary response to the objection is that as I have discussed above, Lakatos/Zahar present a robust methodology of science whose major weakness is how to explain the central notion of novel facts. One remedy for this weakness as pointed out earlier is the need to have access to the biographical accounts of the scientists. That is the right direction to go, but unfortunately it is not practicable for the history of science. What the theory of tacit knowledge does is to save the methodologist from having to account for the biographical details and provide a more general picture of how such details remain rational rather than idiosyncratic.

6.1.4 A richer account of scientific change

Mainline methodologies do not account adequately for change in science. Such methodologies include verificationism, falsificationism, paradigm shifts and revolutions, and the idea that anything goes. The theory of tacit knowledge offers a richer account of scientific change. Within the theory of tacit knowledge, scientists do not go out *deliberately* to abandon one paradigm or research programme for another startlingly different one. Rather, just as their clues remain inarticulate and inarticulable their reasons for choosing the new paradigm or programme remain inarticulate and inarticulable.

The scientist who spearheads the change (or in other words the paradigm shift, revolution, or change of research programme) has actually taken an already accepted theory *more seriously*, concretely or literally than any other scientist. The said scientist proceeds to expand on an already existing position in bolder and more revolutionary manner than others of the same interpretative framework. The transition from one interpretative framework to the next is not always clear cut or deliberate, even though it may appear discrete and even revolutionary in hindsight.

As an example, Polanyi points out that one of the greatest and most surprising discoveries was one in which von Laue discovered the diffraction of X-rays by crystals. He made the discovery simply by believing more concretely in the theories of crystals and X-rays current at his time. In similar fashion, when Einstein produced a theory of Brownian motion in 1905, he came to it by making a very literal articulation of the then current kinetic theory of gases. (1961, pp. 378-379)

Thus far, the scientist who spearheads the change is guided by criteria of scientific merit and by plausibility. Criteria of scientific merit, together with plausibility, require the personal judgment of scientists with knowledge of the area of science concerned. Plausibility has to do with soundness within the limits of prevailing science.

Unlike a few other mainline accounts, this account of change in science provided by the theory of tacit knowledge avoids introducing a new (and ad hoc) element to explain a discrepancy. Kuhn introduces quasi-religious conversion, and Lakatos/Zahar, for example, introduce the notion of 'novel change'. Neither of these new elements helps the respective accounts in the long run. The theory of tacit knowledge, on the contrary, remains within the notions already provided.

Further, the theory of tacit knowledge is economical in resolving some of the discrepancies that mainline methodologies fail to resolve. First of all, Popper's methodology of (naïve) falsificationism cannot make room for the apparent stubbornness of scientists who stick with their otherwise falsified theories as often good practice. Polanyi's solution to this problem is that the scientist spearheading change has widened the meaning of key concepts within the theory (e.g. by taking on a very literal interpretation of the key concepts hitherto used only as models, e.g. in the case where Einstein takes a literal meaning of Brownian theory). The scientist is not consciously abandoning the first theory for a totally different second one. When a theory is apparently falsified, the scientist may still be able to perceive (inarticulately) the validity of the 'falsified' theory. She then holds on doggedly to it in the face of the odds raised by objectors. In some instances in the history of science such apparently untraditional views have been vindicated with time.

Likewise, Kuhn's theory of the incommensurability of paradigms could be resolved by Polanyi's account of change. Briefly, Polanyi shows that the view of the changes as radical revolutions is only partial, selecting only some elements of the change. In the same selectivity, the persisting elements between paradigms are lost sight of and then we have a problem of incommensurability between paradigms. Polanyi's approach makes the Kuhnian quasi-religious conversions from one paradigm to the next irrelevant.

Finally, Lakatos too has problems with demonstrating the objective grounds on which scientists abandon a non-productive research programme for a more fruitful one. In the final analysis, the decision to change to a new research programme seems to be based on reasons other than ones *strictly objective* in the objectivist Lakatosian sense. I have discussed earlier how the introduction of the notion of 'novel facts' does not resolve the problem of how a research programme is abandoned for a more fruitful programme. I have shown how the Lakatos/Zahar methodology would include revolutions within one and the same research programme.

But there is actually a place for a real break with tradition when we look at events with hindsight. We call these developments revolutions. An example of such a break with tradition is when Copernicus chooses the heliocentric over the geocentric world view. We cannot appraise all the clues that Copernicus uses to arrive at this 'decision' any more than he could have articulated all of them. Another even more dramatic example is the way Einstein comes to the idea of relativity. Prosch reports that the account on how Einstein comes to the idea of relativity is fraught with clashing thoughts:

> "Einstein tells in his autobiography that it was the example of the two great fundamental impossibilities underlying thermodynamics that suggested to him the absolute impossibility of observing absolute motion. But today we can see no connection at all between thermodynamics and relativity. Einstein acknowledged his debt to Mach and it is generally thought, therefore, that he confirmed Mach's thesis that the Newtonian doctrine of absolute rest is meaningless; but what Einstein actually proved was, on the contrary, that Newton's doctrine, far from being meaningless, was false. Again, Einstein's redefinition of simultaneity originated modern operationalism; but he himself sharply opposed the way Mach would replace the conception of atoms by their directly observable manifestations." (1986, p. 89)[53]

And so an objectivist methodology that hopes to put a finger on and eventually render articulate (and perhaps manipulable) the creative process through which the scientist goes, is mistaken. The theory of tacit knowledge acknowledges that such lack of articulation can be included in a methodology and thus keep the methodology true to the activity of science. Popper, Kuhn and Lakatos (among others) encounter a problem in their account of how science works because they have overlooked the fact that some knowledge in science is tacit and inarticulable and that this is not a problem for methodology. The clues that the 'revolutionary' scientist uses to undertake a seemingly brand new position, or the clues that she uses to stay with an otherwise 'falsified' theory, are not articulable by her or anybody. But looking back, accounts may seem to make the choice of the clues look like a deliberate activity. Prosch explains:

> "The subsidiary presence of the principles entailed in a discovery show us how change in our standards (in our paradigms) [occurs]. In solving a problem our intuition may respond to our efforts with a solution entailing new standards of coherence, new values. In affirming the solution we may find we have also affirmed the new standards as binding upon us. The new values have entered subsidiarily, embodied in a creative action. After this subsidiary entry, these

[53] Prosch citing Polanyi. (Polanyi, 1966)

> new standards can come to be spelled out and professed in explicit terms. This may make them seem to have been chosen by us. But actually they never were, as such. They were only covertly adopted. Concrete commitments we make to our perceived coherences do bear witness to values – sometimes new values – but the grounds for these values were hidden in the subsidiary clues when these were integrated through our creative action into our perceived coherences." (1986, pp. 104-105)

This explanation by Prosch shows how the theory of tacit knowledge is both consistent and richer than the alternatives. It is consistent and rich because it encompasses the way new values are introduced in science, in the arts and even in human relations. (Prosch, 1986, p. 105) It is consistent with the way language functions and grows as I have discussed earlier in treating the nature of tacit knowledge.

6.1.5 Including the indeterminacy of science

The theory of tacit knowledge does not shy away from indeterminacy in science, for such indeterminacy forms an essential part of how science works. Rather than consign indeterminacy to other areas of research outside the philosophy of science or methodology, the theory of tacit knowledge provides an epistemological structure that can *include* indeterminacy. Within the theory of tacit knowledge, the mechanism of interpretation that is necessary in order to recognize and understand what goes on in the results of an experiment is not explicit. It is *dwelt in*, or assumed, in order to focus on the problems or scientific task at hand. This mechanism is not in one scientist's mind. Rather, it is a shared patrimony among the scientists of the same area of science. It gets passed on implicitly as the scientists become members of the scientific community. It also gets changed implicitly as it is constantly applied to changing situations.

What Polanyi holds here applies to Kuhn's paradigms and Lakatos' MSRPs. Scientists do not externally and deliberately set out to adopt or to change the paradigms or MSRPs of the scientific communities to which they belong. This is at one and the same time the way in which science protects itself from cranks and charlatans, even though in the process it is a way in which truths can be repressed by a system. But perhaps that is the price that must be paid if there is to be science at all. (Prosch, 1986, pp. 108-109)

It should be noted, that the theory of tacit knowledge is neither against nor oblivious of the logical relations between hypotheses and theories, or between theories and methods of measurement, and the whole formalization process of computation, algebra, and instrumentation. These, it must be acknowledged, are important for the sharing of information between scientists. Nevertheless, even in order to interpret these aids to communication, there is a tacit component that scientists bring into play, without which all this structure of formalization and communication would remain meaningless. Their meaning, in other words, is given them by a mind – the mind of a given scientist at each time. (Prosch, 1986, p. 110)

In this sense, science must remain subject to the scrutiny of observational evidence. But again, when evidence is contradictory, science does at times exercise judgment or discretion. As examples, the periodic table of elements and the quantum theory of light were held on to in spite of contradicting evidence. And so at times apparent exceptions to a general rule in science may serve to elucidate rather than refute the rule. In this way, a deeper meaning of the general rule is arrived at with the help of contradicting evidence. At times such contradictions are ignored as mere anomalies that cannot in the moment be accounted for. (Prosch, 1986, p. 111)

In summary, indeterminacy expresses itself in at least five ways. First of all, the affirmations we make about the *real* in nature (i.e. as defined above as what will reveal itself in unforeseen ways in the future) must always remain *indeterminate* for the real we seek to affirm is always richer than the contents of our affirmations. We cannot totally explicate the full extent of the meanings of our present affirmations about the real in nature.

Secondly, there are no explicit rules for deciding whether a perceived pattern in nature is a result of chance or a manifestation of the way nature behaves. The judgment on such a phenomenon is arrived at by a skilful application of values learnt within the scientific community. Thirdly, we may not know the grounds on which we hold our knowledge to be true, when we refer to knowledge of skills and performances. And science is, I argue, in many ways one such kind of knowledge. Fourthly, in order to focus on a given coherence in nature, we must necessarily keep elements of our knowledge (or clues) in the subsidiary realm. Whenever we switch to these subsidiary elements themselves, we lose sight of the coherences. And so the subsidiary elements must remain unspecified in the act of focusing on the goal of our knowledge at each time.

And lastly, the existential elements involved in helping us to modify the grounds of scientific judgment remain unspecifiable. We dwell in the methods i.e. we incorporate them in our being as we change the grounds for making scientific judgments. And so, as we change the grounds, we change ourselves. We are personally involved in the process. And so, for the theory of tacit knowledge as explained by Polanyi, logic has to be understood as going beyond merely explicit or propositional logic. It should include the logic of tacit inference by which a mind dwelling in subsidiary clues crosses a logical gap in discovery and in the justification of discoveries. The same logic of tacit inference operates in perception and in the use of language (for example), even though these two are not explicit or propositional, but skills.

6.1.6 Grounds for answering to skepticism

The theory of tacit knowledge is central to Polanyi's response to the critical and sceptical trends in philosophy as a whole. I argue that his theory of tacit knowledge does lay grounds upon which the problem of persistent skepticism can be addressed and perhaps resolved. The subject of skepticism is much broader than I can attempt to discuss here. But in the theory of tacit knowledge I see a new way out. Polanyi describes his work as 'post-critical'.

Polanyi holds that whenever we say of something that we know it (i.e. in propositional knowledge), we cross a logical gap between our stated belief (i.e. what we know) and the entity itself. Now, this is an indeterminacy we cannot reduce in our activity of knowing if we are to say we have propositional knowledge at all. There is no explicitly or objectively *logical* way to bridge this gap. There are two ways left. We can either acknowledge that our best knowledge about an entity is in fact *true* of the entity, or we can assume a sceptical posture and point out that our knowledge is merely what our epistemological apparatus constrains us to say we know. The latter posture is also shared by the critical approach to epistemology. (Prosch, 1986, p. 223)

An objection to Polanyi's view here is that the gap *persists* between the epistemological and the ontological. But within the theory of tacit knowledge, Polanyi provides a *persuasive* answer to this objection which gives the details of how the logical gap is bridged. Tacit knowledge provides a relationship of *meaning* between two phenomena or entities, i.e. what we know in terms of the internal processes of knowing and the entity out there towards which our knowledge is directed.

Now tacit knowledge goes beyond the internal processes by which we know the entity towards which we direct our attention. Experiments in subliminal perception alluded to by Polanyi have shown that humans are capable of responding to stimuli in ways they cannot articulate. Polanyi speaks in particular about an experiment in which spontaneous muscle twitching that is unfelt by the subject and seen by great visual amplification can be affected through a medium e.g. noise. The twitching could be increased or decreased if it is followed by the silencing of an unpleasant noise. (Polanyi, 1967, p. 14)[54]

Tacit knowledge helps to form the bridge between our internal processes and the stimulus, organizing or integrating them into a meaning that we can perhaps articulate. The way the meaning is organized is *from* our internal processes *to* the stimulus or entity out there. The qualities of the entity do make an impact on our internal processes by the help of tacit knowledge. (Prosch, 1986, p. 224) In the meantime, the idea of the real – i.e. as what may reveal itself in unforeseen ways in the future – should be kept in mind. Also, it should be noted that this explanation allows for fallibility of the process of perception at various points, e.g. in the organisation of the internal processes. And so the theory of tacit knowledge is not replacing skepticism with dogmatism by the use of tacit knowledge. Rather, the role of tacit knowledge is shown to be one of reassurance to the would-be sceptic that we can rely on a part of our knowledge – an important part of our knowledge – to furnish us with information concerning the world out there.

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[54] The subject of subliminal perception has been controversial in the circles of psychology from its inception. But Dixon argues that something about it persists through the controversy, and that perhaps this is a pointer that it ought to be given attention. (Dixon, 1971)

In perceiving a cat, for example, we see many parts which we *integrate* epistemically (i.e. with the help of tacit knowledge) to come up with the entity we call a cat. But in the meantime, the same entity – the cat – is at the ontological level an aggregate, an integration of the parts that form the entity that is the cat. We *dwell* in the various acts that help us form the epistemic integration in order to *see* the cat. We then hold on to this position, namely that we have seen a cat that is out there in reality, with universal intent and in the hope that this vision of the cat is going to confirm itself more meaningfully (e.g. through cat-like behaviour and all else we can study or research to better understand the cat) in the future.

In the same way, scientists who come up with various theories take them to latch onto reality out there – reality as explained in the theory of tacit knowledge. In this sense, Newton believed in the existence of gravitational force and in the view that there are atoms whose only power is inertia. He chose not to suspect his ideas of being mere conceptual constructions – figments of imagination or illusions. Likewise, Einstein believed that his theories were describing the world as it actually is and that quantum mechanics is not correct. For these and similar scientists, their views were believed to describe the world and the scientists remained committed to universal intent – that future manifestations of their views could either confirm or disprove their theories. (Prosch, 1986, p. 232)

6.2 Appraising the weaknesses of the theory of tacit knowledge

6.2.0 Introduction

The main thrust of my argument has been a defence of the theory of tacit knowledge as propounded by Polanyi, against insignificant opposition (i.e. opposition based on misunderstanding of the theory of tacit knowledge) from significant corners (i.e. established or mainline philosophical positions). As Prosch points out, "the only critics [of Polanyi] who I have discovered take issue with his most basic positions do not seem to me to understand these positions, and so are all but worthless in providing any telling objections to his fundamental principles." (1986, p. 9) It is plausible to say that Polanyi's position is complicated but in the main plausible and it opens up new opportunities. What then is significant and fair criticism to the theory of tacit knowledge?

6.2.1 Lack of precision

Polanyi's central concept of tacit knowledge is vague or imprecise on two levels – the micro-level and the macro-level. On the micro-level, Polanyi does not come up with a clear definition of the concept as he applies it in various contexts or to explain the various stages in the activity of science. I have discussed and laid out Polanyi's rendering of the role of tacit knowledge right from the conception of a problem through the discovery of a solution and into the inclusion of the solution or forms of it in the body of scientific knowledge. Coming up with a precise definition or definitions of tacit knowledge would help clarify the debate on its characteristics and its functions. Little wonder that apparently none of his most audible critics have understood the notion. Much of the criticism has thus ended up being irrelevant to the notion and it has further entrenched the critics of tacit knowledge in their attitude of ignoring the notion outright, or only paying it lip service.

A response to this criticism of vagueness at the micro-level can be found from a charitable reading of the theory of tacit knowledge. One of the main objectives in underlining the role of tacit knowledge has been to broaden the concept of knowledge beyond the merely objective (i.e. against objectivism). And in the process, it has been proved that even the most formal knowledge is based on some tacit components that must be assumed in order for the formal knowledge to be held. Seeking to define each element of a formula in physics, for example, would lead to an infinite regress of definitions.

In acknowledging the role of tacit knowledge, we come to a rational way of ending the infinite regress. Tacit knowledge is appraised in such moments not as a notion that explains everything that we fail to explain at the moment, but as a not totally irrational area of knowledge to be left to other areas of research (e.g. psychology as understood by the critical rationalists). I have discussed and agreed with Polanyi that tacit knowledge makes sense and acknowledging it in accounts of how science works is an essential part of methodology. Seen in this light, there is a consistency in not insisting too much on a precise definition of tacit knowledge in its various manifestations in the activity of science. Tacit knowledge defies an objectivist definition and it is thus internally consistent in showing the limits of objectivism. It is enough for the notion to perform a *persuasive* role in our understanding of the activity of science.

The criticism of vagueness could be raised at the macro-level. So far, the question remains open: Does Polanyi's notion of tacit knowledge reduce tacit knowledge to one single phenomenon? What evidence is there to show that the kind of tacit knowledge that operates in heuristics is the very same tacit knowledge that is at work in the organisation of a scientific community? These seem to be intuitively very diverse forms of epistemic activity – the one being at the individual level and the other being at the social level. This is a valid objection. In fact even when we admit the role of tacit knowledge in the activity of science, it does not follow that we have one form of tacit knowledge. The various forms may be irreducible to one another.

In response to this criticism of vagueness at the macro-level, I will borrow the analogy of the idea of objectivity. Heather Douglas has written about the irreducible complexity of the various meanings of the word 'objectivity' as used by philosophers and scientists. (Douglas, 2004)[55] She argues that there are at least eight different categories of 'objectivity' that can be classified into three different modes. The three different modes and their categories are as follows. Douglas isolates objectivity$_1$ in which we look at processes by which we interact with the world (e.g. experimentation, observations, etc.). The categories of objectivity that fall under objectivity$_1$ are: a) where manipulable processes produce reliable results (and this goes in the direction of realism which Douglas admittedly avoids); b) convergent objectivity whereby various processes end up giving the same durable results.

The second mode of objectivity, objectivity$_2$ focuses on the individual's thought processes or reasoning processes and picks out the values in the processes. The categories Douglas includes in objectivity$_2$ are: c) detached objectivity$_2$ and this is rapidly expanded to d) value-free objectivity$_2$ which makes an effort to hide or deny values that are considered non-epistemic, since these may distort knowledge. The preoccupation of holders of this mode of objectivity would look out for and weed out psychologism from epistemology. Linked to (d), Douglas points out another category of objectivity e) value-free objectivity$_2$. In this kind of objectivity, one takes a balanced position about the various values that go into arriving at given results, e.g. when reporting on current peer-reviewed literature. Such value-free objectivity$_2$ can thus mean being reflectively centrist.

[55] I go into the article to some detail, while avoiding a discussion, because it ties in well with my treatment of objectivity as understood in the Polanyian theory of tacit knowledge.

The third mode of objectivity that Douglas proposes is objectivity$_3$ in which we focus on the social processes that engender and structure epistemically important procedures and show us how agreement is reached. The categories included in objectivity$_3$ are the following. There is f) procedural objectivity$_3$ in which the same procedure produces the same result regardless of who uses it, and regardless of their idiosyncrasies. An example of this could be a rigid quantitative process that eliminates the need for personal judgment. Related to procedural objectivity$_3$ is g) concordant objectivity$_3$ in which agreement is sought not through the elimination of individual judgments, but through agreement of such individual judgments. And finally, she proposes h) interactive objectivity$_3$ in which agreement among the individual judgments is sought through discussion of the points of difference (e.g. at conferences, etc.).

Objections to Heather's position can be raised by philosophers who argue for a single or unified meaning of 'objectivity' to which all other variations of it can be reduced. Among these are Nozick (1998) and Nagel (1986). Against these, other arguments are raised in support of objectivity as a complex notion with several interconnected and sometimes overlapping meanings, are raised. Among holders of the latter view are Lloyd (1995) and Megill (1994).

This detailed treatment of objectivity by Douglas is useful for analogical purposes. Her discussion shows that although there are many different (and perhaps irreducible) meanings of *one* central epistemological term of 'objectivity', the term retains a unity of coherence. (Douglas, 2004, p. 465) I suggest that tacit knowledge should be approached in a similar way. The way it is propounded by Polanyi shows that the notion of tacit knowledge has got conceptual coherence, even though a further study may show that the various expressions of tacit knowledge are logically irreducible to one another. They are all interconnected and they sometimes do overlap. Yet the task still remains of classifying the various forms of tacit knowledge and showing the relationship between them.

6.2.2 Insufficient attention to foregoing philosophical thought

I have argued that Polanyi's theory of tacit knowledge promises a *new* epistemology. But the price for this novelty is that Polanyi has only paid passing attention to areas of traditional or mainline philosophy like the philosophy of language and post-Cartesian critical thought. This is in an effort to propound a post-critical philosophy. Even within the philosophy of science, Polanyi labels most of the philosophy of science and methodology as marked by positivism and objectivism.

One may object to this Polanyian approach and point out that in fact it breaches the precepts he himself sets out to propagate – namely the precept of abiding as much as possible by the traditional position (i.e. as held by the scientific community) as a measure against charlatans. Polanyi himself makes a late entry into philosophy. This criticism may go on to point out that there is little wonder that mainline philosophers have ignored Polanyi's theory of tacit knowledge. But Polanyi makes a choice to ignore mainline philosophy in his analysis of the malaise of philosophy, for as Prosch points out, Polanyi traces the problem of objectivism in Western philosophy as far back as Plato. (1986, p. 51) He thus wishes to make a fresh start, based on what he himself would discover. And thus:

> "His eventual discovery of what he believed to be the true epistemology became in his own eyes a totally new philosophical beginning, a backing off from the current ways of going at the problems of knowledge in order to see them from a truly new perspective. His philosophy owes its freshness ... largely to the fact that he did not labour explicitly upon the particular problems

> of knowledge with which most Anglo-American philosophers have been concerned, nor upon those that classic philosophers have worked with either. He tended to outflank these problems and to raise somewhat different ones upon grounds on which many contemporary philosophers have difficulty finding a footing." (1986, p. 51)

Polanyi approaches the entire epistemological enterprise from the point of view of skilful knowledge and practice of science. He hopes that philosophy, one area of cultural endeavour, could learn from the apparent advancement of science, another area of cultural endeavour. In the meantime, science itself is *not* totally detached from other cultural endeavours like art. (Polanyi & Prosch, 1975)

6.2.2.1 Relating to Thomas Kuhn

Some have conflated the methodology of Thomas Kuhn with Polanyi's theory of tacit knowledge. Recent comparative research on the thought of the two philosophers has shown that even when the two were still alive and in correspondence, they were aware of and expressed the fundamental differences there are between their philosophies of science. Archival research has shown that Kuhn was aware of Polanyi's thought.[56] Polanyi saw in Kuhn a philosopher with whom he could join forces and together they influenced each other's thought. It is important to draw the differences between the two in order to answer to the tendency of conflating them.

Moleski has done considerable research on the life and work of Polanyi. He notes that there are important similarities between Polanyi's notion of "interpretative framework" and Kuhn's wider or later notion of "paradigm". He notes:

[56] Some (e.g. Moleski, 2007) carry this relationship to the extent of Kuhn being indebted to Polanyi for his understanding of paradigms and how they operate in science. But I skirt this controversy here because it is still under investigation.

> "In the larger sense of the word, "paradigm" covers much the same ground as "interpretative framework"; both of them: create jargon; identify significant data; suggest canonical interpretations of the data; divide one school from another; define formal operations for practitioners; are surprisingly fruitful, even when wrong; explain the history of science in a satisfying fashion; depend on commitment; exhibit a tacit/articulate structure." (Moleski, 2006-2007, p. 8)

Given these similarities, the conflation of the two approaches is understandable. But the differences are significant. First of all the two differ in metaphysical vision. According to Moleski, Polanyi's understanding of 'interpretative frameworks' offers a richer metaphysical view than Kuhn's notion of 'paradigm'. The reason he advances is that everything Kuhn understands about paradigms can be mapped into Polanyi's notion of interpretative frameworks, whereas not everything in Polanyi's position finds a correlative structure in Kuhn's. (Moleski, 2006-2007, p. 9) For Moleski, Polanyi is obviously a metaphysical realist whereas Kuhn is still stuck in the positivistic resistance to metaphysics. Without going into the realism debate, I do not think that Moleski raises enough reasons to situate Polanyi among the metaphysical realists. It seems to me that Polanyi does take metaphysics seriously – he believes that what scientists are concerned to discover is the truth about the world. But Polanyi can still hold his position validly without thereby becoming an obvious realist. His view of reality has been shown earlier. What is real is tied to what is meaningful and promises to reveal yet unforeseen aspects and meanings of itself with time. That is not strictly entity realism.

Even then, the difference Moleski points out is important, for even though Polanyi cannot be categorized among the realists, he is not averse to realism in principle. Kuhn is reluctant to be classified among realists. When Kuhn tries to extricate himself from the metaphysical problems caused by his notion of paradigm that avoids metaphysics, he resorts to the view of truth in science as comparable to adaptability in species in the Darwinian concept of evolution. The evolved species have neither necessarily arrived at the 'truth', nor at the 'way to be'.

Yet the later results of evolution are meant to bring up more adaptable species. Kuhn points out: "I would argue very strongly that the Darwinian metaphor at the end of the book is right, and should have been taken more seriously than it was; and *nobody* took it seriously. People passed it right by." (Kuhn, 2000, p. 307) It is curious that a methodology that places a lot of significance on *revolutions* and incommensurability (i.e. Kuhn's methodology) should resort to an *evolutionary* approach. This is an inconsistency in method. In contrast, Polanyi's theory of tacit knowledge does not face such a problem of inconsistency.

A second major difference between Kuhn and Polanyi that should stop us from conflating the two is the object towards which scientists are committed. Both methodologies have a place for commitment on the part of the scientist. Kuhn himself points out that there may be differences between his and Polanyi's position on the matter of what scientists are committed to. According to Moleski, Kuhn holds in a paper entitled "The Function of Dogma in Scientific Research" (1961)[57] that "The discussion which follows this paper will indicate that Mr. Polanyi and I differ somewhat about what scientists are committed to, but that should not disguise the very great extent of our agreement about the issues discussed explicitly below." (2006-2007, p. 10) In Kuhn's notion of the paradigm, scientists are committed to the beliefs of the scientific community. Truth is arrived at by consensus among the experts in a field in a given paradigm. Moleski points out that the way Polanyi

[57] Moleski cites from footnote 347 of Kuhn's paper.

perceives Kuhn is such that "Kuhn's concept of paradigm means 'a commitment to a framework of accepted beliefs'." (2006-2007, p. 11)

This view is partially responsible for the quandaries that Kuhn later faces in the incommensurability of paradigms, and the need for quasi-religious conversion for scientists who have to give up one paradigm for a new and better one. There is necessity for revolution at each change of a paradigm. But in the view of Polanyi, the scientist is committed first of all to the beauty of the reality she is discovering. She passionately shares her findings in the scientific community, with some room for *dissent.* The scientist is committed to universal intent – that her theories will be brought to bear on reality in so far unforeseen future instances. In the view of Polanyi, there is no need for incommensurability between interpretative frameworks. There is hardly a need for a religious conversion, as changes take place within a context of discovery that consistently runs through the whole scientific enterprise. Hence, revolutions have a place, but they do not have to be the norm in scientific progress from one paradigm to the next.

And so overall, Kuhn started out by turning his attention to a faithful representation of science in history. He set out to provide a methodology that would be in congruence with the history of science. He did make some interesting observations and he brought some details to clearer light. But somehow, Kuhn appears to introduce an element of irrationalism within his account of the methodologies of science. While his notions of paradigm shift and scientific community were taken up especially by sociologists of science, the wider framework could not withstand criticism. And so Kuhn does not deliver a decisive blow or overall better solution to the problems posed by Popper's approach of falsificationism. In contrast, Polanyi's theory of tacit knowledge, as I have discussed, offers a more tenable position.

Thus far, there are similarities, and an emphasis on the differences is meant more as a clarification of the possible misunderstanding that comes with the conflation of the two. And where there are similarities, Polanyi's treatment of the area of similarity is richer, more far reaching and raises fewer questions than Kuhn's alternative. Polanyi himself comments about the efforts by Kuhn: "I can accept the excellent paper by Mr. Kuhn only as a fragment of an intended revision of the theory of scientific knowledge. Otherwise it would not only fail to answer the questions it raises, but appear altogether to ignore them" (Polanyi, 1963, p. 380)

6.3 Conclusion

Polanyi's theory of tacit knowledge fits in a wider programme in which Polanyi sets out to find a solution to a general social malaise in the way society thinks and acts. The malaise, according to Polanyi and as interpreted by H. Prosch, is responsible for intellectual, moral, social and economic dysfunction of society, especially his contemporary European society. The theory of tacit knowledge is therefore a segment, albeit a key segment, of a more complex whole. Polanyi sets himself this goal (reminiscent of the way ancient philosophers like Socrates set out to be physicians of society) while being in correspondence with other scientists including Fritz Haber, Erwin Schroedinger, Max Planck, and Albert Einstein. As Prosch points out:

> "Polanyi had long been critical of the extreme positivistic view of science, a view most popular in his younger days, but which even now exerts influence upon modern thought, not only as a philosophy of science but in many other ways as well. The intellectual connection of positivism with nihilism and of

nihilism with the ruthless political movements of the left and the right in his day became ever clearer to him. As they did, the importance and intellectual respectability of holding firm beliefs on the ideals essential to science and to a free society – and an acknowledgment of their interconnection – also became ever more clear to him ... No one seemed to see the extent to which not only the existence of a free society, *but also the existence of this presumably verifiable science itself*, rested upon freely held beliefs in ideals and principles that not only could not be proved, but could not even be made wholly explicit. It seemed to him that no one saw that the unprovability of these beliefs did not render them intellectually unrespectable or unworthy of being held. It therefore appeared to him necessary to show people, philosophers included, why and how this was so." (1986, p. 5)[58]

Polanyi was consciously carrying out a complete overhaul of a whole way of thinking in order to preserve freedom as a whole, and the freedom of science in particular. But Polanyi has largely been ignored in philosophical circles. Some recognition has been rendered him in other intellectual circles, e.g. among sociologists, economists, psychologists, scientists and theologians.

An overall advantage of Polanyi's approach is that it helps us to avoid the seemingly unavoidable choice between subjectivism and objectivism. Instead, we have an analysis of perception by a combination of subsidiary and focal awareness. This analysis is supported by the example of how science works. We as persons are involved. We thus avoid such reductionism. We also avoid skepticism by avoiding the positivistic revulsion of referring to the 'real' and to replace this 'real' by an ersatz objectivistic and logical purity. (Prosch, 1986, pp. 272-273)

With this new approach proposed within the theory of tacit knowledge, a number of epistemological problems can be tackled anew with commitment and in hope. And Polanyi has not been alone in this effort. In the same bid to find a

[58] Emphasis in the original

middle ground between rival approaches to the philosophy of science, Ronald Giere proposes a combination that 'gives up the search for criteria of scientific rationality, abandons the attempt to separate the content and methods of science from psychological and sociological reality, but preserves the view of science as a representational activity'. (Giere, 1999, p. 44) Other philosophers who have advocated for a combinatorial approach include: Boyd (1980), Churchland (1989), and Giere (1988).

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