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Aspects Of Victorian Psychologism

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ABSTRACT

In this essay I present revisionary readings of four Victorian philosophers. I argue that each of them is fundamentally committed to a naturalistic philosophical project called psychologism. The psychologistic readings that this critical stance generates offer resources that may be exploited by contemporary philosophers pursuing their own naturalistic projects.

In the first chapter I sketch the structure and main points of the essay. In the second chapter I suggest that Mansel's Kantian psychologism manages to evade the criticisms of Husserl. This serves to highlight the distance between psychologism and contemporary logic. In the third chapter I argue that Whewell embraces a developmental modification of Kantian psychologism. This account undermines reading Whewell as interested only in formal relations between hypotheses and evidence. The fourth chapter contains an alternative to the view that J. S. Mill embraces a covering-law model of explanation. Mill's psychologistic commitments are incompatible with most contemporary analyses of the concepts of science. In the final chapter, I argue that James Clerk Maxwell embraces Whewell's psychologism and actively imports it into his method of research.

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CHAPTER I

INTRODUCTION

1. The project.

Victorian philosophy develops a bold naturalism and is not, as some would have us believe, an inept preamble to logical positivism. The ultimate purpose of this essay is to present the views of four Victorian philosophers, without merely dismissing their psychologism as an unfortunate peripheral mistake. The main conclusion of this essay is that these Victorian philosophers are engaged in a naturalistic project. Their commitment to psychologism is a central feature in their philosophical work and attention to this centrality generates coherent and interesting readings of their works. These philosophers can be seen as developing approaches and results that are useful in augmenting and criticizing the naturalistic philosophy that in the last few decades has begun to eclipse the projects of logical analysis and logical positivism. Textual evidence is presented in support of the claim that each of these thinkers embraces a type of psychologism. A strongly psychologistic reading of aspects of their various works is undertaken. These readings recommend themselves as coherent, provocative and worthy of development. Accepting the fundamental nature of the Victorians' commitment to psychologism proves to provide a viable and much neglected critical apparatus for reading Victorian philosophers.

2. Mansel and Husserl.

The twentieth century is ushered in by a revolution in philosophy. Much of what is distinctive in the philosophical works of our century is to some extent a response to the naturalistic approach that dominated Victorian philosophy. The waxing naturalism of the last few decades might even be interpreted as a return to the line of inquiry so ably developed by the psychologistic philosophers of the nineteenth century.

In the second chapter of this essay I use Husserl's critique of psychologism to draw out the details of Henry Mansel's neo-Kantian psychologism. This exercise yields two results: First, both the evolutionary connections and the revolutionary discontinuities between the philosophical inquiries of the nineteenth and twentieth centuries are exhibited. Second, the weakness of Husserl's arguments against psychological naturalism become apparent. Let me sketch the main points of each of these results.

The foremost discontinuity between the last and this century is the shift from naturalism to absolutism. Mansel, in perfect keeping with his contemporaries, embraces a trust in an empirical dimension to scientific investigation. This forces the rejection of metaphysical speculation and makes the subject matter of all genuine philosophical inquiry--like all genuine scientific inquiry--the sensations or phenomena with which humans are directly acquainted. Thus, philosophy is naturalized in the sense that it shares its subject matter and fundamental approach with the sciences that flourished in the nineteenth century.

This naturalism takes the form of psychologism because the Victorians find the description of the relevant accessible phenomena to be exhausted by mental acts and the content of those acts. (Philosophy must study what Descartes called the formal reality of ideas as well as their objective realities.) Since both of these items are decidedly within the mental realm philosophy becomes continuous with psychology.[1]

The nature of Husserl's criticisms (or perhaps Frege's attack on Husserl's 1891 Philosophy of Arithmetic) have impaired our acquaintance with of the scope of Victorian psychologism. Although Husserl clearly identifies the reductive theses concerning logic as only a small part of psychologism, we have come to view it as fundamentally a

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[1] Of course, Comte in rejecting the viability of psychology espoused a sociological naturalism that takes philosophical issues to be continuous with those of sociology; *viz.* one asks, for example, "How do *we* know?" rather than "How do *I* know?", and so on.

philosophical account of logic. Psychologism is often seen as only a view that takes the subject matter of logic to be patterns of human inference and the modality of logical truths to be the same as other descriptive empirical generalizations. Consider a recent characterization:

> One can begin by distinguishing--the distinction is pretty crude, but nevertheless serviceable as a starting-point--three kinds of position:
>
> (i) logic is descriptive of mental processes (it describes how we _do_, or perhaps how we _must_, think)
>
> (ii) logic is prescriptive of mental processes (it prescribes how we _should_ think)
>
> (iii) logic has nothing to do with mental processes
>
> One might call these _strong psychologism_, _weak psychologism_, and _anti-psychologism_ respectively. (Haack 1978, p. 238)

As will be apparent in the remaining chapters of this essay, and as Husserl recognized, psychologism is a naturalistic project that is much deeper and wider in its concerns than an attempt to found the laws of logic on patterns of human inference. I will argue that the Victorians did not universally defend the thesis that all philosophical questions are reducible to psychological questions--the science of psychology is not strong enough for that. Rather, for the Victorian psychologistic philosopher, psychologism is an acceptable approach in all respectable philosophical inquiry. Epistemology, logic, axiology and philosophy of science are all treated psychologistically. For the Victorian, necessity, possibility, probability, truth, entailment, value, beauty, explanation, evidence, and all the rest are properties of, or relations between, ideas or their contents. For this reason questions concerning necessity, possibility, and so forth must be seen as questions concerning the limits and structure of human cognition.

Husserl's alternative is quite radical. And it shows one just how radical is the great bulk of twentieth-century philosophy.[2] Husserl claims that philosophy is distinct from psychology because it studies a realm distinct from the mental. There are mental acts, and they have contents, but more than this, these contents have objects--they make reference. Twentieth-century philosophy has as a main theme the study of this absolute, mind-independent realm of objectivities, as Husserl calls them. Modality is a

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[2] This follows because there is a strong family resemblance between the fundamental tenets of Husserlian phenomenology and the basic claims of other dominant twentieth-century schools such as Russellian analysis, Carnapian logical syntax, and semantics. For those interested, J. Passemore relates this story in detail and convincingly (1957, Ch. 8).

property of objectivities. Truth is a correspondence relation between objectivities and propositions. Logic becomes the science of the absolute limits of all possible experience. Logic identifies the limits of the structure of any possible knowledge. It is the grammar of possible knowing. Therefore, Husserl moves us from the study of the actual limits of human ideas to the logical preconditions of any cognition at all. The radical nature of such a project is evident. The twentieth-century anti-psychologistic philosopher posits a foundation for the limitations of human conception and knowing, and an object of study, that is outside the natural world as identified by natural science. To take such a radical turn, one would expect completely devastating criticisms of the naturalistic alternative. But I will argue that such criticisms are not to be found in Husserl. Mansel's formulation of psychologism is impervious to Husserl's attacks. And as the sociologically inclined historians are quick to note, this radical move has as its seemingly sole motivation the protection of philosophers from an obsolescence tolled by the advance of the mind sciences. But I am not so pessimistic.

3. Whewell and Maxwell.

In Chapter III and Chapter V of this essay I discuss the psychologistic commitments of Whewell and Maxwell, respectively. The principal importance of these chapters is that they show the resources available in the works of Whewell and Maxwell to generate the groundwork for an understanding of a naturalistic alternative to the genre of philosophy of science and epistemology that has dominated our century. In this alternative, science is an extension of perception and not, as Hobbes would have it, a linguistic entity.

Scientific inquiry extends perception or intuition in that it forces the development or discovery of structures into which phenomena must be fitted in order for humans to "see" them clearly. In Chapter III, I present a reading of Whewell's psychologism; I argue that Whewell unifies what Kant identified as the forms of intuition and concepts of the understanding. I claim that this makes science a "way of seeing" or a species of perception. The ideas of science are the structures into which phenomena must be placed in order to make clear human cognition and perception concerning those phenomena. Theory is imposed on facts. Whewell is an optimistic Kantian because he rejects the view that the concepts Kant identified are exhaustive of all the possible concepts a human might have. Whewell differs from Mansel in holding that the human creation of these concepts or fundamental ideas is open-ended and progressive.

On my reading, Whewell's philosophy of science has three features of prime importance:

(1) Whewell presents a method of scientific investigation that applies colligating ideas to organize groups of phenomena and thereby to generate functions that describe the relations between magnitudes. I argue that Whewell's notion of colligation is derived from methods used in solving certain mathematical problems. Colligation requires the imposition of magnitudes and formulae upon the data and is quite distant from a method in which various scatter diagrams are randomly generated in the hopes of identifying a dependence relation. I argue that colligation is a three step procedure that one would use to solve, say, problems concerning the area of fenced in fields. One takes the first step in colligating by identifying what a Victorian would call the dependent variable. The second step of a colligation is the imposition of a formula that binds the variables together. The final step of a colligation is the imposition of coefficients for magnitudes within the formula.

(2) Whewell argues that in a what he would call a fully developed fundamental idea an ordering of phenomena is identified, while the nature of the items so ordered is left beyond the epistemological pale. Since sientific theories are extremely careful descriptions of fundamental ideas, science can be committed only to the relations in perception and not to claims regarding the nature of the

relata.

(3) Whewell writes that when colligating, the relations we impose on sensations are determined to some degree by an absolute or mind-independent ordering of sensations. Nevertheless, the laws of human thought develop so that the relations imposed on sensations become as necessary as the laws pertaining to logic and mathematics. We cannot colligate the input we receive from the world in just any way we choose; it only fits together in certain ways; for example, our sensations fit together when colligated using the idea of (Euclidean) space and do not fit together when radically different orderings with respect to position are used. To have any clear conception of space, then, one must utilize the ability of the mind to organize sensation spatially and, following Kant, Whewell takes this as the foundation of our a priori knowledge and of the necessity of geometrical truths. Similarly, in order to organize sensations according to relations of, say, force, we must impose the idea of force on the input we receive from the world. We must organize the raw data of experience according to fundamental ideas in order to create a unified perception and also in order to identify what unities exist within that perception. In this Whewell echoes both Hume and Kant.[3] The historical dimension of Whewell's account

[3] Kant's stand on this is well known. It is, however, interesting to note that even Hume takes a similar line: "The

is that we can come to realize the possibility of new organizations of sensation. We can organize the inputs of the world under new descriptions. Hence, science proceeds by imposing new ideas on sensation, and these ideas become the foundations of scientific knowledge. In a sense then, Whewellian science is an extension of the processes that give us spatial and temporal perception; in Whewell's psychologism, philosophy of science is continuous with psychology of perception and cognition.

It is common to identify as the hallmark of Whewellian consilience, the logical entailment of new or unexpected facts by a hypothesis. Thus, consilience is seen as of a piece with simplification and unification. I do not dispute this as an aspect of Whewell's philosophy. Rather, my contention is that his psychologism is central. Hence, the mental or perceptual relations and processes are of more importance to Whewell than these derivative logical relations. Moreover, if the logical-linguistic dimension is interpreted in some kind of post-Husserlian manner, I would argue that a misreading of Whewell is taking place. His home is with the other Victorians--a home filled with the early researchers in what we would today call cognitive scientist, rather than formal logicians.

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conception always precedes the understanding; and where the one is obscure, the other is uncertain; where one fails the other must fail also" (1740, p. 164).

My last chapter is an attempt to show that the Whewellian perception-intuition model of science dominates Maxwell's approach to generating scientific theory. I maintain that Maxwell's use of analogy is a mechanism by which the perceptual abilities of the researcher are developed so as to achieve a Whewellian "point of view". That is, the researcher colligates and then uses analogies to establish the Whewellian fundamental idea as a psychologically necessary law of thought. For the Victorian pursuing the psychologism in Kant's works, science is an extension of the processes of perception rather than particular language and its use.

I further point out that this use of Whewell's philosophy drives Maxwell away from the mechanistic thought of his contemporaries and toward a dynamistic mode of theorizing, which advocates beginning with macroscopic behaviour and moving directly to differential equations. Fundamental laws, such as Newton's laws of motion, are treated as parts of an abstract mathematical system based on axioms that are self-evidently true, and independent of empirical verification. The laws of dynamics are laws describing the inference patterns necessary for any consciousness of dynamic systems and hence are rooted in the mind or rationally grounded. So science has two parts, a priori and a posteriori but their relation is Kantian. I conclude that, in the end, Maxwell identifies the fundamental idea of his investigation as that of a Lagrangian connected system. He holds in a paradigmatically Whewellian manner that one can only come to clearly envisage and think about the phenomena of electromagnetism by viewing them as organized into a Lagrangian connected system.

4. John Stuart Mill.

In my fourth chapter I argue that Mill's genius lies in his attempt to reformulate what were in his time commonly known results of philosophical inquiry into logic, epistemology and philosophy of science, in terms of the associationist psychology that arose from Hobbes and dominated English thought. This makes Mill paradigmatically Victorian: His search is for a natural articulation of the human cognitive processes at work in our gaining and identifying knowledge. His results can be read as an attempt to describe the cognitive processes actually occurring in those activities referred to as deduction and induction. Indeed, he saw these terms as part of a vague, common, or even vulgar usage that is always misleading and often inconsistent with the claims of the psychological theory he embraced.

I argue that the commonly held view that Mill endorses a covering law model of explanation is an example of the kind of misinterpretation that occurs when the fundamental nature of his psychological concerns are overlooked. When we see his project as identifying the actual underlying inference patterns from particular idea to particular idea, we find that Mill puts the covering law model of explanation out of reach by denying the propriety of the resources necessary for an appropriate account of the relation of "covering" requisite for this model.

I close this chapter with what I hope are some ideas for the direction of further research into Mill's psychologism. I contend that Mill's psychologism extends to a naturalistic theory of concepts, articulated in terms of associationist psychology and I sketch an account of evidence and explanation that is consistent with this psychologism of Mill's. The heart of this theory is the view that we define a concept by a method of inference and not its deductive logical relations with other concepts. The concept of causality, for example, is identified by the canons of causal inference. There is not mental representation of a concept _per se_, but only ideas associated according to certain inference patterns. The main implication of this theory is that Husserl's claim that psychologism entails relativism can be avoided.

In this section I also point out one another consequence of the whole essay. If one reads the psychologism of Victorian writers as fundamental to their philosophical views, the debates that arise between them will appear to have more to do with the psychological theories they embrace than the differences between the philosophical positions they promote. I suggest that in a case such as that of Kepler's discovery the much discussed debate between Whewell and Mill is not so much a contest between advocates of different methodologies as one between adherents to conflicting accounts of human psychology. Stripped of their psychological commitments the philosophical tension between these thinkers is much weaker than commonly suggested.

CHAPTER II

PSYCHOLOGISM AND TRANSCENDENTALISM: Contrasts between Mansel and Husserl.

1. Logic and psychology.

At the beginning of this century Husserl urges us to abide by a distinction between issues of logic and matters of psychology. "The idea of a 'pure logic', a theoretical science independent of everything empirical, and hence also of psychology", he maintains, "must be admitted as sound" (1900, p. 212). Although philosophers in our century have for the most part appeared to embrace this view, Husserl's injunction is still not entirely clear. It is not obvious in what this distinction between the logical and the psychological consists: the relation between the studies of philosophical logic and psychology is not manifest. Moreover, as the dominance of an antipsychologistic or structure-theoretic program in philosophy of logic approaches a century in age, the propriety of this distinction comes under more and more harsh criticism by proponents of various sorts of naturalism.

In this light, Victorian philosophical logic and related activity in philosophy of science offer an especially exciting view of the tension between logical and psychological concerns. The Victorians were at the pinnacle of psychologistic analyses of both deductive and inductive logic. And, while John Stuart Mill is one of the named targets of Husserl's mortal attacks on psychologism, an interesting foil to Husserl's views can be found in the psychologism of Henry L. Mansel.

The project of this first chapter, then, is to present the main features of Mansel's Victorian formulation of psychologism by examining in what ways it does, or does not, run afoul of Husserl's critique. This approach will force us to confront what I shall argue is a fundamental shift in the philosophy of logic, that emerges at the turn of the century. It also motivates a reappraisal of Husserl's unquestioned victory in his confrontation with psychologism. The importance of this study of the tensions between psychologism and transcendentalism lies in its ability to help us understand the fundamental shift that founds much of the innovative work of twentieth-century logicians and efficiently analyze our waxing naturalism.

2. What is Psychologism?

Notturno justifiably complains that, "For the past one hundred and fifty years, 'psychologism' has been used as an umbrella term to cover a multitude of philosophical sins, both metaphysical and epistemological. As a result, the meaning of the term has remained systematically obscure" (1985, p. 9). Many authors have merely branded as psychologism any philosophical view that undermines either the absolute truth of their pet beliefs or the independent existence of their favoured ontic commitments. However, even more careful thinkers appear to be in disarray on the nature of psychologism. It seems fair, therefore, to take as a starting position Notturno's very general definition of psychologism as "a family of views, all tending to deprecate or deny distinctions between epistemology and metaphysics on the one hand and psychology on the other" (1985, p. 19). While it is the task of the following pages to sketch what various Victorian philosophers meant by the term psychologism, a somewhat more specific initial definition is possible here.

Psychologism is most usually associated with the attempt to found logic or mathematics on some feature of our minds. This view arises due to a myopic focus on the debate involving Frege and Husserl. The history of psychologism is, however, much more breathtaking than this focus indicates, and the scope of the term is wider than this debate superficially urges. Psychologism has as its general purpose the analysis of the epistemological and ontological status of necessary relations and their relata. Certainly, one of the most interesting classes of these relations consists of mathematical and logical facts, but causal relations as instances of natural laws constitute an equally interesting class of physically necessary relations. Moreover, inquiries into the status of moral laws have also engendered a psychologistic treatment. Thus, as the attempt to answer questions about our knowledge of necessary relations and the nature of those relations by inquiring into the constitution and limitations of the human mind, psychologism has a history that can appear to span virtually all of western philosophy.

The term psychologism is, of course, relatively new. There is no reason to doubt Abbagnano's claim that it emerged in Germany during the early 1800's to refer to the philosophical program of Jakob Friedrich Fries and Friedrich Eduard Beneke (1966, p. 520). In this program, psychological studies become fundamental because, as Beneke puts it, "With all of the concepts of the philosophical disciplines, only what is formed in the human soul according to the laws of its development can be thought; if these laws are understood with certainty and clarity, then a certain and clear knowledge of those disciplines is likewise achieved" (in Abbagnano, 1967, p. 520). Thus, Beneke points out that our possible understanding is limited to those things about which we are able to think. Our reasoning is restricted to operations on our judgments, and our judgments are restricted to operations on our concepts. Thus, our mental constitution restricts not only the kinds of relations we can envisage, but also the things that we can think of as related.

Fries and Beneke give us psychologism with a decidedly epistemological flavour. But, early psychologism also had a metaphysical side. According to Abagnano, "In the same period Vincenzo Gioberti branded as psychologism all of modern philosophy from Descartes on" (1967, p. 520). Gioberti criticized modern philosophy because it appeared to give only psychological reality to the objects of thought. That is, Gioberti resisted the view that we are not directly aware of being that is independent of the human mind and criticized this view's derivative agnosticism towards that mode of being. Psychologism, then, is placed very close to idealism and in conflict with any kind of direct realism. And indeed, this identification of psychologism and idealistic anti-realism is borne out in other philosophical works. In Morris's 1874 translation of Uberweg's history of philosophy, he says, "The philosophic revolution which began with Descartes . . . manifested itself in two forms of Psychologism (or Idealism), and Sensationalism--represented by Descartes and Malebranche on the one side, and by Locke and Condillac on the other" (Vol. II, App. ii, 479).

Thus, in the nineteenth century philosophers saw a long history of psychologistic analysis that concerned the metaphysical status of objects, as well as the epistemological status and genesis of our claims about them. This sense of being involved in a traditional philosophical project was, I would argue, not altogether unwarranted.

3. Husserl's characterization of psychologism.

Husserl can be seen as having a very general notion of psychologism but as concentrating his attacks on a specific part of that larger subject matter. He refers to a general program of psychologizing as "universal epistemological psychologism" and contrasts that with his specific target, "logical psychologism". The attacks on logical psychologism make up much of the first volume of his Logical Investigations of 1900. The clear expression of the relation of these attacks to the more general dismissal of universal epistemological psychologism can be found in his Formal and Transcendental Logic, published twenty-nine years later.

Universal epistemological psychologism includes, in Husserl's words, "any interpretation which converts objectivities into something psychological in the proper sense" (1929, p. 154). Objectivities, for Husserl, can be read as referring to all items that must be both self-identical and numerically distinct from other items in order for our experience to be as it is. In this analysis of objecthood, then, he takes up Kant's mode of argument in "The Transcendental Aesthetic". For Husserl, objectivities are those things that are known by us. They are the things that enter into the knowing relation with consciousness. Thus, physical objects are objectivities. But, so too are numbers, concepts, arguments, proofs and theories. These latter objects are, in Husserl's lexicon, irreal in the sense that they have no space-time location, but this does not diminish their transcendent ontic status.

Notice that this is not meant to be Platonism--there is no postulation of entities beyond what is necessary for knowing. That is to say, entities such as forms, gedanke or propositions, with an existence that is utterly independent of their coming into the knowing relation with humans, are not postulated. Objectivities are unconditioned, but they are not independent. Of course, that is not entirely clear. One main problem of contemporary philosophical logic is to articulate this transcendental position without collapsing into Platonism or psychologism. Moreover, it is not clear that this problem can be solved.[1] It is clear, however, that Husserl attempts to avoid a Platonism that fundamentally postulates an independent and objective reality. Such a view is, for Husserl, naive; in order to avoid subjectivism it constructs a mutilating model of reality as pure object and offers this in place of reality (vide, Husserl, 1936).

In contrast to this view, universal epistemological

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[1] For a detailed discussion of the problems with Husserl's ontology, see J.R. Mensch 1981.

psychologism begins with a radically empirical stance: all our contact with these objectivities is restricted to our having purely mental experience. It then avoids the postulation of all these objects as anything more than purely mental experience. That is to say, universal epistemological psychologism forces a radical anti-realism to emerge. Let me give some examples.

Husserl cites Mach's positivism as a subspecies of universal epistemological psychologism. In this view, says Husserl, "Physical things become reduced to empirically regular complexes of psychic data" (1929, p. 166). In Mach's view, science is about the regularity of sensations, and it need not postulate any existence except that of sensation and sensitive mind. Thus, all the objects of experience are reduced to mental events.

This is, of course, very close to Mill's sensationalism. In his view, physical objects need only be recognized as, "permanent possibilities of sensation". The relation of this view and others of the Victorian psychologists will arise in Chapter IV.[2]

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[2] Throughout this essay I will use the term "psychologist" in referring to researchers in philosophical psychologism. When I wish to speak of those involved in the empirical or scientific analysis of mental processes and functions, and their field, I will use the admittedly cumbersome terms "mind scientist" and "mind sciences",

Husserl's grand notion of psychologism, then, contains a much more sweeping set of theses than those concerning the foundation of logic and the existence of its objects. This view is in line with traditional philosophical psychologism. It is modern, however, in that it is primarily and specifically directed against radically empiricistic epistemology and then only derivatively concerned with its ontological implications. We should not say simply that, "the 'epistemological position' Husserl opposed is that which holds that the laws of logic and mathematics are knowable only a posteriori" (Notturno, 1985, p. 13). The universal epistemological psychologism that Husserl opposed contains theses issuing in radical sensationalism and epistemological relativism.

Nevertheless in 1900 Husserl is more intent on discussing in detail only the issues concerning the component of universal psychologism that deals with the foundation of logic, holding that similar criticism can be applied to psychologizing in any other field and any other naturalizing program in philosophical logic. For Husserl, logical psychologism has two main features: first, it makes our knowledge of logical principles dependent on the discovery of contingent empirical facts concerning our mental constitution. Second, it grounds the status or nature of those principles on the nature of the

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respectively.

psychological processes and acts that make up our mental life. As we shall see, Husserl claims that these two features generate many specific problems.

The central issue that ties this specific attack to his rejection of universal epistemological psychologism is his contention that logical psychologism is an excellent example of the implications of the basic defects of empiricism. Empiricism leads to psychologism and psychologism leads to the epistemological stance of skeptical relativism for, according to Husserl, "Psychologism in all its subvarieties and individual elaborations is in fact the same as relativism" (1900, p. 145). The relativism that Husserl refers to can, I believe, be characterized as follows: relativism exists just in case two subjects can be justified in believing two beliefs, B and B′, respectively, while B is the truth-functional negation of B′. If psychologism allows that each subject's knowledge is founded on contingent features concerning the contitution of the mind of that subject, then this case might arise between individuals of the same species or those of different species.

Like Kant, then, Husserl attempts to defeat the skeptic and relativist. But, Husserl does not wholeheartedly embrace the Kantian stance. "We agree with Kant in his main drift," he says, "though we do not find that he clearly espied the essence of his intended discipline"

(1900, p. 215). Husserl dismisses Kant's view of logic as a closed science. More deeply, however, he distances himself from any psychologistic reading of Kant. Rejecting formal idealism based on transcendental psychology with any taint of empirical psychology, he writes the following of such a doctrine:

> It is likewise relativistic, if, with the apriorists, it deduces these laws, in more or less mythic fashion, from certain 'original forms' or 'modes of functioning' of the (human) understanding, from consciousness as such, conceived as generic (human) reason, from the psycho-physical constitution of man, from the intellectus ipse which, as an innate (generically human) disposition, precedes all actual thought and experience. (1900, p. 145)

His point here is that each of these approaches in some way sees the understanding and reason as faculties of the human soul. That is, they have only the status of the entities discussed in eighteenth- and nineteenth-century faculty psychology.[3] Husserl rejects this reading that takes Kant's results to be founded on the merely contingent nature of our human constitution or the products of that constitution. To avoid psychologism, one must not read

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[3] See my 1989a for a detailed discussion of the epistemological aspects of Hobbes's seventeenth-century faculty psychology.

Kant's words "understanding" and "reason" in what Husserl calls "the natural sense which gives them an essential connection with the human species" (1900, p. 145). Husserl's Kantianism is restricted instead to an account that reads "the terms 'understanding' and 'reason' as merely indicating a direction to 'the form of thinking' and its ideal laws, which logic, as opposed to an empirical psychology of knowledge, must follow" (1900, p. 214).

4. The critique of psychologism.

Commentators often identify only three attacks on psychologism in Husserl's Logical Investigations, but the salient passages can be read as containing many more. There are two main patterns of criticism in this work. One attacks the consequences of psychologism and the other attacks the premisses upon which it is founded. In this latter pattern of attack, Husserl attempts "to show that what it regards as obvious truths are in fact delusive prejudices" (1900, p. 168). Let me examine the details of this claim first.

The foundation of psychologism that Husserl initially attacks is the belief that, "Prescriptions which regulate what is mental must obviously have a mental basis" (1900, p. 168). He claims against this axiom that the laws of logic are not prescriptions. They can be used as guides, but their content has to do only with the relation of truths. This psychologistic prejudice mistakenly ignores what Husserl sees as the fundamental distinction possible between the principles of pure logic and their application by individuals as a technology of rules useful in guiding "the specifically human art of thought" (1900, p. 171). Husserl continues this line noting that rules of judgment can be had by applying general truths from any field. Thus, he concludes, there is reason to admit the existence of rules of judgment which have no basis in psychology.

Psychologism appeals to another fundamental notion that Husserl finds unacceptable: namely, that the subject matter of logic cannot be other than mental activity. The psychological analyst notes that logic concerns itself with judgments, presentations and proofs, and these seem plainly to refer to mental activities. Husserl's response is to point out that if logic is a branch of psychology, then so too is mathematics. But, mathematics cannot be part of psychology because "numbers, sums and products and so forth . . . differ obviously from presentations in which they are given. The number five is not my own or anyone else's counting of five, it is also not my presentation or anyone else's" (1900, p. 180). So, Husserl's response is that the properties investigated by mathematics are not in any way dependent upon the existence of numbers as thoughts or representations in any person's mind. The objectivities of mathematics like those of logic are irreal.

The final unacceptable premiss of psychologism is expressed by Husserl as follows: "All truth pertains to judgment. Judgment, however, is only recognized as true when it is inwardly evident" (1900, p. 187). The psychologist is charged with believing that truth and necessity are merely subjective feelings that accompany some judgments. Certain judgments might feel clear and distinct, for example. Thus, understanding such things as truth and necessity becomes dependent on our ability to explain how these feelings arise as feelings.

Husserl counters with the claim that although there is a relation between purely logical propositions and the psychological datum of inward evidence, this relation does not undermine the transcendentally ideal character of logic. He claims that the truth of a proposition is something more than a subjective feeling about a judgment. Hence, the statement, "A is true", is not equivalent to the statement, "Persons can have the feeling that A is the case". Nevertheless, the first statement can be applied to the various cases in which such feelings arise.

Let me turn to the other pattern of attack in Husserl's Logical Investigations: namely, the criticism by way of the consequences of psychologism. Husserl's arguments in this section can be separated into two classes. First, we have the metaphysical arguments, or those that turn on claims about the nature of the laws of natural science and logic.

Second, there are the epistemological arguments, or those based on views about how we come to know the laws of logic and natural science.

The first among the metaphysical arguments is based on the claim that the precision of the theoretical foundations of an investigation sets an upper limit on the precision of any law grounded on those foundations. "Only vague rules could be based on vague theoretical foundations. If psychological laws lack exactness, the same must be true of the prescriptions of logic", argues Husserl (1900, p. 98). It is a consequence of the attempt to ground logic on psychology, and the empirical vagueness of psychology, that logical laws should be similarly vague. And, according to Husserl they are not. They are absolutely exact.

Husserl is doubtless referring to the theoretical frameworks and laws embraced by nineteenth-century mind scientists such as Wundt. Wundt founded the structuralist school in the then emerging science of experimental psychology. His followers were committed to the view that consciousness was a complex phenomenon that could be analyzed into its separate elementary constituents. For example, in 1896 Titchner published a list of more that 44,000 of these elementary constituents. Through introspection and this theoretical framework they sought to discover the structure of consciousness and the laws that determine that structure. That is to say, they attempted to identify the psychological laws that govern the processes by which a multitude of sensory elements are sythesized forming a unified perception or consciousness. The law of psychic resultants proposed by Wundt is very nearly a restatement of J. S. Mill's idea of creative or "chemical" synthesis. It is merely the claim that the mind is not passive; rather, it actively synthesises sensational elements into unified perceptions, and in doing so creates something new and greater than the sum of its component parts. And, this does seem to be more vague than even the rules that tell us which of the 256 forms of standard syllogisms are valid.

Husserl also points out that, unlike the laws of logic, all laws of psychology, in as much as they are natural laws, can only be our best approximations of nature or idealized fictions. Many laws can fit the same measurements because for any two candidates it can be the case that, as Husserl puts it, "The differentiating factor in both formulae conditions differences in calculated values not exceeding the field of observational error" (1900, p. 100). Hence, none can be identified as exact. The laws of physics which appear more than mere approximations are, Husserl argues, like all laws of the exact sciences, "no more than idealizing fictions" (1900, p. 106). In contrast, the laws of logic are neither fictions nor approximations. They are not fictions because they do not range over idealizations of the physically real, but rather over fundamentally irreal objectivities. That they are not approximations is guaranteed by the absurdity of the alternative and this is why, according to Husserl, there is no sense to any talk of domains where logical principles apply without absolute exactness.

In another criticism, Husserl notes that the laws of natural science have no causal role in the processes they range over. A law itself does nothing. It merely describes the way processes will unfold due to causal powers extant in nature. Often we are not alive to this distinction, and then "we confuse a law as a term in causation with a law as a rule of causation" (1900, p. 102). The psychologistic analysts of logic make just this mistake. They wrongly attribute to logical laws themselves the causal efficacy necessary to have them govern our thinking.

Psychologism also requires that because psychological laws range over the causal relations between states, logical laws must do so as well. Husserl finds two things wrong with this. First, he uses the example of a computer to persuade his audience that an entity can get the results that logical thinking would yield without leading anyone to use logical laws to explain its constitution: "No one, however, who wants to give a physical explanation of the machine's procedures, will appeal to arithmetic instead of mechanical laws" (1900, p. 103). Hence, he concludes that mechanical, and so psychological laws, are distinct from logical laws.

Second, logical laws unlike their psychological counterparts do not imply the existence of mental states. As Husserl puts it, "No logical law implies a 'matter of fact', not even the existence of presentations or judgments or other phenomena of knowledge" (1900, p. 104). Natural laws entail the existence of their subject matter in nature. They have ontological commitments in that physical laws have existential import with respect to physical events or objects. Logical laws on the other hand are, according to Husserl, quite without any physical existential import. They require only the irreal transcendental objectivities.

This argument is echoed in Husserl's claim that no logical laws range over the objects in space-time. All logical laws are of one and the same character according to Husserl. But, it is clear that some principles of logic cannot be regarded as ranging over facts. Hence, none of them are laws ranging over facts. Rather, these laws range over truth; that is, they, "have truth as their regular 'objects'" (1900, p. 109). For example, in Husserl's view the law of non-contradiction is about truths. It must be stated as follows: "for every truth A, its contradictory opposite is no truth" (1900, p. 109).

Now let us turn to the group of criticisms based on Husserl's view of how we come to know the laws of logic or, what I have called, the epistemological arguments. Husserl maintains that, "No natural laws can be known a priori" (1900, p. 99). From this it follows that if logical laws are part of psychology, then they cannot be known a priori. Of course, he believes that the laws of logic are obviously known a priori, and so this consequence of psychologism is absurd.

This claim leads to related arguments. If logical laws are natural laws then they must be established by induction on individual facts. But, they are not. They are established by what Husserl calls, "apodeictic inner evidence" (1900, p. 99). Furthermore, if the laws of logic are inductively based, then they are only probabilistic. Nevertheless, he maintains that they are valid qua apodeicticly certain. Moreover, that they are not based on induction is another reason to believe that they are not about facts.

The most economical way to summarize these arguments is as a series of reductios on each term in a set of biconditionals. The biconditional looks like this: logic is based on psychology if and only if the laws of logic are only known a posteriori if and only if they are inductions if and only if they are probabilistic if and only if they are about facts. Each element is false, hence logic is not based on psychology.

Before turning to the Victorians we should note two arguments that Husserl does not employ in his attack on psychologism. He notes that some have argued that logic cannot be part of psychology because logic is normative and psychology is not. But, Husserl admits that the psychologist has no trouble with this point, and goes on to claim, "logical laws in and for themselves, are not normative propositions" (1900, p. 168). One cannot say, therefore, that Husserl conceived of logical laws as "precise, certain and normative" (my emphasis; Brehier, 1932, p. 207), unless one means the following: Husserl believed that although logical laws refer only to the irreal objectivities and as such have no normative content, they can still be used by humans as norms. Of course, since natural laws can be so used, this property does nothing to distinguish between the logical and psychological.

Neither does Husserl commit himself to the argument that charges psychology with circularity because it uses logic to generate logic. Michael McCarthy (1990, p. 37) errs when he attributes this argument to Hussel because the latter correctly notes that the principles of logic are not premisses in psychologistic justifications. He admits, moreover, that the science of logic would be open to the same charge if merely reasoning logically undermined one's ability to investigate the grounds of logic.

5. The fundamentals of Mansel's view.

Three dominant early Victorian psychologistic philosophers of logic are William Whewell, Henry Mansel and John Stuart Mill. Mill differs from Whewell and Mansel by rejecting conceptualism. He attempts to take much that is commonly accepted and reformulate it in the language he inherits from Hume and the associationalist psychology deriving from Hobbes. For Mill there are no concepts in mental life; there is only a series of impressions, some of which are held together by associative habits described in the laws of association. In contrast, Whewell and Mansel, like Husserl, profess and demonstrate an intellectual debt to Kant. They embrace conceptualism, but come into conflict over the method that philosophical inquiry should follow.

Mansel, then, can be seen as Oxford's foil to Whewell at Cambridge. Born in 1820, Mansel was the Cambridge master's junior by twenty-six years and he survived Whewell by five years, dying in 1871. Mansel's university education took place at St. John's College, Oxford, and he rose to be its Wayneflete professor of moral and metaphysical philosophy by 1859. In 1866 he became St. John's Regius professor of ecclesiastical history and was made dean of St Paul's three years before his death.

In 1851 Mansel published his magnum opus, Prolegomena

<u>Logica: An Inquiry into the Psychological Character of Logical Processes</u>. Throughout this work in philosophical logic, he attempts to utilize the thought of Kant while underlining its harmonies with the Scottish Common Sense philosophy of Hamilton and Reid as understood by the steady English academic. As Mansel puts it, "the future hopes of speculative philosophy rest on the possibility of a union of the principles of Kant with the sober practical spirit which is characteristic of English thinkers" (1851, p. 261). Mansel's project is to present a coherent and compelling psychologistic account of our knowledge while avoiding the errors he saw in Whewell's overzealous a priorism. Reflecting Locke's plan to establish the certainty and extent of human knowledge, Mansel attempts, as he says, "to determine accurately the province and capabilities of logic" (1851, p. 20).

The basis of Mansel's analysis of knowledge is a fourfold segregation of our knowledge claims or judgments. To understand it, however, we need first to identify and define some terms in the thoroughly mentalistic vocabulary Mansel adopts. As an epistemologist Mansel is interested in the nature of all knowledge, but considering our lack of data concerning other intelligent species he opts, rightly I think, to focus his inquiry on human knowledge. The initial subject matter of theory of knowledge is, then, the product of human mental operations. Now, it is possible that one of the products of these processes is an account of what knowledge must be for any being at all. But two facts mitigate against the frontal approach of attempting to analyze this absolute concept of knowledge: first, attempts to provide necessary and sufficient conditions of this concept have all met with accusations of failure. And second, any seemingly successful account will immediately be in need of justification; it will be necessary to identify how that analysis arose through human mental operations. Hence, an account of these operations at the philosophical level is necessary sooner or later.

There are according to Mansel's Victorian viewpoint three main categories of mental operation: conception, judgment and reasoning. In conception, a group of characteristics or properties are joined and recognized either as an object one might perceive in intuition or, in the case of pure conception, as an object characterized using signs or language only. "The act of conception", he says, "consists in regarding certain attributes as coexisting in a possible object of intuition" (1851, p. 165). Following Locke, Mansel insists that the mind is incapable of inventing attributes; and he argues that, since the mind cannot create attributes but only combine them, the concepts of pure conception are entirely composed of signs referring to attributes that have been previously intuited. Thus, the process of conceiving is limited by the accidents of actual intuition and any psychological limitations to possible intuition. This has, as we shall see, implications for the limitations of science.

Judgment, the second main kind of mental operation, is the joining together of concepts. In Mansel's view concepts or mentally represented signs of concepts may be joined, and thus judgment differs from conception in that the latter can operate on attributes. In logic, the verb "is" identifies this joining together. As analyzed in logic, the role of the copula is, in Mansel's words, "to declare the present coexistence of two concepts in the representative act of thought" (1851, p. 71). But, Hobbes's insight into the purely nominal content of ratiocination is also included in Mansel's analysis.

Finally, reasoning is the joining together of judgments. Inferences are made from certain judgments to others. Again, no claim about abstract properties such as entailment are made. Mental activity is the exclusive subject matter of the discussion.

Mansel now categorizes judgments as to their nature and their origins. Necessary judgments are of three kinds: logical and mathematical, psychological, and physical. The foundations of the necessity of logico-mathematical judgments and psychologically necessary judgments are two aspects of our mental constitution. Physically necessary laws are founded on the structure of the material world.

Mansel summarizes these distinctions as follows:

> 1. Judgments necessary in the first degree, or logical and mathematical necessity. These are dependent on the laws of our mental operations; and their contractions are neither conceivable nor supposable.
>
> 2. Judgments necessary in the second degree, or psychological necessity. These are dependent on the restrictions of our mental constitution; and their contradictories are supposable but not conceivable. To this class belong the principles of causality and substance.
>
> 3. Judgments necessary in the third degree, or physical necessity. These are dependent on the laws of the material world; and their contradictories are both supposable and conceivable.
>
> 4. Judgments purely contingent, where either contradictory may be the true or the false alternative. Such are all judgments reducible to no law of causation. (1851, p. 152)

It must be carefully noted that judgment must be read as a process in most of Mansel's work. Although he sometimes goes off the rails, Mansel does attempt to utilize a separate lexicon for each of the studies of psychology, logic and grammar. Let me give some examples.

Judgment is used to denote a psychological process. The static representation of that process is the domain of logic and its relevant item is the proposition. The grammatical term that refers to the linguistic object of equivalent value is the sentence. Psychology, logic and grammar also use other related terms such as conception, concept and word, respectively. There are laws of psychology, principles of logic and rules of grammar. And logic represents in the syllogism the psychological process of reasoning or thought.

Mansel does not treat these lexicons equivalently. He believes that understanding the processes of thought is more important and primitive than understanding either the language of the products of mental operation, namely logic, or the language used in describing human speech, namely grammar. The crucial study for Mansel concerns how the processes of thought alter and generate the form of their products. A hierarchical model emerges, and one way of picturing it is to imagine that each operation takes input and yields an output. There is process and product. The act of conceptualization takes either attributes or signs of them as input and through the process of joining them together, creates a concept as product. This product is handed on to the next level of operation: namely, judgment. This process unites concepts and produces propositions. These propositions are the matter upon which reason works.

The static representation of this process is the syllogism. It is, however, possible to read Mansel as taking the categories of logic to be static idealizations of the more correct process-talk of psychology. Thus, mental action is fundamental--the logical and grammatical stories, in so far as they are told without reference to the psychological story, are derivative or even defective.

Of course, this makes the quotation above more difficult to understand. One can see how a judgment according to processes described by a psychological law is necessary; it has the same necessity as any other nomically ordered physical event. But, in this quotation Mansel separates the four kinds of necessity outlined above by ambivilating between the psychological impossibility of joining some concepts and the logical relation of contradiction obtaining between a proposition representing the product of some impossible mental process and a proposition embodying a necessary relation. Hence, we will have to take a closer look at the means by which Mansel characterizes each of these types of necessity.

6. Mansel on logical and mathematical necessity.

According to Mansel, logical necessity is founded on the laws that govern all of our mental operations of thought. Mathematical necessity arises out of the properties and limitations of our intuitive abilities. That is to say, logic reflects the structure of our processes of conception, judgement, and reasoning; and mathematics does the same for our representative faculties. Mansel says for example that, "by the laws of thought, every part of any given concept, be its origin what it may, must be thought as identical with itself; and hence arises the logical necessity of all analytical judgments" (1851, p. 149). In the case of mathematics, however, he asserts that, "By the laws of our intuitive faculties, all objects of external perception have a certain relation to space, and all objects of internal perception to time; and hence arises the mathematical necessity of geometrical and arithmetical judgments" (1851, p. 149). So, we must distinguish between the laws of thought and the laws of our intuitive faculties.

The laws governing our thought processes are counterparts of the three principles of traditional logic. The principles are those of non-contradiction, identity and excluded middle. The laws can be stated like this: no object can be thought under contradictory attributes; every object of thought is conceived as having characteristics by which it is marked off or distinguished from all others; and, every possible object of thought either has an attribute or does not have that attribute (vide 1851, pp. 167-68). Objects that do not meet these requirements are not concepts. They are but vague proto-concepts.

Psychological necessity, in contrast to its logical and mathematical counterparts, is a function only of these and other limits, rather than the abilities of our intuition. This distinction is most easily understood by recalling the difference between conceptualization and judgment mentioned above. The limits of our intuition can be considered as the limits on the content of our thought; that is, a limitation of our ability to formulate concepts. To use an example of Mansel's, we cannot represent in intuition a being who sees but has no visual sense organs. We can, however, suppose such a being, because the laws that govern our ability to join concepts together in judgments allow us to join the concept of seeing with the concept of being without visual sensory apparatus. The contradictories of judgments with logical or mathematical necessity, then, are just those that we cannot even suppose.

7. Mansel's formulation set against Husserl's criticisms. Let us now return to Husserl's critique. To succeed in avoiding Husserl's challenge, Mansel must answer or deflect the twelve claims we have examined. The first three are the charges that as a psychologist he embraces various prejudices. I will argue that he does not.

Mansel does not claim that logical laws are essentially normative. Instead, he maintains that the laws of logic are, as Husserl insists, not essentially normative, but only used as norms. Logic manifests itself as both a science and an art, and Mansel says that, "the former is an essential, the latter an accidental feature" (1851, p. 17). Mansel argues that the application of the laws of logic in guiding thought is an art. But if there are no laws then there can be no application. The converse, however, is not true: there can be laws without application. So here we have agreement between the transcendentalist and the psychologist. Both Mansel and Husserl see logic as an inquiry into something other than mere rules. This renders innocuous the later charge that the psychologist cannot explain the causal role of the principles of logic in directing thought. Husserl and Mansel can give the same answer to that question.

Mansel would reject Husserl's argument from the ontology of arithmetic. For Mansel, arithmetic is not philosophically part of logic. It may use the art of logic in its inquiries and it has its own art, but it is fundamentally an investigation into succession. According to Mansel, given any series, "[an inquiry] acknowledging no relation between the several steps beyond that of succession to its predecessor . . . [will] contain all the data required for determining the nature of the necessary truths of arithmetical science" (1851, p. 108). The whole edifice of mathematical methods while facilitating calculation does not imply anything not already contained in the concept of succession.

Perhaps this is the idea Peano later makes clear in 1889. Nevertheless, Mansel has given an adequate response to Husserl's claim that mathematics can only be understood if each of its objects (that is, each number) exists as a numerically identical thing for all knowers. Mansel's is a completely Kantian response and it is therefore important to keep in mind that Kant's account of objective knowledge has two equally important strands: first, there is the identification of some relation or ordering principle as fundamental to a body of knowledge; and second, there is the emphasis on the relation or ordering humans are acquainted with in experience as determinig the particular form of the relation or ordering principle applicable to human knowledge. Thus, more than merely seeing succession as important to arithmetic, Mansel, like Kant, relies on inner experience to determine exactly which relation of succession is required for human arithmetic, (vide 1787, pp. 74-82). The succession founding arithmetic is the one that we experience. And for humans, the apprehension of succession is a psychological fact: we are aware of the succession of our mental states. Thus, for us, "the psychological foundation of mathematics is to be found in the consciousness of successive mental states" (1851, p. 107). Arithmetic must be the same for all knowers, if they analyze this relation between any objects of experience whatever.

Finally, Mansel does not embrace the prejudice according to which truth and necessity are merely feelings, and thus capable of only a causal psychological explanation. Necessity is a metaphysical notion, and therefore quite distinct from the epistemic notion of the a priori. For Mansel, necessity has to do with the special nature of certain concepts and judgments. The necessity of judgments is a consequence of our mental constitution and the nature of the world in which we find ourselves. As Mansel puts it, "Necessity in judgments is dependent sometimes on the laws of thought, and sometimes on the laws of other parts of our constitution; and the term may, in another sense, be applied to that character in certain judgments which arises from the limitation of our faculties, and from circumstances in which all men alike are placed" (1851, p. 149).

As we have seen, Mansel builds a threefold hierarchical typology for necessary judgments upon this foundation. Logical and mathematical judgments are necessary in the first degree. They derive their necessity directly from the laws of thought. For example, our thinking is such that, if it is conducted only according to the laws of logic, we must infer certain conclusions from certain premisses. Metaphysical claims such as the judgment that every event must have a cause, are necessary in the second degree. Their necessity depends on limitations in our mental constitution. We cannot but think that when a material cause exists, its effect will follow. Such a law, however, is not generic or empty. It presupposes the concept of cause, and so it is not solely dependent on the "empty" laws of logic. Against Whewell, Mansel claims that physical necessity is embodied in judgments that arise due to regularities in our experience solely due to the structure of the world. Some events always appear togethter, some never so. That the necessity of these judgments lack a mental origin is evidenced in our ability to imagine their contradictories.

In all this it should be clear that according to Mansel's account a judgment may be necessary to any one of these degrees while we are unaware of its necessary status. Thus, for this psychologist, necessity is no mere feeling.

Neither is Mansel inclined to analyze truth as a feeling experienced simultaneously with some judgments. He describes two kinds of truth: namely, material and logical truth. Statements about independent or objective states of affairs are material truths. Since it concerns objectivities, this would be the kind of truth Husserl appeals to in both logic and natural science. Mansel does not, however, take up the usual correspondence story offered in most realist accounts of truth. Material truth, in Mansel's view, "consists rather in the conformity of the object as represented in thought with the object as presented in intuition" (1851, p. 203). If this is correspondence, it is correspondence between two areas of mental life. There are no objectivities beyond the contents of the mind. Mansel's view is, then, better described as a coherence theory in which the truth of the elements of mental life depend on their consistency--and this consistency is not a feeling.

Logical truth is manifested by the conformity of thought to the laws of thought. That is, a judgment is logically true when it proceeds according to the laws of thought. It is not impossible to make logically false judgments because our thinking is not exclusively controlled by the processes exemplified in the principles of logic. We are capable of identifying concepts *via* analogy and induction, and these processes are, according to Mansel, quite outside those embodying sound thought, and so, quite outside the scope of logic.

We can, therefore, read Mansel as avoiding the reduction of truth to a mere feeling. And thus, Husserl's attack again fails to engage Mansel's psychologism.

Let me turn now to Husserl's claims concerning the metaphysical consequences of psychologism. First, there is the claim that if the psychologist is right and logic describes psychological activity, then logical laws should be as vague as other psychological laws or they must be approximations or fictions. Mansel maintains that the laws of logic appear precise because they describe the attributes possible in any object of experience, only at the general level. Moreover, unlike Husserl, Mansel is also sanguine about the idea that the laws of logic are approximations. As he says, "Logic may not unfairly be compared to mechanics treated as a branch of mathematics. As sciences, both proceed deductively from assumptions more or less inconsistent with the actual state of things" (1851, p. 17). He compares logically perfect thought to the mechanically perfect systems that nineteenth-century physicists studied. Actual thought processes like actual machines are related to their idealized models. As he puts it, "The instrument as used may not be identical with the instrument as contemplated, but it must be supposed capable of approximation to it" (1851, p. 17).

Husserl also maintains that logical laws do not entail the existence of the events that make up psychological processes. Hence, they cannot be descriptions of those processes. Here we seem to have not argument but bald assertion. Against this, we need only note that there is a coherent story that includes the opposite position. Mansel's characterization makes logical laws considered psychologically describe such processes. Logical laws, through their foundation, do imply the existence of these processes. In doing this, however, the laws of logic do not entail the existence of Husserl's postulated realm of objective but irreal truth or being. Of course, one must weigh the ontological commitments of each of these programs in philosophical logic and decide which are the more distasteful. However, Mansel's choice is not obviously less preferable.

The other group of Husserl's criticisms focuses on epistemological issues. To him, the psychologist seems committed to characterizing the laws of logic as a posteriori inductive generalizations drawn from psychological facts. Mansel, however, has quite another account of how we know about the laws of logic. Psychology makes acquaintance with the laws of mental processes by critically examining the processes of thought. For Mansel, introspection is the prime method of gaining access to the nature of those processes, but he does not rule out other experimental approaches to psychological research. The logician, in contrast, comes into contact with the laws of thought by critically examining the products of mental processes. The job is then to separate legitimate forms from their illegitimate counterparts. Thus, logic and psychology both lead us to knowledge of ourselves.

The legitimate forms of inference are distinguished by their analytic nature. They have the appearance of tautologies because that appearance is a "necessary consequence of the mind gazing upon its own laws" (1851, p.

19). And, in agreement with Husserl, Mansel claims that these laws are known a priori in as much as they are analytic. They are not knowable by a mind devoid of content, but they can be seen immediately with reflection upon the mind's operations on any content.

Again avoiding Husserl's charges, Mansel distinctly denies that our knowledge of the laws of logic is the product of induction. In fact, he argues that it is positions like Husserl's that lead to that end. If the laws of logic are not the laws of mind, then they are in some sense external to the mind and must be known through some experience by the mind of that external realm of logical objects. It is the ontological commitment to a third realm for logic, and not psychologism, that leads to empiricism like Mill's. As Mansel puts the argument, "If the dictum de omni were, as Mr. Mill supposes, formed on the hypothesis that universals had a distinct existence in nature apart from the mind that contemplates them, Logic . . . and its principles, as mere generalizations from experience, could never attain more than a physical necessity" (1851, p. 159). That is to say, principles concerning the properties of independent entities such as universals can only be generalized through induction from our particular experiences of these entities. Hence, since such principles concerning universal concepts are known a priori, they must not concern independent entities and therefore they are not the products of induction.

Finally, we come to the charge that psychologism requires that the laws of logic are about facts. Mansel, as will now be obvious, answers this in two ways. First, the laws are about facts in the sense that they describe, however indirectly, the contingent constitution of our minds. But, they are not about facts other than those concerning our mental constitution. Using these laws, logic, he writes, "is thus competent to determine the <u>possible existence</u> of a class of objects [as experience]" (1851, p. 237). It is, however, unable to tell us anything about what we will in fact experience. The factual content of these laws, then, does not undermine their status with respect to experience.

8. <u>Psychologism and phenomenology</u>.

While it is clear that Husserl knew of Mansel at least as the editor of Reid's works, there is no reason to believe that Husserl intended to attack Mansel's formulation of psychologism. That is not the point of this study. Husserl's failure to engage anything but a peculiar reading of Mill's version of logical psychologism leads to other ideas. First, it can suggest to us what most distinctively separates the Victorian psychologists. The Husserlian critique misses Mansel because the latter presupposes a psychology or philosophy of mind quite distinct from the anti-conceptualist associationism embraced by Mill. One lesson, then, is that the features of this program in philosophy of logic should not be confused with tenets of a particular psychological theory.

On the other hand, the motivation of the Husserlian attack can allow us to identify what binds the Victorian psychologists together. Clearly, although Husserl names Mill as his villain, some very non-Millian thinkers felt they had been criticized. Brentano is the obvious example, but another might be the author of The Philosophy of Arithmetic (1891). What Husserl objects to in both Brentano and his own earlier philosophy of arithmetic, and what he sees as common to all psychologism is this: the view that logic is the study of inference, whether human or formal. This focus on inference is just what binds together Mansel, Whewell, Mill, Maxwell--and even Boole and Venn. That is quite a claim. And since it is the fundamental idea of the rest of this essay, let me end this chapter by giving it some explanation.

One rejoinder to my demonstration that Husserl fails to engage Mansel's views is to claim that Mansel is not a Victorian psychologist. That is to say, Mansel's view may be seen as having nothing in common with psychologism, and perhaps as more closely resembling a proto-phenomenology that anticipates Husserl's own approach.[4] While containing the grain of truth that phenomenology grows out of psychologism, this response is nevertheless not easily defended. First, it flies in the face of every other method of identifying Victorian psychologists. And second, it leaves us in the uncomfortable position of seeing Husserl's comments on the psychologism in works by the likes of Brentano as mere slips.

However, if we take Husserl seriously, his conflict with Victorian philosophical logic can be made to seem profoundly deep: it is no less than a conflict over the fundamental definition of logic. It concerns, therefore, one of the most fundamental matters separating nineteenth- and twentieth-century philosophy.

Husserl's philosophical motivation is his belief that the foundations of mathematics had been left behind by nineteenth-century progress in the identification of imaginary and irrational numbers as well as the articulation of concepts such as the differential and the integral. He believed that mathematics was plagued with seemingly inconsistent concepts. In the development of his attempts to re-found mathematics on clear and consistent

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[4] Indeed, this is the response offered by M. van de Pitte when commenting on a version of this chapter presented at the 1989 annual meeting of The Canadian Philosophical Association.

concepts, he moved away from his advisor. That is, he gave up Weirstrass' view that numbers must be traced back to the mental act of counting. Instead he attempts to arrive at absolute knowledge of the nature of numbers as ideal objects to which we can refer, and with which we are directly acquainted.

Husserl's innovation is to treat logic as the science of the form into which all deductive systems must fit. It is the analysis of the concept of structure. And so it is the highest constraint on all experience, because, for Husserl, we come into contact with manifolds such as those of physical objects or numbers only by way of a theory qua a deductive system or structure. Let me explain.

A manifold is, for Husserl, a region of objects, whether physical or irreal. A region of objects is completely and uniquely determined by the relations that can hold between them, and those relations can be characterized by a deductive system. J. Philip Miller sets out the classic example:

> the 'real number system' . . . is in fact a manifold. . . . 'Real numbers' are objects in a sense, but they cannot be encountered in the way numbers in elementary arithmetic can. They are not mere fictions void of all reality, but they are entities of a very different character from the numbers presented to us in the act of counting.

(1982, p. 16)

Mansel's psychologistic account of succession as the foundation for arithmetic can be interpreted as an attempt to articulate, perhaps, Peano's postulates. But, it is insufficient as a foundation for all possible arithmetic because other sets of postulates are needed to generate the deductive systems that characterize manifolds such as the real numbers and the complex number system. (For example, to come into contact with the complex numbers, we have to extend the reals using operations on matrices.)

These manifolds do not have unconditional existence, however. As Henry Peitersma writes:

> Husserl's metaphysical thesis is that they do not exist in the true and proper sense of the word. The only existence they can be asserted to enjoy from the philosophical standpoint is existence in a contingent belief context. . . . They cannot exist independent from consciousness. (1987, p. 700)

My reading of this comment is that we cannot come into contact with a manifold unless we are in possession of a deductive system that (perhaps only partially) characterizes it. Being in the possession of a theory or deductive system is the necessary and sufficient condition of being in what Peitersma calls "a doxastic context". And the limit of our knowledge is, therefore, the limit of possible deductive systems or structures. The transcendent nature of objects, that is, their nature outside of all doxastic contexts, is a necessarily open question.

Hence, logic, as the science of the structure of all possible theories, is the science of the absolute limits of all possible experience. Thus, as Husserl remarks in a letter, "the goals which I assign to pure logic coincide essentially with those of the Kantian critique of knowledge" (in De Boer 1978, p. 278). Logic identifies the limits of the structure of any possible knowledge. It is the grammar of possible knowing. Therefore, as De Boer puts it, "the laws of logic are called by Husserl the logical conditions of knowledge" (1978, p. 277).

One might say, then, that the Kantian theme carried through by Husserl is that of seeing space, time and the categories as constraints upon all experience. Husserl takes this and moves it over to the analysis of logic. He makes logical form an ultimate constraint. Mansel, on the other hand, shares with Kant the view that logic is the science of inference or judgment, the limits of inference being held distinct from those of all possible experience. Given this, even if one were to find something similar in the introspective techniques of Husserlian phenomenology and Mansel's psychologism, that alone would not serve to reduce the distance between them on the nature and foundation of logic. In the end, Husserl's version of the twentieth-century's ideal of an absolute knowledge that is neither ascertained using the methods of empirical science, nor reducible to the results of empirical science, would be rejected by Mansel.

So, where Mansel has attempted to avoid Humean scepticism by grounding the universality of our knowledge on the commonalities of all human minds, Husserl has gone on to give up any reference to humans and their minds. Where the psychologist Mansel sees the limits and structure of our knowledge as defined by the capabilities and limitations of human inferences, the transcendentalist Husserl has attempted to formulate the purely structural limits into which any object of human or non-human experience must fit. Where Mansel would say that, as humans, we must interpret nature as a causally connected series of events played out in Euclidean space and metrical time, Husserl seeks to understand the formal limits of any consciousness of being.

CHAPTER III

INFERENCE, INTUITION AND NECESSITY:

Whewell's Developmental Psychologism.

1. Progress.

To characterize Victorian thought as dominated by the notion of progress is hollow. The real work is tying this general observation to the details of Victorian thought. Clearly, it is a misreading of Darwin to see anything like progress as following from his theory of evolution. But of course, Spencer, among others, founded the progress of human culture on murky evolutionary grounds, claiming as he did, survival for the fittest. Nevertheless, Whewell sits nicely within the class of thinkers who strove for a detailed understanding of intellectual progress. He presents a startlingly clear analysis of two facets of the growth of scientific knowledge. First, there is the local progress of each individual inference from a finite set of facts to a universal and necessary law. Second, there is his account of the historical dimension of science. In this, Whewell sees the progress of science as a series of colligations of facts, but also, and more importantly, as the development of the human mind's ability to recognize the necessity and universality of the functions or laws emerging from those colligations. This panoramic attention to the historical dimension of science, then, places Whewell in the company of paradigmatically Victorian thinkers such as Comte, Spencer and Marx.

Fundamentally, Whewell's philosophical project is to reconcile two important views in philosophy of science. The first is an empiricist account of natural philosophy that takes inductive inferences founded on experience to be the obvious motor of theorizing about nature. The second is a theory of knowledge that takes the foundation of physical theory to be a priori.[1] His attempt to carry this through leads him to identify the insights of the empiricists with a genetic account of physical theory. That is, he sees induction as having its sway in the context of discovery. In the context of justification, however, Whewell appeals to a psychologistic reading of Kant in order to ground the necessity of accepted physical laws in a priori intuition.

In explicating this project, I first use Whewell's debate with Mansel as a means of examining some dominant

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[1] Andrew Lugg apparently denies the feasability of even attempting to see Whewell's text as consistent: "Metcalfe is far more sanguine than I am concerning the possibility of extracting a coherent story from Whewell's philosophical writings" (1990). In response I can say only that if there is a systematic philosophical position within Whewell's writing, what I say in this chapter must figure in it.

themes in Whewell's thought. I then attempt to reconstruct a Whewellian concept of evidence, which lies at the center of his psychologistic theory of induction. The main features of Whewell's philosophy of science that emerge on this interpretation are: i) laws of nature are necessary and our knowledge of this necessity has a purely psychological foundation; ii) the committment of science is ideally only to the structure of phenomena rather than to particular postulates concerning the nature of the items related in such structures; iii) while they exhibit the forms of inferences utilized in scientific theorizing, neither a colligation of facts nor a consilience among colligations is, in any straightforward way, evidence for the truth, acceptability or comparative worth of scientific hypotheses; rather, iv) the inferences of discovery and justification are mutually verifying.

2. Whewell's psychologism.

I argued in the second chapter of this essay that a key issue distinguishing Victorian psychologism from more modern characterizations of logic is that the psychologists see logic as the study of inference, while some more modern thinkers see it as the study of all possible structure. It is not difficult to read Whewell, like Mansel, as working within this Victorian tradition. So let me first establish that continuity as a context for their disagreements.

Whewell's psychologism is, as we shall see, multifaceted. But he clearly can be made to seem to take his studies of logic and philosophy of science to be analyses of actual human inference patterns and laws of thought. While criticizing Mill's account of induction Whewell identifies as his own goal the formulation of a "consistent and intelligible view of the nature of Science, and of the mental processes by which Sciences come into being" (1849 p. 270). Tying together human inference and knowledge he says, "[Fundamental] ideas entirely shape and circumscribe our knowledge; they regulate the active operations of our minds, without which our passive sensations do not become knowledge" (1858a, vol. I, p. 69). It is not a matter of sloppiness, then, when he defines induction as "the <u>inference</u> of a more general proposition from less general ones" (my emphasis; ibid.)--that is, as the inferential procedure of an active mind. By taking Whewell's psychologism seriously, there is less need to suppress or reinterpret passages where he might be otherwise taken to "have confused psychological conviction with objective empirical truth, and with inferential validity" (Butts 1968, p. 23).

3. <u>A debate concerning methods</u>.

Mansel likewise sees studies in logic as studies of inference, but he distances himself from his Cambridge counterpart. Whewell sketches the larger Victorian debate in his open letter to Mansel, "The Limits of Demonstrative Science Considered". Referring to himself as Dr. Whewell, he writes the following:

> Kant considers that space and time are conditions of perception, and hence, sources of necessary and universal truth. Dr. Whewell agrees with Kant in placing in the mind certain sources of necessary truth; he calls these Fundamental Ideas, and reckons, besides space and time, others, as cause, likeness, substance, and several more. Mr. Mill, the most recent and able expounder of the opposite doctrine, derives all truths from observation, and denies that there is such a separate source of truth as ideas. Mr. Mansel does not agree either with Mr. Mill or Dr. Whewell; he adheres to the original Kantian thesis, that space and time are sources of necessary truths, but denies the office to the other Fundamental Ideas of Dr. Whewell. (1852, p. 8)

So, here is the crux of it: Mansel sees himself as a conservative Kantian, while Whewell is radical in his identification of forms of intuition or categories that Kant did not admit. Mansel takes his list of categories from the first Critique (1787), and considers them alone as established. Whewell surveys the history of science and inductively draws from it a set of categories allegedly missed by Kant, rather than arguing immediately from the transcendental unity of apperception. These categories are identified by organizing the history of science as a series of colligations, and are then elevated to the staus of forms of the intuition. Thus, the debate between Mansel and Whewell has a methodological dimension.

Although Mansel is not entirely adverse to the establishment of new sciences on the foundations of newly discovered synthetic a priori statements, he argues that Whewell has not sufficiently proven the existence of such statements:

> I do not think that Dr. Whewell has hitherto succeeded in establishing, in the science of Mechanics, a system of a priori synthetical truths derived from the idea of force as distinct from those which are mere applications of the mathematical intuitions of time or space. (Mansel 1851, p. 259)

Future psychological analysis may, according to Mansel, "establish the existence of other subjective conditions of intuition besides space and time" (Mansel 1851, p. 260). Hence, one might generate something like hypothetical or rational mechanics from this basis. But according to Mansel, Whewell's axioms are unlike even the statements that would figure in such an a priori science.

In his defence, Whewell claims that, "There are scientific truths which are seen by intuition, but this intuition is progressive" (1852, p. 8). The history of science shows Whewell that there is progress or development, and that development implies a distinction between the formative power of the researcher's mind as as against some object upon which he works. In this, then, he follows very closely Kant's distinction between the faculties of the active mind and the sensations upon which they work.

Whewell is not, however, an innatist in the sense that Locke attacks, because Whewell's psychologism makes ideas things very different from Lockean objects of understanding. Neither is he an innatist in the sense that can be attributed to Kant. Whewell's vision of the faculties of intuition is developmental.

The fundamental ideas of science arise in minds through progressive work. Conjectures are recognized to be the only way to clearly and distinctly think about certain phenomena. This is an act of insight that comes from careful and orderly thinking about phenomena. "[T]he mind", says Whewell, "under certain circumstances attains a point of view from which it can pronounce mechanical (and other) fundamental ideas to be necessary in their nature, though disclosed to us by experience and observation" (1852, p. 13).

Indeed, articulating something very like Putnam's notion of the contextual a priori (vide, Putnam, 1981, pp. 82-85), Whewell seems to think that one can be in a position where one recognizes not only that our historico-psychological context makes some statements necessary a priori, but also that it places the necessity of some ideas just out of reach for the time being. For example, speaking of his proposed chemical principle that combinations of reagents give rise to only some distinct and definite kinds (i.e. only certain molecules), he says, "*if* we could conceive the composition of bodies *distinctly*, we might be able to see that it is necessary that the modes of this composition should be definite" (1852, p. 12).

Hence, one aspect of Mansel's criticisms amount to a dispute about philosophical methodology. But the charge that there are not transcendental deductions for the fundamental ideas Whewell posits, can be deflected by noting that these deductions may be possible in the future, as the human mind develops; progress occurs as experience becomes such that it supports further presuppositions. (We shall not concern ourselves here with the relation of this claim to Kant's views, but it is possible that Whewell pursues a more psychologistic reading of Kant than is necessitated by the corpus.)

4. Psychological foundations of necessity.

Mansel, it will be recalled, embraces a hierarchy of modal notions. Logical and mathematical judgments are necessary in the first degree. They derive their necessity directly from the laws of thought. When reasoning, one is driven by one's mental constitution to infer certain conclusions from certain givens. Metaphysical claims, such as the judgment that every event must have a cause, are necessary in the second degree. These relate to what Kant saw as the roles of the categories. Their necessity depends on limitations in our mental constitution. One is forced to think that when a material cause exists its effect will follow. This kind of law has a modal character entirely distinct from that of the logical laws. These laws are not conceptually empty, they presuppose concepts. Mansel claims that physical necessity is embodied in judgments that arise due to regularities in our experience solely due to the structure of the world. They are the generalizations we are driven to by our sensory experience, but they are not grounded in the constitution of our minds nor in its limitations. Our ability to imagine only the contradictories of this latter class of statements is, according to Mansel, the sure sign of this fixed distinction between physical laws and higher order modalities.

While Mansel makes the mind the source for necessity in only logical, mathematical and certain metaphysical matters, Whewell, by contrast, allows the mind to contribute necessity to physical laws as well as all the rest--and to contribute exactly the same kind of necessity to the axioms of both logic and those of other developed sciences. The relevant part of his philosophy is his doctrine of fundamental ideas:

> The special and characteristic property of all Fundamental Ideas is what I have already mentioned, that they are the mental sources of necessary and universal scientific truths. I call them _Ideas_, as being something not derived from sensation, but governing sensation, and consequently, giving form to our experience;--_Fundamental_, as being the foundation of knowledge, or at least of Science. (1852, p. 8)

So, fundamental ideas, once established in the mind, organize and unify our experience. But now we sorely need a clearer understanding of Whewell's concept of idea.

Whewellian ideas are fundamental in a sense that separates this psychologist from adherents to the characterization of the nature of mathematical reasoning as hypothetical. These include Hobbes, the Scottish Common Sense Philosphers, Mansel, and, as we can now see, modern conventionalists like Reichenbach. In his, "Essay on Mathematical Reasoning" (1853), Whewell criticizes Stewart's view that mathematical reasoning is a hypothetical pattern of inferences based on definitions. A similar view arises in our own century: one can see concepts as arbitrarily defined and integrated into an uninterpreted calculus. After this is done we can identify our concepts with objects in the world through correspondence rules or what Reichenbach calls coordinative definitions. Speaking of these coordinative definitions, he observes that, "They are arbitrary, like all definitions; on their choice depends the conceptual system which develops with the progress of science" (1927, p. 14). For Whewell, however, definitions do not precede ideas, concepts, or intellectual progress. The fundamental axioms of geometry and the other sciences are not arbitrary definitions. They do not arise from definitions and they cannot be reduced to definitions. Rather they concern the forms one's inferences must take if one is to reason clearly about the subject matter of a science.

> The axioms do not flow from the definitions, but they flow irresistibly along with the definitions. . . . These axioms are not arbitrary assumptions, nor selected hypotheses; but truths which we must see to be necessarily and universally true, before we reason on to anything else" (1837a, p. 43).[2]

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[2] It should be pointed out that this view has been taken up by reflective conventionalists. Reichenbach holds

The first important thing to note about this contrast is that Whewell must be appealing to something more than mere deducibility in his discussion. To reason clearly about a science is not merely to derive theorems from arbitrary axioms according to some set of replacement or inference rules. For example, although I can derive a few theorems regarding highly curved three-spaces, I cannot reason clearly--if at all--about these non-Euclidean spaces. This non-deductive dimension of Whewell's thought will have to be incorporated in our later discussion of consilience. For now, however, it is enough to see that this is not an exclusively Whewellian point. Mansel and the Scottish Common Sense Philosophers were involved in a long fight against the displacement of geometry by mere algebra. Their contention was that the student of geometry learned how to think, whereas the student of algebra learned only how to manipulate meaningless symbols using meaningless rules. While algebra apes the form of some inferences, it is itself devoid of content. Indeed, James Clerk Maxwell's rhetorical question, "Has the multiplication of symbols put

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that we simultaneously use both intra-theoretic and coordinative definitions to develop our conceptual apparatus. Hempel (1966) also has come to see that the introduction of correspondence rules or reduction sentences and purely theoretical connections have to be introduced into a theory holistically.

a stop to the development of ideas?" (1875, p. 307), economically recapitulates this Victorian worry. Clear thinking in a subject is more than the ability to carry though careful deductions.

Another thread woven through this contrast presents definitions as having a heuristic value, but denies them the ability to stand in place of the intuition of an idea. Consider the case of space perception. Whewell follows Berkeley in his account of our perception of depth, noting that we synthesize our visual and motor sensations in order to arrive at perceptions of three-dimensional space. Unlike the associationists, however, Whewell believes that we require concepts prior to perception: "Our sensations need ideas to bind them together, namely, ideas of space, time, number and the like" (1844, p. 60). And these ideas are, in the end, identified with the relations extant between our sensations. "Perception involves sensation, along with the ideas of time, space, and the like", Whewell continues, "or if anyone prefers the expression, involves sensations along with the apprehension of relations" (1844, p. 60). Definitions cannot replace ideas because ideas are, as he puts it, "relations of things or sensations" (1844, p. 60).

Thus we now have a clearer notion of a Whewellian fundamental idea. Each is some structure of, or set of, relations among sensations. A fundamental idea or concept is the limit to our having certain sensations unified into perception and as such the limit of our thinking clearly about the elements of experience under a particular description.

The relations between sensations are not, as Mill and the associationists would have it, merely the habituated connection of sensations that have often been perceived together. Relations between sensations have their source in the mind, whether or not those relations are seen as necessary, and whether or not those relations are extant in the (noumenal) world. Again the Kantian-inspired presentation of the case of spatial perception is Whewell's model:

> Our knowledge of solid space and its properties is not conceivable in any other way than as the result of a mental act, governed by conditions depending on its own nature. . . . [P]erceptions of visible figure are not obtained without an act performed under the same conditions. The sensations of touch and sight are subordinated to an idea which is the basis of our speculative knowledge concerning space and its relations; and this same idea is disclosed to our consciousness by its practically regulating our intercourse with the world. (1858a, vol. I, 124)

Hence, inferences concerning all possible relations of objects in a three-dimensional space require our acquaintance with the fundamental idea of such a space and not merely, as Berkeley and his followers would have it, an acquaintance with the two-dimensional array of retinal experience and the vague feeling of "outness" accompanying motor activity. These experiences must be colligated by the idea of space. Once that colligation is developed into a fixed form of intuition, the set of possible relations according to the fundamental idea becomes the limit of our possible inferences. That is, as Whewell says, fundamental ideas "are not Objects of Thought, but rather Laws of Thought" (in Butts, 1968, p. 5).

Thus, Whewell sees the kind of modality transmitted to physical science as identical to that of mathematics and geometry. These sciences are erected on axioms or truths that have become self-evident and independent of experience:

> [S]ome such sciences at least, as for example Statics, appear to me to rest on foundations exactly similar to Geometry:--that is to say, that they depend on axioms,--self-evident principles, not derived in any immediate manner from experiment, but involved in the very nature of the conceptions which we must possess, in order to reason upon such subjects at all. (1837, p. 42)

And, it is important again to note just how fundamental these ideas are, to Whewell. They are not susceptible to exhaustive definition in isolation from the psychological act of direct intuition.

So, here is another aspect of the Whewell-Mansel debate: all necessity is seen by the former as of a kind, whereas Mansel admits a hierarchy of modal connections. It is crucial to Whewell that this unity of necessity remain intact, for if one can drive a wedge between the kind of necessity that geometry has and the kind of necessity attributed to the causal principle, then Mansel's move to separate the modality and epistemological status of the causal principle and that of physical laws follows easily. There can be no distinction in terms of modality between pure forms of intuition and pure concepts of understanding.

This goes a long way in explaining Whewell's rejection of Kant's distinction between faculties of intuition and categories of the understanding. The distinction between forms of intuition and pure concepts seems to have no other defence than that it serves to keep the modality of causal statements apart from the modality of geometrical and arithmetic statements. Indeed Ralph Walker (1978, pp. 60f) argues that, Kant's distinction between the imposition of space and time by the faculty of sensibility and the imposition of pure concepts by the faculty of understanding is a cumbersome and unnecessary aspect of his philosophy. Moreover, de H. J. Vleeschauwer even argues that Kant repudiates this distinction, (Walker, 1978, pp. 61, 182). Thus, Whewell's portmanteau can seem more justifiable than Mansel's dogmatic Kantianism. Speaking of Whewell, Stoll correctly notes, "If he had accepted Kant's distinction, his readers might have observed that other distinctions between the different Ideas also ought to be noted" (1929, p. 41). This in turn would undermine his attempt to place scientific truth on the secure a priori foundation that Hume seemed to leave as the only viable alternative to skepticism. Hence, Whewell's attempt to collapse the distinction between forms and concepts, making both fundamental ideas, is well motivated.

5. Points of View.

Now one can stop for a moment, and note the roots or anticipations of Gestalttheorie to be found here. That is to say, on matters of perception, or synthetic unity, Whewell is not easily fitted to the empiristic school of Helmholtz. For Whewell, we can conceive of something and have knowledge of it only if we represent it explicity. Whewell's notion of explicit representation involves something like human intentionality, consciousness and perception. This is the nascent Gestalt-like thread in his thought. Moreover, this stance requires that where one is located intellectually and historically must play a major part in deciding which inferences will work next; viz. our scientific history could have been many ways--but it could not proceed in just any old way--and this contextual dimension does not show up in the logico-propositional models of science and absolute concepts of evidence that have dominated this century's philosophy of science. Whewell's characterization of induction, then, could not straightforwardly be modelled in a cognitive entity that had explicit representation defined as something like "X is explicitly represented by Y if and only if Y's behaviour depends upon X". That is, explicit representation cannot occur in an entity with no consciousness or perceptual intuition.

Furthermore, a purely inductive mind emulating Helmholz's characterization of unconscious inference could not use concepts to organize data and generate theory, for as Malcolm Forster notes,

> [T]heory construction always involves the introduction of new concepts. No method of logical construction can do this, since the content of higher level nodes are derived from a limited store of atomic propositions, predicates, names and logical operators. . . . [And] the idea of finding logical definitions of theoretical predicates (like "mass") in terms of more primitive observational vocabulary is hopeless. (1990, p. 32).

For Whewell, this points out just what is wrong with

Helmholtz's approach and Mill's associationist psychology: concepts (fundamental ideas) cannot be reduced to associations of basic sensations because they are orderings of those sensations. Points of view are imposed by the mind.

This implies that one cannot separate the conceptual and factual elements of a theory. But the necessity of the theoretical claims are not in any way dependent on the facts. Hume was right! Rather the necessity must come from the concepts contributed by the mind. Moreover, since the sign of necessity for Mansel and Whewell is the inability of the mind to conceive the contrary, its precondition is the control by concepts already integrated into our conceptual apparatus over our ability to even imagine certain states. (Of course we can describe the contraries to certain necessary laws, but we cannot conceive them.)

This implies that Whewell's doctrine of the fundamental antithesis is consistent with his psychologism. To make this clearer, think of the perceptual case. I cannot see my sensations <u>unless</u> I organize them in space. But that does not undermine the view that any necessity in the science of space arises due to my active organization of these sensations. The fundamental antithesis tells us we cannot clarify our concepts without using them to create experience, but it also tells us that we cannot have experience without concepts. So we find Whewell arguing: "Experience is requisite to the clearness and distinctness of our ideas, not because they are derived from experience, but because they can only be exercised on experience" (1844, p. 70).

In space perception as elsewhere, then, clear cognition concerning a realm of data is not a product of learning, it is the construct arrived at by imposing an idea on sensations. This puts a great distance between Helmholtz's uberwusterscluss (unconscious inference) and Whewell's inductive inference. It makes Whewell much more in line with the so-called nativists such as Hering. Whewell's developmentalism, then, is not to be identified with Helmholtz's unconscious inference, but as a variant of nativism. Thus, "attaining a point of view" can be profitably read as a Gestalt-like process.

The Gestalt thread also brings out the point that Whewell is attempting to ground scientific research on features closely related to those that have more recently been used as criticisms of twentieth-century philosophy of science. Kuhn's Structure of Scientific Revolutions (1970), for instance, can be read as an example of modern psychologistic philosophy of science that has much in common with these aspects of Whewell's thought. One of the main arguments of that work is a psychologistic attack on all foundationalist projects. A paradigm, under this reading, is a thing that ends up in the psychological make-up of the so-called licenced member of a community. Kuhn's seeming innovation is to say that it has both individual and social existence. The paradigm's individual aspect is a set of fundamental ideas that determine the processes of involuntary and immediate experience. As in space perception, it is not interpretation that is the subject of discussion--and so, hermeneutics does not have the resources necessary for a response--rather, it is the processes of human unconscious intuition that are the focus of these kindred works. Kuhn can use these insights as a critique because he is addressing a philosophy of science founded upon logicism, while Whewell's psychologism embodies them as foundations. Preempting Kuhn's attack, then, in Whewell it is the organizing idea or paradigm, rather than the sensations organized, that form the bedrock of his epistemology.

6. Mansel and Whewell on true causes.

Whewell, as we have seen, does not insist on a transcendental deduction of his fundamental ideas. Rather, Whewell's synthesis is an attempt to reconcile a theory of knowledge based on the a priori, with an account of inductive inferences driven by experience that empiricists endorse. Researchers organize sensations using fundamental ideas or concepts and this constitutes their inductions, but these concepts become the a priori foundations of our knowledge of physical laws; through history, these concepts are superceded and replaced. Against Whewell's specific examples of the products of this method, however, Mansel argues that Whewell achieves seemingly a priori knowledge of necessity, "by comprehending under one formula the mere analysis of [equality] and the empirical determination of equality in any particular instance" (1851, p. 259, #1). Let me explain.

Whewell claims that some examples of synthetic necessary laws known a priori concern levers. In statics, the fundamental idea of pressure allows one to see the truth of the claim that two equal weights at the ends of two equal arms will balance. The idea of force gives rise to a similar laws in dynamics. In his Mechanical Euclid, Whewell sets out a number of axioms that he takes to be implicit in the fundamental idea of force. One of these is: "If two equal forces act perpendicularly at the extremities of equal arms of a straight lever to turn it opposite ways, they will keep each other in equilibrium" (1837, p. 28). This is the force-theoretic counterpart of Whewell's claim in Book II, Ch. ix of his Philosophy of the Inductive Sciences--and there he sets out a classically Kantian transcendental argument:

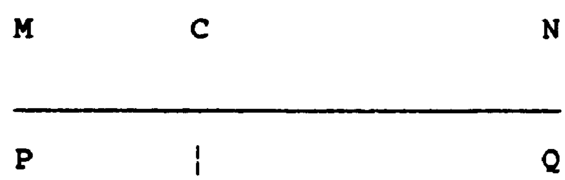
1. We have experience/intuitions about quantities.
2. The claim that "if equals are added to equals, the

wholes are equal" is a necessary precondition to intuitions about quantities.

C. Therefore, if equals are added to equals, the wholes are equal.

The thing to notice is that, for Whewell, these claims are founded as the necessary preconditions of a kind of experience, and not on the analytic principle of identity.

In the <u>Mechanical Euclid</u>, then, Whewell proves what seemed to others such as Mansel an empirical truth by <u>reductio</u>. It is his Proposition IV (1837, p. 34). Given the lever MCN with downward forces or weights P and Q,



he proves the ratio, NC : CM :: P : Q. This truth is not, as Forster (1990) implies, identified by making a huge number of scatter diagrams, in the sense of arbitrarily plotting all sorts of magnitudes against each other. Rather according to Whewell we begin with a conception of a physical or mechanical "system" in mind when we look at the phenomena of the behaviour of the lever. It is the ideas of geometry (<u>qua</u> the science of rigid bodies and space) and those of force that let us organize these phenomena into this kind of system. We are not adrift in a buzzing blooming confusion. The regularity becomes accessible only after the researcher is introduced to the details of Newtonian mechanics. This introduction is, for the Whewellian, part of learning merely how to perceive.

Mansel has difficulty in seeing this law as a truth, let alone a necessary truth:

> Of force . . . we have no positive conception _per se_; we know it only by its effects. Of equal forces we have no positive conception beyond that of the production of equal effects. To assert, therefore, that equal forces will balance each other at the two extremities of a lever, is to assert no more than that effects universally equal will be equal in any particular case. (1851, p. 259)

The law of the lever is characterized by Mansel as either the misstatement of an analytical truth or a purely empirical truth that is "not even universally true" (1851, p. 259 #1). The related analytical truth is, according to Mansel, "bodies acting with equal forces to turn a lever in opposite directions will retain it in equilibrium" (ibid). This he takes to be a case of the law of identity or equality: the addition of equals to equals yields wholes that are equal.

Thus, Mansel takes the laws of logic to endorse the analytic claim because in it we restrict our reference to two forces only insofar as they are acting equally on a lever to turn it in opposite directions. We do not refer to the actual or independent forces. That is to say, we do not speak of the true (component) causes of the equilibrium in the system. And we do not, according to Mansel, because scientific investigation has no access to the level of true causes. For Mansel, "Truth does not consist in the agreement of thoughts with things in themselves, but with intuitions" (1853, p. 97). In Chapter II, I called this a coherence theory of truth because it presents, as our only standard of truth, the compatibility of our perceptions and our theories. Mansel takes this to be the implication of his Kantian denial of the possibility of scientific talk about noumena. This leads him to voice a radical phenomenalism, as when he sarcastically notes that,

> [T]he ideas of Substance and Cause, when they are supposed to imply anything more than mere phenomena, are so far from being treated as foundations of science, that they are regarded as . . . essentially negative and obscure. (1853, p. 98)

This forces us to confront the realism in Whewell's approach to science. Whewell readily admits that the law of the lever is synthetic, and not analytic, just because we can refer to the true causes behind the phenomena. Whewell mentions not merely a force-insofar-as-it-acts-on-a-lever. Rather, he seems to speak of forces that are robust and equal even in the absence of all levers and, one imagines, all other apparatus that might translate their relative strengths into phenomena. Functional representations of the relations among phenomena are, then, only the first step toward true science *qua* the study that develops our knowledge of causal laws.

As Mansel so keenly observes, Whewell's vision of scientific research is one that allows, through science, access to the causal structure behind the phenomena. But, the reader must be wary of attributing to Whewell the simplistic realism that Mansel urges. Force, for Whewell, is a fundamental idea, and fundamental ideas refer to the structure into which sensations must fit. They do not refer straightforwardly to some quasi-substance deserving ontic commitment. Thus, Whewell's realism can be seen as a commitment to the reality of the structure of sensations, and not to their ontological basis in such things as atoms and forces. Of course, this structure or set of relations among sensations is, as we have seen, contributed by the active mind. That is, the structure is in the end a set of laws of thought.

This Whewellian realism, if it is indeed a realism, is very unlike that simple embracing of physical hypotheses that Mansel and the Scottish Common Sense Philosophers ridicule. And Mansel's criticism is, therefore, somewhat misplaced. But, this reading of Whewell does more than merely save him from Mansel. It also serves to make his thought more consistent with what Hendry calls the dynamistic tradition in nineteenth-century physics (1986, pp. 10ff).

Hendry contrasts the mechanistic and dynamistic traditions. He claims that the mechanists, following Locke's interpretation of Newton, build their theories upon hypotheses concerning passive inert matter with forces superimposed. Laplace is the arch-mechanist and he generates the classical mechanist methodology. Wise sketches the method like this:

> One summed over the action of discrete point masses distributed in space to find the total effect at any given point. Integral equations described the physics. In practice it proved more efficient to convert that primary concept to a smoothed partial differential equation. But the Laplacians conceived these differential equations as secondary, purely mathematical constructs. (1982, p. 182)

The mechanist analyzes phenomena as systems of molecules, establishes a set of integral equations for action at each distance from the source, and differentiates to arrive at <u>approximations</u> of the more precise integral equations. The differential equations are merely mathematical in the sense that they are derived from continuous integrations that for absolute accuracy and consistency with the guiding mechanistic physical hypothesis of molecules, must be replaced by non-continuous summations (_vide_ Hendry, 1986, p. 95)

The dynamistic approach follows Leibniz as against Locke, and takes forces to be fundamental or inherent in matter. This gives rise to what might be called the Lagrangian method: start with macroscopic behavior and leap to differential equations. Fundamental laws, such as Newton's laws of motion, are treated as parts of an abstract mathematical system based on axioms that are self-evidently true, _independent of empirical verification_; that is, the laws of dynamics are rooted in the mind and hence rationally grounded. So science has two parts: a priori and a posteriori.

Although he fails to note the structure- or model-theoretic approach hidden in Whewell's concept of the fundamental idea, Hendry rightly points out that Whewell sits within this dynamical tradition. Thus, Whewell promotes the generalized dynamics of Sir Rowan Hamilton over the mechanical approach of Fourier because the latter "mistakes the _use_ of the differential calculus for the _evidence_ of physical truth" (in Hendry, 1986, p. 43). That is, Whewell chides Fourier for seeing the success of his approach in predicting and unifying phenomena as a warrant for ontological commitment to molecules and forces acting at a distance.

Clearly then, Whewell does not suggest that we are directly acquainted with true causes as sensations, and his ontological commitments to true causes are more complex than Mansel sees. But we do know about true causes. The methods of colligation and consilience are the circuitous routes we must follow in arriving at our theories about them. And the development of our intuitions during the regiment of colligation and consilience allows us to reason clearly about the causes *qua* structural relations of phenomena; that is, we come to intuit the nature of these true causes as the necessary connections between events. Hence, it is to the details of the evidence we have for hypotheses about such relations that we now must turn.

7. *Induction and necessity*.

As Butts notes, "The central, and clearly the most intriguing, thesis of Whewell's philosophy of science is that science develops by becoming a more and more comprehensive system of laws that are both universal and necessary, and that are, nevertheless, in some sense the result of induction" (1968, p. 4). In order to understand this claim one has to separate the genetic-empirical features of Whewell's account from its foundational-a priori aspect. But that understanding also requires that we do not lose track of the interrelation between the two stories. "On the one hand", Butts points out,

> Whewell holds that there is a distinction between necessary and empirical truths, and thus that necessary truths cannot depend for their evidence upon appeal to experience; on the other hand, he believes that necessary truths emerge as necessary in the course of the development of this or that empirical science. (1968, p. 7)

There is a symbiotic relationship between what is now distinguished as the two contexts of research.

Whewell's focus on this interdependence of discovery and justification allows him to go beyond more modern accounts of evidence that analyze the relations between propositions in an ideal context of pure justification. His synthesis is facilitated by his attempt to analyze the important features of science in terms of integrated patterns of inference. The cryptic analysis he generates is this: "several Facts are exactly expressed as one Fact if, <u>and only if</u>, we adopt the Conception and the Assertion" (1858, p. 174). In the remainder of this chapter I will try to make sense of this claim.

8. <u>The modern context</u>.

Evidence is, at least in part, a practical notion, it refers to something that provides a reason to believe. Sciantific activity, in so far as it is a process of achieving knowledge, relies on the meaningfulness of a concept of evidence and this reliance can be decomposed into three main relations between hypotheses and evidence: first, evidence plays a (dis)confirmatory role; that is, evidence is taken to confirm or disconfirm some hypothesis to some degree. Second, evidence provides a means of constructing a comparative measure of the relative support available for competing hypotheses. Finally, evidence has an acceptance governing role; evidence can be taken to found decisions concerning belief of an hypothesis.

Carnap (1966) suggests that the confirmatory role of evidence gives rise to both classificatory and quantitative concepts of evidence, and that the former is parasitic on the latter. However, attempts at explicating a classificatory concept of evidence can precede and inform attempts to analyze its quantitative counterpart. So let me set the stage for my discussion of Whewell's theory of evidence by sketching two influential analyses of the classificatory concept of evidence.

Hempel (1945) posits a simple notion of evidence that fits well with a naive (and non-Whewellian) inductive methodology. Hempel accepts Nicod's view that an observation report E is evidence for an hypothesis H just in case

i) E entails H, or if H is a universally quantified statement, one or more of the conjuncts in the

Herbrand expansion of H; or,

ii) H is entailed by a class of statements each of which claims E for evidence *per* clause i).

This concept of evidence gives rise to a number of well-documented difficulties. For example, the so called Paradox of the Ravens is generated. Another difficulty is that there is great difficulty in transmitting evidential support in a well-behaved way to any statement containing essentially non-observational terms. Once non-observational terms are introduced, the fear is that there is no principled way of limiting them to those that intuitively deserve acceptance or belief. Goodman's Grue Paradox (Goodman, 1955) and the problem of the introduction of supernumerary hypotheses (Braithewaite, 1953) warrant this fear.

An instance of this difficulty is the requirement that no non-modal statement can be evidence for one that is modal. If modality is taken to be the form of a statement, then no notion of entailment models the results that we want. If, on the other hand, modality is seen as part of the content of a statement, then where one does not observe necessity in nature, one cannot posit it at any level of generalization. In either case, there is no evidence for necessity.

Glymour (1980) proposes what can be reconstructed as another powerful analysis of the classificatory concept of evidence. Put in Achinstein's (1983, p. 169) simplified terms, for Glymour a statement E is evidence for an hypothesis H with respect to a theory or set of background hypotheses T, if and only if:

i) E and T are together logically consistent;

ii) each quantity in H is computable from the values given in E;

iii) H is not a mathematical identity;

iv) the computation in ii) uses only T and its consequences.

The basic idea is that E is evidence for only the functions that have their every magnitude assigned an exact and explicit quantity by the conjunction of data and the other functions in T through a computation.

Now, there have been claims that this analysis is open to counterexamples (vide. Christensen 1983). But for our purposes, we need only note that this notion of evidence rules out the possibility that there is an evidentially principled way of admitting nomic necessity into science. It is arbitrary whether or not either H is necessary or T contains necessary statements. There is nothing in the computation of the quantities for the magnitudes in any function describing a mere regularity that serves to distinguish it from its nomically necessary counterpart. Since we have evidence only with respect to a theory, the theory is the only possible source of modal content--and the theory is in an important sense independent of the evidence

If, however, evidence is to serve its many roles, it must underwrite the possibility of a principled way of introducing necessity into science. It would be odd and unfortunate if the concept of evidence by its very nature alone should decide the question of the possibility of our knowing of any extant necessary natural laws. Perhaps we cannot know of their existence or perhaps they do not exist, but that surely is something we should find out only after the evidence is in. Thus, Whewell's concept of evidence, which, I will argue, attempts to come to grips with the seemingly ubiquitous scientific inference that moves from a set of utterly contingent statements to statements that are both universal and necessary, may have important implications for contemporary philosophy of science.

9. Colligation as induction.

For Whewell, inductive inference includes a process that he calls the colligation of facts. As Whewell sees it, this process has three aspects: "the selection of the idea, the construction of the conception, and the determination of the magnitudes" (1858, p. 211). In researches susceptible to mathematical descriptions, Whewell reidentifies these aspects as follows: "These three steps correspond to the determination of the Independent Variable, the Formula, and the coefficients" (1858, p. 210). In studies that rely on tables of inequalities, such as astronomy, Whewell identifies the selection of the idea with the argument. He then sets the construction of the conception in line with the law introduced. And finally he identifies the determination of the magnitudes with the units of numerical data exhibited in such tables.

Although Whewell's examples are from the natural sciences and the clear focus of colligation is on inferences in the natural sciences, a prime model of this Whewellian approach comes from simple problem-solving activities. It must be understood that Whewell is the author of a number of undergraduate textbooks in mechanics. He is also the sometimes champion of the mathematical Tripos at Cambridge. He debated with Hamilton concerning the usefulness of mathematical education. He was a fellow and tutor at Cambridge, and his college was prominent in turning out top wranglers and Smith Prize winners. Whewell's day to day involvement in the mathematical training of undergraduates was, therefore, extensive. It is not surprising, then, that a common direction to students attempting to solve certain mathematical problems seems to exactly parallel and explicate Whewell's three-step account of colligation. My claim is, nevertheless, not historical. I do not wish to debate the play of influences. I maintain only that the following problem-solving approach is an accurate model of Whewell's notion of colligation.

Consider the following problem, which is similar to hundreds in introductory physics, calculus and analytic geometry texts. A sheet of metal 8 meters in length and 5 meters in width has equal squares removed from its corners so that it may be folded and soldered to form a rectangular container. At what depth will the container have the greatest capacity?

The standard approach taught to the student is as follows: first, identify the dependent variable. In this case it is capacity or volume. Call this V. Now identify the independent variable(s). In this case they are length, width and depth or height. Second, identify a formula that ties V to the independent variable and the other givens. That formula is, of course, $V = lwh$. Third, use the given data to fix the coefficients of the formula. That is, generate the equation,

$$V = (8 - 2x)(5 - 2x)x.$$

Which is to say, we have the volume function for this case, with coefficients in place:

$$V(x) = 4x^3 - 26x^2 + 40x.$$

The colligation is complete. From this point on the solution is merely a matter of calculation. (I.e. find the derivative and ascertain its maxima.)

This is an almost perfect example of colligation. It reads a differentiable equation off of a state of affairs presentable in direct experience, and so fits the dynamistic tradition. It is what problem-solving wranglers must do and, for Whewell, it is almost identical to the situation of the researcher. Again, listen to Whewell on colligation: "These three steps correspond to the determination of the Independent Variable, the Formula, and the coefficients" (1858, p. 210). Of course the researcher often has to rely on more than memory and a textbook to identify the independent variable and the formula. But these creative procedures are clearly steps of equal if not more significance than the identification of coefficients. Moreover, while Whewell spends time explaining how routines, such as the method of residues, can lead to the identification of interdependent variables, he seems far less interested in the mathematical wrangling needed for applied science. Colligation is interesting as the fundamental creative or interpretive act of the scientific research that dominates the context of discovery.

This reading of Whewell makes sense of the complete absence from his work on induction of any discussion of formal and computational problems in probability theory, of which this extremely adept mathematician would clearly have been aware. Whewellian logic of induction is an account of the inferential processes by which colligations are brought into the fabric of established belief. It is not an account of probabilities and their manipulation in a calculus of evidential support.

This model also offers some insight into Whewell's disparaging comment that, "men often admire the deductive part of the proposition, the geometrical or algebraical demonstration, far more than that part in which the philosophical merit really resides" (1858, p. 174). Whewell can be read as chiding those who dwell on the geometrical or synthetic presentation that constitutes the greatest part of Newton's Principia. For Whewell, the interesting and important part of scientific investigation is not the deductive, algebraic or geometrical entailments of colligating ideas or functions. Rather, focus should fix on the analytical aspects hinted at in Principia and worn on the sleeve by investigators like Lagrange. That is, as John Hendry (1986, pp. 32f) argues, Whewell sides with those dynamical thinkers who treat Newton's theory as a detailed account of principles that are self-evidently true. It is Whewell's project to make sense of the Newtonian Paradox; namely, that the laws of dynamics are firmly based on experience while being independent of empirical verification. And so, while deductive demonstration constitutes explication of an idea, it is not confirmation.

For a colligation in the natural sciences using mathematical apparatus, Forster (1988) has done a very nice job of unpacking the final two steps of this process. The step in which the identification of the independent variable takes place is, however, passed over with surprising speed. Forster merely notes this: "Upon the selection of the independent variable, we may plot many pairs of values of the dependent variable and independent variable on a 'scatter diagram'" (1988, p. 67). This seems to undervalue the interrelatedness of the inferences of discovery and justification. As William Harper notes in his comments on Forster's paper, "The idea or independent variable depends on what we choose to consider. Some choices are more illuminating than others" (1989, p. 119).

This lacuna can be made even more startling, if one recalls two related points. First, Whewell anticipates Hanson's (1958) claim that experience is theory-soaked. As Whewell puts it, "We cannot obtain a sure basis of Facts, by rejecting all inferences and judgments of our own, for such inferences and judgments form an unavoidable element in all Facts" (1858, p. 125). Hence, the generation of colligating ideas is not merely a selection of magnitudes but a necessary condition of experience of the data as such. Moreover, where such choices of magnitudes are in progress, a standard criticism of induction, usually made by those supporting some form of hypothetico-deductivism, is that a hypothesis seems to be needed before one can determine what variables it is reasonable to collect and plot on a scatter diagram. We saw this in our discussion of levers.

There is, however, some reason to believe that Forster sees Whewell as promulgating some rich but essentially empiricist notion of evidence. Forster takes Whewell as allowing higher-level colligations to act as evidence for the propriety of our choice of variables. That is, Forster seems to have Whewell rely on their participation in higher order colligations to determine their membership in a preferred class of physical magnitudes:

> Whewell has provided roughly the following picture of science. At the phenomenological level a colligation of facts will generally introduce the means of measuring formula coefficients, which may be instances of variable quantities when viewed more globally. These 'theoretical' variables may themselves enter into higher-level colligations, which may then introduce further coefficients, and so on. (Forster 1988, p. 77)

This hierarchy becomes a selection device for conjectures, in that a magnitude such as, say, instantaneous acceleration, says Forster, "is a physically significant conception because it enters into the formulation of higher-level laws" (Forster 1988, p. 71). But if this is Whewell's view it is problematic.

The first issue to be met in such a view is that of alternative formulations. It is not only possible, but according to some (e.g. Feynman) even essential, for a good theoretical apparatus to be isomorphic to other theories that colligate the phenomena in question under different magnitudes, formulae and coefficients. If there is no other restriction on a colligating conception but for its subsequent functional relationship to other colligating conceptions, we cannot choose between such equivalent sets of magnitudes. But Forster's realism would force him to choose. Forster's approach similarly makes it impossible to keep competing theories separate; it rules out our establishing a comparative measure of the relative support for competing hypotheses.

Moreover, this approach joins seemingly distinct sciences. Nineteenth-century physical theorists were quite aware of the identical structures, or what Maxwell calls the physical and formal analogies, that hold between fields like thermodynamics, electrodynamics and fluid dynamics. The magnitudes of these studies can be identified with one another by simple functions that correct for scale. And then, although they concern phenomena whose true causes are intuitively unrelated, these magnitudes point to common causes, in Forster's words, "based on the consilience exhibited by the coefficients" (1988, p. 92).

Finally, mere participation in higher order colligations cannot rule out the propriety of any magnitude whatever; a variant of the Grue Paradox arises. It is not difficult to see that any number of intuitively odd magnitudes can be related to other magnitudes through baroque functions. And, measures of complexity won't help because we are coming to believe that the functions that accurately colligate our most trusted and treasured magnitudes in any but the simplest cases, while theoretically computable, are, practically speaking, uncomputable. That is, the complexity of interrelations among phenomena appear to outstrip our mathematical techniques at every turn. But if we allow partially specified relations that merely fit strange attractors to be acceptable colligating conceptions, there seems no limit to the lower-level magnitudes that can be fitted to higher-level inductions.

The alternative is equally unpleasant. If we do not allow looseness in our higher-level colligations, then our most interesting magnitudes lose their status. Moreover, as Cartwright has made clear, the laws that stand as paradigms of the successful relation of magnitudes are often shadowed by ceterus paribus clauses that cover over their inapplicability to cases. Tightening the restricitions on higher order colligations will surely push these laws out of the class of justified claims.

With such room to manouver, we can be virtually assured, then, that higher-level colligations by themselves cannot provide evidence for the propriety of our choices of magnitudes. If this is Whewell's view, he is in trouble; the criterion of participation in higher order colligations allows both too much and too little into the class of acceptable magnitudes. But this is not Whewell's view. Forster has collapsed the distinction between higher-level colligation and consilience. This in turn blurs the Whewellian distinction between discovery and justification and occludes the delicate interrelations between inferences in each of these contexts. These distinctions are essential to Whewell's philosophy of scientific method. They are the foundations of Whewell's attempt to synthesize empiricism and transcendentalism.

Whewell is right to see that participation in a higher-level colligation is not enough to guarantee the propriety of our creative act of generating a colligating idea. A colligating idea is vindicated only when it later plays into a situation in which its relation to other ideas is seen as necessary. As Whewell puts it, "the selected idea is proved to be the right one, only when the true law of nature is established by means of it" (1858, pp. 215-16). Thus, since establishing an actual law of nature requires the identification of a true modal statement, it is to the process of intuiting necessity that our attention must now turn--to the evidence for necessity.

10. <u>Consilience of inductions</u>.

For Whewell, a colligation by itself does not amount to strong evidence for the colligating function or statement. Nevertheless, he claims that the general proposition arrived at by a colligation can be, "more true than the individual facts themselves" (1858, p. 227). This is because the discovery or creation of a fundamental colligating idea can achieve the status of a necessary precondition for clear experience of some class of representations. Thus Whewell's views on the modal component in science cannot be set apart from his account of evidence and scientific inference.

This is not the standard reading of Whewell. It is closer to the line of thought expressed by Butts than that found in Forster. According to Forster,

> Whewell understands the consilience of inductions as a feature of the evidence demonstrated by the successful application of magnitudes determined by the facts of one domain in predicting facts in a different domain. . . . Whewell means that the magnitudes independently measured within separate domains agree with one another, or are connected by some law-like regularity (i.e. connected by some formula). (Forster 1988, p. 74)

On this reading, every colligation is a consilience, because in every colligation we generate and impose a functional relation of two or more magnitudes. Moreover, every consilience is a colligation inasmuch as connecting magnitudes by a formula (with its coefficients fixed) is a colligation. The only separation of consilience and mere prediction is, per Forster, that in a colligation, the researcher does not realize that the colligating concept or magnitude applies to areas of phenomena that are not at present under his study. Now, it may indeed be the case that colligation is a necessary component of consilience, but I will argue that it is not a sufficient condition; that is, there are colligations that are not consiliences.

Harper is more guarded in his characterization of consilience. He endorses Forster saying, "Forster's claim that each case of the consilience of induction is a higher order induction is, surely, a correct explication of Whewell's view" (Harper 1989, p. 126). However, he identifies as more controversial the view that, "any such higher order colligation counts as a consilience of inductions" (ibid.). Still, in the end, Harper maintains that, "This line is attractive even if it means admitting as examples of consilience many higher order colligations which do not have the impressive characteristics of Whewell's favorite examples" (ibid.).

Butts appears more sensitive to the dimensions of Whewell's account of consilience that are concerned with psychologism and the role of the synthetic a priori in science:

> [The] psychologistic character of Whewell's thought is brought out by considering further his view that, in some cases at least, induction allows the properly disciplined scientific mind to see a necessary connection between the facts and the general proposition in which they are included. (1968, p. 23).

Butts also sees this connection between consilience and this intuition of necessity:

> If . . . the general propositions (laws) support one another through what Whewell calls a 'consilience of inductions', and provide the basis for the deductive derivation of further truths, one can see intuitively that these general propositions (or the axioms from which they are derivable) are not only true, but necessarily true. (1968, p. 8)

That is to say, Whewell can be interpreted as setting out the following analysis: the sufficient conditions of the intuition of the necessary truth of a law are first, that the law consiliates other colligations; second, that the law predicts *qua* entails other facts or theories; and third, as we saw before, that the mind is prepared. So the mental act of intuition is tied to consilience--consilience and intuition are, respectively, logical and psychological counterparts.

This mental act can, moreover, be made to seem an essential or necessary condition for induction. As Whewell notes, "the mind must be properly disciplined in order that it may see the necessary connexion between the facts and the general proposition in which they are included. And the perception of this connexion, though treated as one step in our inductive inference, may imply many steps of demonstrative proof" (1858, p. 174). Thus, for Whewell, perception of, or inference to, the modal connection between states of affairs that a physical law colligates is part of the complete process of inductive inference. This fits with his idiosyncratic use of the notion of verification, for Whewell virtually always speaks as if a set of facts verify an inference, rather than an hypothesis. As he writes in Aphorism XXII, "The Inductive Table enables us to . . . determine whether each Induction is verified and justified by the facts" (1858, p. 161).

11. Interrelations of discovery and justification.

This account of consilience leads us to see the rich symmetry of Whewell's sketchy analysis of inductive formula. Once again, that formula is, "[S]everal Facts are exactly expressed as one Fact if, and only if, we adopt the Conception and the Assertion" (1858, p. 174). As we have seen, the conception referred to here is adequately represented as a set of magnitudes in some well-defined functional relation. An assertion, in Whewell's usage, can be rendered as a proposition and a definition. It seems that Whewell includes in the assertion, some ontological commitment to, for example, forces, which are the true causes of regularities among phenomena. Remembering our discussion of the lever law, we can see this in the simple example of an assertion that he offers:

> DEF.-- Two Motions are compounded when each produces its separate effect in a direction parallel to itself.
>
> PROP.--When any Force acts upon a body in motion, the motion which the Force would produce in the body at rest is compounded with the previous motion of the body. (1858, p. 172)

The definition is a refined clarification of the content of the colligating conception, while the proposition goes futher, discussing the true causes *qua* structure of connections between sensations captured in the colligation.

Since definitions do not precede but rather "flow along with" our intuitions of the relations among sensations, the explication of the conception by a definition is not independent of our intuition. We adopt an assertion when the definition and proposition that make a conception clear become forms of intuition. When a colligating idea is fully explicated, it has become a law of thought, the contradiction of which cannot any longer be conceived.

Thus, for Whewell, the necessary and sufficient condition of a complete induction is our psychological incorporation of the conception and assertion--and not some intrinsic relation between the facts mentioned on the left side of the biconditional. But also note that, for Whewell, the necessary and sufficient condition of the adoption is the presence of a completable induction.

> In these cases the proposition is, of course, established, and the definition realized, by enumeration of the facts. And in the case of inferences made in such a form, the Definition of the Conception and the Assertion of the Truth are both requisite and are correlative to one another. (1858, p. 173)

But just as importantly, says Whewell, "Each of the two steps contains the verification and justification of the other" (1858, p. 173).

Whewell is arguing that in the context of discovery we generate the colligating idea by attempts to organize data according to lawful relations. The facts are both a motor and a restriction for colligation. The non-passive mind of the researcher imposes upon selected aspects of the facts (that is to say, magnitudes) various functional relations. These are not arbitrary because they can fail to bring the facts together. But neither are they wholly determined by the facts--many formulae will fit the facts. Nevertheless, since these formulae *qua* fundamental ideas also both empower and limit the mind's ability to contemplate nature, there can be occasions when the researcher both comes to be in a state, and recognizes himself to be in a state, where there is no other way that he can reason clearly about nature. Thus, in the Whewellian picture, the creative process of discovery supports the intuition of necessity. And *vice versa*: the intuition of this necessity is warrant for the propriety of the colligating ideas.

The case of Euclidean geometry as knowledge of space is a simple example. Whewell claims with Kant that the ideas of Euclidean space are presupposed by our experience of space. But, once one has adopted these ideas, it is quite impossible to reason clearly about an alternative spatial structure; Euclidean geometry becomes necessary. For Whewell, to reason clearly about a science is not merely to derive theorems from arbitrary axioms according to some set of replacement or inference rules. Thus, the colligation and the intuition reinforce each other.

We can now sketch the theory of evidence recoverable from Whewell's psychologistic account of scientific research:

E is evidence for H if and only if

i) E characterizes a fact and H predicts or entails E;

ii) H colligates E with other facts; and

iii) H develops through explication, consilience,

> simplification and so on, to a point where it becomes a law of thought.

If these conditions are met, then E endorses an inference to H even if H is modal because an essential condition of a complete induction is embracing the relation in H in such a way that it becomes necessary. Some colligations are not consiliences, then, because the "jumping together" of facts that characterize a consilience is as much a dynamic Gestalt-like mental process as the definition of a functional relation between facts.

Does this analysis meet the practical constraints governing the concept of evidence? It has the resources to do so. First, it is intrinsically tied to acceptance. The interrelation of colligation and intuition includes an acceptance governing mechanism. Second, the confirmation or verification of an hypothesis is tied to non-arbitrary aspects of the fit of hypotheses and facts. Finally, it lends itself to comparative measures of plausibility among hypotheses in two ways. On the one hand, the logico-probabilistic research into colligation and consilience can be embraced. And on the other it can be augmented by a naturalism that does not, as Siegel mistakenly believes all naturalism must, "[abandon] philosophy of science's greatest task: that of contributing to our understanding of the *epistemological* standing of science" (1989, p. 365). Of course, as Carnap believed, this classificatory

Whewellian notion of evidence may be parasitic on a quantitative analysis. But it can still motivate and figure in the project of generating such an analysis.

12. Concluding remarks.

In Whewell we have a fascinating psychologistic philosophy of science. The logic that informs methodology is a study of human concept formation, inference, intuition and perception. It is not the study of the structure into which everything must ultimately fit. The necessity of the statements that these inferences yield is a function of the active human mind. Yet, this is not idealism because the human mind engages a realm of sensation over which it has no absolute control--sensations will not fit together in just any old way. It is, then, by playing off the active processes that create out of sensation an understandable world, and the intuitive process of representation that Whewell arrives at an empirically driven science that achieves a priori foundations. Whewell builds his methodology, then, on the very features of science that thinkers like Hanson and Kuhn later resurrect as criticisms of the logicism of our century. It is misleading, therefore, to present Whewell as an advocate or anticipator of the logico-propositional approach found in Carnap, Hempel and Braithwaite.

It is more charitable and interesting to read Whewell as generating a psychologistic structure-theoretic account of scientific research. He claims that scientific research both uncovers the structure into which all sensations must fit, and promotes the psychological instantiation of some model of that structure. Whewell is thus profoundly involved in the late nineteenth century's growing anti-mechanistic tradition. He is also, as we will see in Chapter V of this essay, the author of an account of philosophy of science that finds some expression in the work of James Clerk Maxwell. His views on the logic of science remain, however, separate from the structure-theoretic account of fundamental ideas. And in this, he is still distant from the twentieth-century philosophy of logic found in the likes of Husserl.

CHAPTER IV

INFERENCE AND EXPLANATION:

John Stuart Mill's Associationist Psychologism.

1. Explanation and psychologism.

On the surface there is very little that is new in Mill's *System of Logic*. The deductive relations captured by syllogisms are clearly expressed in works of philosophers from Aristotle to that of Whatley. The cannons of induction Mill articulates have precedents in the texts of thinkers such as Duns Scotus, Ockam and Grosseteste (*vide*. Losee 1980, Chapter 5). Mill's genius lies elsewhere: namely, in his attempt to reformulate what were in his time commonly known results of philosophical inquiry into logic, epistemology and philosophy of science, in terms of the associationist psychology that arose from Hobbes and dominated English thought. Mill's project, then, might be characterized as an attempt to naturalize these subjects. Moreover, his results can be read as an attempt to show what cognitive processes are actually occurring in those activities referred to by deduction, induction and their cognates. Indeed, he saw these terms as part of a vague, common, or even vulgar usage that is always misleading and often inconsistent with the claims of the psychological theory he embraced.

As an example of the misinterpretation of Mill that occurs when his psychological concerns are overlooked, consider how John Stuart Mill is commonly thought to have endorsed a covering law model of explanation. I argue in this chapter that attributing such a view to him becomes unattractive when proper attention is paid to Mill's psychologism. The general structure of my argument is straightforward: first, I point out some of the important features of the associationist psychology, to which Mill reduces many philosophical issues. I argue that he sees psychology and logic as studies of the same phenomena, namely, human inference. Finally, I explain Mill's claim that all genuine inference is from particulars to particulars, and argue that this undermines efforts to read Mill as embracing the patterns of either deduction or induction that are required for the covering law model of explanation. Thus, I argue that Mill puts the covering law model of explanation out of reach by denying the propriety of the resources necessary for an appropriate account the relation of "covering" requisite for this model.[1]

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[1] Indeed, it is possible to read this chapter as supporting Nancy Cartwright's recent characterization of Mill's philosophy of science as leading to the view that,

> [L]aws ... have no fundamental role to play in scientific theory. In particular, scientific explanation seems to proceed entirely without them.

I believe that we should read Mill as a cognitive scientist and I close this chapter with the contention that Mill's psychologism extends to a naturalistic theory of concepts, articulated in terms of associationist psychology. I sketch an account of evidence and explanation that is consistent with this psychologism of Mill's and point to the light this throws on the debate that exists between Mill and his Kantian adversary, Whewell.

2. Association of ideas.

Mill writes from deep within a British philosophical tradition that is quite unlike the one embraced by Mansel and Whewell. Nevertheless, Mill takes up the Victorian philosophical occupation with psychologistic philosophy. One clear model for J. S. Mill's philosophy of mind is Hobbesian. In discussing Hobbes we can speak of perception as raw experience. It includes the output of our senses and the content of our memory. The faculty of judgment takes the raw phantasms or ideas presented by perceptual mechanisms in experience or memory and performs two operations on them. First, the judgment identifies and records or registers the qualities presented in each phantasm. It takes note of and represents this content of

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They are the end point of explanation and not the source. (1989, p. 185) each idea in a manner allowing for subsequent cognitive activity. The second operation of the judgment is to associate pairs of phantasms according to their registered contents and their contexts. In doing this, the judgment associates phantasms by the order in which they are originally experienced; that is, spatially and temporally contiguous phantasms are associated. It relates what it recognizes as causes and effects. It relates phantasms to phantasms of the purposes to which their referents might be put. And finally, the judgment associates phantasms identified by the directly observable resemblances and differences between the contents of various phantasms. This operation creates the lattice of phantasms or ideas which allows for the trains of thoughts about which Hobbes so often speaks. That is, in guided mental discourse the imagination presents us with one particular idea after another by moving about on the lines of association recorded by the judgment.

Judgment, then, offers nothing new. It adds nothing. Moreover, the connections identified and recorded by the judgment and followed by a directed train of thoughts must always be experienced connections obtaining between pairs of phantasms. As Hobbes puts it, "we have no Transition from one Imagination to another, whereof we never had the like before in our Senses" (1651, p. 94). The judgment's operation is, therefore, utterly constrained by the nature of the material it takes from experience. For this reason, it is common to refer to these processes as passive (e.g., Mill 1843, p. 564).

According to Hobbes, the fancy and judgment are complementary cognitive faculties. The similarities identified by the fancy are not similarities open to direct sensation, for those resemblances are associated by the judgment. For example, the judgment associates phantasms of, say, red things. The fancy by contrast is active because it associates phantasms according to insensible or abstract resemblances. In a sense the fancy imposes associations on ideas that are not similar. For example, the fancy might associate the phantasms of a particular rose and a particular woman on the grounds that they are both beautiful. Thus, Hobbes says, "In Demonstration, in Councell, and in all rigourous search of Truth, Judgement does all; except sometimes the understanding have need to be opened by some apt similitude; and then there is so much use of Fancy" (1651, pp. 136-37). The similitudes identified by the fancy have heuristic value, but are not a proper part of reasoning.[2]

As modern associationism develops, the talk of faculties that identify similarities, like the talk of dormative virtues, is replaced by mere description of mechanical laws of association. The passive systems of the

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[2] For details see my 1989a.

human mind are said to merely record one idea or mental state and associate it with others that come before or after it. The relations of likeness and unlikeness between ideas are still exploited, but rather than posit a faculty responsible for identifying similarities, J. S. Mill, for example, takes them as identified automatically and inexplicably (1843, pp. 70-72).

This associationism is strongly tied to radical empiricism. Recall that, for Locke, empiricism is the antithesis of innatism. Knowledge refers to a special class of ideas, a set of ideas of accurate representations, and contra Descartes, none of these ideas is innate. All of our ideas arise from experience. Ideas that are not the direct result of sensation or reflection (that is, complex ideas) have to be explained as constructions formed by the mind through a process of associating simple ideas. Berkeley and Hume refine this view, but they do not substantially alter its fundamental feature; namely, the ultimate contents of human consciousness are taken to be nothing more than isolated, particular and entirely separate, or atomic, units of experience and various molecular associations or constructions created out of these atoms according to some set of laws that characterize the processes of association. Thus, British empiricism is closely allied to the doctrines of the associationist psychology. The former is unappealing without the latter because if in its knowledge gathering operations the mind are anything but passive, then the source of some of the content of our knowledge might be something other than experience. Indeed when philosophers such as Mansel come to recognize an active role for the mind, because of their empiricist leanings they work diligently to identify the additions made by the mind in order to be able to identify what is contributed by and attributable to the mind-independent world.

It is clear that Mill is acquainted with the classics of British empiricism, as well as other tracts on the associationist "mental philosophy", including the works of Brown, Reid, Condillac and Hartley. As he notes in the Autobiography (1873, pp. 43-44), however, these works were read in the main under his father's direction. It is in James Mill's Analysis of the Phenomena of the Human Mind (1829), then, that we may find the most direct antecedent to Mill's own views. James Mill's associationism is more purely mechanical than that proposed by any of Brown, Reid or Condillac. Any mention of a counterpart of the Hobbesian fancy virtually disappears. Like Hume, James Mill sees ideas as representations of impressions, and these two classes of items as the sole fundamental states of consciousness. He reduces all psychological processes to various applications of a single mechanical law describing the association of these states. All mental phenomena, he claims, are the product of the law of contiguity; that is, our ideas exist or come about in the order in which the impressions that they copy arose. Thus, James Mill views the mind as both purely passive and purely syntactic. The entire content of our consciousness is the determinate result of the order in which impressions happen to have occurred in us, and within mental life there is no role for concepts. Talk about concepts must be translated into talk about sequences of atomic impressions because for both Mills, as Alan Ryan puts it, "what we experience is, strictly, only discrete states of awareness of particular facts" (1974, p. 72). And Burston makes this point dramatically saying that for the Mills,

> We can say the Lord's Prayer forwards purely because we experienced it that way on many occasions, and not because, said forwards it has meaning, and said backwards it has none. (1973, p.174)

Where Brown and even Hartley leave some tiny, although perhaps incoherent, room for active creativity in the associating mind, James Mill's inexorable mechanical psychology leaves none. All of the contents of our consciousness must be, therefore, directly traceable to the very sensitive impressions from which our ideas arose. Given his system, any analysis of the content of science, _qua_ a class of ideas, must be an analysis of the mechanical psycho-logic by means of which the relevant ideas are formed. Our certainty concerning any of these ideas--because that certainty is itself an idea--can be analyzed only through reference to the law of association of ideas. Our certainty must reduce to a set of contiguous impressions because the idea that we are certain of any particular claim has as its only source the peculiarities of the order of impressions in response to which ideas have been associated and the process according to which that association occurs.

The younger Mill modifies the associationism he inherits with a mind to advocating the empiricism that is tied to associationism while simultaneously explaining t introspective evidence of complex ideas seemingly without clear atomic or sensory components.[3] To do this J. S. Mill enriches the purely mechanical model of association with a chemical model:

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[3] Here and below I will use the term "empiricism" to refer to the main tenets of J. S. Mill's epistemological views. To be accurate, however, one must note that Mill referred to himself as an experimentalist in order to distance himself from those who, like Macaulay, claimed the name of empiricism for their unsystematic attempts to draw conclusions regarding the value of traditions and institutions from the historical dominance or catastrophic failures of these these traditions and institutions.

> [T]he laws of the phenomena of mind are sometimes analogous to mechanical, but sometimes also chemical laws. When many impressions or ideas are operating in the mind together, there sometimes takes place a process of a similar kind to chemical combination . . . [where] ideas melt and coalesce into one another, and appear not as several ideas. (1843, p. 853)

Here J. S. Mill sets up at the mental level of associative processes a distinction analogous to the one he sees between mechanical and chemical causal processes. His contention is that some ideas may be formed from sensory components that, like component forces, retain their individuality when composed. For example, the idea of a red patch is easily resolved into the components of colour and shape. Other sensory components, however, like chemical reagents, may suffer transformations that make them difficult if not impossible to recognize in the compound produced. The resolution of, say, the idea of force may not be at all straightforward.

Nevertheless, Mill's chemical model of the association of ideas does not include the Kantian's claim that the mind imposes some necessary and indelible change upon the raw contents of experience. Indeed, Mansel uses the chemical analogy to his own distinct ends saying:

> To speak of our perception as mere modifications of

> mind produced by an unknown cause, would be like maintaining that the acid is modified by the influence of the alkali without entering into combination with it. (1866, p. 72)

That is, the mind combines with its raw materials, lending them form and taking them up as content. For example, Mansel insists that our experience must be framed within space and time. These modes of presentation are neither reducible to the sensory components of experienced ideas, nor are these sensory components open to experience outside of being presented within these modes.

Mill by contrast sees the ideas when combined as substantially the same as their genetic predecessors. As Fred Wilson (1990) makes clear, J. S. Mill's chemical model of association generates a complex account of association. But the fundamental passivity of the Hobbesian judgment is still evident. Wilson (1990, pp. 107-18) shows that Mill rejects the view that the analysis of a complex or molecular idea will always immediately yield the atomic ideas or sensory components actually associated in its genesis. According to Mill, the analysis of an molecular idea may instead yield metaphysical parts that seem quite distant from the sensory components that played a role in the production of the molecular idea. Hence, it is only by retracing the process of association that one can arrive at the sensory ground of some complex ideas. Nevertheless, while the sensory components of a complex idea are not in the idea, they remain connected to that idea by the laws of association. For example, consider the molecular idea of an apple. Mill's point is that analysis of this idea yields ideas like skin, core, seed, and so on. Now, these are not the sensory components of the idea of an apple and in some cases it is very difficult to imagine what the fundamental sensory components of these metaphysical parts are, but eventually we come to things like shape, colour and texture.[4]

The relation between the metaphysical parts and the sensory components is one of associative law, hence as Wilson points out, "Mill gives up the notion that the analysis undertaken by the introspective psychologist is anything like the analysis of a concept" (1990, p. 101). The relation between an idea and its analysis in terms of sensory components is not, as Hobbes or Locke would have it, a matter of definition. Rather, since the idea can resist analysis into its sensory components because it has

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[4] I use these fundamental components as merely suggestive. Much of the battle between various mind scientists in the later nineteenth century focuses on the exact identification of these fundamental components. As I mentioned in Chapter I, in 1896 Wundt's follower Titchner published a list of more that 44,000 of these elementary constituents arrived at by introspective analysis.

developed through a "chemical" association, such analysis depends on the identification of the relevant associative laws. Furthermore, since those laws must be discovered empirically, the idea and its analysis in terms of sensory components are related contingently. The analysis of ideas and, hence, concepts and sentences, relies on an analytic but a posteriori relation, and not as in Whewell one that is synthetic and a priori.

3. Inference and association.

For our purposes here, the central claim arising from Mill's associationist psychology is that all genuine inference is from particulars to particulars I will argue that Mill's psychologism regarding logic is not so much one in which he sees the laws of logic as reducible to laws of psychology, but rather it is a claim that makes the study of logic and the study of psychology continuous. If the subject matter of logic is a fragment of the subject matter of psychology, then, since Mill takes all inference to be from particular to particular, all logic is fundamentally the study of inferences from particular to particular. This position culminates in Mill's view that, as commonly described, neither induction nor deduction is a genuine form of human inference. Hence, neither can be a proper basis for a robust concept of explanation. If this is indeed Mill's position, then a model of explanation that utilizes either inductive or deductive inferences per se between claims must be, for Mill, defective.

Mill is like other Victorian psychologists such as Mansel and Whewell in holding that logic is the study of human inference. Mill naturalizes logic, characterizing it as a study of psychological processes. Psychology and logic are, for Mill, continuous research enterprises with identical epistemological foundations and subject-matters. My initial claim, then, is that in his System of Logic, Mill does not separate logical issues from the psychological concerns of his associationist view. Rather, what may seem to be a distinction between logic and psychology is only an attempt to insulate his views on logic from revisions in, or the defeat of, details within associationist psychology.

In the opening pages of that book he sets out a tentative definition of logic. "Logic", says Mill, "points out what relations must subsist between data and whatever can be concluded from them" (1843, p. 10). But these relations are identified in terms of inference processes:

> Logic must be restricted to that portion of our knowledge which consists of inferences from truths previously known, whether those antecedent data be general propositions or particular observations or perceptions. (1843, p. 9)

Here Mill is pointing to the inferential processes of the mind as the domain of logic. Perceptions are not propositions in the sense of Frege's _Gedanke_ and therefore cannot have entailments. Only a process of mental inference can derive ideas from perceptions. Logic, then, is the science of inference, not the study of entailment. And inference is not a technical term separate by fiat from psychological processes such as associating and perceiving. Like von Helmholtz (1868), Mill argues--against the Kantian and Scottish conceptualists--that spatial perception is the result of inference, concluding that: "The perception of distance by the eye, which seems so like intuition, is thus, in reality, an inference grounded on experience" (1843, p. 8).

For Mill, then, the subject-matter of the _Logic_ is continuous with the subject-matter of psychology. His object is,

> to attempt a correct analysis of the intellectual processes called Reasoning or Inference and of such mental operations as are intended to facilitate this: as well as, on the foundation of this analysis, and _pari passu_ with it, to bring together or frame a set of rules or canons for testing the sufficiency of any given evidence to prove any given proposition. (1843, p. 12)

The rules or canons of inference are to be developed in terms of and in tandem with our understanding of mental operations. As he later puts that:

> So far as [logic] is a science at all, it is part, or branch, of Psychology; differing from it, on the one hand, as a part differs from the whole, and on the other, as an Art differs from a science. Its theoretical grounds are wholly borrowed from Psychology, and include as much of that science as is required to justify the rules of the art. (1865, p. 359)

Mill's wish to distance the rules he develops from the basic theories of empirical and analytic psychology could be mistaken for a separation in kind between logic and psychology.[5] The conclusions of the System, however, merely describe a level of mental operation more complex than that of the fundamental laws of psychology. Thus, regardless of the fundamental model of mind that is finally accepted in psychology, Mill feels that his logic will be unaffected. "Most of the conclusions of this work", he claims,

> have no necessary connexion with any particular views respecting the ulterior analysis. Logic is common ground on which the partisans of Hartley and

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[5] Oddly, von Helmholtz and others make just the opposite slip with Kant's first Critique. There, the avowed distance between Kant's subject-matter and empirical psychology is overlooked.

> of Reid, of Locke and of Kant may meet and join hands. (1843, p. 8)

I am making one main claim then: I am arguing that Mill is essentially a descriptivist. Let me explain that in detail. I understand the term descriptivist to refer to those who deny that the principles of logic are fundamentally prescriptive or formal. In Chapter III of his Metaphysics, Aristotle is the model descriptivist. He holds that logic describes the structure of being, and can be used as a standard of correct reasoning only in a secondary way; viz. the categories of being generate the fabric in which consistency and inconsistency emerge (vide, W. & M. Kneale 1962, pp. 25ff). Mill's psychologism makes him a descriptivist in that it requires that the principles of logic are fundamentally descriptions of psychological processes and can only in a secondary sense be used as prescriptions for correct reasoning. First let us see how psychology is fundamental to logic and then discuss how logic can be prescriptive.

Mill often utilizes a distinction of levels of study within the same subject-matter and this underwrites our reading his logic as a "higher-level" psychology. He notes that in even the most advanced science the fundamental subject-matter can be entirely beyond human understanding while its "higher-level" claims are both understood and supported with evidence. The fundamentals or first principles of a science, says Mill, are usually neither the source of the laws in the sense of being axioms that deductively entail higher-level results, nor the evidence for higher-level laws. In Utilitarianism, he makes this point nicely--and in a way that will fit well with our later discussion of explanation:

> [C]onfusion and uncertainty, and in some cases similar discourse, exist respecting the first principles of all the sciences, not excepting that which is deemed most certain of them, mathematics; without much impairing, generally indeed without impairing at all, the trustworthiness of the conclusions of those sciences. An apparent anomaly, the explanation of which is, that the detailed doctrines of those sciences are not usually deduced from, nor dependent for their evidence upon, what are called its first principles (1861, p. 205)

Mill is not claiming that one's conclusions from first principles can be certain while those first principles are not, for he denies that such doctrines are deduced from first principles. Rather, his point is that the inference to detailed doctrines or higher-level laws is of the same form as that which leads to first principles. Thus, logic as a set of detailed doctrines about inference can exist as a separate science while describing the same phenomena as psychology studies at a more fundamental level. Mill's view of logic, therefore, does not and need not appeal to a post-Husserlian notion of entailment in place of mental operations. Logic is rather an account at a non-fundamental level of mental activity. The upshot of this is that one does not need to know how to reduce one study to another in order to claim that they are both conclusions concerning matters organized according to the same first principles. For example, Mill charges some logicians with the fault of failing to reconcile their logical theories and parts of mind science, but he does not require that one explicitly show how the first principles of psychology deductively entail a theory of concepts, judgment and syllogism.

This reading is supported in that it can make sense of passages in Mill's later An Examination of Sir William Hamilton's Philosophy, as well. His distinction there, between logic and psychology, is still not Husserl's or Frege's. After a number of chapters rehearsing the associationist view, Mill marks a change in subject-matter in a manner that is easily misinterpreted:

> We now arrive at the questions which form the transition from psychology to logic--from the analysis and laws of the mental operations to the theory of the ascertainment of objective truth--the natural link between the two being the theory of

> the particular mental operations whereby truth is ascertained or authenticated. (1865, p. 301)

Psychology is, as one might expect, identified with the analysis of mental operation conducted at the level of laws of association, and logic is said to be the theory of the ascertainment of truth. But, logic is taken to depend on a "theory of the _particular_ mental operations whereby truth is ascertained". Further, if logic is not continuous with fundamental psychology then it seems odd that in what follows this quotation Mill generates an attack on the view that "we think by concepts". Even more surprising that Mill then pursues a _reductio_ of this conceptualist logic of the Kantians by presenting, "a great _psychological_ difficulty to be got over, to which _logicians_ of the conceptualist school have been surprisingly blind" (1865, p. 399; my emphasis); namely, that concepts as defined by Mansel and Hamilton cannot be represented in either thought or imagination (1865, pp. 307-09).

All this can be made to seem straightforward if we take Mill to mean that logic is the systematic study of mental operations at a more general level than that of the associationist laws. That is to say, according to Mill, logicians must at times address deeper psychological issues; for example, logicians cannot, as Mansel does, give full support to a theory of reasoning and judgment based on a notion of concept that is psychologically implausible.

The prescriptive use of the results of a descriptive science does not undermine its status as a descriptive science. But there is a tendency to read Mill as generating two incompatible stories concerning the laws of logic because, as Britton puts it,

> This proposal [of Mill's] for the complete psychologizing of logic must surely end by abandoning the claim that logic settles questions of validity of arguments. For in what way can investigation into our habits of belief have any bearing on their truth or falsity? (1955, p. 143)

In other words, descriptions of psychological processes can have nothing to say about prescriptions concerning logical relations, hence Mill develops independent descriptive and prescriptive accounts. As I understand it, the contention is that Mill only sometimes attempts to identify the laws of logic as inductions concerning the inference patterns of a human mind. On other occasions he instead takes those laws to be inductions on the structure of our experience. Hence, Britton contends that,

> These principles are binding because they are inductions resting upon our past experience; and because that experience is all-pervasive. The test of logical principles lies, not in their inconceivableness, but in the uniformity of experience. (1955, p. 143)

Here, the first thing to note is that psychologism is not essentially tied to the doctrine that the test of logical principles is inconceivability. The continuity of logic and psychology can accommodate the alteration or development of human psychological nature. Secondly, one should understand that even if this distinction between inductions concerning psychology and inductions concerning experience has any credibility at all, it still leaves Mill a descriptivist. Fundamentally, the principles describe the structure of experience, and hence their prescriptive use is only secondary and based upon the claim that logical properties supervene on the structural properties of experience. But more interestingly, the distinction appealed to by Britton overlooks the (passive) activity of the mind in creating experience. We experience according to our psychological processes, and it is a common methodology of nineteenth-century philosophers--attacked vigorously by Comte--to introspect on the content of experience in order to identify the nature of the psychological processes involved in its generation. Hence, psychology and logic have the same subject-matter; they are continuous studies. To say that Mill identified the laws of logic with the mind-independent component of the structure of _mental_ experience is, I believe, an understandable but illicit imposition of the Husserlian and post-Husserlian views outlined in Chapter II of this essay.

Of course this does not rule out the descriptive use of the laws of logic. The use of "ought" in some passages can seem to put pressure on a reading of Mill as descriptive in his psychologism. Indeed, Mill can seem, at times, prescriptivist. Nevertheless, Mill claims that the fruit of his philosophical analysis is, "to enable the mind to command, instead of being commanded by, the laws of the merely passive part of its own nature" (1843, p. 564). We can read this as asserting that the canons of inductive inference, for example, more than outlining how we ought to think, are tools of the thinking mind, hence, part of the description of the mental operations of induction. Logic merely concentrates on the descriptions that will conform to its practical ends. Referring to psychology as "the larger science", he says:

> The extension of Logic as a Science is determined by its necessities as an Art: whatever it does not need for its practical ends, it leaves to the larger science. (1843, pp. 13-14)

The situation is, therefore, analogous to that in contemporary perceptual psychology. Although our intuitions do not lead us to refer to the latest theories of perception as prescriptive, those theories have practical value in preventing us from falling victim to certain perceptual errors or illusions.

Specifically, Mill sees the problem of induction as understanding why it is that we do generalize on only one instance in some cases, and yet in other cases "myriads of concurring instances, without a single exception known or presumed, go such a little way towards establishing an universal proposition" (1843, p. 314). In so far as logic accurately charts our higher-level associative processes, it describes how we make inferences. An analysis of the sensory components of any idea of cause shows us the canons of induction are accurate in identifying that relation. But, all sorts of other associations are taking place. As Hobbes notes, we associate teleologically and analogically. Use of these other inference procedures can generate errors if it wanders into our attempts to identify causal relations. The canons of induction, then, while not primarily rules stating how we ought to think, can be used as guides to help us in our associations of certain particulars into cause and effect relations and in the elimination of faulty inductions. We might see it like this: we unconsciously make causal associations or inductions in just this canonical way, but we are not very good at it in some areas and in these areas the canons can be a crutch or template. By being conscious of the inputs and outputs of our associative processes, we can improve their accuracy by restricting the contexts in which they operate.

To look ahead to Mill's views on inference and explanation then, the laws of logic are a set of descriptive general statements that are short-form records of the characteristics of some of our actual inferences. As such they do not explain our inferences, but rather are explained by them.

4. Inference from particular to particular.

Passmore points out that Mill gives three interpretations of propositions, which he takes to be interchangeable:

> These three interpretations of the proposition, according to Mill, are equivalent; he therefore feels himself free to make use of whichever one of them suits his particular purpose at a given point in his argument. (1957, p. 18)

Passmore sums up these three interpretations as follows:

> The primary import of "all men are mortal", according to Mill, is that the 'attributes of man are always accompanied by the attribute mortality'. Since, however, every attribute is 'grounded' in phenomena, the ultimate, metaphysical, significance of a proposition, he considers, is that certain phenomena--certain 'experiences'--are regularly associated one with another. On the other side, the scientific function of a proposition, as distinct from its metaphysical analysis, is, Mill says, to tell us what to expect in certain

circumstances. (1957, p. 18)

Our discussion so far conforms to this summary. At the level of analysis of molecular ideas into sensory components we have what Passmore calls the "ultimate, metaphysical significance". This is the account that associationist psychology per se yields. At the level of logic we have the acceptance of complex ideas analyzed in terms of what Wilson calls their metaphysical parts, and this coincides with what Passmore refers to as "the primary import" of a proposition; viz. we accept or judge that certain attributes are always accompanied by other attributes. Our discussion shows why Mill thinks these formulations are equivalent. What remains to discuss is the scientific function of propositions, showing that it too is continuous with or equivalent to the psychological and epistemological accounts. That is, we must attempt to understand the interchangeablility that arises through Mill's naturalized notions of induction, deduction, evidence and explanation in terms that are consistent with his associationist psychologism.

Mill is sure that the relations identified in the System of Logic are immune from revision not because they are separate from psychology but because they can operate at the level of language. Thus, some of the data of logic are public and therefore logic need not be restricted only to appeals to introspective results.[6] In virtually all cases, interesting scientific inference is represented as movement from linguistically characterized facts or particular propositions to general propositions or other particular propositions. Thus, Mill's philosophy of science can be tied to the associationist psychology through his analysis of language. Excepting syncategoramatic particles, all words are names or parts of names. Thus, for Mill, every sentence is merely a complex of names. Words are not names of ideas, however, but names of things. Names denote objects, but Mill's sensationalism analyzes an object as a permanent possibility of sensation. Names also connote. They are associated with the phenomenal attributes or sensory components ascribed to those permanent possibilities of sensation. Names, propositions and beliefs can be reduced to the complex mental states referred to in both connotation and denotation (1843, pp. 277f). Indeed Mill criticizes Hamilton for refusing to analyze propositions or beliefs in terms of psychological states (1865, p. 373). Hence, for Mill, language becomes a system of names for denoting potential groups, and so, associations of sensory

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[6] There is some similarity here with the dynamistic tradition in theorizing discussed in Chapter III. But I think it would be going too far to attribute to Mill any conscious membership in that tradition.

components.

Propositions corresponding to the cognitive significance of any sentence, then, have two descriptions: first, they are complex mental states that record some association of the sensations denoted by their constituent names or terms of the sentence; and second, as Passmore says, they can be inference tickets according to which one can be led to expectations about the occurrence of one sensation upon the occurrence of another. This dual analysis of propositions marks, in Mill, the direct connection of the associationist psychology and his theory of scientific inference.

Given an alternate theory of language we might wish to separate the study of the relations between propositions from that of mental operations; that is, we might consider investigating the fixed non-psychological relations between the unchanging contents of various concepts. Or, we might see in the grammar of natural language a foundation for logic.[7] But, for Mill, the logic of the relations between

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[7] Passmore (1957, p. 20) claims that Mill himself slips into this approach in his review of Groate's Aristotle. However, there Mill claims only that while certain axioms are immediately founded on the meanings of words, they are ultimately founded on patterns mental activity: "This identity of the mental operations constitutes the very meaning of the words in which the axioms are expressed" (Mill

propositions has deep ties to this associationist semantics. Logic will unpack the relation between complexes of sensations in memory, imagination or experience. Scientific inference as a means of generating new beliefs is a subset of logic proper. Hence, for Mill, logic and scientific inference, properly speaking, do not properly include either deduction or induction because beneath every seeming inference from a general proposition to a particular instance or vice versa is a real inference from a particular association of sensory components to another particular association of sensory components. Let me explain.

Mill merely gives deference to common usage in allowing for the distinction of induction and deduction:

> Although, therefore, all processes of thought in which the ultimate premises are particulars, whether we conclude from particulars to a general formula, or from particulars to other particulars according to that formula, are equally induction, we shall yet, conformably to usage, consider the name induction as more peculiarly belonging to the process of establishing the general proposition, and the remaining operation, which is substantially interpreting the general proposition, we shall call

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1873a, p. 499, my emphasis). Hence, Passmore's charge seems empty.

> by its usual name, deduction. (1843, p. 203)

This universal scope of inference from particular to particular over all logical mental operations arises because the associationist account of mind forces us to see all mental operations in terms of the association of particulars.

That logic and psychology are continuous areas of research implies that the properties discovered in psychology will emerge in logic. Mill follows Berkeley and James Mill in rejecting Locke's account of abstract ideas. In his notes on James Mill's _Analysis of the Human Mind_, the younger Mill contends that, "All our ideas are of individuals, or of numbers of individuals" (1869, p. 144). This psychological commitment pervades the discussion of deduction in the _Logic_ and leads Mill to this conclusion:

> All inference is from particulars to particulars: General propositions are merely registers of such inferences already made, and short formulae for making more: the major premise of a syllogism, consequently, is a formula of this description; and the conclusion is not an inference drawn _from_ the formulae, but according to the formula; the real logical antecedent or premise being the particular facts from which the general proposition was collected by induction. (1843 p. 193)

According to Mill's philosophy of language, then, general propositions can be equivalent to nothing but collections of particulars:

> [I]n all cases, the general propositions, whether called definitions, axioms, or laws of nature . . . are merely abridged statements, in a kind of shorthand, of the particular facts, which, as the occasion arises, we either think we may proceed on as proved, or intend to assume. (1843, p. 192)

Mill is careful to segregate the mere manipulation of connotations away from inference. In Britton's words, "he refused the name of inference to any form of pure deduction" (1953, p. 122). Verbal transformations using definitional equivalences or "verbal propositions" are only "apparent inferences" and hence, outside the scope of logic. Hence, all inference uses only particulars in the movement to and from only particulars. Which is to say, all logical operations are only processes by which we come to associate particulars. If movement from the particular to the general and from the general to the particular are forms of reasoning, then inference from particular to particular is the "third species of reasoning" that Mill claims as "the foundation of both the others" (1843, p. 162).

5. The covering law model of explanation.

This carries us to the ubiquitous claim that J. S. Mill's theory of explanation is identical to that now referred to as the covering law model. Wilson's claim is indicative of many: "John Stuart Mill holds that the concept of scientific explanation can be explicated in terms of what has come to be called the 'covering law model' of explanation" (1990, p. 84). I will argue that Mill's claim that the ultimate nature of all inference is from particular to particular, undermines this attribution. Moreover a sensitivity to this effect of his psychologism makes sense of the disparaging comments he makes concerning the covering law model of explanation.

For our purposes, the covering law model of explanation can be can be sketched like this: H explains E just in case: 1) H and E constitute a sound deductive or inductive argument; 2) E is the conclusion of that argument; 3) H is a premise or set of premises that contain at least one natural law; and 4) that law is needed to carry through the derivation of the conclusion. Bluntly put, H explains E when H is a law and E can be deduced from H.

The standard quotation used to attribute this view to Mill is his claim that, "An individual fact is said to be explained by pointing out its cause, that is, by stating the law or laws of causation of which its production is an instance" (1843, p. 464). He also goes on to talk about the explanation of laws saying,

[A] law, or uniformity of nature is said to be

> explained, when another law or laws are pointed out, of which that law is but a case, and from which it could be deduced. (ibid)

I suggest that these passages should be taken as places where Mill is deferring to common usage concerning deduction, and that the careful reader must translate this comment into the particular-to-particular vocabulary to which all talk of deduction is reducible. As Mill says,

> An induction from particulars to generals, followed by a syllogism process from those generals to particulars, is a form in which we may always state our reasoning if we please. (1843, p. 198)

That is, the terms induction and deduction can be used as a means of expressing or talking about our actual inferences, but, as we have seen, those actual inferences are from particular to particular.

This oblique reading has the advantage of underlining the pains Mill takes to point out that when he speaks of deductive explanation, he is discussing a peculiar and unnatural notion of explanation:

> The word explanation is here used in its philosophical sense. What is called explaining one law of nature by another, is but substituting one mystery for another. (1843, p. 471)

Thus, deductive-nomological explanation is not in any normal sense explanatory--and this is in itself a good reason to expect the abounding counter-examples to the model. Its genesis as a notion of explanation is only that deductive economy is one criterion for a <u>system</u> of laws that subsume the causal structure of the natural world: one way to select among competing systems of laws that deductively entail the regularities of the world is to favour those that use "the fewest general principles from which all the uniformities existing in nature could be deduced" (1843, p. 472).

Here, Mill is making the observation that within science the notions of simplicity and unification of laws have already by the mid-1800's become muddled with an account of explanation. Thus, if the natural meaning of the term "scientific explanation" is to be given up, its unnatural meaning should be recognized as identifying the determining factors of an analysis of this notion. Mill's clear claim is that the covering-law model is acceptable only because it conduces to unification. In this, he anticipates Hempel's admission that the covering law model seems to have little to do with human understanding, and rather that,

> scientific explanation, especially theoretical explanation, aims at . . . not intuitive and highly subjective kind of understanding, but an objective kind of insight that is achieved by a systematic unification, by exhibiting the phenomena as

> manifestations of common underlying structures and processes. (1966, p. 83)

That is, as Mill characterizes it:

> The laws, thus explained or resolved, are sometimes said to be accounted for; but the expression is incorrect, if taken to mean anything more than what has already been stated. (1843, p. 472)

For Mill, the natural notion of explanation or of accounting for an event is not to show that it is an instance of a general law, but rather to show immediately that it is the effect of some cause (1843, p. 472)--and that explanation must be a *vera causa*. Hence the inductive canons, because they can identify particular causes and their particular effects are the true schemata of explanation.

Nevertheless, John Losee (1983) presents a seemingly compelling argument that Mill is indeed a deductive-nomological theorist and in order to support my reading I will endeavour to undermine his argument in detail. One can represent Losee's argument as follows: Mill presents two mutually irreducible analyses of verification, one, "an ideal based on a deductive model of explanation" (1983, p. 123), the other based on the "satisfaction of the Method of Difference" (ibid.); hence Mill embraces a deductive view of explanation. Moreover, Mill, it is said, is involved in an non-psychologistic project because his view of evidence and explanation is distinct from his psychologistic canonical analysis.

Losee's argument stands or falls on the claim that Mill's two characterizations of verification cannot be made to seem equivalent. Losee presents schemata for the "two distinct concepts of verification" (1983, p. 123) in Mill's Logic. In the first case, where L_1, L_2, ... , L_n are putative laws and p is a correlation, p is evidence for L_1 just in case the following deductive schema (1983, p. 119) is satisfied:

(1) L_1 explains p and p is true.

(2) L_1, L_2, ... L_n are the only possible p-explainers.

(3) Neither L_2 nor L_3 nor ... L_n explains p.

(4) If {(1) and (2) and (3)} then L_1 is true.

Therefore, L_1 is true.

Mill also presents verification in terms of the satisfaction of the following schema for the method of difference (ibid., p. 122):

Instance	Circumstances	Phenomena
1	A B C	a b c
2	B C	b c

Therefore A is a necessary condition of a.

Let us see why one might claim that these schema are distinct.

The first three premises of the deductive model are easily read as one condition; namely, "Only L_1 explains p". That is, in every model M, p is satisfied in M only if L_1 is satisfied in M. Associate the explanadum p of the deductive schema with a of the difference schema, and use A to refer to the causal law L_1. Now, Mill is seen noting cases where, as Losee puts it, A is a necessary condition for a. That is, in every model M, A is satisfied in M only if a is satisfied in M. Thus, in the deductive model the correlation is a necessary condition for the law, whereas in the difference schema the law is a necessary condition for the correlation. Put schematically, the deductive model yields P --> Q, and the difference model yields Q --> P.

This result is tempting, but Mill can be read more charitably so that he achieves to some extent the reduction he claims. Losee's deductive schema is on the face of it invalid due to its incomplete content. It requires an additional premise; namely, that every correlation has an law-like explainer. Moreover, the identification of the connective "explains" and any straightforward connective for entailment is not apparent. For example, if "explains" is replaced by material implication, because the law is said to imply the fact explained, the schema is trivially valid on purely structural grounds because the premises will contain a contradiction. (3) will become:

(3′) $\sim[(L_2 \vee L_3 \vee \ldots \vee L_n) \rightarrow p]$

which simplifies to the negation of the consequent,

(3″) $\sim p$

and is in contradiction with the final conjunct in (1), yielding,

(5) $p \,\&\, \sim p$

Such a schematization of Mill's view is, therefore, certainly uncharitable.[8]

Notice, however, if we replace "explains" with some notion of equivalence (including even the material biconditional), we achieve three agreements with Mill's text. First, the schema presented by Losee becomes valid.

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[8] One might argue that rendering "L explains p" as "p materially implies L" avoids the problems mentioned here. This response, however, makes the deductive and difference schemas identical, contra Losee. Moreover, this seems unattractive in that L will not explain p only in a world where p is true and L is false. But p might be the possible outcome of two separate processes described by L and L1, respectively; i.e., L does not explain p and both p and L are true. This merely points out the need for modally augmented notions of implication in some models explanation, but my point is that something as weak as material equivalence can make sense of Mill's text and notion of explanation.

Second, Mill's comments to the effect that a correlation is explained when it can be deduced from more general laws are allowed. And most importantly, third, this notion of deductive subsumption makes the deductive schemata reducible to its inductive counterpart, consistent with Mill's claim that the hypothetical method is equivalent to induction according to the canons.

The first two points are obvious, but the third requires some explication. The use of equivalence in place of "explains" makes the import of the completed hypothetical method the statement "L_1 if and only if p". The difference schema now must be rewritten so as to incorporate Mill's logical, but not temporal, indifference to the location of cause and effect. Since we can place any sensational complex in either column in the difference schema, we can conclude from that schema not only that A is a necessary condition of a, but, by switching the contents of the two columns, that a is a necessary condition of A; i.e., A if and only if a. Using our previous substitutions, then, the two schemata are logically equivalent. Thus, the two evidential relations to which Mill alludes in the hypothetical method and the method of difference can be made to seem identical.

These results are the details arising from Mill's view that all reasoning is from particulars to particulars. That is to say, they are implications of Mill's anticonceptualism. If all inference is inductive, then the deductive explanatory schema is only shorthand for a perhaps more involved inductive story. A general proposition does not entail all its instantiations, but rather it is equivalent to them. It is a name, record or place-holder for a class of associations of particulars. To find the two schemata irreducible, therefore, is to embrace a defective reading of Mill's work. Mill claims that we do not deduce conclusions from general propositions, but rather that we infer according to them. Not to put too fine a point on it, for Mill, a general proposition H really explains E only if H is a shorthand notation for a set of particular ideas and there is some correct inductive inference possible from each of those particular ideas to E. Moreover, if there is such an inference, then a cause and effect have been isolated. Saying that Mill gives voice to a concept of explanation defined in terms of deduction, then, is not sufficient warrant for the identification of his view of explanation and that expressed in the deductive-nomological model.

6. Knowledge and concepts.

Mill's naturalism is not restricted to his discussion of explanation. It is pervasive. In closing this chapter I will sketch a few other of the more interesting aspects of his fundamental commitment to psychologistic naturalism and point toward their connections with what must be Mill's true views on explanation and evidence.

Mill says that "Logic . . . is not the same thing with knowledge" (1843, p. 9) because logic cannot tell us about the truth of the inputs to our inference procedures; but, he can be interpreted as claiming that logic and epistemology are continuous. He makes use of the result that logic and psychology are continuous to extend the naturalist continuum to include epistemology. "Logic takes cogniz- ce of our intellectual operations, only as they conduce to our knowledge" (1843, p. 6). His psychologism naturalizes epistemology through an identification of the criteria for the justification for beliefs and the mechanisms by which beliefs arise.

Mill sees logic as a general description of the particular mental processes by which beliefs are inferred from other mental states. He claims that logic concerns itself with "both the process itself of advancing from known truths to unknown, and all other intellectual operations in so far as auxiliary to this" (1843, p. 12). He distinguishes between real inference and merely verbal transformations using the criterion that inference yields new knowledge, while merely verbal transformations do not (1843, p. 160). Mill is careful to say that the mental states accessible to these mechanisms of inference include non-belief states: for example, perceptual awareness states and memory states (1843, p. 9). In this, Mill quite radically rejects the doxastic assumption governing much of 20th century epistemology. The dominance of post-Husserlian notions of entailment requires that beliefs be described as propositional and, hence, that beliefs be logically related to other beliefs only. There can be no logical relationships between perceptions and beliefs. Mill's naturalistic logic and epistemological theory allow that the justification of beliefs can be a function of states that include things other than beliefs. Logic for Mill is, therefore, much like the epistemological project of describing what contemporary epistemologists have called doxastic practices (Alston, 1990), j-rules (Goldman, 1990) and epistemic norms (Pollock, 1986). Let me use Pollock's notion of epistemic norms to clarify this claim.

According to Pollock, a subject's belief is justified if and only if the subject's inference to that belief is licenced by correct epistemic norms. Epistemic norms are condition-action rules that describe a procedure we attempt to follow when generating beliefs: "They describe an internalized pattern of behaviour that we automatically follow in reasoning" (Pollock 1986, p. 131). They are condition-action rules in the sense that they describe cognitive processes by which, when certain sensations or memory states obtain, a subject is caused to generate and accept a belief. They are rules in that upon reflection we choose not to undermine the beliefs so caused. For example, there might be a number of sensory components that when perceived lead to the action of forming the belief that there is an apple immediately in front of the subject. The existence and nature of these rules or mechanisms that join beliefs and the reasons on which they are based are contingent facts about human cognition.

This description of epistemic norms is quite compatible with the details of Mill's account of associationism. We notice that associationist psychology discusses the association of sensory components at the level of particular combinations, while logic adds another dimension to these associations: it discusses the generation of molecular ideas under the description of accepted beliefs and as such attempts to identify which processes of association ought to have their outputs accepted. Logic as a general description of the psychological mechanisms by which beliefs arise and are accepted, is a general theory of epistemic norms. The associations that Wilson argues connect complex ideas and their sensory components are just the contingent facts of human cognition to which epistemic norms refer. Hence, logic as a description of epistemic norms consists of epistemological theses. Furthermore, in so far as logic is naturalized in Mill's psychologism, so too is epistemology. Psychology, logic and epistemology are continuous studies.

Mill extends the naturalist project in the Logic to include a theory of concepts, and this theory serves as the foundation of his claims about all inference being from particulars to particulars. Mill is quite explicit in his account of concepts. General names refer not to abstract ideas, nor abstract entities, and neither can concepts be reduced to mere inter-definitions among names.[9] Rather, concepts are attributes consciously held in the consideration of concrete objects. As for Berkeley, concepts are, in Mill's system, merely the restricted attention of the thinker to only some of the aspects of a concrete idea--and these restricted attributes are prior to classes. As Mill says:

> [C]oncrete objects are only known by attributes, are only distinguished by attributes, and . . . the concrete names by which we speak of them mean nothing but attributes, or "bundles of attributes." Our representation in thought of a concrete object is but a representation of attributes, and our concept of a class of concrete objects is but a certain portic ı of those attributes, not, indeed,

[9] With this, then, Mill rejects the view of concepts that dominates early twentieth century philosophy--namely that a concept can only be understood in terms of its truth-conditions as defined in terms of its logical relations to other concepts.

> separately conceived or imaged, but exclusively attended to. There is, therefore, nothing in our mind when we affirm a general proposition, but attributes, and their coexistence or repugnance. (1865, p. 389)

Here then is the reason why all so-called deduction is mentally instantiated as inference from particulars to particulars. There is for Mill "nothing in the mind" that corresponds to the concepts joined by a copula in a general proposition. Rather, we use one concrete molecular idea, say of Robert Butts, and through attention to only a few of the attributes of that idea, represent the content of the proposition "All men are mortal". We then move our attention to another complex idea, say of Socrates, and assert his mortality by attending to the components that mark him as a man. The general proposition remains always mentally uninterpreted. Deductive logic, like algebra, is a set of useful rules for the manipulation of symbols, but it is not akin to any cognitive activity.

This theory of concepts has striking implications for a complete account of Mill's views on the inductive canons that constitute the foundation of his views on evidence and explanation. The concept of causality is under this theory only a restricted set of attributes identified in the mind as it considers a concrete example of causation. Thus, the canons of induction presented in Chapter VIII of Mill's

Logic are not only methods by which one makes an association of two ideas, they are also an analysis of the sensory components of our concept of causality. It is this feature that makes logic as the presentation of actual inference patterns credible in the normative realm. When we define a concept by a method of inference and use that method correctly, we cannot possibly make a mistake. J. S. Mill has developed a theory, therefore, that utilizes what Wittgenstein (1953) calls a private language.

Positing a private language, as Pollock (1986, pp. 147f) points out, protects against epistemological relativism. Mill's definition of concepts in terms of inference procedures makes it impossible for two subjects to be justified in believing contradictory propositions. If they are justified, then they have used the inference patterns that define the concepts they mention. If they disagree, they are _ipso facto_ using different concepts. Relativism is avoided at the price of incommensurability. Thus Husserl's complaint that, "Psychologism in all its subvarieties and individual elaborations is in fact the same as relativism" (1900, p. 145), misses its target.

This procedure of defining concepts through the inference patterns that lead to attributing that concept to any case, opens three further lines of investigation for Mill scholarship: first, Mill's ambivalent attitude toward undefined references to "absolute truth" on the one hand,

and purely pragmatic notions on the other, might be reconciled if this self-vindicating feature of his theory of concepts is exploited. Second, as it is immediately clear that the fundamental problem with Mill's naturalized logic cum epistemology cum theory of concepts is incommensurability rather than relativism, one must ask if Mill attempts to avoid this eventuality. Finally, as Mill points out, there are non-causal relations that emerge through the natural association of ideas. And, one might ask if each of these demands a separate set of canons and a separate epistemic realm.

This last query lets us glimpse the distance between Mill and Whewell--a distance that seems so vast throughout their debate about Kepler's methodology. I argued in the third chapter of this essay that Whewell's theory of concepts was antithetical to associationism. Unlike the associationists, Whewell believes that we require concepts prior to perception: "Our sensations need ideas to bind them together, namely, ideas of space, time, number and the like" (1844, p. 60). These concepts or fundamental ideas are identified with the relations extant between our sensations. A concept is some structure of, or set of, relations among sensations. They restrict the formation of complex ideas and as such both facilitate and limit our ability to have experience. The relations between sensations are not, as Mill and the associationists would have it, merely the habituated connection of sensations that have often been perceived together. Relations between sensations have their source in the mind and have no direct dependence on the mind-independent world.

Mill rejects all of this, but leaves an alluring door open. If concepts other than causality are needed to carry through the project of completing human knowledge, then their generation and analysis will follow that of the analysis of causality. That is, we will be forced to attend to certain groups of attributes in concrete ideas and attempt to formulate canons of inference that articulate what these concepts pick out. If, for example, force cannot be reduced to causal connection, then it is in need of definition through a set of, say, canons of dynamic inference. To Whewell's claim that a structure is imposed upon sensation in order to achieve perception, then, Mill can answer that one must analyze perception in order to arrive at the component structures within it. The debate between them appears to have more to do with the psychological theories and the methodologies those theories dictate than the differences between the notions of evidential support that they wear on their sleeves. And this is not recognized by most commentators.

7. Evidence and explanation naturalized.

This brings us to Mill's notions of evidence and explanation. Mill often presents his psycho-logic as the embodiment of the evidential relation. "Logic", he writes, "is the science of the operations of the understanding which are subservient to the estimation of evidence" (1843, p. 12). It is "not the science of belief, but the science of Proof or Evidence" (1843, p. 9). That is to say, logic, unlike a mere associationist psychology, is the study of the distinguishing features of those psychological processes that deserve to have their outputs accepted as beliefs. For example, it should tell us why wishful thinking is not a good guide for the generation of justified new beliefs concerning causal connections. To discover the identifying features of the sound inductions we have made is to learn "the principles of inductive <u>evidence</u>" (1843, p. 433). If we have established that for Mill logic is a study of mental operations, then it follows that the concept of evidence must be understood in terms of a class of actual mental operations. Evidence does not refer to, "anything and everything which produces belief . . . but that which [the mind] ought to yield to, namely, that by yielding to which, its belief is kept conformable to fact" (1843, p. 564). Thus, evidence is identified only relative to a sound induction. To cast our reading of Mill into a modern form, some proposition, correlation or perception E is evidence for a statement or hypothesis H if and only if some subject S can induce H from E in accordance with the canons of induction.[10]

This reading of Mill allows his analysis of evidence to fulfil one prime criterion of any such analysis. Evidence must be described in a manner such that the possession of evidence for a statement or hypothesis constitutes some reason for believing or accepting that statement or hypothesis. Having a reason to believe or accept something is, on Mill's analysis, being able to infer it using the right kind of epistemic procedure; that is, S is justified in believing H just in case S can induce H from s e reason in accordance with the canons of induction. So, for Mill, the description of evidence turns out to be identical to the description of justified acceptance.

If this is right, however, Mill can develop no other evidential reasons for belief. Hence, a straightforward reading of the following passage must be suppressed: "It may be affirmed as a general principle, that all inductions, whether strong or weak, which can be connected by ratiocination, are confirmatory of one another" (1843, p. 321). Since Mill reduces all deduction to a mere shorthand for inferences from particular to particular, perhaps this passage does not contradict the view that evidence occurs only in inductive relations. This passage

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[10] It follows immediately from this result that Mill can make no thoroughgoing distinction between the context of discovery and the context of justification.

is simply written using that shorthand, "conformably to usage".

Instead, stress must be placed on those passages in the <u>Logic</u> which, <u>contra</u> some of Mansel's and Whewell's comments, do not take verified prediction in the hypothetical method as evidence for an hypothesis, unless the procedure is equivalent to induction. That is,

> This process may evidently be legitimate on one supposition, namely, if the nature of the case be such that the final step, the verification, shall amount to and fulfil the conditions of a complete induction. We want to be assured that the law we have hypothetically assumed is the true one; and its leading deductively to true results will afford this assurance, provided no law except the very one which we have assumed can lead to the true result. (1843, p. 492)

According to Mill, verified prediction is evidence for an hypothesis if "a false law cannot lead to a true result" (ibid.), and "no law except the very one which we have assumed can lead deductively to the same conclusions to which that leads" (ibid.).

If these conditions are met, Mill claims that the hypothetical method is equivalent to an induction because, "it conforms to the method of difference" (1843, 492). Represent three hypotheses as <u>A</u>, <u>B</u>, <u>C</u> and three particulars or lower level generalizations as a, b, c. Suppose A is our hypothesis and a follows from it. Now, if no false or other law can lead to a, then we have a case in which the method of difference can guide us. Without A we have only B, C and b, c, rather than A, B, C and a, b, c. Hence, the particular phenomena a meets with Mill's psychologistic condition of evidence--that is to say, from a an induction to A in accord with the canons can be made.

With respect to explanation, as I mentioned in Section 5, a general proposition H really explains E only if H is a shorthand notation for a set of particular ideas and there is some correct inductive inference possible from each of those particular ideas to E. Hence, laws of nature have virtually no role in the human understanding of events. Explanation is achieved when the cause of an event is identified; and a cause and effect have been isolated if the conditions given in the account of evidence are met. Thus, even in Mill's psychologism, there is a strong relation between evidence and explanation. Moreover, as I pointed out, if other canons can be generated to articulate other concepts, then other kinds of explanation will emerge. There is room in Mill's naturalism even for notions such as teleological explanation.

CHAPTER V

PSYCHOLOGISM, STRUCTURE AND ANALOGY:

Psychologism in James Clerk Maxwell's Method.

1. Maxwell's psychologism.

The discussion of psychologistic philosophy of science that permeates the previous chapters of this essay could be dismissed as of purely historical interest were it not for the work of James Clerk Maxwell. Maxwell's generation of the field equations can be seen as one of the most important acts in the history of physics. But Maxwell's clearly successful methodology cannot be correctly understood or emulated unless one identifies the conscious effort he makes to incorporate and exploit the psychologistic themes that are found in Whewell's and Mansel's work, as well as his relative indifference to the views expressed by Mill. Indeed, Maxwell can be profitably interpreted as consciously attempting to steer a middle course between the conflicting aspects of the thought of Whewell and Mansel's model, Sir William Hamilton.

Identifying the psychologistic commitments in Maxwell's method also serves to bring out connections between this Victorian theme and the dynamistic tradition in nineteenth-century physics. Moreover, Maxwell's synthesis and modification of the philosophical positions of his teachers can illuminate the framework in which early twentieth-century physics occurs. In the second chapter of this essay, we saw both a revolutionary distinction and a family resemblance between nineteenth-century psychologistic and the twentieth-century structure-theoretic philosophies of logic of Mansel and Husserl, respectively. In this, the final chapter, I will set the stage for the perception of a similar resemblance in physics.

In this chapter, I concentrate on the Whewellian aspects of Maxwell's work and their relation to dynamism. In order to carry this out, I first argue that Maxwell uses a Whewellian colligation in his 1855 paper, "On Faraday's lines of force". I argue that Maxwell uses analogies to generate a conscilience in order to make his physical results necessary truths that, as Whewell insisted, have the same necessity as geometrical truths. The consiliating fundamental idea that Maxwell in the end embraces is, I will suggest, that of a Lagrangian connected mechanical system. Finally, I show how this consilience is a commitment to a Whewellian fundamental idea and just as we saw in Whewell it generates only a primary commitment to the reality of the relations between phenomena rather than the items related. It is this final feature that is most clearly in line with the dynamistic tradition.

Before beginning this project, I should like to make clear that I do not wish to argue that Maxwell is influenced by Whewell, Mansel or Hamilton. Historians have shown that kind of argument to be nearly always intractable and usually fruitless. Indeed, in this case it is quite nearly impossible to mount such an argument because of the close relations between Whewell, Hamilton and the other persons that could figure as influences on Maxwell's thought. Norton Wise's critical observation on Richard Olson's Scottish Philosophy and British Physics brings out this point. Wise notes that,

> The same features Olson thought peculiar to Scottish universities appear quite generally at Cambridge, and their connection to Scottish philosophy rests on little more than a correlation of ideas. (1982, p. 176)

Hence, while it is certainly clear that Maxwell was in a position to be influenced by these thinkers, I am content to establish only that Maxwell's work incorporates themes that are also found in Whewell.[1]

2. Lines of force.

If the reading I present in Chapter III of this essay is

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[1] The case for influence is, nonetheless, compelling: Maxwell studied philosophy under Hamilton and physics under Forbes, a sympathetic reviewer of Whewell's works. At Cambridge, Maxwell migrated to Whewell's college and rose to become a fellow of that college while Whewell was its master.

plausible, Whewell's philosophy of science can be read as having three features of prime importance to understanding Maxwell: first, Whewell presents a method of scientific investigation that uses the application of colligating ideas to organize groups of phenomena and thereby to generate functions that describe the relations between magnitudes.

Second, Whewell argues that in a fully developed fundamental idea an ordering of phenomena is identified, while the nature of the items so ordered is left beyond the epistemological pale. Scientific theories are in the end merely extremely careful descriptions of fundamental ideas and, hence, science can be committed only to the relations in perception and not to claims regarding the nature of the relata.

Finally, Whewell avers that while the colligating relations we impose on sensations are determined to some degree by an absolute or mind-independent ordering of sensations, the laws of human thought develop so that the relations imposed on sensations become as necessary as the laws pertaining to logic and mathematics. We cannot colligate the input we receive from the world in just any way we choose; it only fits together in certain ways. For example, our sensations fit together when colligated using the idea of (Euclidean) space and not radically different orderings with respect to position. To have any clear conception of space, then, one must utilize the ability of the mind to organize sensation spatially and, following Kant, Whewell takes this as the foundation of our a priori knowledge and the necessity of geometrical truths. Similarly, according to Whewell, in order to organize sensations according to relations of, say, force, we must impose the concept of force on the input we receive from the world. Moreover, the laws of force must be of the same epistemological and modal character as those of geometry because both are built out of our ability to organize sensation under various descriptions. The historical dimension of Whewell's notions is that we can come to realize the possibility of new organizations of sensation. We can organize the inputs of the world under new descriptions. Hence, science proceeds by imposing new ideas on sensation, and these ideas become the foundations of scientific knowledge. In a sense then, Whewellian science is an extension of the processes that give us spatial and temporal perception--in Whewell's psychologism, philosophy of science is continuous with the psychology of perception and cognition.

Let us first take up the imposition of magnitudes and functions in Maxwell.

Maxwell pays more than mere lip-service to Whewell's account of investigation by colligation. This surprisingly data-independent approach to theorizing can be found throughout Maxwell's work. The famed Maxwellian use of physical analogy is, I will argue, a paradigmatic example of colligation as described in Chapter III. This can be seen in his important paper, "On Faraday's lines of force" (1855). In this paper Maxwell is not, as some commentators seem to feel, intent on unifying the electrical researches that he is reviewing. He is convinced already that the results show the science is unified. Rather he attempts to provide a careful characterization of the fundamental ideas that will allow progress in the field. "The limit of my design," writes Maxwell,

> is to show how, by a strict application of the ideas and methods of Faraday, the connexion of the very different orders of phenomena which he has discovered may be clearly placed before the mathematical mind. (1855, p. 158)

Indicative of this, he comments not on unification, but on the difficulty a human has in forming a useful mental representation of the theoretical fragments available:

> [T]o appreciate the requirements of the science, the student must make himself familiar with a considerable body of most intricate mathematics, the mere retention of which in the memory materially interferes with further progress. The first process therefore in the effectual study of the science, must be one of simplification and

> reduction of the results of previous investigation to a form in which the mind can grasp them. (1855, p. 155)

Maxwell's purpose in entering into these studies is his wish to avoid what he sees as unilluminating, but perfectly viable, attempts to explain and promote the unification of electrodynamics. He finds neither mathematical unification, nor unification based on assumptions concerning the ultimate atomic constituents of the world, to be without problems:

> The results of this simplificatior. may take the form of a purely mathematical formula or of a physical hypothesis. In the first case we entirely lose sight of the phenomena explained. . . . If, on the other hand, we adopt a physical hypothesis, we see the phenomena only through a medium, and are liable to that blindness to facts and rashness in assumption which a partial explanation encourages. (1855, p. 155)

Maxwell sees mere mathematics as empty and physical hypotheses as things that restrict our vision. Thus, Maxwell is moving toward a middle ground between the algebraic and the opulently material. He is pursuing a Whewellian fundamental idea, an idea that organizes sensation both for consciousness and theory--an idea that underwrites the necessity of the laws identified through it, but that makes no claims about the physical objects behind the sensations organized.

The first tool used in Maxwell's pursuit of such an idea is the analysis of Faraday's notion of lines of force in terms of the mechanics of an utterly imaginary fluid, a fluid that is, according to Maxwell, "not even a hypothetical fluid [but] merely a collection of imaginary properties" (1855, p. 160). The properties of this fluid are such that it is completely incompressible and without inertia. These properties allow him initially to identify magnitudes of the imaginary fluid and to predict some of their coefficients and interrelations using a purely geometrical model. Lines of force are then defined in terms of this fluid. Notice that Maxwell does not make the bold conjecture that the electric field is immaterial, a fluid, or a locus of properties. Materiality and similar ontological commitments are not at issue. Only the relations of a set of properties are being geometrically modelled within the fundamental idea of (Euclidean) space.

This geometrical construction is neither mere algebra (i.e., an uninterpreted set of symbols and manipulation rules such as the analytic "geometry" of five-dimensional space), nor a physical hypothesis (i.e., a claim about the nature of the field as, say, an actual fluid permeating space), and it in turn allows Maxwell to colligate various phenomena under the idea of "lines of force" in order to prepare for a consilience. It is rather a model of the relations between the investigated magnitudes, formulated in terms of the purely geometrical magnitudes of length, volume, etc. and their relations.

As an example of this method, let us look at one small aspect of Maxwell's account of electromagnetic repulsion and attraction (1855, p. 167). Consider two points Q and S separated by some distance r. Using Faraday's idea of lines of force, Maxwell envisions each line as a tube containing a fluid in motion. Just as with the calculus problem discussed in Chapter III subsection 9 as a paradigm of Whewellian colligation, Maxwell takes the first step of a colligation by identifying what he would call the dependent variable. He sees it as the repulsion acting on S and identifies this magnitude with the velocity of the fluid at S. Call the fluid's velocity V and denote this velocity at S with V_s. This fluid construction allows Maxwell now to visualize the independent variables as r and the resistance of the medium to the fluid's motion, k. With the identification of variables and their interdependence, the first step in a Whewellian colligation is complete.

The second step of a colligation is the imposition of a formula that binds the variables together. In this case, the visualization of Q and S joined by a line of force (or tube of fluid) with length r leads to the formula,

$$\underline{V}_s = \underline{kVr}$$

The repulsion is the product of the resistance of the medium, the velocity of the fluid and the distance between Q and S.

The final step of Maxwell's colligation is, as Whewell demands, the identification of coefficients. Maxwell enriches the construction in order to achieve this. For the dynamics of repulsion, Maxwell envisages a point-source of fluid at Q, some point in an infinite three-dimensional space. Q is taken to produce one unit of fluid in one unit of time, and that fluid spreads out in all directions saturating the space. Thus, every spherical surface with Q at its centre will pass a unit of fluid in a unit of time. The unimpeded fluid's velocity V at any point some distance r from Q will be,

$$\underline{V} = 1/4\pi\underline{r}^2 .$$

By using the geometrical truths or equations concerning volumes in a Euclidean space and simple fluid kinetics, Maxwell observes that the velocity varies inversely with the square of the distance from the source. The magnitude associated with repulsion now has coefficients. The case of attraction is generated in a similar way by replacing the source with a "sink" or drain.

The rate of the decrease of the fluid's velocity can be dependent on the resistance k of any medium through which it must flow. Hence, the rate of decrease will be kV.

Moreover, since the velocity of the fluid will be the product of the rate of decrease in velocity and the distance from the origin to $\underline{S}$, the velocity of the fluid at $\underline{S}$ will be,

$$\underline{V}_s = \underline{kVr} = \underline{k}(1/4\pi\underline{r}^2)\underline{r} = \underline{k}/4\pi\underline{r} .$$

These, then, are the kinds of results merely geometrical representation (see definition below p. 192) and investigation of distances, surfaces and volumes can yield. By associating velocity in the imaginary fluid with the colligating idea of lines of electro-magnetic force, dynamical magnitudes can be drawn out of a purely kinetic representation. The colligation is therefore complete. The phenomena have been organized in a manner that suggests a single experimental procedure in order to determine $\underline{k}$. The general relation of the magnitudes is not merely a guess--one need not create a series of scatter-diagrams comparing various variables and then try some curve-fitting. Moreover, the process is not an induction in Mill's sense of the term; it is data-driven in only a very limited sense.

Of course, Maxwell's result is closely related to Coulomb's. This kind of formulation convinced Maxwell that electricity and magnetism, like gravity, could be viewed as forces that fall off with the square of the distance. As it turns out, in the electrical case, $\underline{k}$ is $1/\Sigma_0$, where Σ_0 is the electrical permitivity of empty space (considered as a dielectric), that is $\Sigma_0 = 9 \times 10^9$ N m^2/C^2. In the magnetic case, k is $1/\mu$ where μ is 1×10^{-7} N s^2/C^2. These constants later become crucially important when Maxwell attempts to calculate the speed of a wave in one of Faraday's lines of force and notes that $\Sigma_0/\mu = c$.

Relating this colligation to the historical case that dominated all Victorian methodological discussions, viz. the case of Newton, then, it is not the proof of the elliptical orbit of a body acted on by a force varying inversely with the square of the distance that is ultimately interesting. Rather, for the Whewellian, the genius of Newton is his colligation of phenomena of motion under the fundamental idea of a single uniform and universal attractive force. Once the force is postulated, as James Clerk Maxwell shows, geometrical intuitions similar to those that lead to such simple functions as the surface law of containers also lead to the inverse square law. The Newtonian connection should be apparent: once the physical analogy of an incompressible fluid is used to flesh out the idea of a gravitational (or electrostatic) force, a function describing that colligating idea can be read off the geometry. Thus, in this Whewellian colligation, genius occurs before the identification of the coefficients for any particular case. And, this offers some insight into Whewell's disparaging comment that,

> [M]en often admire the deductive part of the

> proposition, the geometrical or algebraical demonstration, far more than that part in which the philosophical merit really resides. (1858, p. 174)

Whewell can be read as chiding those who dwell on the geometrical or synthetic presentation that constitutes the greatest part of Newton's *Principia*. For Whewell the interesting and important part of scientific investigation is not the deductive, algebraic or geometrical entailments of colligating ideas or functions. Rather, focus should fix on the analytical aspects hinted at in *Principia*. Whewell and Maxwell side, then, with those dynamical thinkers who treat Newton's theory as a detailed account of principles that have become self-evidently true. The processes of colligation and consilience have taken the human mind to a point where this theory's axioms are laws of thought. That is, as Maxwell puts it, Newton's laws become "the only system of consistent doctrine about space and time that the human mind is able to form" (1877, p. 29). Maxwell is, therefore, focused on the core of Whewell's project to make sense of what might be called the Newtonian Paradox: namely, that the laws of dynamics are firmly based on experience while seemingly independent of empirical verification. Therefore, while a deductive demonstration constitutes explication of an idea, it is not confirmation.[2]

Let us return to the strongly Whewellian form of the inferential routine Maxwell follows. First, he identifies the dependent variable. In this case it is velocity, and hence pressure or force. Second, using the geometrical construction of the idea of "lines of force", he identifies a formula which ties velocity to the independent variable and the other givens. To complete a Whewellian colligation, therefore, all that remains is to carry out the identification of coefficients; that is, Maxwell needs to draw values for k and r from a given phenomenal situation. Perhaps this is how a wrangler has to think. He must build a model which conforms to the mathematical tools available and then compute the output of the model as an answer to the examination question. It is, however, possible to see this wrangling approach as fundamental in both Whewell and Maxwell.

It is important to note that Maxwell is entirely conscious of the method he uses. He later outlines in a lecture the very Whewellian method he appears to be

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[2]I would argue, therefore, that when in Part IV of his Treatise on Electricity and Magnetism Maxwell deduces the field equations from Lagrange's equations of motion, he is not confirming the field equations, but rather showing that they are of a piece with the fundamental idea of a connected mechanical system.

following in 1855:

> The first part of the growth of a physical science consists in the discovery of a system of quantities on which its phenomena may be conceived to depend. The next stage is the discovery of the mathematical form of the relations between these quantities. After this, the science may be treated as a mathematical science, and the verification of the laws is effected by a theoretical investigation of the conditions under which certain qualities can be most accurately measured, followed by an experimental realization of these conditions, and actual measurement of the quantities. (1874, p. 257)

In the first pages of "Faraday's lines of force", Maxwell derives a formula that demands the measurement of a physical constant: the permitivity of the dielectric. The construction of a geometrical model, or what a Kantian might call a construction in intuition, has led Maxwell to a result that has the possibility of becoming a "geometrical" truth. But, one must now recall Whewell's warning that colligation is not evidence for a colligating formula. The attempt to model non-electromagnetic or atomic repulsion with this construction would lead the researcher ast ay. As even Newton had noted, this kind of repulsion could not fall off as the square of the distance.

Thus, much of Maxwell's further work in electrodynamics can be read as his search for a Whewellian consilience; a consilience that will lend the modality of geometry to his results. Such a consilience would generate the Victorian sine qua non of logico-mathematical necessity, namely, a limitation of the possibility of thinking about the phenomena in question in any other way.

This consilience is, I believe, in the end, the characterization of electromagnetic phenomena as representable only as a Lagrangian continuous connected mechanical system. That is to say, Maxwell's field equations emerge as laws of thought both empowering and limiting the mind in its consideration of electromagnetic phenomena. The field is not a thing, it is a point of view, a fundamental idea or a law of thought. In this light, we can take very seriously Maxwell's claim that,

> [T]he importance of these equations does not depend on their being useful in solving problems in dynamics. A higher function which they must discharge is that of presenting to the mind in the clearest and most general form the fundamental principles of dynamical reasoning. (my emphasis; 1876, p. 309)

Here, as elsewhere, he emphasizes the identification of the form of certain differential equations and the form of human thinking.

3. Relations and relata.

To further support this claim we need to look at the second major Whewellian theme taken up in Maxwell's thought: namely, the committment to relations and not to relata. It is natural to interpret Maxwell's use of imaginary fluids and other mechanical models as various attempts to hypothesize about the nature of the physical mechanisms at work in external reality. Nevertheless, a number of points mitigate against this interpretation. First, as we saw in the last section of this chapter, Maxwell expressly criticizes the use of physical hypotheses. Second, Maxwell changes hypotheses with apparent abandon. Third, he adheres to formulae gleaned from such constructions even in the face of refutations. Quite paradoxically, then, Maxwell's constructions are utterly disposable, yet binding in the face of contradiction by empirical evidence. I will argue that this makes sense if we recognize that Maxwell, like Whewell, sees the proper subject matter of theory as the relation of magnitudes, rather than the existence or nature of the items thought of as generating such relations.

Let me begin by rehearsing the details of the claim that Maxwell both repudiates and retains his constructions. As we have seen, Maxwell begins his electromagnetic theorizing with an account of a hydrodynamic construction that emphasizes the velocity of the flow of a fluid. In other works, Maxwell retains the hydrodynamic model, but concentrates on the tension within a body of fluid under stress. In Part 1 of his "On physical lines of force", Maxwell proposes the vision of a magnetic field as a system of vortices. In Part 2 of the same paper, Maxwell introduces his infamous idling wheels between vortices. He then identifies the motion of these items from vortex to vortex with electric current. In his work on gases, Maxwell generates both molecular and continuum models of gases. There is, then, a notable variety of constructions.

As Jon Dorling convincingly argues, however, these various constructions are, "not a simple-minded application of the hypothetico-deductive method" (1970, p. 229). Dorling's case is based on Maxwell's adherence to the theoretical claims that arise due to the kinetic theory of gases. Maxwell retains these results concerning the relations between magnitudes even though he is aware that his models give an incorrect analysis of gases with respect to their specific heats. Maxwell's commitment continues in the face of inconsistency with empirical results.

This seriously undermines A. F. Chalmers' commentary on Maxwell. Chalmers' blindness to the psychologistic aspect of Victorian philosophy leads to his misinterpretation of the dominant theme in Maxwell's methodology. We must reject Chalmers claim that,

> [Maxwell] believed that there existed a real

> mechanism, involving the interaction of a mechanical aether and the molecules of matter, which was responsible for the observable electromagnetic phenomena, and that a search for the details of this mechanism was a legitimate scientific quest. (1973, p. 109)

Chalmers sees Maxwell's constructions as heuristic but also as candidates for realist committment. His unlikely story must be, however, that Maxwell radically alters his views toward models, suddenly in 1861 giving up his agnosticism and taking up a realism not foreshadowed in his earlier work and then, in writing his later works, merely feigning his earlier Whewellian agnosticism.

My reading also conflicts with Tolstoy's interpretation of Maxwell's use of models. Tolstoy's account errs in the other direction; it is weaker than necessary. Tolstoy rightly sees the models as lacking any ontic commitment to *relata*, but that is because he fails to note in them any commitment at all. Hence, he mistakenly takes Maxwell's modelling as purely heuristic:

> [W]e must not take Maxwell's electromagnetic model-making too much to heart. Models, says he, are fine and useful things, but we must remember that they are the imprint of our minds on external reality which may be ultimately inaccessible. The fluid analogy in *On Faraday's Lines of Force*, or

> the outlandish models used by Maxwell in later writings, need not disturb us; he uses them only as aides--for his own thinking and for his readers. (1981, p. 78)

Something much stronger than this is endorsed by Maxwell's writing. I believe that my account of Maxwell's approach is most closely approximated by Einstein's contention that "[Maxwell] used several of these constructions side by side, and took none of them too seriously; it was clear that the equations themselves were all that was essential" (1931, p. 70). Let me explain.

As Olsen (1975, pp. 175f) points out, Hamilton enjoined his students to use analogies. Nevertheless, he follows the Scottish Common Sense theme of rejecting any inference from analogy in phenomena to identity between underlying mechanisms. An analogical relationship between descriptions of two realms of phenomena does not endorse an inference to the conclusion that the production of those phenomena is dependent on the same mechanism. Hamilton therefore advocates the multiplication and arbitrary replacement of analogies. Hence, Maxwell's changing constructions do not reflect a progressive change in his commitments to views concerning the nature of ultimate physical mechanisms, culminating in his accepting an aether-molecule system. Rather, they show his attempt to follow the advice of the Hamiltonian school, which counted

Mansel among its members. The variety of analogies show off relations among magnitudes that are captured in equations. The shifting analogies are a method by which Maxwell removes the particulars of each as a physical hypothesis, leaving behind unalloyed characterizations of the relations between magnitudes. The similarity between models, Maxwell claims, "is a similarity between relations, not between things related" (1873, p. 52). The models are then abandoned, but the influence they have in shaping the organization of magnitudes by the mind continues. The researcher is not left with merely algebraic representations and manipulation rules.

This is what Turner calls "Maxwell's golden mean" (1955, p. 229). The analogies are not merely heuristic because the equations are always seen to characterize a whole system that is representable. The equations do not allow for manipulations that have steps in which no physical model is possible. That is to say, deductions within the calculus are interpreted at every point. The mathematics is never left to stand completely by itself. In "Faraday's lines of force", for example, as things become more difficult to represent geometrically Maxwell does begin to replace simple geometrical constructions with differential equations. But, he does so reluctantly and without a change in his essential approach. In his attempt to colligate phenomena under Faraday's idea of the "electro-tonic state", he merely wants to talk about the geometrical considerations of the flow of his fluid at any given point and in any given direction. The fundamental notion that Maxwell utilizes in this section is that, "Internal electro-motive forces arise principally from the difference of electric tension at points of the conductor in the immediate neighborhood of the point in question" (1855, p. 190). Thus, the fluid model is applicable but complicated. Still, the partial differential equations Maxwell arrives at are not deduced from fundamental assumptions about the nature of the objects involved; they are not founded on physical hypothesis. Rather, they are drawn from a geometrical model of the structure of the interrelations between those fundamental items whatever they might be--the equations are about differences in magnitudes not properties of molecules.

All this is in line with Whewell. As Wise comments, "In essence Whewell rejected the deductive establishment of partial differential equations in favour of the differential equation itself as the fundamental entity of mathematical physics" (1982, p. 183). With regard to my reading of Whewell, Wise's point must be this: the fundamental idea of a science is an account of the relation between properties and not an ontic commitment to any particular set of relata. Hence, the differential equation, as a careful characterization of some aspect of that structure, is a (partial) definition of the fundamental idea in use. Moreover, since the equation at once describes magnitudes at every possible value, it captures the idea as a unified or whole form of possible relations between magnitudes. The equation describes the possibility of representation. Thus, since in Maxwell too there is no fundamental object of study except the structure described by the equations, Maxwell can be seen as very much in tune with this aspect of Whewell's thought.

This reading leaves Maxwell consistent with the Hamiltonian school in that, as Hendry notes, "For Maxwell as for Hamilton, scientific knowledge was concerned with relations and with analogies between relations, not between the things related" (1986, p. 148). Still, the focus on the Whewellian patterns of colligation and what might be called structural realism with respect to a fundamental idea characterized by a family of mechanical models, gives a more forceful interpretation of Maxwell's work; it also explains why, as I will argue, Maxwell believes that a physically true theory is one that sets out the necessary relations between magnitudes.

4. Laws of Thought and Nature.

As we have seen in earlier chapters, Mansel and Hamilton disagree with Whewell concerning the modality of causal laws. Referring to Hamilton as, "the greatest philosophical critic of the age" (1853, p. 80), Mansel claims that Hamilton's criticisms of Whewell's views are based on the same insights as his own. Whether or not this is so (and I believe it is not) is unimportant; one point made by both critics is that, contra Whewell, the laws of thought cannot underwrite the truth of any objective claim. Mansel follows some of the Scottish Common Sense thinkers in claiming that, "no matter of fact, that is to say, no actual phenomenon of external nature, can in any possible state of human knowledge be a matter of demonstration" (1853, p. 99). This extends even to geometry, which Mansel takes as true only of mental objects and merely an hypothesis concerning the formal features of material objects (vide. 1853, p. 97). In this, Mansel is embracing Hamilton's Principle of Relativity. Mansel admits the Kantian claim that we cannot think about anything without bringing that content under the laws controlling our thinking. There is no unconditioned knowledge of the external world. Causal claims concern themselves with a mixture of mental and nonmental elements, and so the laws of such mixed contents lack the status of laws concerning the pure forms of thought. Thus, while the laws of thought may restrict or facilitate the representations we form, they cannot found claims concerning the form of any law of nature.

Maxwell clearly feels this tension and attempts to respond to it in his 1856 paper, "Are there real analogies in nature?" He transmits an ambivalence toward the view of science as about some external nature, and the seemingly opposed view that science explores some feature of human mentality. "[T]he whole framework of science, up to the pinnacle of philosophy," writes Maxwell, "seems sometimes a dissected model of nature, and sometimes a natural growth on the inner surface of the mind" (1856, p. 348). Furthermore, he identifies the foundation of the unity of science as the fundamental issue in any debate between proponents of these views.

It is clear to Maxwell and many other nineteenth-century thinkers that the fundamental laws identified in many scientific studies share a similar structure. The looming example of this unity for Maxwell is Ohm's (1827) and Thomson's (1842) identification of thermodynamic phenomena and those of electrostatics. Maxwell's question is, then, whether such isomorphism is the product of the make-up of the world, or an artifact of the structure of the human mind. As he notes, if the world is merely matter in motion, then the laws of motion will impose their form on the regularities in every field. If, however, the mind has an active role in our representation of the world, then the isomorphisms between the functions representing regularities in various fields can be seen as having their source in the mind, and as having no objective counterpart.

As he puts the issue:

> If, in examining the admitted truths in science and philosophy, we find certain general principles appearing throughout a vast range of subjects . . . are we to conclude that these various departments of nature in which analogous laws exist, have real interdependence; or that their relation is only apparent and owing to the necessary conditions of thought? (1856, p. 348)

Referring to space and time as Whewellian fundamental ideas, he continues, making the point more clearly,

> [T]o determine whether there is anything in Nature corresponding to them, or whether they are mere projections of our mental machinery on the surface of external things, is absolutely necessary to appease the cravings of intelligence. (1856, p. 349)

Maxwell's early response to the issue is reconciliatory. He claims that founding knowledge on the laws of thought can indeed give us objective knowledge, because thought is an integral part of nature. The essentials of that reconciliation are captured in the following cryptic passage: "the only laws of matter are those which our minds must fabricate, and the only laws of mind are fabricated for it by matter" (1856, p. 355). This statement occurs after what can seem to be a discussion of symmetry.

Tolstoy reads this as the claim that, "the laws of intellect and matter are inseparable--there is, Maxwell says, no objective physics independent of mind" (1981, p. 77). However, the passage seems to be somewhat richer than that. There is a symmetry and a lurking indifference in Maxwell's analysis. If the world is merely matter in motion, then the laws identifying the limits of all motion will find expression in the laws of thought. Maxwell comments as follows:

> [Suppose] all phenomena of nature, being varieties of motion, can only differ in complexity, and therefore the only way of studying nature, is to master the fundamental laws of motion first, and then examine what kind of complication of these laws must be studied in order to obtain true views of the universe. If this theory be true, we must look for indications of these fundamental laws throughout the whole range of science, and not least among those remarkable products of organic life, the results of cerebration (commonly called "thinking"). (1856, pp. 352-53)

Thus, Maxwell brings together Mansel and Whewell. Following the Hamiltonian line he endorses the claim that psychological necessity cannot by itself found a science of the absolute and external. Yet, in a manner very reminiscent of Mansel, necessity seems to be the mind's response to contemplating the very laws that describe its behaviour. At the same time, Maxwell brings these points into agreement with Whewell's doctrine by arguing that the place of mind in nature is such that psychological necessity will reflect the necessary connections between external objects. The structure of thought echoes the structure of matter. Hence, regardless of the passivity or activity of the mind in representing the world, scientific research can and must be founded upon psychological necessity.

Some have noted that Maxwell's views on the relations between physical and mental laws are of fundamental importance in understanding Maxwell. Planck defends the existence of such a relation when he characterizes Maxwell's work as follows:

> By pure reasoning he succeeded in wresting secrets from nature, some of which were only tested a full generation later. . . . That such predictions are at all possible would be quite unintelligible if one did not assume that very close relations exist between the laws of nature and those of mind. (1931, p. 56)

And Wise gestures toward an even greater importance for these relations when he hints that,

> Much of the uniqueness of Maxwell's notion of dynamical theorizing derives from his deeper

> concern over the relation between laws of mind, epitomized in geometry, and laws of matter, epitomized in dynamics. (1982, p. 195)

In Maxwell we see the meeting of psychologism and actual scientific practice. For him, as I will argue, not only the necessity, but the epistemological status, of claims in physical science can be of the same kind as that in the claims of geometry and arithmetic.

One of Maxwell's mechanisms for the transmission of modal and epistemic properties is the geometrical construction. He uses visual representations constructed in intuition in order to identify the properties of the systems so modelled. For example, he uses constructions or geometrical models of "lines of force" in order to arrive at theoretical claims in the field of electrodynamics. The use of these geometrical constructions provides an epistemological base for his theories, because for Maxwell, as for other followers of the psychologistic reading of Kant, geometry is grounded in the laws of thought. Now, as we have seen, according to Maxwell the laws of nature emerging in dynamics mirror laws of thought or their geometrical implications. Thus, for Maxwell, the laws of mind are related to the laws of matter in such a way that the epistemological status of geometry is transferred to properly derived theories in dynamics. In this way, the Whewellian claim of identical modal status for geometry and physical laws is carried into Maxwell's approach.

5. Analogy and law.

Like Whewell, Maxwell believes that scientific theory can become necessarily true. This view is not restricted to these two thinkers; indeed, analyzing scientific claims to knowledge as more or less similar in kind with necessary geometrical theorems is widespread among Victorian natural philosophers. Humeans were not dominant.

Maxwell's peers had by the mid-nineteenth century generated a tripartite analysis of scientific theory. Wise (1982, p. 185f) points out that George Airy had separated the suppositions concerning the mechanical properties of the ether in the theory of light from what he took to be the certain geometrical truth that light is made up of transverse vibrations. The term dynamical theory was appropriated for reference to this geometrical component, which was free of any mechanical hypotheses.

Thomson and Stokes added to this picture, separating Airy's first category into the purely geometrical and the dynamical. They too saw these as distinct from the mechanical aspects of a theory. The geometrical elements of a theory were those directly entailed by the claim that space is made up of an infinite number of points which can be moved in a continuous way in any direction. The dynamical elements consisted of that which followed from the introduction of differential equations describing forces that explained the movements of geometrical points. Finally, the mechanical component of a theory was the set of speculations concerning the constitution of such things as the ether and the mechanical causal systems responsible for the transmissions of forces described in the dynamic component.

Unlike Maxwell, who embraced both the geometrical and dynamic modes of theorizing, Thomson and Stokes restricted proper scientific investigation to the geometrical. They insisted that only the purely geometrical description of motion was defensible. Thomson allowed that consistent and mentally picturable mechanical models could be of heuristic value but he rejected Maxwell's zealous use of dynamic analogy; viz. models like that of his "On Faraday's lines of force", which allow one to read dynamic magnitudes of the target system off kinematic or mechanical magnitudes in an analogical system. Thomson was suspicious of identifying forces in one system with motions in another--but then, he was suspicious of all dynamics. John Hendry sums up the historical place of Thomson on this issue with the following metaphor:

> If we were to consider the pendulum of history as moving between the mechanistic and dynamistic, we might say that it passed Thomson en route from Laplace to Maxwell, but quickly returned to meet

> him again, and indeed to stay with him, as a representative of a new and more cautious mechanism, until swinging back to the more dynamistic outlooks of the founders of quantum mechanics. (1983, p. 144)

Unlike Thomson, Maxwell sits within both the Whewellian and Hamiltonian traditions with their relational bias. For both Whewell and Hamilton, scientific knowledge has as its object the relations between magnitudes and not the objects of those magnitudes. This is the implication of Whewell's doctrine of fundamental ideas and of Hamilton's doctrine of the conditioned. Both these views are built upon considerations of human psychology. A Whewellian fundamental idea is the relation in which humans come necessarily to represent certain classes of phenomena. Hamilton's doctrine is a complex theory addressing the conditions under which humans can claim to have knowledge. Its basic claim is that humans cannot think about things except relationally. We are unable to consider any object unconditionally--that is, without making comparisons.

Maxwell moves beyond the geometrical level of theory, to the dynamic. In so doing he can seem to advocate Whewell's claim that there are more forms of intuition than merely space and time. But Maxwell makes this move without commitment to a mechanical level. He focuses on relations and characterizes the relations between the objects of thought using analogies. Maxwell distinguishes analogies into a number of categories: some are identities. and others, real analogies. Still other relations are physical analogies, mathematical analogies and formal analogies. Thus, the relations seen between magnitudes and sets of magnitudes are diverse in nature, and a question arises as to which of these kinds of relations are of scientific importance and why.

Maxwell's efforts can be seen as an attempt to identify the important relations of magnitudes using physical analogies, and then through clarification to establish these relations as laws of thought. Thus, the physical laws come to have the same kind of modality Victorian Kantians attributed to geometry. To repeat, axioms or special idaalizations such as Newton's laws become, as Maxwell insists in Matter and Motion, components of "the only system of consistent doctrine about space and time that the human mind is able to form" (1877, p. 29). This state of affairs, then, meets the Victorian standard of geometrical necessity: statements are necessary just in case their contradictories are impossible to conceive.

For Maxwell, this modal status is achieved by laws relating magnitudes through, what I called in Whewell, a progressive psychologism. That is, the prepared mind comes to recognize an organization of phenomena or magnitudes as the only one possible. Maxwell saw the use of physical analogies as the exclusive method for facilitating such progress. It is, Maxwell insists, the method that,

> allows the mind at every step to lay hold of a clear physical conception, without being committed to any theory founded on the physical science from which that conception is borrowed, so that it is neither drawn aside from the subject in pursuit of analytical subtleties, nor carried beyond the truth by a favorite hypothesis. (1855, p. 156)

Merely formal analogies lack the clarity necessary to establish a form of thought or fundamental idea. Moreover, they are misleading, allowing a colligation that identifies what should be separate magnitudes. Formal analogies, then, do not lead one to develop laws of thought that emulate the structure of the laws of matter.

Illustrations of a formal analogy come from Victorian theory of light, thermodynamics and dynamics. Maxwell is convinced that the analogy between light and vibrations in an elastic medium and waves in a liquid is not merely formal. However, it can be made formal "by stripping it of its physical dress and reducing it to a theory of 'transverse alterations'" (1855, p. 156). Thus, formal analogy focuses on the similarities in the forms of laws in fundamentally distinct classes of phenomena. The researcher using such an analogy does not have any conception of a physical system; rather, all attention is focused on the form of the laws and the results computed using those laws. As Turner sums this up,

> To strip the theory of light of its dressing, to reduce it to 'theory of "transverse vibration"', is to offer a mathematical description. To ask what property of light it is that vibrates, or to seek a medium in which this vibration occurs, is to resort to physical hypothesis. But to exploit this comparison to the fullest and yet remember that it is but a similarity in mathematical form between two sciences, is to proceed by the method of physical analogy. (1955, p. 228)

In another example of a formal analogy, Maxwell points out the following:

> We have only to substitute source of heat for centre of attraction, flow of heat for accelerating effect of attraction, and temperature for potential, and the solution of a problem in attractions is transformed into that of a problem in heat. (1855, p. 154)

As we would put this, by assigning the appropriate properties to each of the variables, both heat flux and attractive force can be represented by the (negative) gradient of the following function:

$$\int\int p/r \; dw^2$$

where p is a density (the number of lines of force per unit area in the case of electromagnetic attraction), *r* is a distance, and *w* is the surface area giving rise to a potential. (Hence, the vector tells us by how much the force will have been reduced when we move from the surface to some point normal to the surface). Again, Maxwell is not interested in the unification or mutual reduction of thermal and gravitational processes either at the ontological or phenomenal level. He is only interested in the structural similarities of the descriptions of distinct phenomena. But, he takes pains to point out that gravitation and thermal transfer are seen by his contemporaries as fundamentally distinct: the former is achieved by action at a distance while the latter occurs by contiguous activity. Since in this use analogical conceptions about gravitational systems are not deployed to explore the subtleties of thermodynamics, the analogy is only formal. The mathematics of one problem is merely replaced by that of another.

According to Maxwell, analogies of this kind are not of ultimate importance to scientific investigation. He presents two main points in his argument to this end. A system erected on formal analogies is, according to Maxwell, "probably deficient both in the vividness of its conceptions and the fertility of its method" (1855, p. 156). The charge of lack of clarity is a Whewellian point. As I argued in Chapter III, many Victorians felt that merely algebraic or purely mathematical thinking was anemic, offering computation rules but not promoting human understanding. Moreover, in the larger Whewellian scheme of things, formal analogy cannot bring the mind to a position where it identifies the relations between magnitudes in the target system as necessary. It cannot trigger the Gestalt-like process that figures in a complete induction.

The lack of fertility Maxwell disdains encompasses two claims. First, formal analogies do not lead to deeper levels of analysis. They are exhausted as they stand because they are not accompanied by any physical intuitions.

Second, formal analogy can lead the researcher to improper unification of magnitudes or simplification, where the multiplication of magnitudes is the more fruitful and appropriate direction to travel. Colligations can be made using merely formal relations, but Maxwell insists that the colligating idea must be more than a mere formality. One must investigate physical conceptions such as (imaginary) substances, regardless of whether or not the relations between magnitudes give rise to formal analogies with respect to the target system. As Maxwell notes in his "On the mathematical classification of physical quantities":

> In many physical cases, the force and the flux are always in the same direction, and proportional to

> each other. The one is therefore used as a measure of the other, their symbols degenerate into one, and their ideas become confounded together. One of the most important mathematical results of the discovery of substances having different physical properties in different directions has been to enable us to distinguish between the force and the flux, by letting us see that their directions may be different. (1874, p. 261)

That is not to say that Maxwell saw formal analogy as useless. The first steps in a Whewellian colligation are the identification of magnitudes and the characterization of a functional definition of their relations. Maxwell points out that both of these ends can be furthered by attending to formal analogies (1874, p. 257). His point is only that an inordinate affection for formal analogy can lead the researcher astray.

Physical analogies by contrast are those in which one does not merely compute the value for a magnitude using the formulae from another realm of phenomena. Rather one takes the connections between magnitudes to model the relations between other magnitudes. For example, the results of the investigation carried out in "On Faraday's lines of force" for the most part show only that the equations arrived at by considering electrical phenomena as action at a distance between molecules can also be achieved by considering those phenomena as arising from the interaction of continuous substances. Nevertheless, unlike the action at a distance formalism, the colligating idea or physical analogy of a line of force leads one to identify a magnitude with the state of the space between electromagnetically attractive bodies. And this magnitude should take different values depending on the presence of attractive bodies at its perimeters.

The fluidic analogy also shows the two important ways in which physical analogies differ from identities. As Turner states these,

> First, analogies were found incomplete in the sense that the one science has only a special group of instances which are analogous to the other science. . . . Second, the analogies were found incomplete in the sense that certain quantities describing one set of phenomena may represent, to use Maxwell's terminology, 'physical states', while the analogous quantities describing the other set of phenomena are 'mere scientific concepts'. (1955, pp. 230-1)

In the first instance, there is no attempt to identify, say, the temperature of a fluid with any magnitude in the target system. The analogy is quite openly incomplete. With respect to the second kind of incompleteness, the volume of the fluid passing a surface is identified with the flux in the target system, yet, flux, for Maxwell, is a concept without mechanical meaning.

Lines of force, then, are not taken actually to be tubes containing a flow of an incompressible fluid. The idea is not a physical hypothesis, and so the necessity identified is not a physical necessity. The laws identified through a physical analogy are formulae describing the relations of magnitudes and not the properties of the items related. But, neither are the colligating ideas mere mathematical contrivances, and so the necessity articulated is not *ipso facto* that of mathematical truths. Rather, the method of physical analogy leads to a perception of relations, and it is the necessity of perception that is brought out: that is, the necessity of a form of intuition.

6. *Experimental evidence*.

There is a temptation to read Maxwell's allegiance to analogical inference as part of a philosophy of discovery, the seeming lacuna concerning justification in his work being quietly overlooked as an idiosyncracy. The preeminent role of physical analogy as a vehicle of both discovery and justification can, however, be brought out by considering his most Whewellian opinions concerning the role of experimentation in scientific research. These support the view that analogy is meant to work a change in the researcher's mode of perception.

Maxwell admits of only two kinds of experiments: experiments of illustration and experiments of research. This philosophical position controlled his design of the Cavendish Laboratory. What is most interesting in his discussion of these two kinds of experiment is that neither is given any straightforwardly evidential or (dis)confirmatory role. In fact, for Maxwell, as we shall again see, theoretical commitments can overrule even direct observation.

Illustrative experiments are attempts to make a researcher or student follow a colligation previously made by some researcher. In an introductory lecture on empirical physics, when speaking about the purposes of experiments of illustration, Maxwell claims,

> [T]heir aim is to present some phenomenon to the senses of the student in such a way that he may associate with it the appropriate scientific idea. When he has grasped this idea, the experiment which illustrates it has served its purpose. (1873a, p. 243)

The illustrative experiment serves only to allow the scientific investigator to apply a colligating idea. It presents phenomena in a manner that most easily fits the form of phenomena identified in a colligating idea. Moreover, Maxwell attributes this view to Whewell writing,

> He therefore regards experiments on the laws of

> motion as illustrative experiments, meant to make us familiar with the general aspect of certain phenomena, and not as experiments of research from which results are to be deduced by careful measurement and calculation. (1876a, p. 530)

Consider, as an example of an illustrative experiment, the secondary school exercise in which an object is dropped while pulling a ribbon of paper through a bell-clapper that makes a mark on the ribbon at equal time intervals. This experiment forces the student to recognize that, for a falling object, a relation exists between time and velocity. The seemingly benign activity of the clapper as against the activity of the object in its rush toward the floor leads the student to see the time coordinate as an independent variable. Direct observation of the marks on the ribbon can even suggest that the relation between time and velocity is not linear, but exponential. Doubtless, some analogy such as the hydrostatic model of electromagnetic attraction we saw earlier could be used to set the exponent at two. Thus, the student is driven through the three steps of a colligation.[3]

Maxwell is here as elsewhere fully conscious of Whewell's claim that there can be no thoroughgoing separation of fact and theory. Maxwell argues that the

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[3] For an alternative account of this process see Forster 1990.

mind plays a role in forming our knowledge of nature. He maintains with Victorians such as Mansel that even the unity that founds the counting numbers is imposed by the mind--it is in Kant's sense synthetic. This unity in turn is artificially imposed on the visual field. As he notes,

> There is nothing more essential to right understanding of things than a perception of the relations of number. Now the very first notion of number implies a previous act of intelligence. Before we can count any number of things we must pick them out of the universe, and give each of them a fictitious unity by definition. (1856, p. 348)

Hence, even the most primitive perceptual task of breaking experience up into a complex of phenomenal objects presupposes and depends on a previous committment to theory. Moreover, the replacement of one set of theories by another gives the perceiver a different set of objects. Maxwell says all this with his most poetic grace:

> The dimmed outlines of phenomenal things all merge into one another unless we put on the focussing glass of theory, and screw it up sometimes to one pitch of definition and sometimes to another, so as to see down into different depths through the great millstone of the world. (ibid, p. 349)

The other fundamental elements of scientific theory, causes, are also deemed by Maxwell to be mentally derived. Moreover, Maxwell even claims that forces have a merely psychological reality. The possibility of dynamics presupposes a psychological commitment to perceiving some phenomena as if it were a mechanical system in which the motions of bodies are dependent on some transmission of power. "We cannot, however, think of thoughts without conceiving of them as depending on reasons", Maxwell insists, and then continues, saying,

> These reasons when spoken of with relation to objects, get the name _causes_. . . . When the objects are mechanical, or are considered in a mechanical point of view, the causes are still more strictly defined, and are called _forces_. (ibid, p. 350)

The psychological foundation of scientific perception in accepted theories is, from the beginning, then, worn by Maxwell on his sleeve.

Even more strongly, however, as Chalmers (1973, p. 112) notes, Maxwell allows not only for the creation of observation by theory, but also, for the correction of direct observation by theory. Maxwell points out in "On action at a distance" (1876b, pp. 313-14) that advocates of action at a distance use their theoretical commitments to show that objects apparently touching each other are in fact not touching. While two pieces of glass may be observed to be touching, the production of Newton's rings using the composite lens shows that they are not in optical contact. In this, then, the theoretical commitments of the observer can lead him to reject a direct observation at one level of analysis in favour of a theoretical judgment about the state of affairs obtaining at another level of analysis.

Within this context, then, the experiment of illustration is most easily interpreted as a mechanism meant to facilitate the application of theory so as to generate a set of quite openly theory-soaked facts.

This leaves the only other role for an experimental procedure obvious. Its purpose is to measure the physical constants left without evaluation in the illustration. As Maxwell puts it, in experiments of research, "the ultimate object is to measure something which we have already seen--to obtain a numerical estimate of some magnitude" (1873a, p. 243). Moreover, this procedure fits into general methodology as follows: first one colligates; as Maxwell puts it:

> we have first to make our sense familiar with the phenomenon, but we must not stop there, we must find out which of its features are capable of measurement, and what measurements are required to make a complete specification of the phenomena. (1873a. p. 244)

And then one identifies values for magnitudes suggested by the colligation: "We must then make these measurements and deduce from them the result which we require to find" (1873a, p. 244). Thus, the experiment of research may even fail to allow for the direct measurement of a magnitude. The experiment of research can open the way to what we might today call a bootstrap computation, (vide. Glymour, 1980). But the colligation and this ancillary operation are distinct. Furthermore, within the Whewell-Maxwell approach, a bootstrap computation will, perhaps, increase confidence in the value assigned to some magnitude, but it will not act as evidence for the colligating formula.

Experiments, then, help the mind to internalize a theory, but it is the mind's prerogative to give the theory a modal status. In reinforcing this point, Maxwell notes Todhunter's criticism of Whewell: while Whewell claims that the weight of a whole is identical to the sum of the weights of its parts, Todhunter points out that Whewell also claims that weight is dependent on the position of the parts. Maxwell repairs Whewell's claim stating that what he should have said is that "The mass of the whole compound must be equal to the sum of the masses of the separate elements" (1876a, p. 532). But, then Maxwell notes that no experiment is done to confirm this claim. Rather, all our experiments use measurements or comparisons of weight. Maxwell concludes,

> Thus, then, we are led by experiments which are not only liable to error, but which are to some extent erroneous in principle, to a statement which is universally acknowledged to be strictly true. Our convictions of its truth must therefore rest on some deeper foundation than the experiments which suggested it to our minds. (1876a, p. 532)

And, in my reading, the psychological foundation of this necessity is, for Maxwell, the accepted Whewellian fundamental idea.

7. <u>True physical theory</u>.

How, then, is it that some theories or colligating ideas are selected as superior and eventually accepted? I believe that Maxwell's answer here can also be read as owing much to Whewell's psychologism. True physical theories are those whose contraries cannot be conceived. They are laws of thought; that is, the formal limits of our thinking clearly about some realm of phenomena.

In Hendry's words, a true physical theory is one "in which physical facts will be physically explained" (1986, p. 154). But, as Hendry is quick to note, "By a 'physical explanation', however, Maxwell did not mean a theory based on physical hypotheses, but rather one based on the necessary connection between empirical phenomena" (1986, p. 154). In my reading, then, a true physical theory completely characterizes the relations between phenomena in a realm or natural kind. Given the values for some set of magnitudes the complete state of the set is defined. Moreover, the relations of the phenomena are seen as the only ones able to be conceived clearly.

For Maxwell, the clear conception of a system is that characterization of all possible relations of a system of phenomena, which uses as many magnitudes as needed but as few as possible. That is to say, the limits of the human mind in formulating descriptions of systems are the limits of physically true theories. Maxwell's career appears to be, then, a movement toward the identification of Lagrangian mechanics with the limits of (physical) thought. The equations of motion given in their Lagrangian formulation constitute the limits of human conception concerning mechanical systems as a natural kind. As I have already noted, Maxwell says, in "On the proof of equations of motion", that these equations present "to the mind in the clearest and most general form the fundamental principles of dynamical reasoning" (1876, p. 307).

Lagrangian formulations characterize a connected mechanical system using a number of generalized coordinates or magnitudes just equal to the number of degrees of freedom determined by the connections within the system. The generalized coordinates therefore exhaustively identify the state of such a system using the smallest set of magnitudes sufficient to the task. Since only the number of degrees of freedom of the system rigidly constrains the number of magnitudes used in the description of the system, there is no justifiable inference from the Lagrangian characterization to any one of the infinite number of micro-systems of component causes that might emulate the behaviour of the macro-system under study. Thus, as Simpson points out, in a Lagrangian characterization,

> the velocities, moments and forces related to [the generalized coordinates] in the equations of motion are not literally what their names indicate, but new, generalized quantities only metaphorically related to their Newtonian originals. (1970, p. 252)

It is the structure of the phenomena, and not the properties of the items related, that takes precedence.

Maxwell's famous explication of this situation uses the analogy of a belfry in which we have observational access only to the behaviour of the bells and the positions of the ropes. The mechanical apparatus connecting the bells and ropes is not open to our observation. Maxwell identifies the positions of the ropes with values for the generalized coordinates. Thus, given the positions of the ropes and a true physical theory of the belfry, one can determine the state of the bells. A true physical theory, then, has two conditions: first, the behaviour of the system must be dependent on all the ropes (generalized coordinates) considered. Second, there are no ropes (generalized coordinates) of which we are not aware.

In its ultimate form, then, Maxwell's electromagnetic theory is dynamical, but once formulated within Lagrangian mechanics, Maxwell can avoid any mention of the infinite number of possible mechanisms, or single actual mechanical system, that underlie the connected phenomena. As Maxwell puts his project in the fourth section of the Treatise,

> What I propose to do now, is examine the consequances of the assumption that the phenomena of the electric current are those of a moving system, the motion being communicated from one part of the system to another by forces, the nature of which we do not yet even attempt to define, because we can eliminate these forces from the equations of motion by the method given by Lagrange for any connected system. (1873, II, p. 198)

8. Maxwell's dynamism.

To set this in a context, we must compare the dynamistic approach of Maxwell to its methodological competitor, mechanism. As Hendry (1986, pp. 38f) argues at length, the mechanistic program is identified by ontologizing about only passive inert matter upon which forces are superimposed. This program counts among its adherents, Locke, Laplace, Cauchy and Poisson. This approach advocates a method that includes these steps: 1) analyze phenomena as systems of molecules or atoms; 2) establish a set of integral equations for action at each distance from the source; 3) differentiate to arrive at approximations of the precise integral equations. These continuous integrations make ineliminable approximations because according to the metaphysics of mechanism, they should be replaced by non-continuous summations of the discrete forces acting between each pair of molecules. In Norton Wise's words, as a Laplacian mechanist,

> One summed over the action of discrete point masses distributed in space to find the total effect at any given point. Integral equations described the physics. In practice it proved more efficient to convert that primary concept to a smoothed partial differential equation. But the Laplacians conceived these differential equations as secondary, purely mathematical constructs. (Wise, 1982, p. 182)

On the other hand, dynamistic theorizing proceeds by starting with an ontology in which forces are elemental, fundamental, inherent in matter. Witn these Leibnizian foundations, there follows a rejection of detailed, experimentally unverifiable mechanical hypotheses such as atomic interaction. The followers of this approach include among their number Lagrange, Fresnel, Fourier, Ohm, and Ampere. All of these to some extent advocate beginning with macroscopic behaviour and moving directly to differential equations (vide. Hendry, p. 38). Fundamental laws, such as Newton's laws of motion, are treated as parts of an abstract mathematical system based on axioms that are self-evidently true and independent of empirical verification; that is, the laws of dynamics are rooted in the mind, rationally grounded. So, science has two parts, a priori and a posteriori, but their relation is Kantian.

Maxwell's methodological commitments can thus be seen as an extension of this dynamistic approach. He mixes it with views found in the psychologisms of both Mansel's and Whewell's schools. As we have seen, in Maxwell's hands the dependence on analogy becomes a mechanism by which the perceptual abilities of the researcher are developed so as to achieve a Whewellian "point of view". For the Victorian pursuing the psychologism in Kant's works, then, science is a kind of perception rather than a kind of language.

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