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On the Disunity of the Sciences and *Ceteris Paribus* Laws

Doctor in Philosophy
2005

Emma Tobin

Declaration:

This thesis, which has been composed by myself, Emma Tobin, is my own work, except where acknowledged to the contrary in the text.

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Summary

This thesis examines the claim that the sciences are disunified. **Chapter 1** outlines and introduces different accounts of the stratification of the sciences in the literature, in particular, *Unificationism*, *Disunificationism*, *Eliminativism* and *Human Science Disunificationism*. I argue that all of these competing views are informed by an ideal model for successful science. In particular, all of the views discussed are committed to the claim that a science requires laws to be considered scientifically legitimate. At the end of this chapter, the narrower topic of the thesis is revealed: do the special sciences have real legitimate *ceteris paribus* laws?

In **Chapter 2**, I examine Fodor's (1974) disunity of the sciences hypothesis, which introduces a distinction between the physical sciences like physics and the special sciences such as geology, biology, psychology and the various medical sciences. These later special sciences are considered 'special' in so far as they cannot be straightforwardly reduced to physics. In this chapter, I claim that Fodor's account requires a more precise analysis of the relationship between the laws and the natural kinds of the special sciences. I claim that if Fodor is to save the special sciences from reductionism, then the burden of proof lies with him to provide an account of *ceteris paribus* laws in the special sciences. Therefore, *Special Science Realism* is reliant on an account of *ceteris paribus* laws.

Chapter 3 assesses the semantic challenge to *ceteris paribus* laws; namely that we cannot provide a non-vacuous account of their semantics. I argue that any attempt to provide the truth conditions by hedging *cp* laws (e.g. Fodor (1991) & Pietroski and Rey (1995)) faces a dilemma: either it is not "*cp* laws all the way down" and *cp* laws are placeholders for the real laws of physics, or it is "*cp* laws all the way down" and all laws are vacuous. I analyse a weaker semantic account (Lange (1993)), but I reveal that this leads to a problem for testing *cp* laws. Consequently, *cp* laws are faced with a pejorative dilemma which combines their *semantic* and *epistemic* problems: either *cp* laws can be completed prior to testing, but only either in a vacuous way or in such a way that they are made placeholders for the real laws of physics or *cp* laws can be completed within testing, but consequently, the authenticity of the scientific investigation cannot be preserved.

Chapter 4 argues that it would be premature, given the semantic problems of the last chapter to argue that there are no *ceteris paribus* laws. There is a further metaphysical question, which needs to be addressed concerning whether there are truthmakers for *cp* laws. I argue that both of the *contingentist* accounts of laws of nature (i.e. the *regularity* theory (Mill (1846), Ramsey (1929), Lewis (1973) and the *nomic necessitation* view (Dretske (1977), Tooley (1977), Armstrong (1968)) cannot save *cp* laws from vacuity. However, I examine the view that dispositions are the truthmakers for *cp* laws (Lipton (1991), (Bird (1998)). I argue that an analysis of the relationship between dispositions and laws reveals that a less stringent account is required, if our theoretical account is to be made to fit with scientific practice. I argue for an *a posteriori* account of dispositions, which significantly effects how we construe the relationship between dispositions and their corresponding laws, which I call V*ariant Dispositionalism*. The dispositional account of *cp* laws is rejected in favour of a looser account of *cp* generalisations.

In **Chapter 5**, I argue that the latter *semantic*, *epistemic* and *metaphysical* problems that face *cp* laws may be the result of an equivocation of importantly different kinds of *cp* laws. I propose the *heterogeneity* problem for *cp* generalisations which claims that candidates for *cp* laws function in very different ways in different disciplines. The difficulty is that all of these generalisations are given a single analysis *qua cp* laws. I make a distinction between *idealized and contextual cp laws.* The *cp* clause in an idealised *cp* law plays the role of indicating that the relationship described in the law is an idealization. The *cp* clause in a contextual *cp* law is standing in for the context; the absence, presence or constancy of which may contravene the law. Moreover, I claim that the laws of physics and the laws of the special sciences are distinct: *cp* laws in physics are idealized *cp* laws whereas the laws of the special sciences are contextual *cp* laws. The upshot is that although we might claim that it is "*cp* all the way down" (Cartwright (1989)) this does not entail that all sciences are made equally. The disunification of the special sciences is supported by my argument that the laws of the special sciences have an entirely different methodology to the laws of physics.

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TABLE OF CONTENTS:

INTRODUCTION: **1**

CHAPTER 1: SCIENTIFIC FUNDAMENTALISM AND THE STRATIFICATION OF THE SCIENCES **22**

1:1 INTRODUCTION 22
1:2 THE VIEWS 30
1:2:1 The Unity of the Sciences 30
1:2:2 Unificationism and the Ideal view of science 41
1:2:3 Disunity and Anti-Reductionism: The Special Sciences 43
1:2:3 Eliminativism 44
1:2:4 Special Science Disunificationism: 47
1:2:5 Human Science Disunification: 51
1:3 DISUNIFICATIONISM AND INTENTIONAL PSYCHOLOGY 55
1:4 SCIENTIFIC FUNDAMENTALISM: 61
1:4:1 Logical Positivism & The Scientific Method: 62
1:5 THESIS OVERVIEW: 68

CHAPTER 2 : THE DISUNITY OF THE SPECIAL SCIENCES: LAWS AND KINDS **69**

2:1 INTRODUCTION: 69
2:2 THE IRREDUCIBILITY OF THE SPECIAL SCIENCES: 70
2:3 DISUNIFICATION OF THE SPECIAL SCIENCES: 76
2:4 THE THREE DISUNITY CLAIMS: 79
2:4:1 Language Disunificationism: 79
2:4:2 Laws and Kind Disunificationism: 80
2:4:3 Special Science Kinds: Jade And Pain 83
2:5 SPECIAL SCIENCE KINDS AND SCIENTIFIC PRACTICE: 90
2:5:1 Domain-Specificity: 90
2:6 COVERING LAW EXPLANATIONS: 99
2:6:1 Covering Law Models and *Ceteris Paribus* Laws: 100
2:6:2 *Ceteris Paribus* Laws & The Semantic Problem: 106
2:7 CONCLUSION: 106

CHAPTER 3: *CETERIS PARIBUS* LAWS: THE SEMANTIC PROBLEM **108**

3.1 INTRODUCTION 108
3:2 *CETERIS PARIBUS* LAWS: 110
3:2:1 Laws vs. Accidents: 110
3:2:2 The etymology of *Ceteris Paribus* 115
3:2:3 *Ceteris Paribus* Special Science Laws: 117
3:2:4 Mendel's Laws & Exceptions: 119
3:2:5 *Ceteris Paribus* 'all the way down'. 121
3:2:6 *Ceteris Paribus* Laws and Probabilistic Laws 122
3:3 THE SEMANTIC PROBLEM 124
3:3:1 The Context-Specificity of *Cp* Laws 125
3:3:2 *Cp* Laws are Indeterminate Propositions: 129

3:3:3 *Cp* Laws are Tautologies 129
3:3:4 *Cp* laws are Vacuous 130
3:4 PROVIDING THE TRUTH CONDITIONS: 131
3:4:1 Hedged *Cp* Laws: Providing the Truth Conditions in principle 134
3:4:2 *Cp* Laws in Scientific Practice 143
3:5 *CP* LAWS "ALL THE WAY DOWN" 148
3:6 TRUTH-CONDITIONAL ACCOUNTS & SCIENTIFIC FUNDAMENTALISM: 150
3:7 EPISTEMIC SEMANTICS FOR CP LAWS: 150
3:8 THE PRINCIPAL DILEMMA: 154
3:9 CONCLUSION: 155

CHAPTER 4: *CETERIS PARIBUS* LAWS AND TRUTHMAKERS 157

4:1 INTRODUCTION: 157
4:2 LAWS OF NATURE AND ACCIDENTAL GENERALISATIONS: 163
4:3 THE REGULARITY THEORY OF LAWS OF NATURE: 163
4:3:1 The Regularity View and *Cp* Laws: 170
4:4 NOMIC NECESSITATION 175
4:4:1 Nomic Necessitation and *Cp* Laws: 177
4:5 THE DISPOSITIONAL VIEW OF LAWS: 183
4:5:1 Dispositional View and *Cp* Laws 186
4:5:2 *Cp* Dispositions and Verification: 189
4:5:3 Dispositional Essentialism: 194
4:6 VARIANT DISPOSITIONALISM: 197
4:7 CONCLUSION: 201

CHAPTER FIVE: THE JANUS-FACED *CETERIS PARIBUS* CONDITION: 203

5:1 INTRODUCTION: 203
5:2 THE MEANINGS OF *CETERIS PARIBUS*: 205
5:3 THE HETEROGENEITY PROBLEM: 206
5:4 THE *CP* PROVISO: CONTEXTUALISATION OR IDEALISATION. 216
5:4:1 Contextual *Cp* Laws 216
5:4:2 Idealised *Cp* Laws: 219
5:4:3 The Application of Idealised *Cp* laws: 223
5:4:4 Contextual *Cp* Laws & Genuine Exceptional Cases: 227
5:5 CONTEXTUAL & IDEALIZED *CP* LAWS & THE SEMANTIC PROBLEM 230
5:5:1 Contextual *Cp* Laws and the Semantic Problem: 230
5:5:2 Idealised *Cp* Laws and the Semantic Problem: 234
5:5:3 The Methodological Problem of Application: 238
5:6 "*CP* ALL THE WAY DOWN" 239
5:7 THE DISUNITY OF THE SPECIAL SCIENCES & *CP* GENERALISATIONS: 240

CONCLUSION 242

APPENDICES 246

APPENDIX I : THE DERIVATION OF KEPLER'S LAWS FROM NEWTON'S LAWS .. 246
APPENDIX II: A VARIETY OF *CETERIS PARIBUS* GENERALISATIONS FROM DIFFERENT SCIENCES: 247

BIBLIOGRAPHY 250

List of Illustrations

Fig 1.1	*Unificationism*
Fig 1.2	*Special Science (SS) Disunificationism*
Fig 1.3	*Human Science(HS) Disunificationism*
Fig 1.4	*Special (SS) & Human (HS) Science Disunificationism*
Fig 1.5	*Natural Special Science (NSS) and Human Special Science (HSS) Disunificationism.*
Fig 1.6	*Natural Special Science (NSS) and Human Special Science (HSS) Disunificationism and Both (B)*
Fig 1.7	*Inverted Pyramid of the Sciences*
Fig 1.8	*Illustration of the Relationship between the Special Sciences and the Reducing Science in Special Science Disunificationism*
Fig 2.0	*Illustration of the Relationship between the Special Science and the Reducing Science Under the Paradigm of Reductionism*
Fig 2.1	*Illustration of the Relationship between the Special Science and the Reducing Science in Special Science Realism*

INTRODUCTION:

The common sense view of the world that we are accustomed to is that the universe is ordered. Because the universe is ordered we believe that we can predict and explain this order. Both philosophers and scientists are tempted to believe that laws of nature play a governing role in reality and consequently, that the order and uniformity that we observe in nature is the result of or is best explained by laws of nature. One of the chief aims of scientific enquiry then is to discover nature's laws.

Examples of order in the universe are widespread. The planets in our solar system move in ellipses. Light reflects from surfaces in a regular manner. Metals expand when heated. In a sequence of sedimentary rocks, the deeper layers are older than the shallower layers. The price of goods is increased by consumer demand in a market economy. Tyranny causes revolution. Frustration leads to aggression. Speed causes road accidents. Moa's never reach longevity, since they all die before the age of fifty. We can see then that there are many examples of regularities in the world.

The problem of laws of nature is how we are to distinguish between those regularities in nature that are merely accidental as opposed to those that are lawful. For example, we believe that metals expand when heated because there is a corresponding law, which states that 'All metals expand when heated'.

Conversely, there is an intuitive sense that some of the regularities above are merely accidental. For instance, Popper ((1959) : 427-8) has rightly pointed out that even though all known Moa's (birds from New Zealand that have been extinct for centuries) have actually died before the age of fifty that this regularity is not supported by a law. However, the reason why all Moa's died before fifty was because of a rare virus in the New Zealand environment (i.e. an accident). The traditional problem of laws of nature is how precisely we ought to draw the laws/accidents distinction given that our evidence for both laws and accidents is often the same (i.e. universal regularities).

Philosophers and scientists alike have perpetuated the common sense belief that we occupy a tidy and ordered universe. Moreover, there is a belief or at least an aspiration that someday we will discover a theory that explains this order in its entirety. Since physics is the paramount science, this future theory is most likely to come from future fundamental physics. Consequently, it is often supposed that the successful mathematical and physical sciences will provide us with the real laws of the universe, those governing principles from which all else ought to flow naturally and in an ordered fashion.

However, the universe as we know it is indeed complex and in order to understand it, physics is often put to one side in favour of other scientific disciplines, like economics, psychology, the medical sciences and biology. There are therefore important issues to be raised about the scientific status of these so-called *special sciences*. For example, can we hope that these sciences will one day be reduced to physics? Furthermore, if they do not reduce to physics, how are

we to assess their scientific legitimacy? Moreover, what characteristics must a special science possess to be considered scientifically self-sufficient? This thesis is primarily concerned with these issues. I wish to examine these broader issues in terms of a more specific, but crucial question regarding the special sciences; namely, do the special sciences have empirical laws?

Why is the question of laws important? Firstly, since one of the chief aims of scientific enquiry is considered to be the discovery of laws of nature, it might be argued that the scientific legitimacy of any discipline is dependent on whether that discipline has empirical laws. Secondly, the converse is also thought to be true: if a scientific discipline does not have empirical laws, then we ought to eliminate it altogether and consider it to be pseudo-scientific. For this reason, I see the question of empirical laws as one of fundamental importance to the larger enquiry about the status of the special sciences.

This specific debate about laws in the special sciences also has a larger philosophical importance for other issues in the philosophy of science, other than the sole issue of the status of scientific disciplines alone. In fact, a deeper analysis of this issue than has been provided in the previous literature has important implications for other debates in the philosophy of science too. For this reason, it is a topic worthy of more attention than it has been given in general philosophy of science research. Indeed, the issue of how we construe the "special" laws of the special sciences occupies a pivotal position at the juncture of some important issues in the literature.

These issues are as follows: The first issue is whether the sciences can be unified or whether they will remain disunified even in the event of a completed physics. The second issue is how we are to understand the disunity of the sciences claim. The third issue is whether those paradigmatically exception-ridden generalisations of the special sciences can be construed as a specific kind of law; namely a *ceteris paribus* law (a law that holds only all other things being equal). This also poses questions concerning the characteristics a generalisation ought to possess in order to be considered as a law. The fourth issue is whether the laws of physics are any different to the laws of the special sciences or in Cartwright's terminology whether it might be "*ceteris paribus* all the way down" (even in physics). I will firstly, introduce these issues to you and at the end of this introduction, I will introduce the main tenets of my position and explain how this new account can provide new ways of interpreting these olds problems in the philosophy of science.

Unity vs Disunity:

The first issue I wish to introduce is the question of how we should stratify the sciences. The traditional view of the sciences, known as *unificationism*, is the view that ideally all scientific disciplines will be unified by a completed physics or a grand theory of everything (Putnam & Oppenheim (1948)). Unification may however, entail different accounts of the special sciences. The standard *reductionist* view is that the special sciences will be made reducible to some ideal future physical theory. Conversely the *anti-reductionist* view claims that the special sciences cannot be reduced to physics.

The *anti-reductionist* viewpoint is motivated by the empirical hypothesis that the kinds of the special sciences could be implemented in vastly different physical mechanisms. Take for example, Farr's law, which describes the exponential spread of epidemics. It does not, in fact, matter which physical mechanism implements a given epidemic, (e.g. whether it is an influenza epidemic, a polio epidemic or an obesity epidemic) but only that it is an epidemic and that it spreads exponentially.

For this reason, there will be no simple correspondence between the physical mechanism that happens to implement a given special science kind on a given occasion and the special science kind itself. Therefore, it will not be possible to reduce the special science kind to a physical mechanism as such because there is always the open possibility that a special science kind could be implemented some other mechanism. Consequently, a one-to-one reduction of any given special science to physics is *ipso facto* impossible.

Broadly speaking, in the literature, there are two accounts of the special sciences that can be given, assuming anti-reductionism. The first is *Eliminativism* and the second *Special Science Realism*. *Eliminativism* is the view that the failure of the special sciences to reduce to physics ought to be taken as evidence for their scientific illegitimacy (Churchland (1981)). According to this view, the special sciences have no more scientific prowess than those now outmoded theories (e.g. medieval medicine), which have been eliminated in favour of new and better theories as science progresses.

The opposite view is that the failure of the special sciences to reduce to physics ought to be considered as evidence for the scientific prowess and hence, autonomy of the special sciences (Fodor (1974)). Fodor moves from the irreducibility of the special sciences to the claim that the special sciences can be analysed according to the exact same criteria with which we analyse physics. In particular, he claims that the special sciences can support scientific explanations, because they have real empirical laws that refer to natural kinds. It is the latter view that is the primary focus of this thesis.

The Disunity of the Sciences and Scientific Fundamentalism:

This brings me to the second issue that I wish to introduce: how we are to understand the disunity of the sciences claim. The choice between elimination, on the one hand and realism, on the other, oversimplifies the possible ways we might analyse the special sciences. In fact, I wish to argue, in this thesis, that the disunity of the sciences, as an empirical hypothesis, provides only a starting point for analysing the scientific legitimacy of the special sciences.

My position is that there have been major oversights in the assessment of what makes a special science "special". The most serious issue is that all of the latter ways of dividing the sciences work within a pre-conceived notion of what science is ideally meant to look like, which has been referred to as *Scientific Fundamentalism* (Cartwright (1999)). Scientific fundamentalism presupposes that there is an uncontentious model for successful science; namely that we know the characteristics a science should *ideally* have.

The *ideal* model for successful science is motivated by unificationism and by the view that future science will provide us with a theory that is uniform, over-arching and explanatorily wide. However, taking future physics as the ideal model for all sciences, results in a rather stringent and rigid classification of the language of science. In fact, it sets science up so that there is no alternative to future physics fundamentalism.

Moreover, this ideal is so pervasive that ironically, even though disunification presents a challenge to the unity of sciences view, we are still coerced into analysing the special sciences in terms of the criteria set down for physics by the *unificationists*. For example, it is assumed that a special science must satisfy the stringent positivistic criteria set forth by successful scientific practice in the more fundamental sciences (i.e. the hard physical sciences like physics), such as explanatory rigour, wide scope, testability, projectibility, universal generalisations (laws) and so forth.

However, I argue that the chief constraint placed on successful science by the *ideal* science model, is that a science must contain laws to be considered scientifically legitimate. Moreover, not just any old generalisation will count as lawful; rather there are also theoretical constraints from the *ideal* science model that constrict the kinds of generalisations that will count as genuine empirical laws. In particular, a law ought to be formulated syntactically as a universally quantified conditional generalisation in the first order predicate calculus e.g. $\forall x$

($Fx \rightarrow Gx$), it should have wide scope, explanatory power and not be restricted to spatio-temporal domains.

Special Science Laws:

Now the problem that faces the special sciences is that their generalisations have *ipso-facto* exceptions and so it is difficult to construe them as laws in terms of these rather rigid criteria. They only hold for the most part or 'all other things being equal'. This is because these generalisations are removed from the complex situations that special science kinds find themselves in, in diverse contextual situations. Something can always intervene in reality that prevents a generalisation from being applicable in a certain context.

Take an example from a special science like biology. Bergmann's Law describes the size differences in populations of species of animals in restricted climactic conditions. It states that for species of warm-blooded vertebrates, races living in cooler climates are larger than races living in warmer climates. However, there are exceptional cases to this law. For example, some vertebrates can build burrows and consequently, the body size of these animals does not alter in cooler climates, as they do not need the higher body mass-to-surface area ratio that favours heat retention. Therefore, we have a case where something changes in the environment and contravenes the law from being applicable to certain vertebrates in that environment.

The problem then is that we do not know how to construe these kinds of generalisations. It certainly would be incorrect to construe Bergmann's law as a

universal generalisation, since burrowing animals present an obvious counterexample. For this reason, an eliminativist would argue that these generalisations are simply heuristic rules of thumb, possibly placeholders for the real laws of physics, which will provide an exhaustive account of the mechanisms that implement such regularities.

There is an implicit assumption made here that since the generalisations of the special science cannot compete with the laws of physics for law status, then we ought to conclude that the special sciences do not have the same claim to scientific legitimacy. Moreover, in the event of our best knowledge it is probable that the special sciences will be eliminated in favour of the comprehensive and completed theory.

The Disunity of the Sciences: *Ceteris Paribus* Laws and Natural Kinds

This brings me to the third issue that I want to introduce namely, whether the paradigmatically exception-ridden generalisations of the special sciences can be construed as *ceteris paribus* laws. Fodor (1974) (1991) argues, *pace* the eliminativist view, that the exception-ridden generalisations of the special sciences can be construed as genuine *ceteris paribus* laws (laws that hold "all other things being equal"). So, despite the fact that the laws of the special sciences have exceptions, Fodor argues that we can nevertheless provide an account of them in terms of the constraints of the ideal science model. For Fodor, any special science law can be formulated syntactically as a universally quantified conditional generalisation in the first order predicate calculus, with an added

ceteris paribus appendage, {e.g. $\forall x\ (Fx \rightarrow\ Gx\ )\ cp\ \}$ where the *ceteris paribus* appendage indicates that the law may have exceptions.

Additionally, Fodor argues that the predicates contained in these special science laws are genuine natural kinds. However, Fodor accepts a weak construal of special science kinds, such that the multiply realised kinds of a special science are no more than "the bound variables in its proper laws" (Fodor (1981): 132). Therefore, Fodor makes the kinds of the special sciences parasitic on the laws. For this reason, he argues that once a case for *ceteris paribus* laws in the special sciences can be made, he is content to reify special science kinds and grant these sciences "autonomy". Consequently, Fodor argues for a view that might be called *Special Science Realism*. Nevertheless, the burden of proof is on theorists like Fodor to corroborate *Special Science Realism* by providing an account of special science laws. The challenge then that *Special Science Realism* faces is to provide an account of *ceteris paribus* laws.

Ceteris Paribus Laws and Their Problems:

So why are *ceteris paribus* laws problematic? Firstly, there is a *semantic* problem in providing truth conditions for *cp* laws. It can be argued that *cp* clauses have no clearly determined content in so far as "all else being equal" refers to an indeterminate set of affairs (Schiffer (1991), Earman & Roberts (1999)). So, 'All *F*'s are *G*'s *cp*' could be read as 'All *F*'s are *G*'s unless they are not', or 'All *F*'s are *G*'s *iff* All F's are G's' or indeed as 'All *F*'s & {……} are *G*'s' and thus the statements that express *cp* laws are vacuous. Therefore, it can be argued that

either *cp* laws can be given a determinate content, but that in doing so they are rendered vacuous or that they cannot be given a determinate content and so they are open-ended and lack the precision required for scientific explanation.

Secondly, there is an *epistemic* problem in so far as it is not clear how these *cp* clauses can be tested. Since, very often, we do not know what it means for all else to be equal in advance of testing any given *cp* law, then how can it ever be confirmed? Clearly, there is no finite set of empirical observations that can verify such claims. Even if such claims were testable, how could we know that the tests were legitimate, since scientists can count just about anything as an exception so as to retain the legitimacy of the law?

Providing Truth Conditions for *Ceteris Paribus* Laws:

Nevertheless, in the literature, there have been many attempts to solve the semantic problem (and to prevent the epistemic problem from arising) by providing the truth conditions for *cp* clauses. In this thesis, I consider two attempts which provide the truth conditions for *cp* laws (Fodor (1991) and Pietroski and Rey (1995)). One possibility is to specify some further condition *C*, which in conjunction with the antecedent *F* will always bring it about that *G (e.g.* 'All (*F*&*C*) are *G*'). [1] However, this move makes no real semantic advance, since we are still unsure of what *C* means on this account, i.e. we have replaced *cp* with an equally problematic *C* and so we could just ask what the semantic content of *C* is?

[1] Fodor (1991) calls the condition *C* the "completer". However, he argues that the completer will be from the realising science and that the realiser and the completer together will be jointly sufficient for the implementation of a given *cp* law. This is supposed to leave open the possibility of exceptions because there could be realisers that are not accompanied by completers.

The second possibility, as argued by Pietroski and Rey (1995), is to claim that we can explain why an exception is abnormal in scientific practice by citing the independent factors that occurred to contravene the law. This ability in scientific practice is a sufficient condition for a given *cp* law being non-vacuous. So, given the law 'All *F*'s are *G*'s *cp*' and given an exceptional case of $F \& \neg G$ then we will have a further condition *C* that will explain $\neg G$. Importantly, *C* must do some other independent explanatory work (in science) so that it is not *ad hoc*. However, this simply defers the problem at hand, because the independent 'scientifically legitimate' theory has to be itself given a justification. In giving an account of laws, we are essentially trying to give an account of scientific explanation. So, to assume that the 'scientific' legitimacy is not itself problematic requires justification.

I suggest, however, that these two attempts to provide the truth conditions in principle assume that *ceteris paribus* laws are supported by a secure underlying bedrock of scientifically legitimate theories (with strict laws) in physics. In other words, these attempts to provide the truth conditions for *cp* laws assume that it is not "*cp* all the way down".

Ceteris Paribus "all the way down"?

This brings me to the fourth and final issue that I wish to introduce; namely whether it is "*cp* all the way down", even in fundamental physics and how precisely we are to understand that claim. It is interesting to note that others (e.g.

Earman et al (1999), (2002)) coming from the same premise as Fodor (1991) and Pietroski and Rey (1995) (i.e. that it is not *cp* all the way down) come to the opposite conclusion; namely that there are no genuine self-standing *cp* empirical laws.

The idea is that if we hedge *cp* laws so that they require physics for completion, then they really are not self-sufficient. They are at best 'autonomous' in so far as they are good heuristic devices. However, at worst they are merely placeholders for the real laws of physics. Therefore, it would seem that if *cp* laws require strict laws in physics for completion, then they lose their autonomy as self-standing empirical laws, in that they will be reduced to the laws of physics that complete them.

On the other hand, we might chose to argue that it is "*cp* all the way down", as theorists like Cartwright (1989) have claimed, so that even the laws of physics require the *ceteris paribus* proviso. The issue then would be that the semantic problem for *cp* laws in the special sciences becomes a problem for *all* laws in all of the sciences, since all laws are *cp*. Therefore, I claim that *cp* laws face a dilemma, either it is not "*cp* all the way down" and *cp* laws are placeholders for the real laws of physics, or it is "*cp* laws all the way down" and the semantic problem cannot be solved by reference to more the fundamental strict laws in physics.

Semantics in Practice?

Now one alternative possibility is to argue that perhaps the requirement that *cp* laws need truth conditions is too stringent a requirement for meaning in science. Lange (1993a) has argued that *cp* claims are perfectly meaningful in so far as there is tacit agreement in terms of what constitutes a counterexample to any given *cp* law in scientific practice. He takes the Wittgensteinian paradigm of meaning as rule following and applies it to scientific practice.

So, despite the fact that we cannot enumerate every theoretically possible exception to a *cp* law in advance of applying it in scientific practice, we can nevertheless successfully use *cp* laws in scientific practice to generate scientific explanations and predictions. Since the *cp* generalisations of the special sciences satisfy these criteria they ought to be deemed lawful. Now the problem is that on this account, it is unclear why we should construe these 'rules' as laws at all, rather than heuristic devices for the guidance of scientific practice. These rules merely provide an ability to distinguish between exceptions and falsifications.

Moreover, there is a genuine problem in testing these claims. Indeed, the falsifibility of claims will be even more pertinent if *cp* laws are only epistemically justified. Clearly, if *cp* clauses are considered true only because of some tacit background epistemic commitments, then when the law is tested the scientist can always justify the law by appealing to his own epistemic inkling.

Therefore, I will argue that *cp* laws are faced with a pejorative dilemma which combines the *semantic* and *epistemic* problems for them: either (a) *cp* laws

can be completed prior to testing, but only either in a vacuous way or in such a way that they are made placeholders for the real laws of physics (the *semantic* problem) or (b) *cp* laws can be completed within testing, but consequently, the authenticity of the scientific investigation cannot be preserved (the *epistemic* problem). Taking either horn of the dilemma construing *cp* generalisations as laws does not make them scientifically legitimate.

Metaphysics to the Rescue?

So should we conclude that there are no *cp* laws and that special science realism cannot be sustained? There is another possible line of response; namely, that the semantic problem is only problematic for an account of *ceteris paribus* law statements rather than *cp* laws of nature themselves. This claim involves a distinction between an *epistemic* account of law statements and a *metaphysical* account of laws. However, whether there is conceptual room between these two accounts depends very much on the account of laws of nature that one is committed to more generally. We might question then whether there are ontological properties in nature to which law statements are reducible.

On a closer examination, I will argue that neither of the contingentist accounts of laws of nature (i.e. the regularity view of laws (Hume, Mill, Ramsey, Lewis) and the nomic necessitation view of laws (Armstrong, Drestke, Tooley)) can provide ontologically basic properties that might act as truthmakers for *cp* laws.

Theorists such as Ellis (2002) and Mumford (2004) have, I think rightly, argued that in fact both regularity and nomic necessitation views are committed to an empiricist metaphysics, where the laws are made more metaphysically basic than the properties (or the properties of the kinds) referred to in those laws. It is the laws (or the regularities described by laws) about categorical properties, which make the properties, behave in the way they do rather than any essential metaphysical facts about those same properties. For this reason, neither of these accounts can provide properties that are more basic than *cp* laws, which might act as truthmakers for *cp* laws.

An alternative view as argued by Lipton (1995), Ellis (2001) and Bird (2004) is that laws are descriptions of underlying stable dispositions. According to Bird (2004), the truth conditions for *cp* laws become unproblematic given the assumption that they describe underlying stable dispositions and given a conditional analysis of those dispositions. This is because it is still true to say that an object possesses a certain disposition, even if the disposition is not manifested in a certain situation. The conditional in a *cp* law refers to an underlying disposition by capturing the manifestation of the disposition given the presence of a certain stimulus, "all other things being equal", where the "all the other things being equal" refers to situations when the disposition fails to manifest itself. It is still *true* to say that a property or object possesses a disposition even if that disposition is not right now manifesting itself.

However, there is a second problem for the dispositionalist view as outlined in Lipton (1999), which is to claim that a theory of dispositions faces its

own semantic problem; namely, how are we to give content to our dispositional terms? What does it mean to say that something is fragile? Well surely it means nothing more that the corresponding conditional i.e. 'If it is fragile, then it will break'. The dispositionalist makes a metaphysical assumption that the disposition is more ontologically basic that the corresponding law.

A Humean about laws can straightforwardly deny this and ask how we can rationally justify our assumption that there are dispositional essential properties in the world, when our only way of knowing about them is via the generalisations that we make, i.e. by *cp* laws. So the dispositionalist analysis is circular; *cp* laws need dispositions to be meaningful and in order for us to know about dispositions we need *cp* laws.

I argue that a closer analysis of the relationship between dispositions and laws reveals that a less stringent account of their relationship is required if our theoretical account must be made to fit with how generalisations capture dispositions in scientific practice. Therefore, even if dispositions did provide truthmakers for *cp* laws, I argue that this account would not be consistent with scientific practice, particularly in the special sciences. Moreover, it would involve a metaphysical assumption about dispositions that cannot be verified. The dispositional account is not exhaustive, because we can imagine cases of conditionals which describe dispositions which are not lawful and we can also imagine conditionals which we think are lawful, which cannot be given a dispositional analysis. Therefore, the dispositional analysis cannot provide a uniform analysis of *cp* laws. It is misleading and ought to be rejected.

Cp Generalisations and Scientific Practice:

The thesis then aims to bring together distinct *semantic*, *epistemic* and *metaphysical* problems for *cp* laws. In the end, however, I propose that an analysis of the different *cp* generalisations that compete for *cp* law status in scientific practice is required. In fact, I argue that one of the chief problems with the analysis of many problems in the philosophy of science is that they do not pay sufficient attention to scientific practice. A closer examination of diverse *cp* generalisations exposes a new problem for *cp* laws which reveals that the uniform analysis of diverse *cp* generalisations *qua cp* laws is misleading in that it homogenises different kinds of generalisations, so that important differences in content are not recognised. I call this the *heterogeneity* problem.

In the first instance, this might sound promising in that the earlier problems addressed in the thesis may be the upshot of our failure to distinguish between different kinds of *cp* law in the first place. I wish to distinguish at least five different kinds of *cp* generalisations that involve different kinds of claims: causal (e.g. Law of Tumour Remission in Oncology), non-causal (e.g. Farr's Law in Epidemiology), functional (e.g. Gay Lussac's Law in Chemistry), idealised (e.g. Ohm's Law in electronic engineering) and restricted spatio-temporal claims (e.g. Bergmann's Law in Biology). It is indeed important to point out that these generalisations which all compete for *cp* law status function quite differently. However, it is only of significance if the differences between these generalisations involve a difference in the content of their *cp* clauses.

Contextual vs. Idealised *Cp* Laws -- Application and Exceptions:

I will argue that the attempt to provide a uniform account of *cp* laws has been misled by an equivocation between two different kinds of *cp* law: *Contextual cp* Laws and *Idealised cp* Laws. This distinction can be seen by looking at the differences in how these laws' clauses function. The *cp* clause in an *idealised cp* law plays the role of indicating that the relationship described in the law is an idealization. The *cp* clause in a contextual *cp* law is standing in for the context; the absence, presence or constancy of which may contravene the law.

There are two further differences that make this distinction clearer. The first difference regards how these laws apply in scientific practice. *Idealised cp* laws only apply to situations where the variables in the generalisation would happen in isolation. Importantly, they make *no* claim about what would happen if that generalisation were applied in a context. This is importantly different to the contextual conception because contextual *cp* laws stipulate that the context will count, whereas the *cp* in idealised *cp* laws stipulates that, were there no context other than the one described explicitly in the generalisation, then the generalisation would hold.

The second difference regards the role of the exceptional cases to these *cp* laws. *Contextual cp* laws have 'genuine' exceptions, whereas *idealised cp* laws do not. A context can dictate a genuine exception to a law, if there is a case where the law is literally false in that context. The role of the *cp* clause in a contextual generalisation is to indicate that in some contexts there are 'genuine' exceptions. In contrast, the role of the *cp* proviso in an *Idealised cp* laws is as an

approximation, which indicates that the regularity described in the generalisation is an idealisation.

The Disunity of the Sciences and Scientific Practice

I argue that, since there are importantly different kinds of *cp* law, the claim that it is "*cp* all the way down" need not entail that all sciences are made equally, nor that all of the sciences should be provided with a uniform analysis. So, the distinction between *idealized* and *contextual cp* laws underscores a fundamental difference in the methodology of physics in contrast to the special sciences. The special sciences function by using generalisations that involve contextualisation, whereas physics functions by using generalisations that involve idealisations. Therefore, the feature that makes the special sciences "special" is that they have their own kind of laws; namely contextual *cp* laws. The position that I defend in this thesis is that the disunity of the sciences is a disunity of methodologies, where the laws in physics function in an entirely different way to the laws of the special sciences.

It may be helpful to bring together the basic arguments that I shall defend in the thesis before I begin. They are as follows:

1) The disunity of the sciences ought to be defended.
2) The laws of the special sciences are more basic than special science natural kinds.
3) The disunity of the sciences depends on an account of special science laws.

4) The generalisations of the special sciences are *ceteris paribus* and so cannot be treated as strict laws.
5) *Cp* generalisations have semantic, epistemic and metaphysical problems that make it difficult to construe them as laws.
6) The uniform analysis of all laws in science as *ceteris paribus* laws is misleading.
7) The claim that it is "*cp* all the way down" does not entail that the laws of physics are of the same kind as the laws of the special sciences.
8) The disunity of the sciences is made apparent by the fact that the generalisations of the special sciences function differently to the generalisations in physics.
9) Realism about the special sciences does not require an analysis of the special sciences in terms of the criteria that are laid down by the *ideal* model of future physics.
10) The special sciences are "special" because they have their own distinct kind of contextual *cp* generalisation.

CHAPTER 1: SCIENTIFIC FUNDAMENTALISM AND THE STRATIFICATION OF THE SCIENCES

1:1 Introduction

There is much debate in the philosophy of science regarding whether science will ever be unified. Science in the modern world had been compartmentalised into a heterogeneous group of distinct disciplines; with rather rigid boundaries in terms of the subject matter that they deal with. Moreover, the more specific these disciplines become the more heterogeneous scientific enquiry looks more generally. Traditionally[1], the ideal of science was unification, namely, to specify some unified theory or equation to which all other scientific disciplines could be reduced. This question is clearly an important one for the significance that we grant to our present scientific disciplines and theories.

The question of whether science can be unified and so understood as a homogeneous discipline is closely linked to the claim that science could one day be completed; in so far as future completed physics would provide one homogeneous, true theory that would exhaust all scientific explanation. However, unification need not entail completion, since even in the future event that science were completed, in so far as future physics provided us with a theory of the ultimate constituents of reality, the question of whether all scientific disciplines ought to, or whether indeed could be unified with physics will remain. Therefore,

[1] The movement known as the Vienna Circle published its chief manifesto in 1929 advocating the unity of the sciences hypothesis. This was based on the rejection of traditional metaphysics and a strict adherence to physicalism.

this question is an important conceptual one, whatever may happen with future empirical science.

There is a distinct competing hypothesis *disunificationism,* which is the claim that even in the event of our best knowledge of physics that scientific knowledge will involve a disunified cluster of disciplines, each with their own theoretical and practical interests. Clearly, our commitments to unification or disunification will depend on prior judgements about what should really be counted as science. Whether or not these prior judgements should be considered defensible is something that is rarely considered. In this chapter, I wish to look at these prior judgements about scientific legitimacy and see how they inform our classification of the different scientific disciplines.

At the heyday of positivism, the aim of scientific enquiry was to work towards the completion of science. The finished product of future science was to be a unified theory of everything, conceived of by Putnam & Oppenheim (1958) as the reduction (in principle) of all scientific disciplines to fundamental particle physics. This hypothesis might be dubbed *microphysical fundamentalism or unificationism*. The claim here is that understanding our most fundamental representation of the world in terms of a completed particle physics, will be the best understanding of the world that we can have because it will cut nature at its very joints.

Moreover, all the other competing scientific disciplines will be in principle reducible (via some schema for reduction) to the completed physical unifying

theory. Completed physics will be a grand unifying theory, a deductively closed set of laws that governs all the phenomena of nature and picks out nature as it actually is. The unified theory will provide an Archimedean point for the scientific community, so that the truth of the theory (provided by fundamental physics) will be in complete correspondence with reality. It can be represented in the following way:

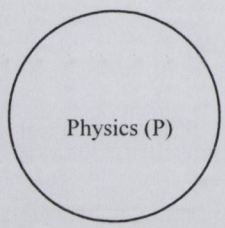

Fig 1:1 Unificationism

A competing hypothesis argues that science will involve a loosely related finite number of disciplines each governing their own phenomena defined in terms of the theoretical and practical interests of individual theories. It is motivated by the possibility that the reduction of all other scientific theories to physics will be prohibited not only in practice, but also in principle. This can be termed *disunificationism*.

Disunificationism can be understood in several ways. It is important to conceive of the possible classifications that disunification might yield. The first conception as conceived by Fodor (1974) introduces a distinction between the special sciences (geology, meteorology, psychology and so on) and fundamental sciences (physics), but classifies the special sciences in terms of the exact same criteria for legitimate science that are characteristic of physics; namely the special sciences should have laws that refer to genuine natural kinds, which support

explanation, are confirmed by their instances and so forth. The special sciences remain distinct from fundamental physics because the predicates of the special sciences are multiply realisable in the predicates of fundamental physics.

This characterisation implies that a complete understanding of the universe will involve a matrix of explanations from all of the scientific disciplines physics; biology, geology, the medical sciences, chemistry etc. Moreover, were the correct basic fundamental science theory to be provided, it would not replace the special sciences in so far as these scientific disciplines would remain distinct and autonomous. Call this version of disunification *special science disunificationism*. It can be illustrated as follows:

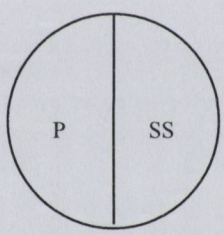

Fig 1:2 Special Science (SS) Disunificationism

The second kind of disunificationism involves a rather different claim. It motivates the view that there is a distinction between what might be called the natural sciences (physics, biology, chemistry) and the human sciences (psychology, economics), which for that very reason precludes reduction[2]. The human sciences describe a distinctly rational world, which involves a reference to norms and practices, as well as context-specific and environmental factors. For

[2] This kind of distinction between natural science and social science is seen in Wilkerson (1995), where he argues for the metaphysical view that the natural kinds of the natural sciences (physics, chemistry and biology) are a different kind of natural kind to those we see in the social sciences (psychology, economics, sociology).

this reason, the human sciences are *toto coelo* different in methodology to the fundamental sciences (Schutz (1954)). Since, the human sciences are by definition relative to the viewpoint of the ideally rational human agent then they can never attain the kind of objectivity and generality that is characteristic of fundamental physics. Let's call this version of disunification *human science disunificationism.* It can be illustrated in the following way:

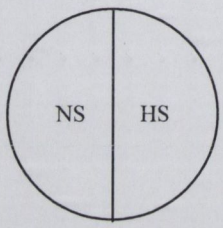

Fig 1:3: Human Science (HS) Disunificationism

Furthermore, it is certainly possible to combine some of these views to take hybrid positions that combine the latter views about disunification. For example, we could coherently hold a tripartite disunificationism which admits that there is a perfectly coherent way of distinguishing between the human sciences and the special sciences. On this view, the failure to reduce both the special sciences and the social sciences to physics entails that both of the latter are distinct from physics. It can be illustrated in the following way:

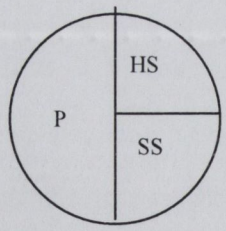

Fig 1.4 Special (SS) & Human (HS) Science Disunificationism

However, it is certainly an open question (and may have to be decided on a case-by-case basis) as to which special sciences should be cashed out in terms of natural special sciences as opposed to those that ought to be cashed out in terms of social special sciences. It is indeed possible to view the special sciences as further compartmentalised in terms of which ones operate (in so far as they share a common methodology) in the same way as the fundamental natural sciences and which ones do not; namely those that function in a similar vein to the social sciences.

We might see, for example, a tripartite division of the sciences. This would be to argue that reduction introduces a distinction between the fundamental sciences (physics) and the natural special sciences (biology, geology, the medical sciences, like pathology, epidemiology, cardiology and so forth), but that there is a further distinction between special sciences like psychology and economics and the natural special sciences, in so far as psychology is specific to norms and practices similarly, economics explains and predicts in terms of the ideal rational agent. It can be illustrated as follows:

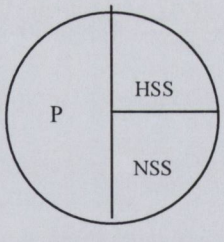

Fig 1. 5 Natural Special Science (NSS) and Human Special Science (HSS) Disunificationism.

Moreover, there is a possible argument that suggests that there is not a clear-cut line between the natural and human special sciences as suggested above.

Some of the special sciences might present a problem in that some scientific explanations in these disciplines are provided by theories that fit the natural science schema whereas others involve distinctly anthropocentric explanations. It can be illustrated in the following way:

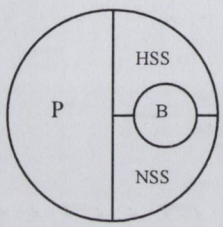

Fig 1.6 Natural Special Science (NSS) and Human Special Science (HSS) Disunificationism and Both (B).

Take psychology and economics as exemplary cases. In economics, the supply and demand curve, originally developed by Alfred Marshall (1890)[3], attempts to describe and explain the influence of supply and demand on prices in a competitive economy. So take supply and demand explanations: An increase in cash flow leads to an increase in prices (or inflation). We can explain this in a naturalistic way if we are to explain why this happens in terms of market trends, the increase in capital and the effect on the supply of goods. However, we can clearly also explain it in terms of the norms, preferences and dispositions of consumers in a market economy.

Clearly then, the possibility of these different ways of stratifying the sciences establishes that any claim about disunity must only be taken as a starting

[3] Alfred Marshall (1980) formulated the law of supply and demand in his *Principles of Political Economy*.

point for enquiry. A further deliberation is required to get from the premise of disunification, to an analysis of what disunification (understood as irreducibility) means for how we should think about the stratification of the sciences.

In Section **1:2**, I will analyse the different forms of disunification that are possible given the premise of irreducibility. More specifically, I will question what disunification entails for how we classify the special sciences (such as biology, epidemiology, cardiology, psychology and so on). I will argue that how we understand the disunification claim is informed by background assumptions about what criteria are constitutive of successful science. The so-called special sciences lie at the juncture of an alleged disjunction between hard physical science on the one hand and social science on the other. In section **1:3**, I will analyse the pivotal position that intentional psychology has in our possible accounts of disunity. In section **1:4**, I will reveal that the false disjunction between hard physical science on the one hand and social science on the other, is based on a rather rigid form of physics chauvinism, where it is assumed that the criteria for successful science are those criteria that make physics successful such as explanatory rigour, wide scope, testability, and projectibility.

These criteria limit the possible ways we might understand the disunification hypothesis. In fact, in Chapter 2, I will reveal that there have been major oversights in the assessment of what makes a special science "special" for this very reason. I will be arguing against the stratification of the sciences as presented by theorists like Fodor (*Fig 1.2* above) where the special sciences are "autonomous" or distinct from physics, but are analysed in terms of the exact

same criteria which we use to analyse the fundamental sciences. The most serious issue, I will argue, is the assumption that a special science must satisfy *all* of the stringent positivistic criteria set forth by successful fundamental science (i.e. the hard physical sciences like physics), such as explanatory rigour, wide scope, testability, projectibility, universal generalisations (laws) and so forth, that is to say the criteria that we use to assess the natural sciences.

Moreover, I will argue that to deny the special sciences the methodology we associate with the hard physical sciences need not entail that all the special sciences emulate the social sciences in methodology (Fig 3 above). In fact, once the assumption that a science is only legitimate in so far as it approximates the methods of physics is abandoned, then new ways of thinking about disunification are opened up (i.e. *Figs 1.4*, *1.5* and *1.6* above). First and foremost, we must look at the possible ways that we can stratify the sciences and disclose the above disjunction.

1:2 The Views

1:2:1 The Unity of the Sciences

The first view I wish to examine is the standard positivist view of the unity of the sciences. Positivism's legacy is that the unification of the sciences should make everything reducible to physics[4]. Physics would eventually provide an exhaustive explanation of the entire universe, and macro-level descriptions (such

[4] The expression 'unity of science' was introduced by Neurath and deemed the most important aim of logical empiricism in the 1930's. For a comprehensive account of logical empiricism see Neurath, Carnap & Morris (eds.) (1938)

as those explanations that are characteristic of special science disciplines) would in principle be replaceable by some comprehensive theory of everything. In principle, there should be a strict demarcation between laws and accidents and the only real laws of nature would be those of the correct theory of everything provided by fundamental physics.

Indeed, were the theory to be suitably comprehensive, then it could turn out that there is only one (or a few) genuine law(s) of nature[5]. Importantly, the great aim of the unity of science project was to make all scientific disciplines reducible (in principle) to this grand unifying theory. The schema for reduction was the Nagelian model and the failure of a theory (or scientific discipline) to reduce according to it, would result in elimination and the claim that the discipline was pseudo-scientific.

Reduction, according to Nagel (1961) is the 'explanation of a theory or a set of experimental laws established in one area of inquiry, by a theory usually though not invariably formulated for some other domain.' (Nagel 1961 : 338). The laws of the old theory ought to be straightforwardly derivable from the laws of the new theory[6]. Nagel views reduction as a type of deductive explanation,

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[5] The most respected laws are those that have the widest scope and application. For this reason, they are often referred to as fundamental laws of nature. An example is Lavoisier's law of the conservation of mass in chemistry, which states that in a closed system, mass always remains constant, no matter what processes occur internal to the system (Lavoisier's *Elementary Treatise on Chemistry* (1789)). Should physicists discover a unified theory of everything, then we would want the law(s) to be of the same ilk; namely, they should determine clearly and without ambiguity the elements of a closed system.

[6] Whether or not the Nagelian sense of reduction is the one we ought to be implementing when thinking about science is something I will not consider here. For alternative models see especially Bickle, John. (1998). Psychoneural Reduction: The New Wave, MIT Press, Cambridge, MA & Kemeny, J. & Oppenheim, P. (1956), 'On Reduction', *Philosophical Studies,* 7, p. 6-19.

where he accepts Hempel's Deductive-Nomological model of explanation as the explanation paradigm.

According to Hempel's Deductive-Nomological (D-N) model the *explanandum* (fact-to-be-explained) is the conclusion of a logical argument that is derived from a law in conjunction with initial conditions (which make up the premises). The D-N model must satisfy the following requirements. The premises in the explanans must logically entail the explanandum. The premises of the explanans must be true. At least one of the premises must be a universal law, with unlimited scope that does not refer to particulars. The explanans must have empirical content, which means that they must be testable and observable. Consider the observation that a spoon-handle is bent at the point where it emerges from a glass of water (Hempel (1962) : 16). This explanandum can be deductively explained from the laws of refraction and reflection in conjunction with initial conditions such as the spoon handle being placed in the water by agent x at time t_1.

Given two theories α and β, there are two sets of laws $L\alpha$ and $L\beta$, $L\alpha$ is expressed in the terminology of theory α and $L\beta$ is expressed in the terminology of theory β, where there are also existential commitments to the kinds expressed in the terminology of both theories. The reduction of α to β means that we can

translate the set of laws Lα to the set of laws Lβ[7]. Commonsense often provides examples of simple reduction. Millikan (1999) provides the following example:

> The cooks law that if you mix baking soda with something sour it bubbles up is reduced to chemistry by identifying baking soda with NaHCO3, identifying the sour taste with the presence of H+ ions, adding enough about valences, to derive that CO2, will form if you mix these, and identifying CO2 with what's in the bubbles. (Millikan (1999) : 45)

However, there are also examples of textbooks reductions in science. Nagel uses the following example:

> 'As a consequence of the reductive process, the theory of heat is commonly said to be but a branch of Newtonian Mechanics, physical optics a branch of electromagnetic theory and chemical laws a branch of quantum mechanics. Moreover even biological processes have been given physicochemical explanations and there is a continuing debate as to the possibility of giving such explanations for the entire domain of biological phenomena. There have been repeated though still unsuccessful attempts to exhibit various patterns of men's social behaviour as examples of psychological laws. (Nagel (1961) : 96).

Nagel (1961: 98) distinguishes between homogeneous and inhomogeneous reductions. A reduction is homogeneous, if all the descriptive terms in the conclusion are either present in the premises or can be defined explicitly using terms that are in the premises[8]. An example of this type of reduction is the explanation by Newtonian Mechanics and in particular, the Newtonian law of universal gravitation of various laws about the motion of bodies; like Kepler's laws of planetary motion.

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[7] The ideal of reduction that we want in science fits best in mathematics, where the formal language is clearly axiomatic so that the derivation is one of straightforward reduction (Azzouni (2000)).

[8] Lawrence Sklar (1967 : 110-111) argues that the number of actual homogenous reductions in the history of science are in fact rare.

Kepler's laws are generally taken as empirical generalisations that followed the observations of Tycho Brahe, the Danish Astronomer. Kepler's laws were able to provide a unified framework for predicting and explaining the motions of the planets, but could not explain why these generalisations were true; namely, why the planet's orbits were elliptical, why the sun was at one focus and so on. Newton's theory of universal gravitation in conjunction with his three laws of motion explained why the planets moved in ellipses by introducing a theory of gravity. However, Kepler's theory of the solar system was not then replaced, but rather was reduced to Newton's theory, in so far as Kepler's laws could be derived from Newton's law of gravitation in conjunction with his laws of motion. A*ppendix I* illustrates an informal account of this reduction.

In contrast, a reduction is inhomogeneous, if at least one descriptive term in the conclusion neither occurs in the premises nor is definable by those that do occur in them. The problem in spelling out the logic of these reductions is that there are concepts employed in the reduced laws that are absent from the reducing theories. For example, the notion of heat in thermodynamics is totally absent from the explanation provided by statistical mechanics. Therefore, it is necessary to provide bridge principles, which connect the terms of the reducing theory and the reduced theory. However, according to Nagel, once these bridge principles are provided there will be no problem in principle in providing the reductions that are needed for inhomogeneous deductive derivations. He states:

> 'Bridge Laws state what relations presumably obtain between the extensions of their terms, so that in favourable cases laws of the "narrower" theory (with suitable qualifications about their approximate character) can be deduced from the "wider" theory, and thereby make intelligible why the two theories may have a common field of application.' (Nagel (1961: 113)

So, theory reduction is possible in two ways. Two theories that have homogeneous terms can be straightforwardly reduced by deduction. Two theories that have inhomogeneous terms can be reduced with the aid of bridge principles. So the ideal of reduction then for Nagel is that it ought to be always possible (in the limit) to reduce an older theory by a new and more comprehensive one in either of these two ways.

The ideal of unification is that all scientific theories (and eventually all scientific disciplines) will preferably be reduced in either of the two ways that satisfy the ideal of reduction. Since this process ought to be considered transitive, then it follows that if the process were continued to completion that all of science would be reducible to fundamental particle physics. Moreover, in the short term if a new theory is able to reduce a well-established theory, then science can be said to be cumulative in so far as there is a paradigm for scientific progress. The details of the reductions may be complicated, but the ideal of unification entails that the transitivity of scientific theories will at least in the limit make it possible. The upshot is that any new scientific problem that might arise at the macro-level will always be in principle solvable by determining whatever microphysical process is responsible for it.

The classic statement of the latter reductionist paradigm is what has become known as the unity of science hypothesis. (Putnam and Oppenheim

(1958)) We can conceive of the stratification of the sciences as an inverted pyramid (see *Fig 1.7* below), where the concepts and laws of each scientific discipline are reducible to the more fundamental concepts and laws of a lower-level discipline, until we reach physics at the base. The inverted pyramid illustrates how unificationism sees the scientific system. Physics at the bottom of the inverted pyramid is conceived of as a single point, the grand unifying theory where everything comes together and everything is explained. Moreover, the sciences increase in abstraction as we ascend the pyramid, in so far as theories at the top lack the precision of those at the pinnacle. The ideal of unification is that at some point in future science, the pyramid will converge to the base[9].

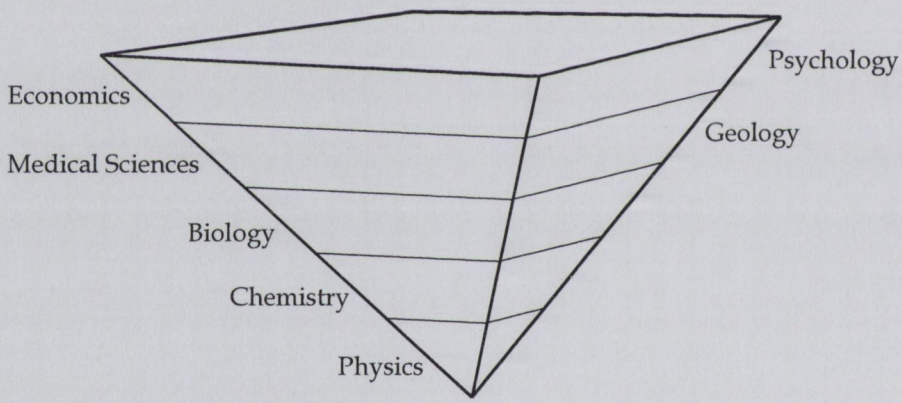

Fig 1.7 Inverted Pyramid of the Sciences

We have seen in Nagel that reductions can be either homogenous or inhomogeneous. However, even if reduction is achieved there is a further question about how we should read our unification claim. Carnap (1938: 61) suggests that

[9] Cartwright (1999 : 7) symbolises this as a normal non-inverted pyramid where the scope of physics is symbolised by the wide base. However, I wish to suggest that the unity of the sciences in so far as it is understood as a claim about laws is better illustrated as an inverted pyramid.

unification can be understood in three different ways. The first claim is the unity of language, *viz.* a common reduction basis of all the terms of the distinct sciences and disciplines to a common scientific language.

The first claim can be seen as a necessary condition for the second stronger claim. The second is the claim that all the laws of special and social sciences will be made reducible to the laws of physics; call this *laws unificationism.* The third claim is the strongest one, which states that once the transitive process of theory reduction reaches the ideal limit (unification), the only natural kinds that will exist will be those of physics; call this *kind unificationism*. The third claim is a controversial one and most advocates of the unity of science leave it an open question whether we would have unity of kinds in the event of our best knowledge. I will look at how each of these is understood in terms of the unification of the scientific disciplines.

Presumably, the first claim of the unity of language is the most uncontroversial. We can claim that the aim of unification is to discover or formulate a scientifically legitimate language of science *as such.* In other words, we should only allow terms that are directly verifiable to constitute knowledge. A second way of understanding the claim is that there will be some language or logic of science, which is used, in all scientifically legitimate disciplines[10]. The

[10] Carnap (1938) argued that this narrow and homogeneous class of terms would be the terms of the physical thing-language. The thing language is the language that cuts across the technical language used by scientists and the common-sense language used in everyday language. This language is used and in principle could be used by all competent language users to speak about 'things' or objects.

role of philosophers of science is then to delineate this logic of science[11]. Importantly, mathematical logic, according to the positivists could throw much light on the logic of science that would in principle unify it.

The received view of the logic of science entails the second claim to unificationism; namely laws unificationism. The logic of science makes the claim that scientific disciplines progress by the formulation of scientific theories (hypotheses), which are in turn defined as bodies of laws. According to this hypothetico-deductive view of theories, the axioms of a theory are a set of universally true laws. Therefore, all sciences should have laws, where the laws are understood as universally quantified generalisations. Unification of the laws implies that all the laws will reduce in a straightforward way (according to the Nagelian schema) to the fundamental laws of physics. An example of laws unificationism is the deduction of Kepler's laws of planetary motion to the Newtonian theory of gravitation[12].

The third claim, kind unificationism, holds that there is an identity relation between the kind predicates of the reducing theory and those of the new completed theory, so that the predicates of the old theory literally refer to the same natural kinds which the new theory gives a better and more accurate explanation of. For example, the theory of optics is considered to be straightforwardly reducible to the theory of electromagnetism. This reduction is a kind reduction because it reduces the kinds or entities of optics (i.e. light waves)

[11] The belief in *the* logic of science can be seen as early as Newton (1687) in the ideal of a universalised scientific language or calculus.
[12] See *Appendix 1*, for an informal account of the derivation.

to the kinds of electromagnetism (i.e. radiation)[13]. Consequently, the claim of kind unificationism is the claim that the kinds that are referred to by the final theory of everything will be the only kinds that are genuinely natural.

Another question that emerges from how we think about these different unity claims is whether the unity of sciences should be committed to reductionism, in principle, as opposed to reduction in practice (Dupré (1983)). This is an important distinction. If the claim of reduction is only theoretical then science may look very much in the future like it does now, even when we have a complete theory of everything. The sciences will be apportioned into disciplines that govern distinct entities and properties at different levels of abstraction. However, all theories will be, in principle, reducible to particle physics, which will be able to explain the underlying microphysical mechanism that is responsible for everything.

So, our ontological commitments to the natural kinds of the special sciences will obviously be at a minimum. The macro-level sciences (like biology, psychology, the medical sciences and so forth) become merely heuristic devices that provide useful ways of carving up the world for certain purposes. Clearly, disunification is then conceived as a weaker epistemic claim, which delineates the so-called special sciences in an instrumentalist way. The claim for special science autonomy will be abandoned in so far as reduction to physics could always, in principle, be achieved.

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[13] See Sklar (1967) : 120 for a discussion of this reduction of entities.

Clearly, on this construal all macro-level sciences will be merely heuristic devices that may prove useful when in scientific practice we want to screen off the more complex story about what is going on at the microphysical level. This type of reductionism means that all of the so-called special sciences and social sciences, which by and large explain the macro-world, will in theory supervene on the microphysical facts. We can therefore, depend on macro-level explanations for the most part, only in so far as we know that there *is* a fundamental physical explanation at the microphysical level which they could in principle be reduced to. The rest of science (biology, the medical sciences and so forth) will consist of deductive derivations of the laws of particle physics in conjunction with bridge principles, which will relate the natural kinds of the macro-level sciences with those of physics.

However, if unification were possible also in practice, then science should be overhauled. At the ideal point that future science was able to explain everything, then all other disciplines would be eliminated. Reduction here is conceived as replacement. The idea here is that most of science (as we now conceive it) will be rendered obsolete, as the completed physics is better understood. In other words, once the theory of everything is supplied there will be a scientific revolution (in the Kuhnian sense)[14], which will involve a paradigm

[14] Kuhn (1962) argued that when the differences between successive paradigms (the old and new theory) are both necessary and irreconcilable, then the only option available is theory replacement. Theory replacement requires a scientific revolution, in so far as the old theory is abandoned in favour of the new one. However, in Kuhn's account of scientific revolutions, he denied that when the paradigms change so too does the world. In other words, scientific revolutions only involved an improvement of the instruments with which we examine the world. Clearly, were ALL sciences to reduce to physics and were this reduction to be possible both in principle and practice, then we would have to conclude that this scientific revolution were the final one, in so far as the instrument provided by the theory of everything should cut nature at its very joints. Old theories (or sciences) like Biology might be used when scientists require approximate *explanations* of the

shift in our concept of science. Theoretically, we should be able to internalise the theory and in so doing explain and understand the world in *the* accurate way. A failure to do so would either be a failure in human knowledge (in so far as ignorance prevented people from internalising the theory) or would be the result of scientific conservatism, where the scientific community would take quite some time to accept the revolution. Clearly, whether unification can be achieved in practice or in principle, the metaphysical commitments of science will be fundamentally changed in so far as the natural kinds referred to by physics will be the only natural kinds that are to be had. The claim is that once the fundamental physical theory is provided, then nature's joints will be found.

1:2:2 Unificationism and the *Ideal* view of science

So, what would the *ideal* theory look like? In other words, what are the ideal criteria that we would expect this theory to have? We would expect the theory to be universally true and to have universal scope, in so far as the law or set of laws of nature that make up the theory will provide a deductive basis from which all else can be derived. The theory should be simple, unified and all embracing. The language of the theory will be in complete correspondence with the realm of space-time events, which make up the actual (real) world and in so far as the predicates converge on 'kinds' these kinds will be natural. Additionally, we will expect the theory to have closure in so far as no possible effects (not explainable by the theory) are possible. Therefore, the theory will

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macro-world. Biology could still be done in terms of genes, chromosomes etc. but only in so far as the scientists were interested in explanations. The genes, chromosomes etc. could not be literally said to exist, they would just be the outmoded posits of an old instrument.

have no anomalies. Complete correspondence with reality will entail that all causal relations should be predictable and explainable, in terms of the fundamental laws of nature. Methodologically, we would expect there to be a single logic of confirmation and so a glimpse at the theory in scientific practice should exemplify *the* scientific method.

The picture of unified physics as a tool of unlimited utility in capturing a perfectly ordered reality is greatly challenged by theorists like Cartwright (1999). There are implicit assumptions made in the unification programme. In particular, the claims that it is, in fact, an ordered universe that we are trying to capture and that we do in fact have the capacity in our scientific theorising to capture it. The problem is that this view of a neatly ordered universe may be just wishful thinking on our part. In fact Cartwright (1999) argues for the view that the world is dappled and that physics, as we use it in everyday scientific practice, although a useful tool can only ever hope to have a limited application. It is indeed an open empirical question whether science is progressing towards an all-encompassing explanatory paradigm, if it should turn out that the world is not as a matter of fact an ordered place.

Therefore, the traditional unificationist picture of science is very much guided by *a priori* theorising about what science would be like were it ideal. Moreover, these *ideal* criteria inform and shape how we assess not only our present theories in physics, but also, I wish to argue, how we assess the special and social sciences. Whether we are indeed warranted in assessing the special and indeed the social sciences in this way and so, according to these criteria is a

question I wish to address. Let's take a closer look at the different views about the how the sciences might be stratified given the premise of disunification.

1:2:3 Disunity and Anti-Reductionism: The Special Sciences

So how likely is the ideal of unification? *Pace* Putnam & Oppenheim (1958), Putnam (1973) himself argues that multiple realisability presents a problem for the ideal of reduction and the unity of science. This discussion began in the philosophy of mind in a discussion about the problems that faced central state materialism (type-type identity theory) (Place (1956), Smart (1959)). According to central state materialism, the mind-brain relation is one of straightforward reduction. So, in saying that pain = *c*-fibres firing, the view was that pain could be straightforwardly reduced to its neurophysiological counterpart. But, Putnam (1973) argues that in the case of intentional psychology, there will not be straightforward reductions between mental kind predicates and neurophysiological ones.

Comparative anatomy and neurophysiology suggests that the possibility of reducing all instances of pain (in all species) to *c*-fibres firing is *de facto* an impossible task. Given what we do know about the heterogeneity of neuroanatomy across species, it is empirically inadequate to hold strict reduction as a serious empirical hypothesis. Moreover, thought experiments about the possibility of organisms (Martians & Robots) with different neuroanatomy than that of the anthropocentric physico-chemical one, open up the possibility of psychological kinds being multiply realised in different neurophysiological kinds.

Fodor (1974) argues that multiply realisability may even be so extreme as to prohibit reduction within specific species, so that psychological kinds may be realised differently in the same token system over time. Therefore, this stronger multiple realisability claim makes even species-specific reductions *prima facie* impossible. For this reason, it is argued that the special sciences are irreducible.

So what does the failure of reduction mean for the stratification of the sciences? There are two views that can be taken. The first view is *Eliminativism*, which claims that the failure of reduction should be taken seriously and so we ought to eliminate those scientific disciplines, which fail to reduce to physics according to the Hempelian deductive paradigm (Churchland (1981)). The second view is *Special Science Disunificationism*, which claims that the special sciences are distinct autonomous disciplines, which remain irreducible to physics. Fodor takes on the burden of proof in defending the scientific prowess of the special sciences.

1:2:3 Eliminativism

Eliminativism is the view that the failure of reduction and indeed the open empirical possibility of multiple realisability should not entail disunification, but rather elimination. We often find that the concepts in the reduced theory do not correlate in any simple way with the concepts employed in the reducing theory.[15] For example, if the laws of the reduced science contain terms that do not occur in

[15] This is obvious in the mind/brain case. The *vocabulary* we use to describe intentional and neurophysiological reactions is so different, that it would be hard to imagine a law that had the intentional state as antecedent and a neurophysiological state as consequent.

the theoretical assumptions of the reducing science, then at least a homogenous reduction is *prima facie* impossible.

Churchland (1981) insists, *pace* Fodor (1974), that the *prima facie* failure of reduction should not lead to disunification, but rather to elimination. So, the special sciences either reduce in a straightforward way to physics, or they should be eliminated and regarded as pseudo-scientific. The inclination here is to make the irreducible special sciences comparable to discredited folk theories from the past. He states:

> Eliminative Materialism is the thesis that our commonsense conception of psychological phenomena constitutes a radically false theory, a theory so fundamentally defective that both the principles and the ontology of that theory will eventually be displaced, rather than smoothly reduced, by completed neuroscience. Our mutual understanding and even our introspection may then be reconstituted within the conceptual framework of completed neuroscience, a theory we may expect to be more powerful by far than the common-sense theory it displaces. (Churchland (1981): 67)

For example, intentional psychology is, according to Churchland, an outmoded and radically false theory, which is in the long run utterly replaceable by neuroscience. "Folk Psychology" by analogy with something like say "folk medicine" is meant to tempt us to the view that intentional state ascription is just a bad scientific theory of our behaviour. Consider the following example; the underlying principles of medieval medicine were four humors - black bile, yellow bile, phlegm, and blood. According, to the theory, it was the balance of these four humors that allowed for the well being of a person. (Margotta (1996)) Illnesses were explained by the fact that there was an imbalance in the humors of the individual.

Churchland argues that this 'folk' theory could not be reduced to any modern account of medicine, because it is so obviously false. The purpose of an analogy like this is twofold. Firstly, it is supposed to demonstrate that recalcitrance to reduction does not entail that the theory is veridical and ineliminable. Secondly, the analogy with the *weakest* of theories is supposed to show that the explanations of intentional psychology are unfalsifiable and hence, unscientific.

Furthermore, Churchland argues that the special sciences do not have real empirical laws. The real laws that govern the phenomena that are explained by these outmoded theories are at the microphysical level. Churchland takes, for example, the fact that intentional explanation remains resistant to reduction, as a testimony against, rather than in favour of its scientific rigour. He states:

> The real laws governing over our internal activities are defined over different and much more complex kinematical states and configurations, as are the normative criteria for developmental integrity and intellectual virtue. (Churchland (1981) : 85)

Finally, Churchland argues that the empirical failure of the scientific theories in the special sciences is sufficient for us to reject them. Taking the example of intentional psychology, he argues that the empirical failures of the theory are widespread. This is evident from the fact that we are largely ignorant of many phenomena that pervade out mental lives. Commonsense psychology provides no explanation for why we need sleep, the nature of mental illness, creative imagination, memory, or perceptual illusions. Moreover, it sheds little light on the learning process itself, or how we store information. Consequently, commonsense psychology is 'at best a highly superficial theory, a partial and

unpenetrating gloss on a deeper and more complex reality.' (Churchland (1981) : 74.)

However, the reasons for this extreme conclusion are based on a strict demarcation between science and pseudo-science, which again is informed by what science should be like (our *ideal* model from future physics which we discussed earlier) rather than what it is, in fact, like in scientific practice. The special sciences are rejected as genuine scientific disciplines because they fail to satisfy certain criteria that are considered constitutive of legitimate science. In particular, there is an assumption that a science should have wide explanatory power and empirical laws.

1:2:4 Special Science Disunificationism:

Fodor (1974)[16] argues *pace* Churchland (1981) that the special sciences are real autonomous disciplines whose failure to reduce to physics ought not to entail elimination. Fodor (1974) argues from the multiple realisability hypothesis to the disunity of the sciences hypothesis, which introduces a distinction between fundamental sciences like physics and *all* of the special sciences such as intentional psychology, geology, meteorology and so on. These later special sciences are considered 'special' in so far as they cannot be reduced to physics.

[16] Fodor (1974) is reprinted in Fodor (1981). All page references and citations are from the (1981) edition.

As we have said, reduction is prohibited by the empirical hypothesis that special science kinds are multiply realisable in physical kinds, known as the multiple realisability thesis (henceforth MRT). The special sciences are genuine empirical sciences, whose explanatory success is undeniable according to Fodor. The question of how we are to classify the special sciences is an important one. However, it is a challenge that Fodor thinks he can meet. He argues that the special sciences can meet the stringent requirements for science, in so far as we can discover real *ceteris paribus* laws in the special sciences that quantify over special science natural kinds.

Let's take a closer look at the disunity of the sciences hypothesis. The multiple realisability thesis (henceforth MRT) claims that it is an open empirical possibility that the kind predicate(s) of the reduced science (i.e. the special science) may correspond to a widely disjunctive and heterogeneous set of kind predicate(s) in the reducing science (i.e. physics) and hence, that there is no one-to-one correlation between the predicates of physics and those of the special sciences. Fodor provides the following example:

> Gresham's Law says something about what will happen in monetary exchanges under certain conditions. I am willing to believe that physics is general in the sense that it implies that any event which consists of a monetary exchange (hence any event which falls under Gresham's law) has a true description in the vocabulary of physics and in virtue of which it falls under the laws of physics. But banal considerations suggest that a physical description which covers all such events must be wildly disjunctive. Some monetary exchanges involve strings of wampum. Some involve dollar bills. And some involve signing one's name to a check. What are the chances that a disjunction of physical predicates which covers all these events (i.e. a disjunctive predicate which can form the right hand side of a bridge law of the form 'x is a monetary exchange $\leftrightarrow$ .....') expresses a physical kind? (Fodor (1981): 134)

Therefore, the economic kind 'monetary exchange' is multiply realisable in the heterogeneous set of physical kinds that could be counted as examples of monetary exchanges.

What does MRT mean for how we think about the stratification of the sciences? Fodor argues that the distinction is definitive, in so far as MRT provides a strict demarcation between the special sciences and the fundamental ones. Fodor (1974) (1991) takes the empirical hypothesis of MRT seriously and uses it to argue that the special sciences are scientifically legitimate, in so far as there are genuine empirical special science laws that refer to real natural kinds. Therefore, Fodor argues for realism about the special sciences since he thinks that there is no particular problem in disclosing their laws. Fodor (1974) illustrates it in the following way (see *Fig 1.8* below)[17]:

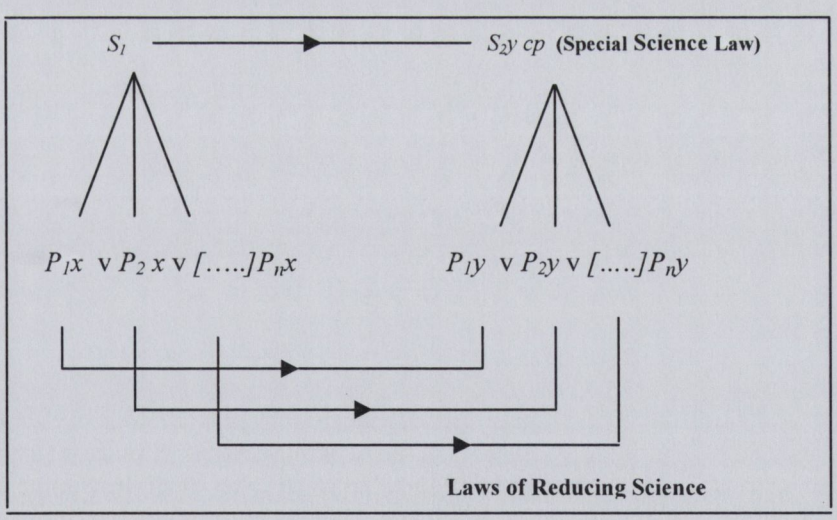

Fig 1.8 Illustration of the Relationship between the Special Sciences and the Reducing Science in Special Science Disunificationism

[17] I will examine the details of this illustration in greater detail in chapter two.

However, why the special sciences should require the particular characteristics (i.e. laws and natural kinds) attributed to them by Fodor is an assumption that is built into the understanding of the honorific of "legitimate science". In other words, Fodor assumes that the special sciences should fulfil the exact same criteria as their fundamental science counterpart, in order for them to be considered 'scientific'. The problem here is that background commitments to the *ideal* of successful science makes theorists like Fodor apprehensive about classifying the special sciences as anything less than (or indeed different to) their fundamental science counterparts.

Fodor thinks that there are only two routes for him to take. He can reject MRT and the distinction between the fundamental and the special sciences that it motivates (i.e. accept the elimination of the special sciences), or he can painstakingly give an account of how the special sciences fulfil the positivistic model for successful science, such as providing an account of empirical laws in the special sciences. Fodor argues that the latter account can be provided[18].

The reason why Fodor requires an account of special science laws is because he believes that if he cannot make a case for empirical laws in the special sciences that he will face the following dilemma:

> Either special sciences have no ontological independence from lower-level sciences or, worse, they have no empirical laws, which is just to say that they are not empirical sciences at all. (Shapiro (2000) : 160)[19]

[18] I will examine Fodor's account in Chapter two.

[19] Rosenberg (2001) argues that Shapiro's dilemma for the special sciences is unreal. He claims that in fact Shapiro and indeed Fodor have not located the most important source of multiple realisation in nature which he takes to be natural selection. He claims that natural selection will, in the long run, ensure that multiple realizers have causally relevant differences from one another and moreover, that these causal differences will be significant enough to obstruct the *ceteris paribus* laws that are needed to secure the autonomy of the special sciences. (Rosenberg (2001: 366).

I contend that this is a false disjunction. It hails from logical positivism's stringent policing of the language of science, such that it is assumed that the only alternative to controlled experiments that test universally quantified laws in an objective way, is the description (rather than explanation) of accidental and contingent generalisations in a subjective way.

1:2:5 Human Science Disunification:

The latter false disjunction hails from the supposed alternative view of the special sciences; namely that disunification might result in an altogether different distinction between the fundamental and special sciences. The idea is that the special sciences (psychology, economics) are *toto coelo* different from the hard physical sciences and for this reason need to be analysed in a similar vein to the social sciences. What is implied here is that there is a fundamental distinction between the methodologies of the social sciences and the hard physical sciences, in so far as the social sciences are idiographic, characterized by singular assertory propositions and individualising conceptualisation and the natural sciences are nomothetic, characterised by general apodictic propositions and generalising conceptualisation (Schutz (1954) : 257)[20]. This distinction between the human sciences and the natural sciences (as we saw earlier in *Fig 1.3*) can be represented as follows:

[20] Schutz rejected positivism and agreed with Dilthey on the priority of the cultural over the natural sciences. Analysing the social sciences in terms of the logical structure of the natural sciences really left something out; namely the phenomenological understanding of human beings.

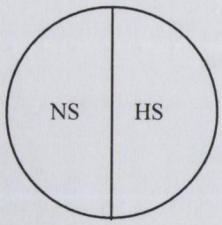

Fig 1. 3 Natural and Human Science Disunificationism

However, it is important to note that this distinction involves a stronger claim for the social scientist; namely that interpreting the social sciences in terms of natural science would actually leave something out about how we interpret and understand the social world. Importantly, the social scientist would resist all attempts to subject the social world to scientism. Weber (1905: 128-29) explained it in the following way:

> Suppose that somehow an empirical-statistical demonstration of the strictest sort is produced, showing that all men everywhere who have ever been placed in a certain situation have invariably reacted in the same way and to the same extent. Suppose that whenever this situation is experimentally reproduced, the same reaction invariably follows [……..] By itself, such a demonstration would contribute absolutely nothing to the project of "understanding" "why" it invariably occurs in the same way. (Weber (1905) : 128-129)

Therefore, the claim made on behalf of the social scientist here is complete resistance to the reduction of understanding to explanation. However, in the philosophy of science, the converse is implied; namely try at all costs to reduce understanding to explanation if the understanding is to be deemed genuinely 'scientific'. In order for a scientific explanation to be taken seriously it must approximate the methodologies of the hard physical sciences and in so far as it fails to do so, it is no longer to be thought of as legitimate science.

Therefore, there is a stand off between natural science and social science, where both parties involved insist on an absolute distinction between the two albeit for very different reasons. This standoff is motivated in both a bottom up and top down way. The standoff is motivated in a bottom up way, since there must be a clear distinction between what is scientific on the one hand and what is merely descriptive on the other. It is motivated in a top down way, because the social scientist feels that the reduction of the social sciences to the natural sciences would leave something important out, namely human *understanding* of human affairs, as opposed to a mere scientific explanation of them.

There are two aspects of the social sciences, which will preclude the reduction of the social sciences to the natural sciences and so motivate the distinction between the two in a bottom up way. Firstly, there is a concern that explanation in the social sciences is not genuinely nomological[21]. Social theory is open and incomplete by its very nature. In the social sciences, all the possible factors that might intervene to effect explanation cannot be explained in terms of social theory itself. Secondly, there is a concern that social theory is somehow parasitic upon our account of human psychology (folk psychology); in so far as other social sciences involve an implicit reference to human norms, practices and preferences. Consider an economic explanation of a certain occurrence. (Hempel (1965) uses the following example:

[21] Of course, social scientists like Weber will claim that the problem here is in analysing the social sciences in terms of the criteria that we use to analyse natural science in the first place.

> In the fall of 1946, there occurred at the cotton exchanges of the United States a price drop which was so severe that the exchanges in New York, New Orleans, and Chicago had to suspend their activities altogether. In an attempt to explain this occurrence, newspapers traced it back to a large scale speculator in New Orleans, who had feared that his holdings were too large and had therefore, begun to liquidate his stocks, smaller speculators had then followed his example in a panic and had thus touched off the critical decline. (Hempel (1965) : 251)

Implicitly, the law of supply and demand is the regularity that is exhibited. The drop in cotton prices can be explained by the increase in supply and unchanged demand. The initial conditions are the fact that speculators liquidated their stocks. These factors together explain the drop in prices (the-fact-to-be-explained). The problem is that the explanandum could have been different, in so far as the law of supply and demand is not a strict law. This first problem then amounts to the claim that putative social science generalisations (like the law of supply and demand) are incomplete and thus not laws. Therefore, there is an implicit assumption that a science must have laws to be genuinely scientific.

The second problem is that the social sciences are somehow reducible or at the very least dependent on psychology[22]. This assumption relies on the intuition that the behaviour of social groups (or kinds) is the sum of the individuals whose behaviour is explainable and predictable by means of intentional psychology (folk psychology). In other words, the behaviour of the group is nothing over and above the behaviour(s) of its constituent members. Therefore, the scientific status that we give to our explanations in the social sciences will be parasitic on whether we can make a case for intentional psychology as a science[23]. Consequently, there

[22] This can be seen in Porpora (1983)

[23] It is this idea that has made the campaign for the institution of a genuine special science of intentional psychology by theorists like Fodor (as discussed above).

is a further problem with regard to whether psychology itself can be interpreted as a natural science.

1:3 Disunificationism and Intentional Psychology

So, first and foremost disunificationism (because of multiply realisability) involves a distinction between the fundamental sciences and the special and social sciences. However, it is then problematic as to how we ought to construe the relationship between the latter two. The first possibility is to treat of these two as absolutely distinct; namely in so far as we think that there is a *toto coelo* difference between the social and special sciences. We should say that there are two types of disunificationism, both special science disunificationism and human science disunificationism. Therefore, we could legitimately hold these both forms of disunifictionism.

The second possibility is to say that intentional psychology is the point of segregation; namely in so far as all social sciences could in principle be reduced to intentional psychology and so there is only one genuine claim to disunification; namely special science disunificationism. Mill (1843) held the latter view:

> The laws of the phenomena of society are and can be, nothing but the laws and the actions and passions of human beings united together in the social state[…]Human beings in society have no properties but those which are derived from and may be resolved into, the laws of individual man.
> (Mill (1843), VI, vii, I)

On this construal, the case of intentional psychology and in particular, its status as either a natural or social science occupies a pivotal position in this standoff

between the natural and social sciences, as though it cuts the divide between the natural and social sciences.

However, this is informed by the same paradigm of reductionism that we examined earlier. The idea is that we can somehow reduce the social sciences to psychology and then the project of reducing (or at the very least accounting for) psychology becomes the most important task in establishing how we ought to understand the claim of disunificationism. Therefore, the special sciences are thought to occupy a pivotal equidistant position in the standoff.

A further possibility is to deny the special science claim to disunificationism and to argue that there is a distinction between the natural sciences and the social sciences and that psychology occupies the same conceptual position as the other social sciences, economics, sociology, criminology and so forth. Davidson (1970) supports this view in so for as he argues for a conceptual distinction between the natural sciences and human social sciences like psychology.

Davidson takes the multiple realisability thesis to be fatal to mind/brain reducibility, however he argues *pace* Fodor, that we should not initiate psychology as an authentic special science with empirical laws. Rather, the failure of reduction introduces a conceptual distinction between psychology and the natural sciences. This conceptual distinction is the result of the agent-centred concepts that are characteristic of psychology as a discipline, namely the failure of

psychology to have any candidates for empirical laws, what he terms the "anomalism of the mental".

Davidson claims that the chief assumption that motivates the claim that intentional psychology is a natural science is that all scientific explanation must involve genuine causal explanation which itself must be a species of nomological explanation. Davidson rejects this assumption and argues *pace* Fodor that there is more to causal explanation than nomic support. Therefore, we should not invoke special science laws in order to corroborate the causal explanations that are used so successfully in the special sciences. He states:

> The most primitive explanation of an action gives its cause; more elaborate explanations may tell more of the story, or defend the singular causal claim by producing a relevant law or by giving reasons for believing such exists. But it is an error to think that no explanation has been given until a law has been produced. Linked with these errors is the idea that singular causal statements necessarily indicate by the concepts they employ, the concepts that will occur in the entailed law. (Davidson (1970) : 17)

Davidson takes the autonomy of the special sciences to be a totally conceptual claim. The inclination is to think that the special and social sciences are not legitimate sciences, if they cannot be analysed in terms of those characteristics that we think are constitutive of successful science; namely that a science should involve causal explanations that are corroborated by laws.

However, in the latter passage, Davidson unveils an important equivocation in our ideas about scientific explanation. Nomological explanation is not all there is to explanation for Davidson, so the special and social sciences can be considered as genuinely explanatory disciplines, despite the fact that there

are no serious candidates for laws in them. Moreover, once we reject the campaign for laws, then there is no longer a distinction to be made between the social and special sciences. For Davidson, the only laws that are forthcoming, are the laws that govern the causal and deterministic mechanism, which realise the psychological generalisations; namely the laws of physics[24]. Consequently, disunificationism, on the Davidsonian view, is an entirely conceptual claim.

Davidson is concerned with psychology alone here, but we could take this view to generalise, if it should turn out that some or all of the other special sciences also fail to discover any candidates for laws. The idea here is that the special sciences are analogous to the social sciences in so far as they refer to context specific variables, which are quantified over by individualising concepts. The equivalence between the special and social sciences is meant to imply that the special sciences are not as scientifically legitimate as their more fundamental science counterparts. Once we give up on general laws in the special sciences, then they become idiographic sciences that are specific only to a human point of view. Hence, while these disciplines might be perfectly valid as heuristic devices for describing a distinctly human world, they are not the subjects of real science, in so far as science should discover universally true explanations of the universe[25].

The problem then is that disunification becomes merely a conceptual notion, rather than an empirical hypothesis and the special sciences are of mere

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[24] Or at least the laws of neurophysiology, which can in turn be reduced to the laws of physics.
[25] Harold Kincaid (1990) argues against the view that the social sciences are different in methodology from the hard physical sciences. In fact, he takes Fodor's view about the special sciences to generalise even to the social sciences. Therefore, Kincaid defends *ceteris paribus* laws in the social sciences that refer to real natural kinds.

instrumental value. The difficulty that theorists like Fodor would have with the latter if we classify the disunification hypothesis in the latter way, then the distinction that it introduces will not be strong enough to rebut the reductionist claim for unification.

The problem is that we could consistently have unification, where the ontologically important general hypotheses are explained by particle physics and also disunification of the social sciences, where the failure of the latter to reduce to physics can be explained away by the fact that they are *conceptually* distinct. They quantify over phenomena that will remain descriptively important from a human point of view, even if unification were to take place. Importantly, there will not be any metaphysically important discoveries or explanations to be provided by these sciences and so there will not be any metaphysical reason to think that these sciences are *ontologically* distinct. On this view, it is difficult to conceive how the social sciences (including all those that were earlier considered to be 'special sciences') would become any more than heuristic devices.

In fact, some philosophers and social scientists take this problem to be a serious challenge to the legitimacy of not only the special sciences, but also the social sciences themselves. In order to combat this claim, Kincaid (1996) argues that the social sciences satisfy the constraints placed upon science by the *ideal* model for successful science; namely he argues for the view that the social sciences are nomological and so have genuine *cp* laws. Kincaid takes Fodor's understanding of disunificationism even further, in so far as he wishes to examine

all sciences in terms of the same criteria with which we assess the hard physical sciences. He states:

> *Ceteris Paribus* qualifications surely do plague the social sciences. That, however, does not separate them from the natural sciences, for *ceteris paribus* clauses are endemic even in out best physics. (Kincaid (1996 : 64).

In economics, the law of supply and demand for example is a genuine law in so far as it is explanatory. It is explanatory, according to Kincaid, because it is confirmed by its instances[26].

In all of the above views, there is a background assumption that there are definite criteria a science must fulfil to be scientific. More particularly, the status of a science as either natural or social science respectively, hinges upon whether we can give an account of the laws of that science or not. In the case that we can, then we can legitimately call it a natural science and in the case that we cannot, it crosses the divided line and is regarded as a social science.

This particular background assumption about laws stems from the background assumption that there are clearly delineated criteria, which are constitutive of successful science. These criteria are extrapolated from the past empirical successes of the natural sciences and from the aspiration that there is one complete overarching theory forthcoming in physics. It is assumed that a

[26] Kincaid thinks that there is no problem in confirming the putative laws of the social sciences. What the *ceteris paribus* clause does in scientific practice is control interfering factors. In social science, empirical research can corroborate the claim to social explanation quite easily, according to Kincaid. For example, studies, which control certain interfering factors, can corroborate a social *cp* law. Moreover, because these laws can be used to make predictions of social phenomena, then we should conclude that they are genuine laws. Therefore, in scientific practice the methodology of the social sciences can corroborate our claim that the social sciences are genuine sciences.

legitimate science should have one and only one logic of confirmation, it should exemplify *the scientific method* and there should be a sharp and clearly delineated line between laws and accidents.

The *ideal* science paradigm informs and shapes how we position the sciences. Moreover, we are faced always with a disjunction between those sciences, which rigidly fit the paradigm, and those that do not. We can either bring the special and social science into line with the ideal science paradigm so that it is preserved, or we admit that the failure of reduction leads to either elimination or scientific illegitimacy[27]. Let's take a closer look at the paradigm of successful science.

1:4 Scientific Fundamentalism:

The underlying problem, as I see here, it is that all of the latter ways of dividing the sciences work within a pre-conceived notion of what science is ideally meant to look like, which Cartwright (1999) has referred to as *Scientific Fundamentalism.* Scientific Fundamentalism presupposes that there is an uncontentious model for successful science; namely that we know the characteristics a science should *ideally* have. There are three background assumptions that inform this view. The first is *an empiricist view of science*, the second a belief in *the scientific method* and the third *physics chauvinism*, which

[27] Otherwise, we admit that disunificationism is merely a conceptual claim and while the social sciences may continue to give interesting descriptive taxonomies of a distinctly human world, they do not compete for scientific legitimacy with their fundamental science counterparts.

takes physics as the ideal model for successful science. Let's look at how each of these influences the stratification of the sciences.

1:4:1 Logical Positivism & The Scientific Method:

The *logical positivist* view of knowledge, building on earlier empiricist theories of knowledge was to influence the philosophy of science greatly and indeed could be seen as the pervading view of science right up until the 1950's[28]. Positivism renounced all metaphysically dubious claims to knowledge, embracing both empiricism and logicism.

All knowledge should be equated with scientific knowledge. For example, any proposition that could not be empirically verified and yet, made existential claims about metaphysical entities like powers, capacities and dispositions was to be considered a pseudo-proposition. On the other hand, for a proposition *P*, to be meaningful, according to the logical empiricist, that proposition had to be empirically verifiable[29]. Moreover, the theoretical part of good science should be

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[28] Logical positivism is a field of philosophy, which developed in Vienna in the 1920's around the work of the Vienna Circle. Central figures in its development included Rudolf Carnap, Hans Reichenbach, Carl Gustav Hempel and Otto Von Neurath. Most importantly, the positivists thought that there were only two sources of all knowledge; namely logical reasoning (analytic *a priori* truths such as logical truths (the truths of mathematics being in turn reducible to the truths of logic) and (synthetic *a posteriori* truths such as empirical facts.) Importantly, then the positivists would deny the Kantian claim that we could know truths that were both synthetic and *a priori.* See (Blumberg & Feigl (1931) : 282.)

[29] Importantly, a proposition does not have to be conclusively verifiable. Rather, it is sufficient, that some possible observations should be relevant to its truth or falsehood. Ayer, for example, makes this clear in the following passage:

> 'Let us call a proposition which records an actual or possible observation an experimental observation. Then we may say that it is the mark of a genuine factual proposition, not that it be equivalent to an experimental proposition, or any finite number of experimental propositions, but simply that some experimental propositions can be deduced from it in conjunction with certain other premises without being deducible from those other premises alone. (Ayer: (1946) : 26)

translatable into observation sentences. Any proposition that failed to satisfy these constraints should be deemed vacuous.

The great belief in *the scientific method*[30] is supported by verificationism. Most scientists and indeed philosophers of science believe that there is *a* scientific method, which is constitutive of successful scientific methodology. The belief in the scientific method is informed by the belief in a scientific attitude to discovery and investigation. Dewey (1938) defines it in the following way:

> 'The scientific attitude [....] is manifested in any walk of life. What then is it? On its negative side, it is the freedom from control by routine, prejudice, dogma, unexamined tradition, sheer self-interest. Positively, it is the will to inquire, to examine, to discriminate, to draw conclusions only on the basis of evidence after taking pains to gather all available evidence. It is the intention to reach beliefs and to test those that are entertained, on the basis of observed fact, recognizing also that facts are without meaning save as they point to ideas. It is, in turn, the experimental attitude, which recognizes that while ideas are necessary to deal with facts, yet they are working hypotheses to be tested by the consequences they produce. (Dewey (1938) : 31)

So, there is an attitude to knowledge and the empirical world that the scientist must always take to free him/herself from dogma and prejudice. This

[30] A belief in *the scientific method* is consistent with many views about science. This belief in *the* scientific method goes back to the Greeks. Aristotle, for example, believed that knowledge of essences was grasped by induction from sensible things. However, he also gave a role to deduction, in so far as there was a finite set of propositions about any given essence, from which all other propositions could be derived. See Aristotle (*Posterior Analytics*). With the Copernican Revolution and the advances of the Seventeenth Century, the conception of the scientific method would change rapidly. Galileo believed that the Copernican system of the universe was *the* true description of reality. Therefore, for this naïve realist view the scientific method could yield absolute truth of reality. The method of getting to this truth for Galileo was by induction. In contrast, the instrumentalist view of science holds that a hypothesis need not be strictly speaking true, but rather that it should yield calculations, which agree with the empirical observations. Therefore, the scientific method does not yield absolute truth, but allows calculations that are consistent with empirical observations. Popper himself would re-describe scientific method yet again; in so far as he argued that scientific laws and theories were not confirmed by their instances, but rather that the possibility that theories might be falsified by new data is always left open. Theories then are not confirmed but are corroborated. What is common across all of these views about science is that there is the belief in *a scientific method,* which is implicit in all good scientific practice. (For a clear history of the different views of science and their commitments see Popper (1963) : 130-153)

idea about the scientific attitude informs the idea of a scientific method. The scientific method is the perceived correct methodology incorporated by scientists to discover a complete and true description of the scientific world. Moreover, it is rational for scientists to incorporate the scientific method, since this is an idealisation of the scientific attitude. Therefore, the goal of the scientist is to rid him/herself of all prejudice and work towards theories, which are testable and empirically verifiable. The best way the scientist can achieve this kind of objectivity in methodology is in terms of *the* scientific method.

Secondly, the belief in the scientific method is informed by the idea that scientific knowledge is cumulative; namely in so far as new theories give better and closer approximations to the truth than older theories. So, the belief in the scientific method is marked by a belief in scientific progress. The goal of science is to construct theories; where a theory T is a set of organising principles (laws) about a given phenomena P.

A belief in the scientific method, which as an instrument will eventually achieve the true description of the universe, is exemplified in the following quote from Einstein:

> Have we any right to hope that experience will guide us aright, when there are theories (like classical mechanics) which agree with experience to a very great extent, even without comprehending the experience in its depths? To this answer with complete assurance, that in my opinion there is *the* correct path, and moreover, that it is in our power to find it. Our experience up to date justifies us in feeling sure that in Nature is actualized the ideal of mathematical simplicity. It is my conviction that pure mathematical construction enables us to discover the concepts and laws connecting them with the key to understanding of the phenomena of nature. (Einstein (1934): 167)

This belief in "the correct path" that science has the power to discover leads to the kind of optimism that is characteristic of future physics fundamentalism. The positivists were to give a formulation to this commitment to "the correct path" in terms of the ideal of unificationism. Moreover, the strict requirements of verification in the empiricist theory of knowledge precluded all metaphysically dubious claims to knowledge. Consequently, a model for successful science was built upon the ideal aspiration that we would, in the end, discover a unified theory of everything.

Since, the ideal of the unificationists is that all present day theories would be made reducible to the ideal theory of particle physics, then positivists were more interested in assessing how science should be or would be when an ideal completed theory would be supplied in the future, than in the present scientific theories which they held would eventually be eliminated, when the completed theory was provided.

Consequently, taking future physics as the ideal model for science and the model for all sciences, results in a rather stringent and rigid classification of the language of science. It sets science up so that there is no alternative to physics fundamentalism. Moreover, in so far as there is an alternative (i.e. in so far as disunification allows us to make a case for less rigorous sciences such as economics, psychology, geology, biology and so forth), we are coerced into analysing them in terms of the criteria set down by physics.

For example, we must discover laws of nature with wide applicability, which are confirmed by their instances. Moreover, if the laws of the putative special science theory are correct then the classification of types that they refer to should be 'natural'. In other words, these other special sciences should have genuine natural kinds. Furthermore, the special sciences should provide self-standing explanations that have sufficient empirical content to be testable and so on. By contrast, if we decide that the special sciences do not satisfy the rigid criteria set forth by the honorific term 'legitimate science', then the only conceptual classifications remaining for them are either as human descriptive explanatory theories or as pseudo-sciences.

The belief that an over-arching unified theory should have the ideal characteristics that we would want a scientific theory to have stems from the yearning for an ordered world that is rationally comprehensible. It is this obsession with the unified theory that blocks progress on asking the right kinds of questions (epistemic, methodological and perhaps, also metaphysical) about the special sciences. As Cartwright (1999) rightly urges:

> The pernicious effects of the belief in the single universal rule of law and the single scientific system are spread across the sciences.[....] The theory consumes resources and efforts that could go into the hundreds of other enterprises in physics that ask different kinds of questions and solve different kinds of problems.
> (Cartwright (1999) : 16)

I wish to take this point further and suggest that the 'pernicious effects' of the belief in the single scientific system, not only consumes resources in the sciences themselves, but indeed in the philosophy of science. I contend that our

understanding of the stratification of the sciences is limited by these prejudices about what makes a discipline scientifically legitimate. In particular, we take disunification to mean what it does because we think we have a prior stable account of what science should *ideally* be.

My claim is that we are not warranted in assuming the ideal of successful science laid down by the language of logical empiricism. Moreover, at present the unrelenting belief in it is blocking progress on altogether different issues that might otherwise be raised. Given the hypothesis of disunification, a further enquiry is required, which asks the question whether the criteria that we use to assess the fundamental sciences capture the phenomena that support the inductive inferences we make in the special sciences. Moreover, rather than assuming that these sciences function in the same way as their physical science counterparts we might ask whether they in fact do.

1:5 Thesis Overview:

The particular focus will be on whether a science needs empirical laws to be rendered scientific. In the next chapter, I will argue that Fodor's (1981) reading of the disunity of the sciences hypothesis is solely dependent on an account of *ceteris paribus* laws in the special sciences. I will argue that the account of special sciences kinds and laws that emerges on this view is deeply pervaded by the kind of scientific fundamentalism that I have looked at in this introductory chapter. I will argue that the kinds of natural kind found in the special sciences are not robust enough to underwrite scientific explanations and predictions. Therefore, special science disunificationism is solely dependent on a construal of the empirical laws of the special sciences. Consequently, the remainder of the thesis will examine the issue of *ceteris paribus* laws.

Chapter's *Three* and *Four* will examine some independent semantic, epistemic and metaphysical reasons for rejecting *ceteris paribus* laws in the special sciences. However, in Chapter *Five*, I argue that a glimpse at how we use *ceteris paribus* clauses in different generalisations suggests that it is simply false to assume that all laws are the same and indeed that all sciences ought to be given the same analysis. I propose that *Special Science Realism* can be defended from a methodological point of view, where it is claimed that the generalisations in the special sciences function in an entirely different way to the generalisations in physics.

CHAPTER 2 : THE DISUNITY OF THE SPECIAL SCIENCES: LAWS AND KINDS

2:1 Introduction:

In the last chapter, I examined how scientific fundamentalism shapes and informs the possible ways we might delineate the sciences. In this chapter, I will look more particularly at the case of the special sciences. As we have seen, the disunity of the sciences hypothesis introduces a distinction between the fundamental sciences like physics and the special sciences such as geology, meteorology, psychology and the medical sciences. These later special sciences are considered 'special' in so far as they cannot be straightforwardly reduced to physics.

In section **2:2**, I will examine the claim that reduction is prohibited by the empirical hypothesis that special science kinds are multiply realisable in physical kinds, known as the multiple realisability thesis (henceforth MRT). In section **2:3**, I will assess how Fodor (1981) has proceeded to cash out the notion of a special science given the premise of disunification. I will reveal that the burden of proof lies with theorists like Fodor to give an account of empirical laws in the special sciences.

The most serious issue, I will argue, is the assumption that a special science must satisfy the stringent criteria set forth by successful fundamental

science (i.e. the hard physical sciences like physics), such as wide scope explanations by an appeal universal generalisations (laws) which in turn delineate real natural kinds. More particularly, in section **2:4**, I will reveal that a deeper analysis of the relationship between laws and kinds in the special sciences entails that Fodor's realist account of the special sciences is solely reliant on an account of their empirical laws.

In section **2:5**, I will examine whether Fodor's theoretical account of the special sciences is consistent with how these sciences function in scientific practice. In Section **2:6**, I reveal that our understanding of the disunity of the sciences is motivated by the biased assumption that all genuine scientific explanation must be nomological. However, a glimpse at *ceteris paribus* covering law models reveals that there is a grave semantic problem that faces *cp* laws.

2:2 The Irreducibility of the Special Sciences:

In the last chapter, we saw that the great aim of the unity of science project was to make all scientific disciplines reducible via a Nagelian Schema for reduction (at least in principle) to a grand unifying physical theory. Fodor (1974) argues that reductionism and the resultant unity of the sciences programme are gravely misleading. If reductionism were true then every special science kind would be a physical kind, or would at the very least be coextensive with one. So, every predicate, which appears in the antecedent of a special science law, should appear as one of the reduced predicates in a bridge principle, together with the

assumption that the kind predicates are the ones whose terms are the bound variables in proper laws.

What would the relationship between a reduced special science and a reducing physical science look like under the paradigm of reductionism? Fodor (1974 : 139) symbolises it in the following way:

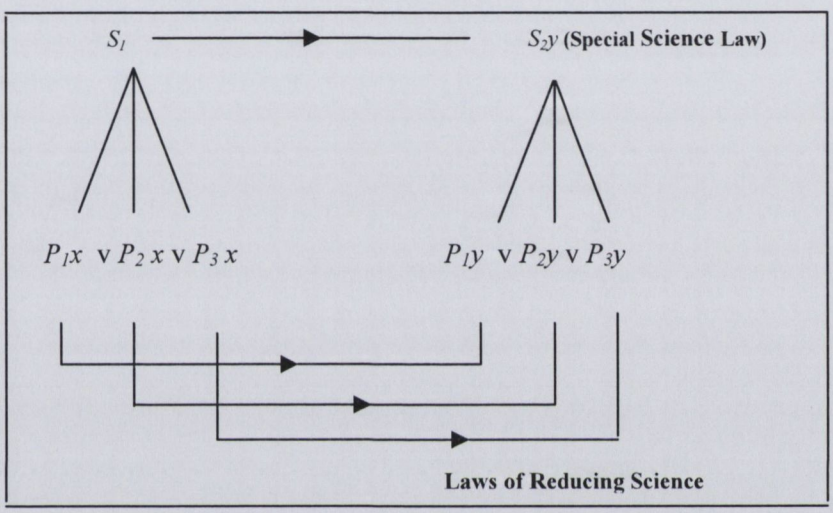

Fig 2:1 Illustration of the Relationship between the Special Science and the Reducing Science Under the Paradigm of Reductionism

The conditional in (1) below is a genuine law of the special science (S).

(1) $S_1x \rightarrow S_2y$

Where (1) should be read as 'all events which consist of x's being S_1 bring about events which consist of y's being S_2.' Both S_1x and S_2y pick out real special science natural kinds. Were the paradigm of reductionism to be correct then we should have a candidate physical law, which should in principle reduce the special science law. A conditional like (2) would be such a law of physics (P).

(2) $P_1x \rightarrow P_1y$[1].

Where (2) should be read as 'all events which consist of x's being P_1 bring about events which consist of y's being P_2, where P_1 and P_2 are predicates of physics and (2) is a physical law.

In the last chapter, we saw that according to Nagel (1961) all reductions must either be *homogeneous* or *inhomogeneous.* A reduction is *homogeneous* if all the descriptive terms in the conclusion are either present in the premises or can be defined explicitly using terms that are in the premises. In contrast, a reduction is *inhomogeneous* if at least one descriptive term in the conclusion neither occurs in the premises nor is definable by those that do occur in them. Therefore, it is necessary to provide bridge principles, which connect the terms of the reducing theory and the reduced theory in inhomogeneous reductions.

So according to the Nagelian schema for reduction, it must be possible to provide either homogeneous or inhomogeneous reductions for intertheoretic reduction to be possible. However, it is not possible to provide a homogenous reduction for any special science because there are terms in the conclusion of the deduction which are not present in the premises. If S_1x were reduced to P_1x then there would be no mention of P_1x in the premises, nor could P_1x be defined explicitly using terms that are in the premises (i.e. S_1x).

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[1] According to Fodor, the connective in the conditional must be transitive. This is important because it is not literally presumed that any given special science would reduce to physics directly, but rather that the special science proceeds via bridge laws, which connect their predicates with those of intermediary theories. Thus psychology will reduce to neurology, which will reduce to biochemistry, which will reduce to physics. (See Fodor (1981) : 129)

At best, therefore, any possibility of reducing a law of the special science (1) to a law of physics (2) will be inhomogeneous. So, if the special science (S) were reducible to physics (P) then laws like (1) above should be reducible to laws like (2) above via biconditional bridge principles like (3) & (4) below:

(3) $S_1x \leftrightarrow P_1x$[2]

and

(4) $S_2y \leftrightarrow P_1y$

Nevertheless, Fodor (1974) opens up the possibility that even inhomogenous reductions are impossible. Reduction, according to Fodor, is precluded by the multiple realisability thesis (MRT). *MRT* claims that it is an open empirical possibility that the kind predicate(s) of a reduced science (i.e. a special science) may correspond to a widely disjunctive and heterogeneous set of kind predicate(s) in a reducing science (i.e. physics) and hence, that there is no one-to-one correlation between the predicates of physics and those of the special sciences. Fodor argues that there is an empirical possibility that every special science kind (such as S_1x) is not coextensive with a physical kind predicate (such as P_1x) where some bridge principles like (3) expresses a nomologically necessary property identity.

Fig 2.1 above then will be re-formulated as *Fig. 2.2* below where the physical predicates satisfy an open-ended disjunction.

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[2] Bridge laws will hold by virtue of the identity of the events that satisfy their antecedents, because Fodor is a token physicalist, rather than a type physicalist.

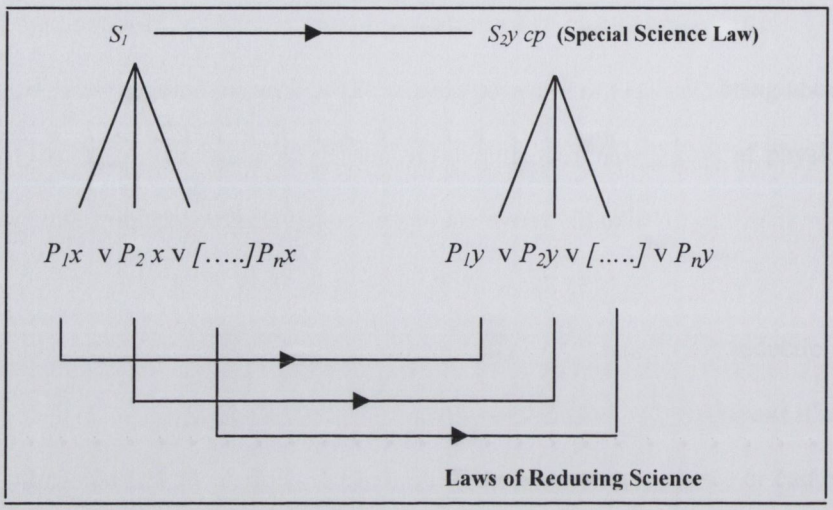

Fig 2.2 Illustration of the Relationship between the Special Science and the Reducing Science in Special Science Realism

Fodor uses the example of Gresham's law in economics, which can be stated in the following way:

> (5) *Ceteris Paribus*, good money (money that has little difference between its exchange value and its commodity value) drives bad money (money that has a substantial difference between its commodity value and its market value, where market value is lower than exchange value) out of circulation.

Fodor argues that it is empirically possible that the predicate 'money' in this law is multiply realised by different physical predicates. He states:

> I am willing to believe that physics is general in the sense that it implies that any event which consists of a monetary exchange (hence any event which falls under Gresham's law) has a true description in the vocabulary of physics and in virtue of which it falls under the laws of physics. But banal considerations suggest that a physical description which covers all such events must be wildly disjunctive. Some monetary exchanges involve strings of wampum. Some involve dollar bills. And some involve signing one's name to a check. What are the chances that a disjunction of physical predicates which covers all these events (i.e. a disjunctive predicate which can form the right hand side of a bridge law of the form '*x* is a monetary exchange ↔ .....') expresses a physical kind? (Fodor (1981): 134)

One possible response might be that Gresham's law is somehow just a placeholder for some future law of economics that would be more rigorous (e.g. a law of microeconomics). However, Fodor is making a stronger theoretical claim namely that *any* law of economics would be similarly open-ended, so even if one were unimpressed with the empirical rigour of Gresham's law, any other true and counterfactual supporting generalization of economics would face the same challenge of multiple realisations[3].

The consequence then for the stratification of the sciences is that multiple realisability precludes the "in principle" reduction of any special science to a lower-level science (e.g. physics). Rosenberg (1985) provides an excellent example of the failure of the reduction of a special science (Mendelian genetics) to a lower-level science (molecular genetics), in so far as it would be impossible to reduce the terms of one theory to those of another. He states:

> Such vast, unwieldy, general statements as we might construct - in which a Mendelian gene is equated with a molecular one - will be full of disjunctions, conjunctions, negations, exceptions, qualifications. It will make so many appeals to stages in the pathway between the DNA and the phenotypic endpoint of the pathway that it will be without any scientific standing. (Rosenberg (1985) : 107).

[3] Fodor would take his argument to generalise to other laws in economics too. For example, "*Ceteris Paribus*, a high minimum wage increases unemployment. A high minimum wage makes labour intensive processes more expensive and tends to cause an unemployment increase because staff become a more expensive business commodity. However, we can imagine exceptional cases to this generalisation. for example, if the wages of employees are raised, then employees will be able to afford to purchase more goods and so the demand for goods will increase and since an increase in demand will require an increase in supply then more workers will be needed. Therefore, we can see that "A high minimum wage increases unemployment" is true "all other things being equal."

2:3 Disunification of the Special Sciences:

Despite the failure of reduction, Fodor argues *pace* Churchland (1981) that a failure to reduce ought not to entail that these sciences are simply so outmoded that we ought to eliminate them. Fodor takes the explanatory rigour of the special sciences as evidence of their scientific prowess. Taking explanatory rigour and irreducibility as a starting point, Fodor is convinced that the scientific "autonomy" of the sciences can be secured.

Fodor argues that the laws of the special science like (1) above express interesting generalisations about special science kinds whose physical descriptions have as a matter of fact got absolutely nothing in common[4]. Consequently, Fodor (1974) (1991) argues that we ought to take the empirical hypothesis of *MRT* seriously and conclude that the special sciences are scientifically legitimate, in so far as there have genuine empirical special science laws that refer to real natural kinds.

An example may prove helpful here. In intentional psychology, a putative law will be something like the following:

(6) (x) (p) [(x fears that p) $\supset$ (x desires that $\neg p$) *ceteris paribus*]

The suggestion is that the special science unifies and generalises about various properties that might be considered disparate at the micro-physiological level. So,

[4] Special science generalisations are considered 'interesting' according to Fodor, because often whether the physical kinds that implement the special science kinds on different occasions have anything in common is entirely irrelevant to the truth of the generalisation, its degree of confirmation, its testability, its projectibility and so on.

if we look at all the neurophysiological states that implement different tokens of fear in different human beings, or even different tokens of fear in the same human being over time, there will be many candidate realising states ($P_1x \vee P_2x$ [.....] $\vee$ P_nx). Moreover, laws like:

(7) $(\forall x)(\forall y)[(P_1x \vee P_2x \vee [.....]P_nx) \rightarrow (P_1y \vee P_2y \vee [.....]P_ny)]$

ought not to be considered to be genuine laws because the antecedent and the consequent formulated in this way are wildly disjunctive.

Fodor argues that a disjunctive generalisation cannot be considered to be a law because disjunctive predicates cannot refer to real natural kinds. Moreover, open generalisations (i.e. laws with disjunctives) fail to pick out the truth of higher-order generalisations. So, the higher-order generalisation is still required. Fodor (1997) explains this in the following passage. He states:

> For, to offer a law of the form $(R_1 \vee R_2 \vee \ldots\ldots) \rightarrow Q$ is to invite the charge that one has failed to correctly identify the property in virtue of which the antecedent of the law necessitates the consequent. Or to say the same thing the other way around: Someone who offers such a law undertakes the burden to provide a positive reason that there isn't a higher level but nondisjunctive property of things that are $R_1 \vee R_2$....in virtue of which they bring it about that Q. (Fodor (1997): 18-19.)

Consequently, the burden of proof is on the reductionist to explain how macro-level natural kinds are to be locally reduced to their micro-level disjunctive properties[5]. In order to do this, Fodor argues that we have to first of all attack the

[5] Kim ((1993) p. 10) argues that reduction is not prohibited by the multiple realisability thesis and that disunificationism is consequently an incoherent claim. Although, there cannot be global bridge principles that connect special science laws with the reducing scientific laws that implement them, we can specify bridge laws that are local and context-specific. Therefore, according to Kim, we can give a perfectly coherent account of disjunctive heterogeneous kinds that operate in laws. However, Fodor's (1998) reply is that the burden of proof is on the reductionist to explain why the special sciences do not have self-standing empirical laws. I will examine this in greater detail later on in this chapter.

generalisations that those natural kinds feature in. In other words, in order to show that there are no self-standing natural kinds in the special sciences, we have to make a compelling case for why there are no genuine special science laws[6].

Fodor is motivating a very weak construal of natural kinds here where a kind will possess kind membership if it is one of the "bound variables in a proper law"[7]. The lower level disjunctive generalisation is not a proper law because disjunctive predicates do not correspond to 'natural kinds'. For this reason, Fodor suggests that the special sciences are "autonomous". Evidently, Fodor views the relationship between laws and kinds as rather reciprocal. However, I wish to argue that the strength of Fodor's disunity claim is reliant on precisely how we are to construe the relationship between special science natural kinds and special science laws.

In the last chapter, we examined Carnap's (1938) different unification claims. The first claim, *language unificationism,* is the unification of scientific language, *viz.* a common reduction basis of all the terms of the distinct sciences and disciplines to a common scientific language. The second claim, *laws*

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[6] Fodor sees no problem in making a case for special science laws. He maintains that theorist have been mislead in thinking that laws only cover causal relations. For Fodor, they also function as the expression of non-causal higher-order generalisations that can only be viewed and interpreted only from that level. Fodor maintains that a law may be essential for the purposes of explanation and prediction, despite the fact that the predicates contained in the law are strictly speaking causally superfluous. For example, he states

> What's gone wrong, I think is failing to notice that laws do more than cover singular causal relations[.....]the function of laws in explaining and predicting events that fall under them need not be its *whole* function. A law may be essential for capturing generalistations even where overdetermination would in principle make it superfluous for causal explanation. When this is the case we put up with the overdetermination because we want the generalisation.' (Fodor 1991: 30)

[7] A metaphysically stronger notion of natural kinds would take kind membership to be defined by the possession of some (perhaps micro-structural) properties that are considered necessary and sufficient for kind membership.

unificationism, which is entailed by the first, is the claim that all the laws of special and social sciences will be made reducible to the laws of physics. The third is the strongest claim, *kind unificationism*, which states that once the transitive process of theory reduction reaches the ideal limit (unification, the only natural kinds that will exist will be those of physics. Conversely, in order to assess the strength of Fodor's disunity claim we must assess whether his claim to disunity ought to be read as a disunity of language, laws or kinds.

2:4 The Three Disunity Claims:

2:4:1 Language Disunificationism:

To examine the strength of the disunity of the sciences claim that Fodor makes it is important to analyse his account so as to get clear on the strength of the thesis that he is making. Take *language disunification* firstly. On Fodor's (1974) account, there is a disunification of language in so far as the terms in the reducing theory do not correspond to the terms in the reduced theory. Fodor (1974 : 134) uses Gresham's law as an example of an autonomous law in economics. As we have seen above, Fodor argues that the descriptive term 'money' will correspond to a wildly disjunctive set of physical possibilities (e.g. strings of wampum, signing one's name to a check, a dollar bill, a euro etc.)[8] So, the terms of the reducing theory do not correspond to the terms of the reduced theory in any straightforward way. This, in effect, is what precludes homogenous

[8] Rosenberg's above example of the attempted reduction of Mendelian genetics to molecular genetics is also accommodated here. The descriptive term 'gene' will correspond to various stages in the pathway between the DNA and the phenotypic endpoint of the pathway. There is no straightforward translation of the terms in the language of the theory of Mendelian Genetics into the terms of the language of the theory of molecular genetics.

reductions of the special sciences to any realising physical sciences, according to Fodor. Therefore, we can take the weakest claim that Fodor is making to be that of *language disunification*[9].

2:4:2 Laws and Kind Disunificationism:

However, *laws disunificationism* is entailed by *language disunificationism*[10]. Since, the predicates of the reduced science correspond to a wildly heterogeneous group of predicates in the reducing science (i.e. multiple realisability) it will not be possible to provide an inhomogeneous reduction *via* bridge principles. Take Fodor's own example of Gresham's law (5) above:

> (5) *Ceteris Paribus*, good money (money that has little difference between its exchange value and its commodity value) drives bad money (money that has a substantial difference between its commodity value and its market value, where market value is lower than exchange value) out of circulation.

The suggestion is that since the predicate 'money' is multiply realisable in many different physical predicates, then there will be no possibility of providing bridge principles between the predicates in the higher-level law and those in the lower-level ones. Moreover, since the lower-level laws will be disjunctive, they cannot genuinely delineate natural kinds, so for this reason it is preferable to retain the higher-order *special science* law and reify the natural kinds of the

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[9] If the disunity claim were merely that of *Language disunification*, then the autonomy of the special sciences could be interpreted as a linguistic or explanatory claim where the unity of the sciences was precluded by a semantic failure to provide a homogeneous and determinate language that would be applicable in all sciences. So, in the theoretical case where it was possible to provide a unification of kinds and laws, but impossible to provide a unification of language the 'autonomy' of the special sciences would not be a very strong claim.

[10] This is because the language used in the special sciences is different to the language used in physics and there is no means of translating the descriptive terms of the special sciences into the descriptive terms of physics without a subsequent loss of meaning.

special sciences. Therefore, in Fodor's earlier work he appears to be committed to all three theses, language disunification, laws disunification and kinds' disunification.

However, Fodor (1974), (1981) leaves the relationship between laws and kinds rather unexplained in his earlier work. He accepts a weak construal of special science kinds, such that the multiply realised kinds of a special science are no more than "the bound variables in its proper laws". (Fodor (1981) : 132) Nevertheless, interestingly enough he does admit that he is building into his account a rather murky construal of this relationship.

> If I knew what a law was, and I believed that scientific theories consist just of bodies of laws then I would say [.....] roughly, the kind predicates of a science are the ones whose terms are the bound variables in its proper laws. I am inclined to say that even in my present state of ignorance, accepting the consequence that it makes the murky notion of a kind viciously dependent on the equally murky notion of *law* and *theory*. There is no firm footing here. (Fodor (1981) : 132)

Although, the relationship is left rather murky, the weaker notion of natural kind that is being incorporated here means that Fodor is making the laws of the special sciences more basic than the natural kind predicates that figure in these laws. I wish to argue that an argument for the disunity of the sciences cannot afford to leave this relationship murky. It is important to consider whether laws of nature or natural kinds are indeed more basic. On the one hand, if the laws of the special sciences are more basic, then Fodor must provide us with an account of special science laws in order to corroborate his disunity claim. Moreover, this account must not presuppose any account of natural kinds. On the other hand, if the natural kinds of the special sciences are more basic he must

provide an account of natural kinds that does not presuppose laws of nature and which corroborates his disunity claim. In the end, the commitments made here will determine how we should understand the disunity of the sciences, given *MRT (Multiple Realisability Thesis)*.

Therefore, whether or not *MRT* entails the disunity of the sciences and how strong the claim of disunity in fact is, requires a deeper analysis of the relationship between the laws and kinds of the special sciences than Fodor provides in his earlier work (Fodor (1974). Determining the precise relationship between laws and natural kinds is clearly problematic.

It is important to consider whether laws of nature or natural kinds are indeed more basic. On the one hand, we want the laws of a theory to be those that allow us to make inductive inferences to future instances of the kind predicates expressed in the law. We want the laws somehow to corroborate our inductive claims. On the other hand, in so far as we want to be able to make stable inductive inferences, we want some ontological account to corroborate our realism about scientific knowledge.

It is rather difficult to pin down precisely whether it is laws or kinds that are more basic, that is to say, which one, if either, can provide some ontological ground to underpin the claims we make to scientific explanation in science more generally. Moreover, if there are no *a priori* reasons why we would should chose one to be more basic than the other, then at the very least we should have *a posteriori* reasons in that the account we choose should be consistent with how

the special sciences proceed in scientific practice. Let's take a closer look at natural kinds first and foremost.

2:4:3 Special Science Kinds: Jade And Pain

So one counterargument to Fodor's account of the disunity of the special sciences is to claim that in fact there is nothing special about the multiply realisability of special science properties. Bickle (1992) has argued that paradigmatic examples of properties in physics are also multiply realisable. For example, "having a certain mass *x*" or "having a certain acceleration *y*" are realised by many different physical properties. Nevertheless, we consider the mass and acceleration of an object to be respectable physical properties of that object.

From the latter claim that there is in fact nothing special about the multiply realisability of the special sciences, Kim argues that the multiple realisability of special sciences kinds does not in fact preclude reduction. He agrees with theorists like Fodor that multiple realisability makes reduction *ipso facto* impossible. However, Kim (1992) has argued *pace* Fodor that special science kinds are not themselves nomic kinds. However, he argues that local reductions are possible. In fact, special science kinds can be locally reduced to their disjunctive realisers. So, special science kinds are purely conventional, in so far as future science should provide an account of these realising kinds.

Take the example of Jade. We now know, contrary to what was once believed, that jade is not a mineral kind rather it is composed of Jadeite and

Nephrite. We can rightly say that Jade is multiply realisable (henceforth *MR*) in Jadeite and Nephrite. Kim argues that since Jade is paradigmatically *MR*, then it is unprojectible and so, a proposition like (8) below:

(8) Jade is green

cannot be a law. Firstly, genuine laws are projectible; in so far as observation of positive instances of *F*'s that are *G*'s increases our credence that the next *F* will be a *G*. Kim imagines the theoretical possibility that all the past positive instance of (8) were really instances of Jadeite. So (9) below is confirmed rather than (8):

(9) Jadeite is green.

So, we cannot say that what we thought in the past to be positive instances of Jade really confirmed (8). Consequently, (8) is not confirmed by its instances and so is not a law. Kim argues then that there are no laws about Jade and that Jade itself is not a genuine mineral kind. Indeed, Kim takes this to generalise to other special science laws and indeed to generalise to the view that there are no 'autonomous' special sciences too. Therefore, the *ipso facto* failure to reduce special science kinds need not entail special science realism. He states:

> We put the following question to Fodor and like-minded philosophers: if pain is nomically equivalent to N, the property claimed to be wildly disjunctive and obviously nonnomic, *why isn't pain itself equally heterogeneous and nonnomic as a kind?* (Kim (1993) : 323).

Fodor's (1998 : 12) first response is that Kim has not really shown that (8) is unprojectible, but rather makes the methodological point that in scientific practice we would realise that we have in fact made a sampling error, in so far as our past testing involved a biased sample (i.e. all samples of Jade were in fact Jadeite). Nevertheless, Fodor will have to concede that as his account stands,

there is no means of distinguishing between which generalisations are lawful and which are merely accidental. Although, green Jadeite samples tell us that Jadeite is green, they tell us nothing about the colour of nephrite and nephrite samples tell us that nephrite is green, but they tell us nothing about the colour of Jadeite. For this reason:

(10) Jadeite is green *iff* nephrite is green

although true, is a purely accidental generalisation. Similarly, we could say that (11) below is of the same ilk.

(11) Jadeite is green *iff* grass is green.

It is in response to this article that Fodor (1998) pins down his view on the relationship between laws and kinds. Fodor introduces, by way of response, a distinction between a multiply based property that is disjunctive *(MD)* and a multiply based property that is disjunctively realised *(MDR)*. I wish to formulate the distinction in terms of the following two biconditionals:

(MD): Ψ is a multiply based property that is disjunctive *iff* there is no possible world W_2 where there is some property β that realises Ψ, which is not a property that realises Ψ in the actual world W_1.

(MDR): Ψ is a multiply based property that is disjunctively realised *iff* there is some metaphysically possible world W_2, where there is some property β that realises Ψ, which is not a property, which realises Ψ in the actual world W_1.

Jade is an instance of *MD* rather than *MDR*, because there is no instance of Jade in any metaphysically possible world where it has any realizer that it lacks in the actual world (i.e. there is no possible world where we have an instance of Jade, which is not realised by either nephrite or jadeite). So, something with all the other qualitative properties of Jade in another possible world, but which

lacked the essential properties (i.e. Jadeite or Nephrite) would simple not be an instance of Jade.

In contrast, pain is an instance of *MDR* because we can imagine some metaphysically possible world where a token pain state is realised by silicon chips or *z*-fibres firing, or whatever. It is this broader understanding of the multiple realisability thesis that was taken to preclude the intertheoretic reduction (including the local reduction)[11] of intentional psychology to neurophysiology. Presumably, Fodor would take it that the other special sciences are also 'autonomous' because special sciences properties are multiply based properties that are disjunctively realised (*MDR*) rather than multiply based properties that are disjunctive (*MD)*. Therefore, the disunity of the special sciences is motivated by *MDR* understood as a trans-global claim.

Importantly, Fodor is arguing here that the kinds of the special sciences should not be understood as kinds defined by their essential properties. Kinds that are (*MD*) are understood in this way because if you want a sample of Jade, then it must be composed essentially of either Jadeite or Nephrite. In other words, if *J* (Jade) is realised by the disjunction *P* (Jadeite) or (*Q*) Nephrite, then it is metaphysically necessary that the properties a thing has *qua J* are either properties it has *qua P* or properties it has *qua Q*.

[11] The whole point of functionalism in the philosophy of mind was that the multiple realisability thesis is to be understood in this broader sense.

In contrast, kinds that are *(MDR)* are defined by their functional roles, rather than by the essential properties of the kinds that realise them. So, it does not matter if pains are realised by silicon chips, neurons, *c*-fibres or whatever, rather it matters that pains obey a certain causal role. Similarly, in economics it does not matter if money is exchanged in terms of dollar bills, strings of wampum, cheques, credit cards or whatever. What matters is that money performs a certain functional role in a market economy. Even in the medical sciences, (e.g. in a medical science like epidemiology), it doesn't matter what virus, bacteria, or toxin causes an epidemic, but only that epidemics have a certain functional role (i.e. that it spreads exponentially).

Consequently, the natural kinds of the special sciences are not defined by their essential properties, but rather by their functional roles and so it is theoretically possible that these roles could be implemented by heterogeneous mechanisms. As Fodor puts it:

> The physical mechanisms whereby events conform to the laws of the special sciences are heterogeneous (Fodor (1981) : 138).

Moreover, the functional role is revealed in the propositions expressed in *cp* special science laws. This makes it clearer why Fodor gives laws (rather than natural kinds) ontological priority. Laws are of fundamental importance in the special sciences, because it is the laws that capture the functional role that is definitive of kind membership. For example, one aspect of the functional role of pain is expressed in the following proposition:

(12) Pain causes avoidance *cp*.

'Pain' is not understood in terms of some essential properties of natural kinds that make any given instance of pain an instance of pain; rather, it is understood as the kind of mental state that functions to cause pain avoidance behaviour.

Similarly, in economics, one of the functional roles of 'money' is defined by Gresham's law (5), which states that:

> (5) *Ceteris Paribus*, good money (money that has little difference between its exchange value and its commodity value) drives bad money (money that has a substantial difference between its commodity value and its market value, where market value is lower than exchange value) out of circulation.

Equally, in epidemiology, Farr's Law describes one of the functional roles of epidemics:

> (13) *Ceteris Paribus*, all epidemics increase their incident rates exponentially.

The reason why the special sciences remain 'autonomous' is not only that there cannot be any reduction of special science kinds to the disjunctive kinds that realise them at the physical level, but also because it is the special science kind which is the bound variable in the special science law, (whatever way it is realised), that is projectible. Therefore, special science laws are more basic than special science natural kinds on Fodor's account of the disunity of the sciences.

Fodor supports his view by a further methodological argument that if there is a higher-level property that subsumes all the states that satisfy an open disjunction then we will want to formulate our laws in terms of it. So, even if there is no reason for us to reject laws that have disjunctions *as such*, then

methodologically we ought to, because the special science generalisation (law) is stronger and is confirmed by more evidence than the corresponding disjunction. Therefore, if special science laws are legitimate in so far as they are confirmed by their instances and support and corroborate the causal-explanatory claims of the special sciences, then this should be sufficient to reify the properties of special science kinds. He states:

> Why are we prepared to buy nondisjunctive laws at the cost of reifying high-level properties? My story is that this policy complies with a dictate that inductive practice obeys quite generally: *Prefer the strongest claim compatible with the evidence, all else being equal.* Quantification over instances is one aspect of rational compliance with this injunction; reification of higher level kinds is another. (Fodor (1998) : 20; *emphasis Fodor's*)

Therefore, if the evidence supports the generalisations that we make in the special sciences, then we should treat of those generalisations as laws of nature. So, we can see that Fodor is making two claims here. Firstly, the generalisations of the special sciences are projectible, in so far as they are confirmed by their instances and so should be analysed as laws of nature. Secondly, because the laws of the special sciences support their instances more than any corresponding physical (disjunctive) laws, then reifying the kinds of the special sciences is a rational inference to make, when weighed up against scientific evidence and explanation. Importantly, on the Fodorian account the laws turn out to be more basic than the kinds, in so far as the kinds are parasitic on the *ceteris paribus* special science laws that are confirmed by their instances.

Fodor is committed to rejecting the claim that kinds are more basic that laws because a commitment to this claim entails an essentialist view of natural kinds where a natural kind is defined by its essential properties. Moreover, a

commitment to this stronger essentialist notion of natural kinds would entail that the multiple realisable properties are merely *MD* and so such an account would be faced with Kim's challenge of local reductionism. On the other hand, in so far as he makes the laws of the special sciences more basic, he must provide an account of why the generalisations of the special sciences can be construed as genuine *cp* laws, which does not presuppose a stronger account of natural kinds.[12]

2:5 Special Science Kinds and Scientific Practice:

2:5:1 Domain-Specificity:

However, before I assess whether Fodor can indeed provide an account of laws in the special sciences, I wish to address another potential problem with his account. A natural enquiry at this juncture is to question whether the theoretical account of the special sciences that Fodor has provided, conforms to how these special sciences actually function in scientific practice. In particular, are the empirical generalisations of the special sciences related to special science kinds in the way that Fodor construes them? Undoubtedly, if the theoretical account of the special sciences that Fodor has motivated is to be accepted then at the very least it ought to be consistent with scientific practice in the special sciences.

What does it mean to say that the putative *cp* laws of the special sciences refer to natural kinds? The most characteristic feature of the generalisations that are used in special science disciplines is that they have *ipso facto* exceptions. So,

[12] In the next chapter, I will analyse the semantic and epistemic problems in providing an account of the putative *cp* laws of the special sciences. Fodor thinks that he can provide truth conditions for *cp* laws, which makes them both meaningful and falsifiable. See Fodor (1991) for this account.

the putative laws of the special sciences only hold "all else being equal" or *ceteris paribus*. Reading this latter construal into the Fodorian account, we can say that on Fodor's account the laws of the special sciences succeed "all else being equal" in picking out the functional properties common to special science natural kinds in a certain environment or domain. Exceptions would not present a problem as such, if we were able to assess all of the possible exceptions to a given *cp* law so that the *cp* clause in any given *cp* law would be in principle eliminable.

However, *cp* generalisations in the special sciences are also domain-specific, that is to say they are relative to a certain spatio-temporal domain. The reason why this presents a problem is because one of the paradigmatic features that laws are supposed to possess as opposed to accidental generalisations is that of wide scope and applicability. The generalisations of the special sciences are narrow scope in that the functional roles expressed in *cp* laws only hold relative to a specific environmental domain. Lange (2002) has noted that in scientific practice this is how all of the special sciences work. He uses as examples the domain-specificity of enquiry in the special sciences such as "Establishment and maintenance of claw bilateral symmetry in snapping shrimps". (Lange (2002) : 247.)

Why is domain-specificity an issue for the special sciences? The first problem is that the laws of the special sciences are not exceptionless and so not *all* of the members of the kind will have *all* of the functional roles expressed by the laws *all* of the time and in *all* settings. The second problem is that if the generalisations are subject to exceptions and relative to a given contextual

domain, then there is an open possibility that the functional roles attributed to special science kinds may change or cease over time or at least given certain contextual situations.

Rosenberg (2001) argues that in fact all cases of multiple realizations in the special sciences are the result of natural selection. Consequently, there will always be a heterogeneous group of physical mechanisms that are functionally equivalent because they are trying to solve the very same design problem. The problem that Rosenberg raises is that the degree of stability of this functional role would be dependent on a stable and unchanging environment. However, the environments or domains that the mechanisms occupy are ever changing and presenting new design problems. So, it is always possible that any given functional role is merely relative to that specific environment at a specific evolutionary stage. There is no reason in principle why that functional role should remain in the future, if it were no longer required.

Undoubtedly then, there is an issue regarding whether or not the domain specificity of the special sciences entails that the functional role of certain special science kinds is variable. The degree of stability of the functional roles expressed in *cp* generalisations is an important issue. To make this clearer, consider the theoretical case that a given functional role (expressed in a *cp* generalisation which is thought to pick out a special science kind) was to cease because of some environmental intervention or change. In the case that we were never to have evidence for this functional role again would we be correct in holding nevertheless that the special science generalisation that describes it is a law?

Rosenberg (2001) argues that this does present a problem for special science laws. He states:

> What this means, of course, is that any generalization linking a single functional kind to a set of physical realizations will be at best a temporarily true claim about a historically limited pattern to which, over evolutionary time periods, the number of exceptions will mount until its subject becomes extinct. (Rosenberg (2001) : 367).

An example will prove helpful here. Consider Bergmann's Law in Biology, which describes the size differences in populations of species of animals in restricted climactic conditions. It states that:

> (14) For species of warm-blooded vertebrates, races living in cooler climates are larger than races living in warmer climates.

There are however, genuine exceptions to this putative special science law. For example, a number of animals such as groundhogs, foxes, chipmunks, and moles dig burrows. The body size of these animals does not alter in cooler climates, as they do not need the higher body mass-to-surface area ratio that favours heat retention. Therefore, there are actual exceptional cases that are not accommodated by this generalisation.

Secondly, this law is restricted to a certain domain (i.e. warm blooded vertebrates in cooler or warmer climates). The contextual setting or environment can also dictate whether the law is applicable in a given case. For example, the soil type that the vertebrates inhabit can influence whether the weight of vertebrates varies according to Bergmann's law. According to Hamilton (1957) a study of bobwhites in glaciated regions of Ohio suggested that they are larger than bobwhites in other parts of Ohio. In this counterexample, we have an alteration in body size, which is not the result of the animal being in a cooler climate, but in

fact is influenced by the soil type. In other words, the contextual conditions may make the law false in certain circumstances.

In a similar vein, Millikan (1999) argues that this context-specificity of the special sciences in scientific practice entails there are in fact no special sciences in Fodor's theoretical sense. Millikan claims that "the theoretical idea that there could be a univocal empirical science that ranged over multiple realizations of a functional property is quite problematic" (Millikan (1999 : 47). In particular, it is not clear why a variety of objects that exhibit the same functional property should be alike in any other respects. Millikan agrees that a functional property may be instantiated by different mechanisms, but queries the entailment from the propositional fact that they exhibit these functional roles (i.e. the empirical law) to the claim that they form a natural kind which would ground our inductions to further functional properties of the same kind.

Essentially the disagreement between theorists like Millikan and Fodor amounts to a disagreement about how we ought to cash out functional role that is described in the generalisations of the special sciences. Fodor argues that functional role is simply the role attributed to special science kinds in special science laws. On the contrary, Millikan argues that Fodor's account is too stringent and that special sciences kinds are defined by reference to historical relations among the members, what she terms their 'proper function'. Rosenberg (2001) would agree with Millikan *pace* Fodor that since the environment in which a functional role is implemented is constantly changed, it is always an open

empirical possibility that the functional role expressed in the generalisation may change or cease altogether.

Millikan (1999) claims that special science kinds ought to be described as 'historical kinds'. The 'proper function' of a historical kind is determined by the historical function that has been associated with a kind in a given special science. She argues that special sciences kinds are defined by reference to historical relations among the members, rather than by reference to functional properties that are attributed to the members in special science laws[13].

Millikan's notion of 'proper' function is a looser notion than that of functional role in Fodor's account. Importantly, the theoretical possibility that 'proper function' might change or indeed that the functional role is not always instantiated in every kind member is left open. This leaves ample room for the *ipso facto* exceptional cases to the generalisations that are so characteristic of special science disciplines.

In a similar vein, Dupré (1983) argues that we can never hope to distinguish completely homogeneous kinds in the special sciences. The putative laws of the special sciences, which would delineate homogeneous kinds, are restricted to abstract models, which idealise away from what actually happens in scientific practice in the special sciences.

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[13] In a similar vein, there is a considerable debate in the philosophy of biology regarding whether species should rightly be considered kinds. One view is that species are not kinds, but individuals. See Ghiselin (1974) and Hull (1976). This reading of biological individuals reveals that we should not even seek to discover laws in biology, because essentially biology just does not work that way. The claim is that biology has an altogether different methodology.

So, we could formulate laws in the special science, in terms of the abstract models that try to impose order on nature. However, to do this would be essentially to omit important characteristics of the special sciences; namely their ability to explain not only why things happen when they do, but also why things don't happen when they do not. Therefore, what is characteristic about the special sciences is that they are interested not just in when a generalisation holds but also when it does not.

Dupré (1983 : 331-332) reveals that in a special science like ecology, formulating laws that refer to homogeneous kinds in actual fact leaves something out. If we were to suppose that the ecological generalisation:

(15) 'Lynxes eat hares'

were a special science law that picks out two homogeneous kinds, we would suppose that all lynxes should eat hares *cp.* However, Dupré makes the interesting observation that an ecologist would not be surprised when some lynxes or hares deviate from the norm. In fact, it is deviations from the norm that make up the rich tapestry that makes ecology such an interesting discipline. He states:

> [....] we would not assume that the same conditions apply to every lynx. Some lynxes do not have hungry kittens; some can run faster than others and therefore catch more distant hares; and so on. Moreover, some hares, such as elderly sick ones, will no doubt be easier to catch than others, who might be young and healthy. And finally it is more than doubtful whether even a complete specification of all the relevant properties and capacities of both animals would be sufficient to determine whether the hare gets eaten. One or other might trip; the hare might run into a cul-de-sac, or discover an escape hole; etc. (Sometimes you get a bear, sometimes the bear gets you.)" (Dupré (1983) : 331-332.)

A glimpse at special science kinds in scientific practice then is informative for our account of the special sciences. We saw earlier that Fodor's account of the

special sciences makes special science kinds dependent on special science laws. In fact, a glimpse at the special sciences in scientific practice would suggest that Fodor's theoretical account states the case too strongly.

Fodor states the cases as strongly as he does because he wishes to avoid the immediate consequence of the weaker account of the special science, which is that the special sciences become *descriptive* rather than *explanatory* on this account. It might be argued that, on these weaker accounts of the special science (Dupré (1983) Millikan (1999), Rosenberg (2001)) normal science within the special sciences becomes the task of taxonomising the universe within specific domains. In other words, the task of the special sciences is a purely contingent historical one, namely that of sorting out the macro-level distribution of matter in certain spatio-temporal zones. For this reason, the special sciences become more like the social sciences than the fundamental sciences in methodology.

In the last chapter, we saw that there is a competing view of the stratification of the sciences (human science disunificationism) which claims that there is a fundamental distinction between the methodologies of the social sciences and the hard physical sciences, in so far as the social sciences are idiographic, characterized by singular assertory propositions and individualising conceptualisation and the natural sciences are nomothetic, characterised by general apodictic propositions and generalising conceptualisation. (Schutz (1954): 257).

The claim then would be to argue that the so-called special sciences are similar to the social sciences in methodology, rather than to the physical sciences. This analogy between the special and social sciences is meant to imply that the special sciences are not scientifically legitimate. The idea is that once we give up on general laws, the special sciences become idiosyncratic sciences. Hence, while these disciplines might be perfectly valid for describing as heuristic devices for the scientist to aid in describing the macro-world, they are not the subjects of real science, in so far as science should discover universally true explanations of the universe.

The assumption here is that because the kinds of the special sciences are only projectible within a narrow scope that they consequently, cannot appear in genuine nomological explanations. The inference then is that since the special sciences (so-described) do not generate nomological explanations, then they are not scientifically legitimate. The idea is that an explanation that is generated from a generalisation with restricted scope (i.e. one that is domain-specific) is not legitimate.

The converse then is that if the special sciences are to be rendered explanatorily legitimate then they must have empirical laws[14]. Ironically then, both accounts of disunity of the sciences (i.e. both human science disunificationism and special science disunificationism) are motivated by an

[14] Fodor's assumption that genuine scientific explanations must involve laws of some sort is what motivates his stronger account of the disunity of the sciences. Moreover, Fodor believes that he can in fact make a case for *ceteris paribus* special science laws. We will examine the case of *ceteris paribus* laws in the next chapter.

assumption which involves a bias about what genuine explanation involves; namely that all genuine explanation must be nomological.

2:6 Covering Law Explanations:

What does the assumption that all scientific explanation is nomological involve? What is being assumed here is that all explanation should involve either induction or deduction from universal or statistical laws and initial conditions, namely; a Hempelian covering-law model of explanation. As we saw in the last chapter, according to Hempel's *Deductive Nomological* and *Inductive Statistical* model's of explanation, an explanation ought to be a deductive or inductive argument respectively, such that an explanation can be generated from a universal or probabilistic law in conjunction with prior and relevant initial conditions. They can be formulated in the following way:

Deductive-Nomological (D-N) Model:

$L_1 L_2 L_3$	Laws
$C_1 C_2 C_3$	+ Relevant initial conditions
	(*Explanans*)
F_1	Fact-to-be-explained
	(*Explanandum*)[15]

[15] The D-N model must satisfy the following requirements. The premises in the explanans must logically entail the explanandum. The premises of the explanans must be true. At least one of the premises must be a universal law, with unlimited scope that does not refer to particulars. The explanans must have empirical content, which means that they must be testable and observable. Consider the observation that a spoon-handle is bent at the point where it emerges from a glass of water (Hempel (1962) p. 16). This explanandum can be deductively explained from the laws of refraction and reflection in conjunction with initial conditions such as the spoon handle being placed in the water by agent x at time t_1.

Inductive-Statistical (I-S) Model:

$P\ (F,\ C) = r$	Probabilistic Laws
$C_1 \quad [r]$ +	Relevant initial conditions (*Explanans*)
F_1	Fact-to-be-explained (*Explanandum*)[16]

2:6:1 Covering Law Models and *Ceteris Paribus* Laws:

Importantly, since *cp* laws are thought to be neither universal nor probabilistic, an alternative covering-law model is required. Clearly then another motivation for the unrelenting campaign for *ceteris paribus* laws that quantify over real natural kinds is an attempt to fit the explanations of the special sciences into some sort of covering law model of explanation[17]. So what is this assumed *ceteris paribus* covering law model? It might be formulated in the following way:

***Ceteris-Paribus* Covering Law (C-P) Models:**

$CP_1\ CP_2\ CP_3$	Laws + *ceteris paribus* qualifiers
$C_1 \quad C_2 \quad C_3$	+ Relevant initial conditions (*Explanans*)
F_1	Fact-to-be-explained (*Explanandum*)

[16] Inductive-Statistical explanations must satisfy the following criteria. The explanandum must follow the explanans with high inductive probability. The premises of the argument must be true. The explanans must contain at least one probabilistic law. The explanans must have empirical content and so be testable. Furthermore, I-S explanations must have maximal specificity. To have maximal specificity an explanation must not lose force once additional facts are known. Consider the subsiding of an asthma attack following the administration of 200 micrograms of salbutamol. How do we explain this phenomenon? There is no universal law that can be formulated such that "All attacks of asthma subside with the administration of 200 micrograms of salbutamol". Rather, the administration of salbutamol makes recovery from an asthma-attack highly probable.

[17] For a detailed version of this argument see Tobin. E. (forthcoming) 'Towards a New Model Of Explanation for the Special Sciences', Causality & Explanation : Essays in Honor of Wesley Salmon, Autonomous University Press.

Let's consider an example from the special sciences in order to assess how such a model ought to be construed. In a special science like epidemiology, consider the following:

> "Epidemics increase their incidence rates exponentially *cp.* " (Farr's Law)
> X is an epidemic (Initial Conditions)
> X will increase exponentially.

However, there is a semantic problem with postulating *cp* laws to support special science explanations in that exceptions can occur that contravene the law. The problem is that the putative *cp* law can yield true explanations given some initial conditions and false explanations given others. Now the problem that emerges here is that we literally do not know what the *cp* clause refers to. Since we do not know how to cash out the appended *cp* clause, then we literally do not know what it means. This is the semantic problem that faces any construal of *cp* laws[18]. Consider the following examples:

> "Epidemics increase their incidence rates exponentially *cp*" (Farr's Law)
> Bubonic plague is an epidemic caused by the germ *Yersinia Pestis*
> Bubonic Plague increases exponentially.

> "Epidemics increase their incidence rates exponentially *cp*" (Farr's Law)
> Polio is a viral epidemic.
> Polio increases exponentially.

In the first example, we can imagine cases in which the *explanandum* is true such as the Black Death in the 14th Century, i.e. where the bubonic plague did increase exponentially. However, we can also imagine how a change in environmental factors may make it false in other situations. For example, the bubonic plague may not increase exponentially, if certain preventative measures were taken. For example, health authorities might ensure that all people who are

[18] I will consider this problem in detail in the next chapter.

exposed to plague are given antibiotic treatment for seven days, or if hygiene and environmental sanitation are extremely good then the chances of an epidemic are rare. So, given this particular set of initial conditions the law is only true "in the absence of disturbing factors".

However, the second example is a case in point where an epidemic has not increased exponentially, in other words where the *explanandum* has turned out to be false. In other words, polio is one of the exceptional cases that the *cp* clause is meant to accommodate. However, the reason why polio is in fact an exceptional case is highly context specific. Since, Polio has been ubiquitous since the beginning of human history; its incidence rate should have peaked in the past.

However, improvements in sanitation have meant that outbreaks of Polio have occurred most rapidly in the last century. In highly unsanitary circumstances, virtually all children are exposed to the virus during infancy, when infection with polio is most likely to be asymptomatic, which results in lifelong immunity. (Menkes (1995)). Ironically then, improvements in sanitation have meant that people not immuned to the disease contract it later on in life and become infected by it when it is more likely to be symptomatic. For this reason, epidemics of polio have increased their incidence rates non-exponentially.

Consequently, we cannot say which variables must be in place in order for all other things to be equal nor can we say which variables must be absent to make all other things not equal. There is no prior stable and statable way of expressing what *cp* means in advance of applying it in specific contexts. Consequently, we

cannot provide truth conditions for *cp* laws. We literally do not know what it means when we say that a law holds *cp*. This is to argue that *cp* laws are vacuous, in so far as they have no empirical content[19].

One problem then for postulating a model of explanation with *cp* laws is in assessing how we can accommodate such exceptions if we implement the standard covering-law methodology. Another problem is in assessing how we accommodate unstatable context-dependent exceptions, which will affect the probability with which any given *cp* law will hold. Such a model needs to accept low probability explanations without accommodating extraneous ones. Let's take a closer look at two possibilities. How might the *cp* covering law model be formulated?

Firstly, let's take a look at the Inductive-*Ceteris Paribus* Model. This would be formulated as follows:

I-CP

$(P\ (F,\ C)\ = r\ )$ *Cp Ceteris Paribus* Laws
C_1 + Relevant initial conditions (*Explanans*)

F_1 Fact to be explained (*Explanandum*)

This would state that a given *cp* law $(P\ (F,\ C)\ = r\ )$ *Cp* in conjunction with relevant initial conditions (C_1) make the fact-to-be explained (F_1) highly probable. One chief objection to the standard inductive-statistical model is the problem of low probabilities. The problem of low probabilities is that explanations are possible even if the *explanandum* is not highly probable.

[19] This is the standard semantic objection to the construal of *ceteris paribus* laws. See Schiffer (1991), Earman & Roberts (1999) & Earman et al (2002).

Clearly, this problem also applies to the inductive-*ceteris paribus* variant. Since, we cannot know all the exceptions to a *cp* law or when they might occur then we have no idea of the probability of the *cp* law holding. Therefore, we cannot say what the probability of the *explanandum* is in light of the *explanans*. This is because we cannot complete the *explanans*. Special science laws apply to every probability except their out and out negation. So, the most we can say about *cp* laws is that they hold unless they don't. For this reason, an inductive *cp* model will not work, since *cp* laws cannot in conjunction with initial conditions make the fact-to-be-explained highly probable.

One possible response to this worry is that probabilities can be assessed on a case-by-case basis. However, a problem with this approach will arise in scientific methodology in that if it is indeed possible to classify all exceptions in a given context then, scientists can classify just about anything as an exception to the law, in order to retain the legitimacy of the law. In other words, scientists can rig their results to fit the law. In this respect, the vagueness of *cp* special science laws would make the special sciences unfalsifiable. Therefore, construing the explanations of the special sciences in terms of an inductive *cp* model does little to validate their explanatory role.

Let's take a look at the Deductive-*Ceteris Paribus* model. It can be formulated as follows:

D-CP

$CP_1\ CP_2\ CP_3$ *Ceteris Paribus* Laws
$C_1\ \ C_2\ \ C_3$ + Relevant initial conditions (Explanans)

F_1 Fact-to-be-explained (Explanandum)

According to this model, the fact-to-be-explained (F_1) can be straightforwardly deduced from the *cp* law(s) ($CP_1\ CP_2\ CP_3$) in conjunction with relevant initial conditions ($C_1\ \ C_2\ \ C_3$) . However, again the semantic problem for *cp* laws outlined above entails that we cannot say what all of the exceptions are in order to be able to conclude that all other things were equal, that is to say that even if we were able to cash out every possible exception that might occur this would be insufficient for explaining why the law applies when it does[20].

Since, the special science *cp* clause cannot be 'theoretically' completed, then any deductive explanation containing *cp* laws will contain an unstatable first premise. Clearly, if you cannot articulate the premises of the argument, then you don't generate an explanation based on those premises. Therefore, the deductive *cp* model does not validate special science explanations either.

[20] Indeed the relevance of some exceptions might be apparent only when expressed in the language of some more basic science. This is why *cp* laws are sometimes expressed as hedged laws. (e.g. Fodor (1991). I will examine the attempt to save *cp* laws from vacuity by hedging them in the next chapter.

2:6:2 *Ceteris Paribus* Laws & The Semantic Problem:

One obvious response to the above argument is to suggest that perhaps the problem is the commitment to a now much discredited model of explanation[21]. Nevertheless, the interesting upshot of the discussion is that we have honed on a very particular semantic problem that faces any construal of *ceteris paribus* laws in the special sciences. The semantic problem is essentially that we literally do not know what the *ceteris paribus* proviso replaces and consequently, the meaning of the *ceteris paribus* clause is indeterminate.

We saw earlier that Fodor's disunity of the sciences claim is solely reliant on an account of empirical laws in the special sciences. However, it now emerges that the putative laws of the special sciences face a serious semantic challenge. The burden of proof lies with Fodor to answer the semantic challenge for *ceteris paribus* laws in order to defend his account of the autonomy of the special sciences.

2:7 Conclusion:

In this chapter, I have argued that Fodor's account of the disunity of the sciences is solely reliant on an account of their empirical laws. I argued that Fodor is forced to make the laws of the special sciences more basic than their kinds. The

[21] The covering law model of explanation has been discredited and many alternative accounts of scientific explanation have emerged. Some examples are Statistical Relevance and Causal Relevance Models (Salmon (1984), Unificatory Accounts (Kitcher (1981) and Mechanistic Models (Woodward (2003).

alternative possibility that the natural kinds were more basic was precluded by Kim's argument for local reductionism, which coerced Fodor into making a stronger multiple realisability claim in his distinction between MDR (a multiply based property that is disjunctively realised e.g. pain) and MD (a multiply disjunctive property e.g. Jade).

I argued that a glimpse at how the special sciences function in scientific practice suggests a discrepancy between Fodor's theoretical account of the special sciences and an empirical account of them in scientific practice. Moreover, I claimed that Fodor's campaign for *ceteris paribus* laws is the result of the biased assumption that all legitimate scientific explanation must be nomological. The upshot of this analysis is that the putative *ceteris paribus* laws of the special sciences face a serious semantic problem, that we literally do not know what the *ceteris paribus* proviso means. I now turn to this issue in the next chapter.

CHAPTER 3: *CETERIS PARIBUS* LAWS: THE SEMANTIC PROBLEM

3.1 Introduction

Natural and social sciences often hedge their laws with provisos in so far as any law only holds in the absence of disturbing factors or 'all else being equal' (i.e. in Latin, *ceteris paribus*). For this reason, many argue that the scientific status of these sciences is dependant on whether a legitimate construal of *cp* laws can be made. This problem rests on the methodological assumption that any mature science should discover laws of nature, defined in terms of universally quantified conditionals and that such laws are fundamental for the purposes of scientific explanation and prediction in that science. A further difficulty arises in so far as it can be argued that there are *cp* laws even in physics and hence, as Cartwright claims (1983) that it is "*cp* all the way down"[1]. Clearly, if the latter hypothesis were correct then we require an account of *cp* laws if we want to be realists in any regard about scientific knowledge. Consequently, an account of

[1] Some of the laws of physics can be interpreted as *cp* laws. Cartwright's own example is Snell's law in physics. Snell's Law states that:

> At an interface between dielectric media, there is also a refracted ray in the second media, lying in the plane of incidence making an angel θ_t with the normal and obeying Snell's law:
>
> $$Sin\ \theta/\theta_t = n_2/n_1$$
>
> Where v_1 and v_2 are the velocities of propagation in the two media and $n_1 = (c/v_1)$, $n_2 = (c/v_2)$ are the indices of refraction. (Cartwright (1983) : 46).

The reason why this law is *cp* is because it only holds for isotropic media (that is media where only one wave transmitted).

ceteris paribus laws has important implications for the philosophy of all the sciences.

In Section **3:2**, I will outline the notion of a *ceteris paribus* law. In section **3:3**, I will analyse the semantic challenge for *cp* laws, namely that we cannot provide a non-vacuous account of their semantics. The semantic problem amounts to the claim that *ceteris paribus* does not have a determinate meaning. Consequently, it can be argued that we literally do not know what it means to say that a law holds *ceteris paribus*. In Section **3:4**, I will illustrate that any attempt to render *cp* clauses determinate by providing their truth conditions renders them vacuous. In Section **3:5**, I argue that providing the truth conditions by hedging *cp* laws face a dilemma: either it is not *cp* laws all the way down and *cp* laws are placeholders for the real laws of physics, or it is *cp* laws all the way down and the semantic problem cannot be solved by reference to any more fundamental laws and so *cp* laws remain vacuous. In sections **3:6** and **3:7**, I analyse whether a weaker semantic account might be provided. This weaker notion leads to a problem for testing *cp* laws.

Therefore, in section **3:9** I argue that *cp* laws are faced with a pejorative dilemma which combines the *semantic* and *epistemic* problems for them: either (a) *cp* laws can be completed prior to testing, but only either in a vacuous way or in such a way that they are made placeholders for the real laws of physics (the semantic problem) or (b) *cp* laws can be completed within testing, but consequently, the authenticity of the scientific investigation cannot be preserved

(the epistemic problem). Taking either horn of the dilemma construing *cp* generalisations as laws does not make them scientifically legitimate.

3:2 *Ceteris Paribus* Laws:

3:2:1 Laws vs. Accidents:

The standard philosophical problem about laws of nature is how we are to distinguish between those generalisations that we consider lawful and those that we consider merely accidental. Take the statement:

(1) 'All the coins in my pocket are euros'.

We would not wish to consider this statement a law of nature in so far as the claim is purely coincidental. We think that it is, because there is no reason to believe that if I were to take a 50-cent coin and place it in my pocket that it would, by some necessity spontaneously transform itself into a euro coin. In contrast, consider a claim like:

(2) 'All planets move in ellipses'.

We tend to think that a claim like this has a rather different modal status to a claim like 'All the coins in my pockets are euros'. Why do we call the former lawful and the latter accidental?

There is at least minimal agreement in terms of how philosophers of science answer this question. Most philosophers will agree that there are certain normative characteristics a generalisation ought to have to be considered a law of nature. Firstly, a law of nature should be universally true, it should have wide scope and be applicable to a wide range of phenomena. Laws are supposed to be

general and so not to be specific to certain places or times. So, in terms of the above example, we would expect that *all* planets should move in ellipses and we expect this to be a true statement that applies across all circumstances and to all things that we consider to be members of the kind planet. Ideally then laws should be formulated syntactically as universally quantified conditionals in first-order predicate calculus (e.g. $\forall x\ (Fx \supset Gx)$).

Moreover, we expect laws to play a heuristic role in scientific enquiry and experimentation. In terms of scientific practice, then we expect laws to be those true statements that will be confirmed by their instances, and will generate explanations and predictions in conjunction with prior and relevant initial conditions. Laws rule out pseudo-scientific explanations. They allow us to make inductive inferences about the phenomena perceived in nature. In this respect, such laws are projectible, in so far as we expect any future F to be G, in so far as we apply the law that 'All F's are G's'. Consequently, true laws of nature should be confirmed by their instances.[2].

However, I think that this stark contrast between laws and accidental generalisations is misleading in so far as it makes the criteria that a generalisation must satisfy in order to count as a law far too strict. I contend that, in fact, the rigidity of these criteria come from the ideal model for science that we examined

[2] Broadly speaking, it might be argued that laws of nature are different from accidents in that they have modal force that constitutes their lawlikeness. However, what the mark of lawlikeness is (i.e. what gives laws their modal force) is a much-debated matter. There are (generalising somewhat) three competing views about what makes a law distinct from an accidental regularity, the *regularity theory*, the *nomic necessitarianism theory* and *dispositionalism*. The important question of what the truthmakers for *cp* laws might be will be returned to in Chapter 4.

in the first chapter. For this reason, all laws are modelled on the best possible laws that we might achieve in some future comprehensive physical theory.

The problem is that most (or possibly all) of our present "textbook" laws do not satisfy these rigid criteria. Consequently, we are not sure what kind of analysis we should give those present empirical generalisations that we call 'laws' in the textbooks. One possible view is to say that all present empirical laws are of merely instrumental value. We can use these laws to generate reliable explanations. Moreover, we can use them to manipulate and control our environments. However, the claim is that ultimately they are merely placeholders for the future laws of physics.

The suggestion then is that we must bring the laws of different sciences into line with the laws we expect to have in physics. We might have permission to use incomplete generalisations in the interim, but always with the realisation that these laws are in fact not strictly speaking true and that at the first sign of some much improved scientific theory in the future we will in fact be able to eliminate them altogether.

This more general problem that faces our current laws is an even more pertinent problem for the putative laws of the special sciences (e.g. psychology, the medical sciences, geology, economics and so on). The laws of the special sciences not only merely approximate the truth, but also they are in fact descriptively false when read as universal statements. There are widespread exceptions to the laws of these latter sciences. In fact, what is most characteristic

of these putative laws is that they are only true 'all else being equal' or 'in the absence of disturbing factors' or in Latin '*ceteris paribus*'.

The laws of the special sciences deviate from the ideal in so far as they are marked by heterogeneity and exceptions. Special science generalisations predict and explain in an approximate way such with the stipulation that all interfering factors are constant or absent. Take the following example from a special science like chemistry. The law of definite proportions states that:

> (3) In a chemical compound, the masses of the elements are in the same proportions *cp*.

For example, oxygen makes up $^{8}/_{9}$ of the mass of any sample of pure water, while hydrogen makes up the remaining $^{1}/_{9}$ of the mass. Importantly, we can project that in future samples of water that the masses of the elements will be of the ratio 8 : 1 (Oxygen : Water).

However, the reason why this law needs the *cp* proviso is because of the existence of some compounds which are nonstoichiometric, in other words, where the masses of the elements can vary from sample to sample[3]. So, we can predict that "all else being equal" the masses of the elements will be in the same proportions. The problem is that if we read (3) literally without the *ceteris paribus* proviso it would in fact be false. The reason why this is false is because there are cases of nonstoichiometric chemical compounds where the masses of the elements are not in the same proportions. So there are *ipso facto* counterexamples to this putative law.

[3] Examples of nonstoichiometric compounds are magnetite and iron oxide.

Nevertheless, Fodor (1974) (1991) argues that *ceteris paribus* clauses present no insurmountable problems for the special sciences. As we saw in Chapter 2, he argues that despite the possibility of exceptional cases, the extraordinary empirical successes of the special sciences are undeniable[4]. For this reason, he is convinced that realism about the laws of the special sciences is justified.

Taking intentional psychology as an exemplary special science, he argues, for example, that the predictive and explanatory successes of our intentional psychology run so deep, that to us they are practically invisible. (Fodor, (1987) : 3). He motivates the view that we tacitly assume the applicability of commonsense psychology in our everyday interactions with the world and in our intersubjective relations with other human beings. This, according to Fodor, is how we infer people's intentions from the words they utter and people's behaviour from their intentions.

If someone utters the words, "I will see you at 3 p.m. outside the café", then barring confusion, distraction or other interfering factors, that same person will be standing outside the café at 3 p.m. Moreover, this prediction will work across the board, not only with intimate best friends and relatives, but even with complete strangers. Intentional psychology would work even better in a laboratory situation, where any interfering factors could be controlled.

[4] On the contrary, Churchland (1981 : 73-74) argues that in fact the empirical success of a special science like psychology is indeed questionable.

Consequently, Fodor construes the generalisations of the special sciences as genuine empirical *ceteris paribus* laws. So, what are putative *ceteris paribus* laws?

3:2:2 The etymology of *Ceteris Paribus*

The Latin term *ceteris paribus* (henceforth *cp*) which originated in the 17th century is literally translated as "other things the same". However, it can actually be interpreted in many ways. In English, the standard translation is as 'all other things being equal". However, it may also be translated as "all other things being constant", or "operating on the ideal assumption that nothing else changes" or as 'other things being right'[5]. A statement about the logical connections between two states of affairs is qualified by a *ceteris paribus* clause, in order to rule out the possibility of other factors, which could affect, change or override the relationship described between the antecedent and the consequent in the statement.

The *Oxford English Dictionary* quotes the definition of *ceteris paribus* from *The Economist* (1969): 828:2) as "that comfortable hold all, *ceteris paribus*". *Ceteris paribus* is used to replace an indeterminate list of possible interfering factors. We cannot state explicitly what the *cp* proviso is introduced to replace, but we know that there are factors (whatever they might be) which might intervene. Therefore, once the *cp* clause is added on these possible impediments are accounted for.

[5] Cartwright (1983 : 45) argues that it is more apt to translate *ceteris paribus* as 'other things being right' rather than as 'other things being equal', since she understands the *ceteris paribus* clause to indicate that the generalisation only holds in special or unusually ideal conditions.

First uses of the term can be traced back as far as 1662 to William Petty's *Treatise of Taxes and Contributions* (Persky (1990)). However, it is John Cairnes (1857) who first explicitly expounds the idea that the explanations provided by investigations in economics can only be said to hold true in the absence of disturbing causes. Alfred Marshall (1890) is usually credited with popularising the phrase as it pertained to economic methodology. Marshall defined the law of supply and demand with the implicit *ceteris paribus* proviso in the following way:

> When the price of a good is raised (at the same time when all other things are held constant) less of it is demanded. Or, what is the same thing; if a greater quantity of a good is put on the market, then -other things being equal-, it can be sold only at a lower price. (Marshall (1890) : 164).

The *ceteris paribus* proviso here is meant to screen off those factors that could contravene the law from holding.

Although it is unclear when exactly the term *ceteris paribus* made its way into the philosophical literature, it can be seen as early as Mill's *System of Logic* (1843). Mill introduces the notion of an inexact science like Tidology, where the laws are loose and qualified. He states:

> No one doubts that Tidology is really a science. [.....] But circumstances of a local or causal nature, such as the configuration of the bottom of the ocean, the degree of confinement from shores, the direction of the wind [...], influence in many or in all places the height and time of the tide. General laws may be laid down respecting the tides; predictions may be founded on those laws, and the result will *in the main* [...] correspond to the predictions. (Mill (1843), 6:3:1: 553 – Italics added)[6]

[6] The observation that Mill initiated the notion of *cp* laws in the philosophy of science was made first by (Lange (2002) : 412)

Mill explains as early as this that inexact sciences are marked by their qualified laws, laws that have to stipulate the presence of normalising features, and the absence of disturbing ones or as he puts it, laws that only hold "in the main". Herein lays the rather murky notion of a qualified empirical law in the inexact sciences.

3:2:3 *Ceteris Paribus* Special Science Laws:

There are perfectly good generalisations that are used in the special sciences to furnish us with explanations, that by and large deviate from the 'ideal' characteristics that we think a generalisation should have in order to qualify as lawful. For example, there are some generalisations that do not hold universally, or that hold only "in the absence of disturbing factors or "all else being equal". There are *ipso facto* counterexamples to these laws. In the literature, these are referred to as *ceteris paribus* laws.

As we saw in the last chapter, the reason behind the campaign for inexact *cp* laws, by theorists like Fodor, stems from a larger worry about the status of the so-called special sciences, such as biology, epidemiology, geology, astronomy, the medical sciences and so on. The idea is that mature science should discover laws of nature that govern the phenomena it investigates. In so far as the special sciences can be said to be mature and irreducible to the more fundamental sciences, it is argued that the special sciences must have empirical laws. The converse of the disjunction is that, if the special sciences do not have laws, then

they are similar to the social sciences in methodology and so should not be analysed as genuine nomological disciplines.

The problem is that the generalisations that have been discovered in these sciences are not serious candidates for law hood according to strict criteria required for law status (i.e universal generalisations that have wide scope and explanatory power). Moreover, we have reason to believe that there are no such generalisations forthcoming in the special sciences, in so far as the special sciences are just not that exact.

There are two reasons why the laws of the special sciences lack the precision required. Firstly, in so far as we think that laws should be universal generalisations, it is clear that the putative laws of the specials sciences are not universal. In fact, any candidates for laws in the special sciences are rife with exceptions, in that there are *ipso facto* counterexamples to any given special science generalisation. Secondly, in so far as generalisations in the special sciences do support explanations and predictions, they only do so within a narrow domain or in terms of specific contexts and environments. Consequently, any special science law only holds *ceteris paribus* (all other things being equal), where the *cp* proviso is a catchall phrase for those exceptions that might occur and so contravene the law

3:2:4 Mendel's Laws & Exceptions:

Examples of putative *cp* law's are Mendel's laws in genetics[7]. Mendel's first law; the law of segregation states that:

> (4) In *normal* Mendelian segregation, at a genetic locus, half of an organism's offspring inherit one of the alleles and the other half the other allele. (Mendel (1866)) [Italics added]

However, Mendel uses the term 'normal' because he is aware that there are exceptional cases in which the law fails to hold. In these cases, one of the alleles is consistently found in more than half the offspring. Mendel's experiments consisted of interbreeding pea plants in order to observe the traits (characteristics) that would appear in subsequent generations. He noticed that when he crossbred plants with round peas and those with wrinkled peas, all of the resulting offspring had round peas.

He concluded that when an organism has two different alleles corresponding to the same genetic trait, one of the two would be dominant, which geneticists symbolise as 'R'. The other allele is called the recessive allele 'r' and will only be passed on if present in both parents. Consequently, the trait of the dominant allele will have a selective advantage. This sole exception itself may not present a serious problem. In fact, Mendel himself supplemented the original generalisation with one about dominant/recessive genes. Mendel's law of dominance states that:

> (5) In *normal* Mendelian segregation, where one allele is dominant, all of an organism's offspring inherit the dominant allele. [Italics added]

[7] For other examples of *ceteris paribus* laws see *Appendix* II.

However, this itself is also *cp* in so far as two parents that possess a recessive allele may pass this trait on to their offspring. Moreover, there are also other exceptions, for example, in cases of *incomplete dominance*, the inheritance of a dominant and a recessive allele results in a blending of traits to produce intermediate characteristics. For instance, in snapdragons incomplete dominance of the traits is seen when red and white are crossed. The offspring are pink, therefore blending the two traits[8].

Another exception to Mendelian genetics involves genes with *multiple alleles*. Certain traits are controlled by multiple alleles that have complex rules of dominance. Examples are the alleles for human skin and blood groups[9]. Another exception to Mendel's law is the case of *gene linkage*. Sometimes when alleles are situated close to one another on a chromosome they produce offspring with unexpected allele combinations. This is because of a process called crossing-over[10]. An example of an exception to this rule is the fruit fly[11].

Consequently, Mendel's laws only hold given the proviso that some genetic variation does not occur and given that a system of sexual reproduction is in place in the first place *etc*. So, it is more correct to say that Mendel's law of

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[8] Plants with red flowers have two copies of the dominant allele *R* for red flower colour (*RR*). Plants with white flowers have two copies of the recessive allele *r* for white flower colour (*rr*). Pink flowers result in plants with one copy of each allele (*Rr*), with each allele contributing to a blending of colours.

[9] Karl Landsteiner first discovered blood groups with complex multiple alleles.

[10] This is a process during meiosis when two chromosomes (situated close together) may interconnect and exchange places on the chromosome. The pair then breaks apart to form two chromosomes with different genes.

[11] T.H. Morgan (1910) discovered the fruit fly *(Drosophila)*. Drosophila eyes are normally bright red. Morgan discovered an exceptional white-eyed male.

segregation only holds *cp* i.e. in the absence of disturbing factors (where the proviso should list all the possible exceptions that might occur for example, provided that there is not incomplete dominance, provided that there are not multiple alleles in place and provided that gene linkage does not occur and so on).

When Mendel's law is classified as a universal generalisation, then strictly speaking it is false. However, if the *cp* proviso accompanies it, then the thought is that such generalisations can be given the same logical structure as those laws that furnish us with explanations in physics, those laws that are thought to provide the model for the honorific of 'law'. The *cp* proviso screens off the disturbing factors, so that science can show why the law holds in the idealised setting where "all else is equal."

3:2:5 *Ceteris Paribus* 'all the way down'.

However, some theorists argue that in fact it is *ceteris paribus* "all the way down" even in fundamental physics (Cartwright (1983) : 47)[12]. For example, most laws assume that the law of inertia (*Newton's First Law*) holds, namely, that things tend to remain either at motion or at rest.[13] Such laws describe ideal experimental situations. For example, Boyle's ideal gas law states that:

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[12] Hempel (1988) is often read as claiming that it is "*Cp* all the way down". However, Earman et al (1999), (2002) claim that this is an incorrect reading. I will argue in Chapter 5, that the claim that it is "cp all the way down" has been misunderstood in the literature.

[13] Equally, they would assume for example that the conservation laws (i.e. of mass and energy) are in place. In physics, these laws are ones that have the widest scope and application. For this reason, they are often called the most fundamental laws of nature. An example is Lavoisier's law of the conservation of mass, which states that in a closed system, mass always remains constant, no matter what processes occur internal to the system.

(6) If the temperature of a gas is constant, then the product of pressure and volume must remain constant.

The reason why this law might only be thought to hold *cp* is because it assumes that the gas is tested within normal densities and pressures and that the purity of the gas is maintained. Consequently, there are exceptions to the law such as if a real gas is compressed to an excessively high density, then its behaviour will deviate from the law.

3:2:6 *Ceteris Paribus* Laws and Probabilistic Laws

It is tempting to understand *cp* laws as some species of probabilistic law. Clearly, the exceptional cases to *cp* laws present a problem for construing them as universal statements. For this reason, it might be promising to argue that *cp* laws are some kind of statistical/probabilistic law i.e. that we can say that a given *cp* law will hold "for the most part".

Probabilistic laws have the form $P(F, C) = r$, where the probability P of an F being a C is r.

(7) The probability of a given C_{14} atom (F) of disintegrating (C) is 0.5 (r).

So the inclination is to think that the *ceteris paribus* clause replaces some unknown probability. Consequently, it would be true to claim that *cp* laws do hold for the most part.

However, as Cartwright (1983: 47) I think rightly points outs, it is simply false to say that any putative *cp* law holds 'for the most part' (or given a certain

probability). In fact, sometimes the exceptional cases to a *cp* law are more applicable in most real life cases and the generalisation in the *cp* law is rarely true except in certain ideal circumstances. For this reason, it would in fact be false to say that a *cp* law holds "for the most part". According to Snell's law of optics, the angle of refraction can be explained by the angle of incidence according to the equation $Sin\ \theta/\theta_t = n_2/n_1$. If we were to construe this law as a statistical one, we might claim that this explanation of the angle of refraction is true "for the most part".

However, Snell's law in optics only describe media whose optical properties are isotopic (where there is only one wave of light transmitted). In reality, most media are in fact anisotropic (e.g. non cubic crystals) and therefore, constitute exceptions to Snell's law. So, interpreting the *cp* clause in Snell's law as true "for the most part" would be misleading.

The further problem in conflating probabilistic laws and *cp* ones is that in the case of *cp* laws, it is not just epistemic ignorance that prevents us from assessing the probability of the *cp* law holding or not, rather, we have reason to think that even in the event of our best knowledge that the *cp* law does not in fact have a determinate probability of holding. We just say that a *cp* law holds sometimes (where we mean that it happens enough for us to make inductive inferences about it). The difference is that there is no objective chance of a *cp* law holding.

3:3 The Semantic Problem

The standard objection to the construal of *cp* laws is that providing an account of the semantics of *cp* sentences is problematic because we do not know what the *ceteris paribus* appendage means[14]. It is argued that in order to make the *cp* generalisation into a universally true law, we should be able to state the conditions in which the law is true.

However, there is a background assumption here that laws of nature are to be construed as universally quantified conditionals. This assumption is corroborated by a rather rigid positivist view of science. In so far as science should be interested in truth, then the laws of a science should be strictly speaking true we should be able to provide an account of their truth conditions. Take a law such as:

(8) All metals conduct electricity

We can provide truth functional semantics for universally quantified laws like (8) because they have a determinate meaning; and since they are universally true then they can always be verified, in so for as any x that is a metal will conduct electricity. So (8) becomes:

(8)* "All metals conduct electricity is true iff all metals conduct electricity"

[14] As a caveat to those readers that may be concerned that the semantic worry is not the only challenge that *cp* laws face, let me quell your worries by assuring that the further challenges which face *cp* laws will be dealt with in later chapters (e.g. whether cp laws can be saved from vacuity by a stronger metaphysical account of laws of nature).

The reason why (8)* has empirical content is because it is universally quantified, i.e. universally true for all metals.

In contrast since, there are *ipso facto* exceptions to any given *cp* law, then the law is strictly speaking false (as a universal generalisation). Moreover, we cannot tell the context in which it is true, because it is true in some contexts and false in others. So trying to give an account of the conditions under which it is meaningful (which will provide an account of when it is true) is a more challenging task. Clearly, a false generalisation cannot be part of a mature science.

Consequently, in order to construe putative *cp* generalisations as laws, we must provide an account of their truth conditions. In order to do this, we must get clearer on what it is that *cp* refers to. So the truth conditions for a *cp* law should be provided such that "$\forall x ((Fx \ \& \ N) \rightarrow Gx)$ *cp iff* $((Fx \ \& \ N) \rightarrow Gx$" where "*N*" is the determinate description of what *cp* means, which has independent empirical content and is testable. For example, in order to provide truth conditions for Farr's Law we would have to be able to specify that for all cases of x, if x is an epidemic and *N* conditions obtain, then x will increase exponentially.

3:3:1 The Context-Specificity of *Cp* Laws

The first problem in providing the truth conditions is the fact that what the *cp* appendage means is context specific. In so far as meaning is determined in a highly contextual way, it could be argued that the *cp* clause as such has an

indeterminate meaning. An example makes this clear. Take Von Baer's Law of embryonic similarity in evolutionary biology, which states that:

> (9) "Characters common to a large group of species appear earlier in the development of the embryo than the special features which arrive later in the stages of development *cp*"

There is much evidence to think that this *cp* law is true. The generalisation has sufficient generality to be used in explanation in evolutionary biology. In fact, Stephen Jay Gould claims that the laws of Von Baer 'in refurbished evolutionary dress, are now more widely accepted than ever before' (Gould, *Ontogeny and Phylogeny* : 59.)

Von Baer's law explains a large percentage of the parallels between ontogeny (the history or lineage of a specific organism) and phylogeny (the history or lineage of the race). It shows that all animals of a type should follow similar trajectories, in their early embryonic development. For example, vertebrates, such as fish, lizards and humans, develop in ways that are remarkably similar during early stages, but they become more and more differentiated as the embryos approach maturity. (Ayala (1988) : 987).

For example, cartilage is a common character found in all fish, in cartilaginous fish such as rays and sharks as well as in bony fish like herring, eels, sturgeon and salmon. Cartilage therefore, would be considered to be a general character. In contrast, bone is a special character, being found only in bony fish. Von Baer's law correctly predicts that, in bony fish, cartilage will appear earlier in individual development, and will transform into bone.

So, why add the *cp* proviso? There are exceptions to Von Baer's law that occur for context-specific reasons. For example, neotony is one type of genetic mutation that occurs in certain amphibians, which is the attainment of sexual maturity by an organism still in its larval stage. Neotony is defined as the phenomenon of attaining reproductive maturity while retaining the larval external morphology (Duellman & Trueb, 1986). This means that the adult looks exactly as it normally would in the later stages of development, in its earlier stages of development. In other words, it develops special characters in early stages of development.

Therefore, the law is contravened because we have an exceptional case where development does not go from general features to individual features. Moreover, the reason(s) why neotony (and indeed other genetic mutations like progenesis and so forth) occur in any given situation are highly context specific. For example, it is thought that neotony is induced by environmental factors, such as very low temperatures or lack of iodine. The problem is that whether the law will hold or not depends upon these independent context-specific variables.

Since, the exceptions to Von Baer's law are highly context-specific it can be argued that the *ceteris paribus* appendage has no determinate meaning. There is no definite proposition in the offing that might replace the *cp* clause. Consequently, the law could be reformulated, as "Characters common to a large group of species appear earlier in the development of the embryo than the special features which arrive later in the stages of development *iff* characters common to a large group of species appear earlier in the development of the embryo than the

special features which arrive later in the stages of development ". Put the other way round, we could say that the law hold's unless it doesn't". Put more formally, $\forall x$ $((Fx \rightarrow Gx)$ $cp \Leftrightarrow$ $\forall x$ $((Fx \rightarrow Gx)$ cp is true, but it is a tautology. Clearly, the most we can say in advance of applying the law to specific circumstances is that the law will hold if and only if the law will hold unless it doesn't which is clearly vacuous.

It can be argued then that we do not know what it means to say that Von Baer's law holds *ceteris paribus* in advance of applying that law to specific contexts, at least in so far as there is no prior stable and statable version of a *ceteris paribus* generalisation that gives it empirical content. Any way we formulate it, it seems that the generalisation has no empirical content. Consequently, it is not the kind of candidate that we would usually deem as an empirical law in a mature science.

The charge of vacuity involves some distinct semantic claims about *cp* sentences. These semantic arguments are usually run together in the literature. However, it is worth pulling them apart to see that there are quite distinct arguments being used. These arguments cumulate to make the semantic problems an overwhelming bulwark to progress on the issue of giving *cp* laws any analysis whatsoever. There are three arguments that motivate the semantic problem for *cp* laws.

(1) Cp Laws are indeterminate propositions

(2) Cp laws are tautologies

(3) Cp laws are vacuous

3:3:2 *Cp* Laws are Indeterminate Propositions:

Firstly, we might say that *cp* laws express indeterminate propositions. This is to argue that we cannot provide a non-vacuous account of the truth conditions of *cp* laws. The idea is that we cannot complete the *cp* law, whilst retaining empirical content. The principal problem is that clauses annexed with *ceteris paribus* provisos look like they are expressing determinate propositions, when, in fact, they are vague and indeterminate propositions. According to Schiffer (1991), in intentional psychology a *cp* law is tantamount to saying "If a person wants something and [....], then she will take steps to get it". Since, this proposition is inherently incomplete it is strictly speaking meaningless, and would not be believed, let alone considered a law.

3:3:3 *Cp* Laws are Tautologies

The second argument is that *cp* laws express determinate propositions but that they are tautologous truisms, with no empirical content. In other words, there is no problem in providing the truth conditions, but the account we provide makes the *cp* sentence tautologous. *Cp* laws are true in the case where there are no exceptions to the law, that is to say a given law. For example, consider Bergmann's law in biology which state:

> (10)*Ceteris Paribus*, for species of warm-blooded vertebrates, races living in cooler climates are larger than races living in warmer climates.

This amounts to (11):

(11) [$\forall x(Fx \rightarrow Gx)$ *cp*]

which is true in all cases where there are no circumstances like (12) :

> (12) A warm-blooded vertebrate in a cooler climate that is not larger than races living in a warmer climate. [$\exists x(Fx \ \& \ \neg \ Gx)$]

However, this is the same as saying that a given *cp* law is true *iff* a given *cp* law is true (13):

> (13) For species of warm-blooded vertebrates, races living in cooler climates are larger than races living in warmer climates *iff* races living in cooler climates are larger than races living in warmer climates. [$\forall x(Fx \rightarrow Gx) \Leftrightarrow \forall x(Fx \rightarrow Gx)$]

Therefore, formulating the truth conditions in this way renders the *cp* law tautologous or at least something very like that[15]. Tautologies have no empirical content and so we would not want the laws of an empirical science to be formulated in this way. This is clearly tautologous.

3:3:4 *Cp* laws are Vacuous

The third argument is that *cp* laws express determinate truth conditions, but that they are vacuous. The idea here is similar to the last argument and in fact is logically equivalent. What we mean when we say that a law holds *cp* is that it holds true unless there is an exception. However, since what is constitutive of an

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[15] It has been pointed out to me that *cp* laws so formulated may not be strictly tautologous in the following sense. Formally we can take it that a tautology is of the form (1) $A \supset A$. However, what a *cp* law says is (2) $A \supset B$ where there are no exceptions. So strictly (10) above is a case of (2) rather than (1). My attempt to reduce (2) back to (1) amounts to saying that if (2) *Ceteris paribus,* for species of warm-blooded vertebrates, races living in cooler climates are larger than races living in warmer climates = (2)* *Ceteris paribus,* for species of warm-blooded vertebrates, races living in cooler climates are larger than races living in warmer climates, if circumstances are exceptionless. So essentially I am equating (2) & (2*). However, saying that circumstances are exceptionless means nothing if we do not know what it is to say that situations are exceptionless. So, if there is no non-vacuous way of saying that situations are exceptionless, then we are just saying that (2) is true if and only if (2) is true $(A \supset B) \leftrightarrow (A \supset B)$, which is tautologous.

exception is highly context-specific, the most we can say in advance of applying the law to specific circumstances is that the law will hold unless it doesn't. So (10) becomes:

> (14) For species of warm-blooded vertebrates, races living in cooler climates are larger than races living in warmer climates, unless they are not.

This claim is clearly vacuous and again we would not want the laws of an empirical science to be formulated in this way.

Therefore, the semantic problem is a genuine concern for providing any analysis of *cp* laws. There are genuine difficulties for providing the truth conditions both in advance of applying it to specific contexts (in principle) and applying it to specific practice (in practice). I will now examine in greater detail some attempts in the literature to provide the truth conditions both in practice and in principle. I will argue that neither approaches save *cp* laws from vacuity.

3:4 Providing the Truth Conditions:

In order to cash out the c*eteris paribus* clause we would have to be able to list every possible variable that could contravene the law so as to provide the truth conditions for it. The idea is that we should be able to say what it is that the *ceteris paribus* proviso is replacing. Formally the law would be 'All x's are y's, unless there is a *z*, or some circumstances such that there occurs an *x* that is not a *y*. We would now know what the *ceteris paribus* clause means because we can see what it replaces, namely the disjunctive set of qualifier clauses (that encapsulate a *z* circumstance) that would explain how an exception to the law

might occur. In the literature, these have been called 'closure clauses'. The thought is that if we can provide a list of all the exceptions that may occur, in terms of these closure clauses, then we can provide a true confirmable law. So the law, becomes 'All *x*'s are *y*'s, (unless every theoretically and physically possible exception)".

According to Earman et al. (2002 : 284-285) there are two distinct reasons why we might think that the conditional of any given *cp* law cannot be made explicit. The first is that we do not now know the conditions under which the law would hold, and the second is that we would not even in the event of our best knowledge of science know the conditions in which a law might hold because they will comprise an indefinitely large and perhaps indeterminate set. Earman et al. argue that the first issue is not relevant since it is may be only the result of our ignorance, and there is nothing preventing future empirical research from in principle eliminating the *cp* proviso. Moreover, Earman et al. continue to argue that there is no good argument for why *cp* laws are 'in principle' incompletable either. Consequently, they think that '*cp*' laws are merely placeholders for the real laws of physics[16].

Nevertheless, much of the literature is motivated by the belief that there are laws that are "in principle" incompletable (Giere (1999) & Lange (1993)). According to Lange, "the number of provisos is 'indefinitely large' which makes it impossible to offer them as premises' (Lange (1993a) : 240). He argues that

[16] Earman et. al argue *pace* Cartwright (1989) that it is not "*cp* all the way down". Even if there appear to be *cp* clauses in the present laws of physics they are in principle eliminable since they will eventually be stated more explicitly in terms of closed strict laws provided by the future laws of physics.

there is an indefinite number of interfering factors, which we cannot anticipate in advance of scientific practice. His example is the law of thermal expansion, which states that the change in length of an expanding metal bar is directly proportional to the change in temperature. However, there are possible interferences that we could not account for in advance of applying the law in a given context. So, he states that in order to "complete" the law:

> One would have need to specify not only that no one is hammering the bar on one end, but also that the bar is not encased on four of its six sides in a rigid material that will not yield as the bar is heated, and so on. (Lange (1993a : 243).

For this reason, Lange argues that we cannot ever provide the truth conditions for *cp* laws in advance of applying them in scientific practice. Moreover, according to Lange, there is sufficient reason to believe that laws are " in principle" incompletable.

However, Earman et. al (2002 : 284) argue that the list is only indefinite if expressed in a terminology that avoids the language of physics. They state:

> If one helps oneself to technical terms from physics, the condition is easily stated: The "law" of thermal expansion is rigorously true if there are no external boundary stresses on the bar throughout the process. (Earman et. al (2002) : 284)

In the end, they argue that all real laws will be ones where the *cp* proviso will be "in principle" eliminable, once our knowledge is complete. So, apparent *cp* clauses in the putative laws of our present theories are merely a sign of our current scientific ignorance.

There is a conflict then between what we can know in practice as opposed to what we could know in principle. The idea is that we should not argue from the limits of our present knowledge to the impossibility of surpassing those limits. It is clear that whether an account can be provided by future empirical research remains an open question. This is why the second issue is indeed more pressing. If there is a reason to believe that even *in principle* we could not complete the *cp* clause, then the semantic problem is a real problem.

3:4:1 Hedged *Cp* Laws: Providing the Truth Conditions in principle

Let's take a closer look at the idea that it should be at least, in principle, possible to provide the truth conditions for *cp* laws. Fodor (1991) sees no problem in principle in providing the closure clauses and hence, truth conditions for *cp* laws. He insists that any *cp* special science law is, in principle, completable in terms of one or more of the more basic sciences (physics)[17]. Fodor is speaking more about intentional psychology and its relation to neuroscience, as opposed to the special sciences in general. But, presumably, he would take his account to generalise; since he previously argued in (1974) that all of the special sciences have genuine *cp* empirical laws.

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[17] The fact that Fodor thinks of *cp* laws as hedged laws was already hinted at in Fodor (1987) : 6. He states:

> The same pattern that holds for the special sciences seems to holds for intentional psychology as well. On the one hand, its ceteris paribus clauses are ineliminable from the point of view of its proprietary conceptual resources. But, on the other hand, we have –so far at least- no reason to doubt that they be discharged in the vocabulary of some lower-level science.

What is his account? Take the intentional psychological generalisation "if you are thirsty, then you will drink *cp*"; put formally *(∀x (Ax→Bx) cp)*. According to Fodor, this is a genuine *cp* law. Schiffer (1991), as we have seen above would argue that this proposition looks as though it is determinate when in fact it is not. Since, we have no way of enumerating every theoretically possible exception to the law, it is incompletable, has no empirical content and is vacuous.

Pace Schiffer (1991), Fodor argues that the necessary and sufficient conditions for the validity of a *cp* intentional law can be outlined such that a *cp* law is *in principle* completable, where *A* is a propositional attitude and *B* an action, then the proposition $A \rightarrow B$ is a *ceteris paribus* intentional law, in so far as it is true that *A* causes *B behaviour* 'all other things being equal'.

Given the law *∀x (Ax →Bx) cp, A* is multiply realisable in a set of properties of the fundamental science; let's call each of these a realiser of the functional state *A*. $A(R_1)$ is an event where A is realised by R_1 (e.g where the intentional state of "being thirsty" is implemented in the brain by some physiological state). *K* (the completer) is some further set of physical circumstances that in conjunction with the realiser *R* are causally sufficient for the instantiation of $A \rightarrow B$. The *ceteris paribus* clause is thus completable, once every realiser of *A* has a completer *K*. In other words, *K* will be the set of physically favourable circumstances that make "all things remain equal".

In this respect, when we use the *ceteris paribus* law $A \rightarrow B$, it is more informative to us than focusing in on the complex neurophysiological cause of the action, which will be a compound of the brain state that realises the intentional state and also the completer which would theoretically explain why all other things are equal (or not). Accordingly, there is no problem in providing the truth conditions, namely, $(A \rightarrow B)$ *cp* is *true* provided every realiser of A has a completer K.

It is important that both the realiser and the completer are causally necessary. This precludes the reduction of the intentional state to either the completer or the realiser alone. The realiser of A does not alone guarantee B, because the completer may not occur with the realiser, so B may not occur, leaving open the possibility of $A \rightarrow \neg B$, (i.e. exceptions to the *cp* law can occur.) Additionally, the completer alone does not guarantee B, because this would allow too many things to count as *cp* laws. In other words, this will mean that we can guarantee just about any generalisation.

For example, "if I am thirsty, then pigs will fly *cp*" could then be corroborated in a rather *ad hoc* way. Let K (the completer = genetically engineered flying pigs have been created). Obviously, in this regard, however the fact that you are thirsty is realised, pigs will fly. So, the completer alone is not causally sufficient. Therefore, any *cp* law is true once its realiser is accompanied by its completer. Both the realiser and the completer are causally necessary[18].

[18] For another critique of this account see Mott (1992).

Let me illustrate this with an example. Take the *cp* generalisation, "if I am hungry, then I will take steps to eat *cp*." There will be some realiser (brain state) R_1 in the brain, which implements my feeling of hunger on a given occasion. Furthermore, there will be some further set of neurophysiological facts, which in conjunction with R_1 will bring it about that I take steps to get food. Whenever, I am hungry and fail to take steps to get food then the realiser is not accompanied by it completer (i.e. in this instance there are no physically favourable conditions that will implement the law in conjunction with the realiser.)

Schiffer (1991) argues that there are possible realisers that never have a completer for the law *(A→B) cp*. For example, take the hunger striker that even though he is hungry will nevertheless, always refuse to eat[19]. This is a counterexample to the Fodorian account because there is no completer (i.e. there is no further set of neurophysiological facts) that in conjunction with the realiser of the intentional state could even in principle explain why the hunger striker fails to eat. According to Schiffer, this will be an "absolute exception" to the *cp* law.

However, Fodor replies that any intentional state '*A*' will figure in more than one law, that is to say that there will never just be one generalisation about the antecedent of any *cp* law. In other words, there are no isolated *cp* laws involving any given intentional state, since if a given intentional state figures in any law it will figure in many laws. There will always be other laws about a given intentional state that are in principle completable.

[19] The example is Mott's (1992) : 337.

Fodor argues that if we take the network (set) of all laws about a given intentional state A, that many or most of the laws in that network will have realisers that are accompanied by completers. So some *cp* laws can fail on every occasion, once the bulk of the laws hold true. If R_1 is never completed for any of the laws that have A as their antecedent in the network, then R_1 would simply not be a realiser of A. For, example, if the example of the hunger striker can never be explained by a neurophysiological completer, there will nevertheless be many other laws about "being hungry" that will be completable.

Consequently, since neither the realiser, nor the completer are alone sufficient for the action, the intentional law postulating the regularity between the intentional state and the action is more informative than focusing in on either the completer or the realiser alone at the neurophysiological or biochemical level. The intentional law picks out the higher-order functional natural kind that could not be picked out at the neurophysiological level. The generalisation then is necessary to determine what realisers count as realisers of A. Without the intentional law this would be impossible. As Fodor puts it:

> The sum and substance is the truism that we need psychological vocabulary to say what the realizers of a psychological state have in common. (Fodor (1991) : 30)).

This allows Fodor to argue for a real special science of intentional psychology that has real empirical laws, which govern over distinct natural kinds. The reason why *cp* intentional laws are not vacuous, according to Fodor is because they are in principle completable in terms of the complex neurophysiological cause of the action. Fodor is content that intentional laws are heteronomic, so that in order to

provide the truth conditions for *cp* laws it is necessary to go down a level. However, Fodor is assuming here that there are strict laws in neurophysiology, which ground the *cp* laws of intentional psychology.

The merits of Fodor's account is that he tries to get around an impasse in terms of trying to provide truth conditions for *cp* laws, in the language of the special science that they are formulated alone. The problem in trying to do this is that the propositions that you end up with are not clearly defensible in their own right. So we can deny them or affirm them, but we really cannot justify them either. Louise Antony (1995) has elegantly unveiled this impasse in terms of the debate about special science laws particularly as it pertains to intentional psychology.

> No one wishes to be caught having to deny the obvious facts, and in that case, one must either impugn the inference from truisms to laws ("why *should* there be laws in psychology?") or refute the arguments against them ("well why *shouldn't* there be laws in psychology?") (Antony (1995) : 429)

Fodor's account tries to get around this impasse by suggesting that any account of the truth conditions of *ceteris paribus* psychological laws has to take neurophysiological strict laws into account. Fodor knows that the truth conditions of *cp* laws in the special sciences cannot be provided in terms of the language of the macro-level special science, without resulting in vacuity. However, they can be provided in terms of the micro-level mechanism (the realiser and the completer) that implements the functional regularity that is exhibited by the *cp* law at the macro-level.

However, the burden of proof lies with Fodor to (a) provide such an account of non-strict laws and (b) to explain why *ceteris paribus* intentional laws inevitably remain distinct from underlying neurophysiological ones. I think that Fodor does not give an adequate argument for (b). Let me make this clear. Since, in order to make intentional *cp* laws non-vacuous, Fodor is reliant on neurophysiology, it seems then that the interesting consequence is not only whether or not the *ceteris paribus* laws in intentional psychology can themselves be completed, but also how these clauses relate to laws in neurophysiology that ground them.

The problem is that the Fodorian account takes it for granted that there are strict laws in physics upon which his account can rely, or it at least takes for granted that such physical exceptionless laws are forthcoming. However, it at least remains an open question whether such laws can indeed be depended upon. So my problem then is if it turns out that there are *cp* laws in neurophysiology and indeed in physics, then how can the *cp* laws of intentional psychology (and indeed the other special sciences) ever be grounded?

As we saw earlier, it is still considered an open question in philosophy of science, for example, whether we are warranted in holding that the basic laws of physics are exceptionless. As we saw earlier, laws such as Boyle's law, Ohm's laws and Snell's law can be analysed in such a way that they too need to be annexed with *ceteris paribus* clauses[20].

[20] In fact, I will argue in Chapter 5, that even Newton's Second Law of Motion can be construed as a *ceteris paribus* law.

Moreover, it is plausible that in Fodor's specific case (i.e. neurophysiology) that the putative neurophysiological laws would also have exceptions, because they govern complex processes in the brain rather than singular causal events. Consider the following:

> (15) "When an action potential is sent down an axon, then depolarisation occurs at the terminal button."

It would be more correct to add, "all other things being equal" to this law, because there are exceptions that might occur. For example:

> (16) "When an action potential is sent down an axon, then depolarisation occurs at the terminal button, unless there is a deficiency of neurotransmitter."

The issue here is that Fodor's account requires that there will turn out to be strict laws in neurophysiology, and that it is by reference to these that the *ceteris paribus* clauses in intentional laws can be completed. The problem is that the laws of neurophysiology and indeed those of physics can also be construed as *ceteris paribus* laws. Fodor (1991) does consider this possibility in the following passage:

> The present worry is that if Hempel is right about there not being any strict laws, and Schiffer is right about there not being any hedged laws, then between them, they've licked the platter clean and there aren't any laws at all. (Fodor (1991) : 21-22)

If some or all of the laws of physics are *ceteris paribus* laws then this would mean that absolute exceptions cannot be accounted for by virtue of belonging to a network that is composed of strict laws that do not have exceptions and *cp* laws that are accompanied by both realisers and completers. This is so because the network will be composed of strict and non-strict laws. Fodor

thought that he could make intentional laws non-vacuous by suggesting that their exceptions could be catered for by virtue of belonging to a network of neurophysiological strict laws. However, if these laws might themselves be *ceteris paribus*, then the vacuity objection returns, because we cannot complete intentional laws by making them part of a network of lower-level neurophysiological ones.

Moreover, Fodor's account is problematic when viewed from the other direction. Take it that what Fodor says is correct and that it turns out that the *ceteris paribus* clause is in principle eliminable, because there is so to speak a bedrock of strict laws which ground *cp* laws. This leads to rather a different analysis of *cp* laws, since they are only legitimate in so far as there are further strict or statistical laws, which ground them. The problem with this construal is that strictly speaking *cp* laws only become placeholders for the "real" strict laws that will be provided by physics.

It can then be argued that c*p* laws are no more than heuristic devices that allow us to make rather reliable explanations and predictions, but only in so far as we know that strict laws ground them. They make interesting claims about the macro-world and are useful in scientific practice, but only in so far as it is sometimes helpful to ignore or screen off interferences. Additionally, these generalisations are interesting from a methodological point of view in so far as they initiate enquiry into the underlying causal mechanism that implements the regularity at the microphysical level. *Cp* laws become interesting generalisations that supervene upon the fundamental laws of the hard physical sciences.

However, there is nothing in the analysis provided to claim that *cp* laws will remain autonomous in the event of our best knowledge of science. So, when all is said and done and science reaches its terminus, then *cp* generalisations could be merely useful, rather than lawful.

Fodor is faced with the following dilemma; on the one hand should it turn out that there are strict laws, which ground *cp* laws, and his account is correct, then *cp* laws will become no more than placeholders for the fundamental laws of physics, and on the other hand, should it turn out that there are no strict laws which ground *cp* laws, then the problems recurs in physics and we have no laws of nature at all. Therefore, whichever way you look at it, providing the truth conditions in principle, by hedging *cp* clauses is a futile task.

3:4:2 *Cp* Laws in Scientific Practice

An alternative possibility is that even if there is no context-independent way of providing the truth conditions for *cp* laws that we can, nevertheless, illustrate in scientific experimentation and practice that a given *cp* law is empirically non-vacuous. The idea is that we cannot, outside of the context of scientific practice provide a set of jointly necessary and sufficient conditions for *cp* laws *tout court*. The claim is that scientific enquiry is set by abstracting away from the details that matter. Therefore, when a *cp* law is discovered, exceptions are idealised away so that theoretically important phenomena can be discovered. However, in scientific practice we can justify the prior law in an empirically informative way.

Pietroski and Rey (1995) argue that there is an unproblematic way of providing the truth conditions for *cp* laws in scientific practice. The reason why *cp* laws are genuine laws is because we can explain why an exception is abnormal in scientific practice by citing the independent factors that occurred to contravene the law. This ability in scientific practice is a sufficient condition for a given *cp* law being non-vacuous. The idea is that when we are face to face with a counterexample to any given *cp* law, we immediately know that it is one. In fact, it is this that initiates enquiry into what went wrong. Once, the factors can be enumerated that constituted the exception, then we can see why all else was not equal[21].

However, there is a problem with this construal too in that it takes too many generalisations in. Consider a vacuous truism from astrology like:

(17) "All Sagittarians (ruled by Jupiter) are extroverted *cp*".

An exception to the rule is an introverted self-conscious Sagittarian. The astrologer can always find "some other" set of "independent" factors that explain the exception. This is exactly why we don't want to consider astrology a science and why we certainly don't think any of its generalisations are candidates for laws. In other words, these generalisations are unfalsifiable. Pietroski & Rey would have to say that the fact that the exceptions can be explained away in

[21] It should be noted that this is the most promising approach. If we apply a bootstrapping model of confirmation {e.g. Glymour (1980)} there is no reason why nomological claims or theories cannot be corroborated by making an assumption that we can rely on further nomological claims of theories to bootstrap them. Woodward (1991) discusses this as an objection to the cruder empiricist account of confirmation, which requires direct verifiability. Nevertheless, the problem with this approach in giving an account of *cp* laws is that firstly it is an open question whether it is *cp* "all the way down" as Cartwright (1989) has argued. Presumably, what would be required is that *cp* laws are bootstrapped by strict laws, but this is also problematic in that it might entail reductionism.

practice is a sufficient condition to make the putative astrological *cp* law non-vacuous.

However, Pietroski & Rey, add a further condition to eliminate the possibility of construing the latter truisms as genuine *cp* laws. This further condition that explains why the *cp* law fails to hold in a given circumstance must do other independent explanatory work in science apart from explaining why all things were not equal. Putting their account more formally will be helpful. Take the law $\forall x\ (Ax \rightarrow Bx)\ cp$. When A fails to bring it about that B, there must be a further condition C, which explains $\neg B$. Moreover, C must do other explanatory work in science, other than explaining $\neg B$. This condition is meant to eliminate the kind of *ad hocery* that makes the generalisations of astrology (17 above) pseudo-scientific and shows that there is a non *ad hoc* way that *cp* laws can be empirically informative.

> Let's take an example. Boyle's Law states:
>
> (18) Under conditions of constant temperature and quantity, there is an inverse relationship between the volume and pressure for an ideal gas *cp* ($PV = nRT$).

Suppose that we place an amount of gas in a container of volume V at a temperature T. The gas will then exert a pressure P. Experiments show that – *ceteris paribus*—the pressure p, the volume V and the temperature T of the n moles of gas are related by the ideal gas law $PV = nRT$ (if the temperature of a gas is constant, then the product of pressure and volume must remain constant). However, in an experiment suppose that we have a case where $PV \neq nRT$. For

example, at 20-atm pressure the volume is about 2.3% smaller than predicted, by the law. The kinetic-molecular theory of gases, which is an independent explanatory theory, helps to explain the deviation from the *ideal*, namely in so far as it explains the force exerted by the molecules on each other, in situations of high pressure.

According to the Kinetic Theory of gases, pressure is the force exerted against the wall of a container by the continual collision of molecules against it. The ideal gas law can be viewed as arising from the kinetic pressure of gas molecules colliding with the walls of a container in accordance with Newton's laws. Newton's second law of motion tells us that the force exerted on a wall by a single gas molecule, is equal to the mass of the molecule multiplied by the velocity. The pressure-volume product of the ideal gas equation is directly proportional to the total kinetic energy of a gas, since the total kinetic energy is the temperature. Therefore, the reason for the deviation is explainable in terms of a scientifically legitimate independent theory, namely Newtonian mechanics for molecules.

I wish to argue however, that although this provides a methodology for coping with the *ceteris paribus* clauses in *cp* laws that nevertheless, it simply defers the problem at hand, because the independent 'scientifically legitimate' theory has to be itself given a justification. Whether we can indeed rely on Newtonian mechanics requires independent validation. Newton's second law states that:

> (19) The acceleration of an object is dependent upon two variables the net force acting upon the object and the mass of the object.

The equation for Newton's second law is $F = Ma$, the net force *(F)* is equated to the product of the mass times the acceleration *(Ma)*.

The problem is that we could rightly construe Newton's second law as a *ceteris paribus* law. To explain the behaviour of the molecules of the gas in terms of Newton's second law, we must introduce extra assumptions such as that the gas is in *equilibrium*, or that molecules are *distributed uniformly* within the container, or that each molecule has an *equal probability* for going in any direction. Only with such assumptions can we derive the statistical distribution of velocities within the gas. (Uchii (1999)).

So, it would be more correct to say that the *ceteris paribus* clause of the ideal gas law is completable in practice if it can be explained by Newton's second law-*ceteris paribus*. Therefore, the explanation of *ceteris paribus* is deferred. So, the question would again arise as to whether we could explain the instances when Newton's law would fail to explain the ideal gas law (when *cetera* were not *pariba*), in terms of some independent theory.

The problem is that explanation must stop somewhere; so Pietroski & Rey face the same dilemma as Fodor. There will either be on the one hand strict laws that render some theories scientifically legitimate which will provide us with a means of explaining away exceptions to *ceteris paribus* generalisations. The issue then is whether the *cp* laws are merely placeholders for the real laws of these

"scientifically legitimate" theories. On the other hand, if the problem of coping with *ceteris paribus* clauses is merely deferred, but there is no scientifically legitimate theory without *cp* laws, then the problems recurs in physics and we have no laws of nature at all.

3:5 *Cp* Laws "all the way down"

So, both Fodor (1991) and Pietroski and Rey (1995) have attempted to provide truth conditions for *ceteris paribus* laws by presuming that *cp* laws can be completed because we can rely on the strict laws of "scientifically legitimate" theories in physics. They both assume that there is an important distinction between the laws of the special sciences and the laws of physics. In other words, there is an assumption that it is not *ceteris paribus* "all the way down". They move from the premise that it is not *cp* all the way down to the conclusion that since physics either provides (or at will provide at some point in future physics) a bedrock of strict laws, that the *cp* clauses in our present special science laws are in principle completable.

It is interesting to note that others coming from the same premise (i.e. that it is not *cp* all the way down) come to the opposite conclusion; namely that there are no genuine self-standing *cp* empirical laws. Earman et al. (2002) argue that there is actually a paucity of examples from physics to support the view that it is indeed *cp* all the way down.

Nevertheless, what the semantic objection achieves is it forces *cp* laws into closure. Any law with an ineliminable *cp* clause will be vacuous, whereas any *cp* law that is semantically closed, in so far as it has empirical content will essentially be closed only in its relation to further strict or probabilistic laws. The problem then is that the *cp* law is only a placeholder for the strict law, the function of which is to signal the presence of the strict laws that exhaustively explain the micro-level mechanism that implements the regularity. So you either have tautologous truisms that are empirically vacuous, or generalisations that signal the presence of further laws. So, either way it seems that there are no self-standing *cp* laws. So why not eliminate them altogether? This is exactly the move that Earman et. al (2002) make.

For this reason, I contend that that any attempt to provide truth conditions for *cp* laws in a strict truth-conditional way is doomed to failure, since it makes *cp* laws placeholders for the real laws of physics. Moreover, in so far as *cp* laws are meant to be introduced to corroborate the special sciences, conceiving of them as truth-conditional only by reference to the harder more fundamental sciences leads to a crude implied reductionism, that was characteristic of the very enterprise that the disunity of science was meant to combat; namely fundamentalism about physics. Therefore, *cp* laws face a dilemma, either it is not *cp* laws all the way down and *cp* laws are placeholders for the real laws of physics, or it is *cp* laws all the way down and the semantic problem cannot be solved by reference to any more fundamental laws.

3:6 Truth-Conditional Accounts & Scientific Fundamentalism:

It is now clearer that attempts to provide the truth conditions, both in practice and in principle, depend upon the kind of reductionism that is characteristic of scientific fundamentalism. *Cp* laws are being conceived in a rather different way than we had originally imagined; namely as macro-level generalisations that are grounded in micro-level laws. Indeed, the ideal of unificationism so pervades scientific ideology that ironically even those philosophers who argue against it (namely in favour of disunificationism), end up relying on physics to make their case against physics fundamentalism. Fundamentalism polices the language of science in such a way that we cannot but look at science in the strictest way. So, in order to save *cp* laws from vacuity, we are forced into the same rigid classifications and criteria that are characteristic of physics, namely the idea that a law must be a universally true conditional.

3:7 Epistemic Semantics for *cp* Laws:

However, there is an alternative possibility. This is to argue that a strict truth-conditional account of the semantics of *cp* sentences is not required. In other words, perhaps the requirement of a strict truth-conditional semantics is too stringent a policing of the language of science. The idea is that we should open up the conceptual ground somewhat towards providing a less stringent account of the semantics of *cp* clauses. So, forget about the strict requirement for truth conditions and instead look towards an epistemic account of the semantics of *cp* sentences in scientific practice. This is to argue that we do not have to enumerate

all of the circumstances that would make the sentence true; namely that there is somehow tacit agreement on what "all else being equal" means in scientific practice. (Spohn (2002), Lange (2002), Glymour (2002)).

Thus far, we have concentrated on those views that try to make *cp* laws fit into the rigid schema set out by theories about what laws of nature *should* be. In other words, the concentration has been on which criteria a *cp* law should fulfil to be considered law-like in principle, namely there must be a prior stable and statable description of the *cp* law, which squeezes it into a universal generalisation.

A rather different approach is not so much to obsess about the criteria for lawhood that should be satisfied in order to deem *cp* laws legitimate, but rather to look at how these generalisations function in scientific practice, and thereby examine the epistemic attitude of the scientist to *cp* claims in practice. Clearly, the semantic problem might be considered a rather innocuous one if we are not so obsessed by the stringent criteria that we have set ourselves for lawhood.

In this vein, an alternative approach is to consider the role of our epistemic attitudes to *cp* generalisations in order to illustrate why we consider *cp* sentences meaningful. To bring this approach into clearer view, the idea is that we should not approach the problem from the point of view of which characteristics a claim must have to be considered law like rather than accidental, but rather concentrate on why we make the inductive inference that we do from an epistemic point of view, when we perceive the regularity that is exhibited in a *cp* generalisation.

We should rather look at the meaning of *cp* generalisations and consider what it is about our own epistemic attitudes to them that makes us intuitively distinguish them from accidental generalisations. Lange (1993a) has argued that *cp* claims are perfectly meaningful in so far there is tacit agreement in terms of what constitutes a counterexample to any given *cp* law in scientific practice. Lange's argues that the *ceteris paribus* proviso is a replacement for a store of background knowledge that the scientist draws upon and tacitly adheres to in scientific practice. It cannot be explicitly formulated in the strict way that is required to provide truth conditions.

However, we can imagine what would constitute a counterexample to a *cp* law and once a given 'variable' is considered there will be unanimous concurrence that it is exceptional. Lange is committed here to a Wittgensteinian conception of meaning as use. So, we cannot cash out precisely the rules that the scientist follows, but we can watch the scientist in practice and it becomes clear that he/she is tacitly following rules that all other scientists concur upon.

Of course, in the strictest sense, this does not provide any truth conditions for a given *cp* law, because, unanimity is neither necessary nor sufficient for the truth of the *cp* clause. There is no fully explicit list of closure clauses that would render the *cp* clause eliminable. Rather, the *cp* clause is a placeholder for the implicit background understanding (both common-sense and scientific) that would constitute compelling reasons for taking the law at face value and classifying disturbing factors as exceptional.

The problem with this approach is that it makes the distinction between laws and accidents very subjective and anthropocentric. This looser analysis certainly could be made consistent with the view that *cp* laws are really only heuristic devices, that although might very well be explanatorily relevant, provide no metaphysically substantial information about the world[22]. Therefore, ironically, Lange's account could be made consistent with Earman et. al's view that *cp* laws are merely placeholders for the real laws of physics. It is therefore, pertinent at this point to ask why we should construe these 'rules' as laws at all. Couldn't we just construe *cp* generalisations as heuristic rules of thumb?

Again, the question of the falsifiability of these claims will be even more relevant were *cp* laws to be epistemically justified. If *cp* clauses are considered true only because of some tacit background epistemic commitments, then when the law is tested the scientist can always justify the law by appealing to his own epistemic inkling. A counterexample can always be explained away as an exception.

Another immediate difficulty here is that we can only provide an epistemic semantics for instantiated laws on this account. Consider Boyle's gas law again which is true of an ideal gas, even if there never were an actual instantiation of an ideal gas. This kind of law would be strictly speaking vacuous, until it is

[22] Traditionally, in so far as we think a generalisation should be considered a law of nature, then we think that there is some objective relationship that is being explained (be it a regularity, a relation of nomic necessitation between universals or whatever). However, it seems that on this account the distinction between a generalisation that is lawful and one that is accidental is made purely in an epistemic manner on the part of the scientist. I will return to this debate in the next chapter.

instantiated in the actual world (or at least until the scientist tests it in scientific practice).

3:8 The Principal Dilemma:

Therefore, we can see that the requirement for strict truth-conditional accounts is motivated by a further methodological problem about how we are to test *cp* laws. I wish to argue that the vacuity objection is motivated in another direction. There is a methodological problem in so far as it is not clear how these *cp* clauses can be tested. Since, very often, we do not know what it means for all else to be equal in advance of testing any given *cp* law, then how can it ever be confirmed? Clearly, there is no finite set of empirical observations that can verify such claims.

Even if such claims were testable, how could we know that the tests are legitimate, since scientists can count just about anything as an exception so as to retain the legitimacy of the law? Scientists could build in just about anything as an exception to their experimental results, so as to substantiate the law. Scientists can rig their results to fit the law. In other words, this *post-hoc* theorising leads to the irrefutability of the very scientific theory that the *cp* laws are introduced to corroborate. Clearly, if the sciences that the *cp* laws are introduced to corroborate are not testable, then they are essentially unfalsifiable.

Therefore, *cp* laws are faced with a pejorative dilemma which combines semantic and epistemic problems for them: either (a) *cp* laws can be completed prior to testing, but only in a vacuous way and so the clauses that express them are not truth-conditional, (the *semantic* problem) or (b) *cp* laws can be completed within testing, but consequently, the authenticity of the scientific investigation cannot be preserved (the *epistemic* problem). Taking either horn of the dilemma construing *cp* generalisations as laws does not make them scientifically legitimate.

3:9 Conclusion:

In conclusion then, we cannot provide an account of the truth conditions of *cp* laws, either in practice or in principle, while respecting the kinds of generalisations that the *cp* proviso was introduced to accommodate. Moreover, any attempt at providing an epistemic semantics for *cp* laws results in laws that are untestable. Therefore, introducing *cp* laws into any science would render it unfalsifiable. Indeed, the semantic problem for *cp* laws is a fatal one. So should we conclude that there are no *cp* laws? In so far as we should be able to provide an independent semantic analysis of *cp* laws, I have revealed that the semantic problem is insurmountable. In this Chapter, I have argued that in so far as the semantic problem is addressed to *ceteris paribus* laws *tout court* that it is fatal.

However, one suggestion is that the problem of vacuity may be addressed by some more metaphysically substantial account of what makes a *cp* law true, namely in terms of whatever objective relations the *cp* law quantifies over. Once

we can in turn provide an account of these objective relations (be it in terms of universals, dispositional properties, capacities, regularities or whatever), which does not already presuppose laws of nature, then we will have saved *cp* laws from vacuity by providing their truthmakers. However, if we introduce a metaphysical account to save *cp* laws from vacuity, then we must not presuppose that which we want to explain; namely *cp* laws. In the next chapter, I will survey whether this task can be done.

CHAPTER 4: *CETERIS PARIBUS* LAWS AND TRUTHMAKERS

4:1 Introduction:

Most of the laws of the so-called special sciences (such as biology, psychology, geology and the medical sciences) only hold *cp*. In fact, it is plausible to suggest that even some (or most) of the laws of physics are *cp* laws[1]. Nevertheless, we saw in the last chapter that *cp* laws face a serious semantic challenge; namely that we cannot give an account of their truth conditions {Schiffer (1991), Earman & Roberts (2001)}.

The semantic problem that was addressed in the last chapter is that "all else being equal" appears to be a catchall phrase for all the possible exceptional cases that might contravene the law, so, 'All *F*'s are *G*'s *cp*' could be read as 'All *F*'s are *G*'s, unless they are not', or 'All *F*'s are *G*'s *iff* All F's are G's' or indeed as 'All *F*'s & {.....} are *G*'s' and thus the statements that express *cp* laws are vacuous. This semantic challenge means that we simply do not know what we mean when we say that a law holds *cp* because the *ceteris paribus* appendage has no determinate content.

[1] A commonly cited example is Boyle's Law which states that 'Under conditions of constant temperature and quantity, there is an inverse relationship between the volume and pressure for an ideal gas *cp (PV = nRT)*. The reason why this law is *cp* is because in conditions of high densities and pressures the law breaks down.

How seriously ought we to take this semantic problem? In the last chapter, I argued that the semantic challenge to *cp* laws as it stands is fatal. The principal semantic dilemma that faced *cp* laws was that *either* the *cp* clauses in *cp* laws can be in principle completed prior to the application of the law in scientific practice but only in a vacuous way, or in such a way that they become placeholders for the laws of physics *or* the *cp* clauses in *cp* laws can be completed within testing, but there is no way of testing whether exceptional cases are explained away in an *ad hoc* manner.

However, there is one possible response which might claim that the semantic problem is only problematic for an account of *ceteris paribus* law statements rather than *cp* laws of nature themselves. This claim involves a subtle distinction between an *epistemic* account of law statements and a *metaphysical* account of laws. The idea is that the semantic problem presents a serious problem in providing an epistemic account of *cp* laws, but that nevertheless questions regarding the metaphysics of *cp* laws need yet be answered. Whether there is conceptual scope for such a distinction depends upon your account of laws of nature more generally.

The most serious question then is whether there are ontological properties in nature that law statements are reducible to. If such an account were provided, then it may be possible to accept the semantic and epistemic problems, whilst remaining committed to an ontology that requires an account of *cp* laws. It may even be more promising if an ontological account of *cp* laws were to provide truthmakers for *cp* law statements, so that a metaphysical account of *cp* laws may

solve the earlier semantic and epistemic problems that *cp* statements faced. The latter would be to argue that the semantic problem puts the epistemological cart before the ontological horse.

However, whether this response is available to the *cp* law theorist depends rather tentatively on the account of laws of nature that is assumed more generally. Therefore, it is necessary to analyse the more general accounts of laws and to question how commitments about this more general question affects the inferences we might make about the more particular case of *cp* laws[2]. Therefore, the more general philosophical question of what is a law of nature must be addressed.

In chapter one, we saw that physics chauvinism pervades how we characterise the sciences particularly since physics is taken as the *ideal* model for successful science. This is even more evident in the assessment of scientific laws, where it is customary to take the ideal laws of physics (e.g. Maxwell's equations, conservation laws etc.) as paradigms upon which we should model all other scientific laws.

We have seen in earlier chapters that in the tradition of logical empiricism there are certain normative features that a law *ought* to have. In particular, there are certain constitutive factors which, broadly speaking, are supposed to

[2] In most of the literature, the question of the semantics of *cp* laws is discussed without any reference to the more general question of what distinguishes a lawful generalisation from an accidental one. I wish to argue that this is misleading because background commitments to different theories of laws of nature deeply motivate how seriously we ought to take the semantic challenge.

distinguish lawful generalisations about facts from accidental generalisations about facts. A genuine law *should have* the following characteristics: it should be formulated syntactically as a universal quantified conditional generalisation in the first order predicate calculus (e.g. $\forall x\ (Fx \rightarrow Gx)$), it should support counterfactuals, have wide scope and not be restricted to spatio-temporal domains[3]. Moreover, a law statement should also support our inductive inferences and have explanatory and predictive power. Broadly speaking, laws of nature are different from accidents in that they have modal force that constitutes their lawlikeness.

Cartwright (1983 : 54-55) argues that the paradigm of ideal science has instituted a certain view about laws, which she calls the *facticity* view of laws. The facticity view of laws is committed to the scientific realist view that laws of nature describe facts about reality. Importantly, the laws of nature should tell us concrete facts about reality and we should not consider a law to be true unless it does in fact sufficiently capture these facts about reality. Moreover, it is committed to the assumption that science is cumulative and that our law statements are proceeding towards better and better covering-law explanations of reality. A commitment to the facticity view about laws is consistent with different ontological claims about firstly what these 'facts' that laws refer to might be and secondly, how these facts can make the conditional in law statements true. It is the latter part that concerns me in this chapter.

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[3] This is the classic view taken by Hempel and Oppenheim (1948) For example "All Metals Conduct Electricity" is a law because it is expressed in an unproblematic way as a universally quantified conditional. So let F stand for the metal and G the conduction of electricity, then this law can be expressed straightforwardly as $\forall x\ (Fx \rightarrow Gx)$. Importantly, this statement has wide scope is not restricted to spatio-temporal domains and it entails the counterfactual statement that if x were a metal then x would conduct electricity.

There is disagreement concerning whether the normative features we believe a law ought to have are epistemic or ontological; namely in relation to whether the modal force that distinguishes laws from accidents is epistemic or whether there are real facts or properties in nature that give laws their modal force. On the one hand, if we think that the modal force is a matter of an epistemic difference that *we* recognise in lawful generalisations as opposed to accidental ones, then the semantic problems that a putative law statement faces may challenge its eligibility as a genuine law. On the other hand, if there are further ontological properties, then there may be truthmakers for law statements in the external world. This general issue about the modality of laws will influence how seriously we ought to take our semantic problem in the more particular case of *cp* laws. In particular, the question of whether there are any plausible ontological candidates which might act as truthmakers for *cp* laws must be addressed.

In this chapter, I will examine the plausibility of providing truthmakers for *cp* laws. In particular, I will examine reductive accounts of laws of nature where the law statement is reduced to the ontological facts in the world that it describes. In so far as we would require a truthmaker for laws then we require an account of those facts in the world that make statements about laws true.

In section **4:2,** I will examine the neo-Humean view that there are no truthmakers for *cp* laws, because laws supervene on regularities. In section **4:3**, I will look at two commonly held views about laws of nature, namely nomic

necessitation and dispositionalism and analyse whether they might provide us with the relevant truthmakers to sidestep the semantic worry that faces *cp* laws.

I will argue that there is no room for *cp* laws on a nomic necessitation view of laws. However, I will suggest that dispositionalism is intuitively appealing as a solution to our problem. In particular, I will examine the view that dispositions are the truthmakers for *cp* laws, which is corroborated by the claim that a metaphysical assumption about dispositions explains so much, that the richer ontology is corroborated by the amount it explains.

Nevertheless, in section **4:4**, I analyse two problems that face the view that dispositions are truthmakers for *cp* laws. Firstly, there is a problem in verifying non-categorical properties, which means that dispositions remain a rather tenuous metaphysical assumption. Secondly, I examine how this view is constrained by the precise way in which we construe the disposition-law relationship.

In section **4:5,** I argue that a view of dispositions where the causal and nomic powers of the disposition may vary is more consistent with the intuition that there are heterogeneous kinds of dispositions a view, which I call *Variable Dispositionalism.* It becomes clearer then that even if dispositions did provide truthmakers for *cp* laws that this analysis would leave out important points about how these generalisations function in scientific practice.

4:2 Laws of Nature and Accidental Generalisations:

As we saw in the last chapter, the standard philosophical problem about laws of nature is to assess why laws are different from accidents. Take the statement:

(1) 'All sandwiches are made from bread'.

We would not wish to consider this statement a law of nature in so far as the claim is purely coincidental. In contrast, consider a claim like:

(2) 'All planets move in ellipses'.

The standard problem of laws of nature then is a question about what grounds the intuitive distinction between (1) and (2).

4:3 The Regularity Theory of Laws of Nature:

I will turn firstly to the regularity theory of laws of nature. According to the regularity account of laws, laws of nature manifest themselves in regularities. The naïve regularity theory of laws makes no distinction between the statement of the law and its manifestation. So, it is law that *F*'s are *G*'s *iff* all *F*'s are *G*'s, where the latter is a regularity in the world. Taking (2) above we would say:

(3) All planets move in ellipses *iff* all planets move in ellipses

where the consequent of the biconditional is a real regularity that manifests itself in the natural world.

This regularity view of laws is based on Humean scepticism about causation. The Humean problem of induction bans objective necessity from

nature and so concludes that we cannot justify our belief in the necessity of causation in the external world. All induction is based on the assumption that there is uniformity in nature. So, we believe that the sun will rise tomorrow, because it has done in the past and we assume that nature remains uniform. The inference from the constant conjunction of regularities to the positing of a power of necessary connection (which he calls "the Principle of the Uniformity of Nature") that underpins those regularities is often made. Hume's sceptical problem for induction is a problem about the justification for our assumption that nature will remain uniform. He states it in the following way:

> We have sought in vain for an idea of power or necessary connection in all the sources from which we could suppose it to be derived. It appears that, in single instances of the operation of bodies, we never can, by our utmost scrutiny, discover any thing but one event following another; without being able to comprehend any force or power by which the cause operates, or any connection between it and its supposed effect.
> (E: Sec VII, Pt II: 58)

Therefore, according to Hume, we *know* "one little event following another". In other words, we only know regularities and we cannot rationally justify our belief in anything beyond that. Even though Hume himself did not explicitly discuss the issue of laws of nature, we might extrapolate from this view that the laws of nature will also turn out to be regularities on this metaphysical view[4]. The naïve regularity view can be formulated in the following way:

> (4) It is a law that all *F*'s are *G*'s *iff* all instances of *F*'s are *G*'s.

[4] Although, Hume does not explicitly speak about laws of nature it is often taken that a regularity theory of laws is the direct upshot of a Humean metaphysics. For an example of this construal, see Psillos (2002: 137-159.) However, Mumford (2004: 21-31) has recently argued that in fact we ought to read the Humean metaphysics as a lawless one.

We can know that All *F*'s are *G*'s by enumerative induction i.e. *F*'s have regularly been *G*'s in the past without exception and there is some regularity in the world such that *F*'s are *G*'s.

The chief difficulty with the naïve regularity view of laws is that it is unclear, given that laws simply are regularities, how we should distinguish between lawful regularities and accidental ones. Popper ((1959) : 427-8) provides the following example. Moa's are birds from New Zealand that have been extinct for some centuries. We know that all Moa's died before fifty. So, it is a regularity that:

(5) "All Moa's die before fifty".

However, according to the regularity view of laws, this would turn out to be law of nature i.e.

(6) "It is a law that All Moa's die before fifty because all Moa's have died before fifty."

Nevertheless, the reason why all Moa's died before fifty was because of a rare virus in the New Zealand environment (i.e. an accident). Nevertheless, according to the naïve regularity view we would still have to assume that 4) is a law since it is an exceptionless regularity. Therefore, there is no way of distinguishing between those regularities that are genuinely lawful and those that are merely accidental. This counterexample illustrates that the fact that a generalisation is exceptionless does not entail that it is a law.

Therefore, the chief problem that faces the naïve regularity view is that we cannot distinguish between those exceptionless regularities that are accidental and

those exceptionless regularities that are lawful. One possible attempt to solve this difficulty is to claim that the property of lawfulness could be identified by examining our distinct epistemic attitudes to laws in comparison to our attitudes to accidents. For example, *our* ability to use law statements to generate predictions and explanations constitutes a significant difference in our epistemic attitudes to laws as opposed to accidents.

Goodman (1979 : 20) uses the following example to illustrate the different *epistemic* role that lawful generalisations play as opposed to accidents. Goodman asks us to compare a law statement like "Butter melts at 150°" with an accidental generalisation like "All the coins in my pockets are silver". He claims that the first is a law because it is accepted as true and will yield further predictions about further instances of butter, even though we could not possibly have studied all possible instances of butter. In contrast, the second example merely describes a contingent fact after the determination of all cases (i.e. an examination of all the coins in my pocket).

The chief problem with making the distinction between laws and accidents epistemic is that it is too subjective. It may be the case that other beings (other than humans in some other possible world perhaps) would construe different generalisations as laws. Indeed, we could just be totally wrong (or ignorant) in thinking that certain laws play the explanatory role that we think they do. Accidental generalisations could wrongfully be construed as laws, on this account, simply because of human ignorance.

More sophisticated regularity theorists attempt then to forge a more robust account of laws of nature. Mill (1846) firstly outlined the more sophisticated regularity theory in the 19th century and later on in the early 20th century, theorists like Ramsey (1928) and Lewis (1973) elaborated upon the Millean account. The earliest account of this sophisticated regularity theory can be seen in Mill's *System of Logic*. Mill claims that asking whether a given generalisation is lawful or accidental is in fact a misleading question. We ought not to examine any given regularity in isolation, but rather we ought to examine *regularities* (Mill call's them *uniformities)* in the plural.

Lawlikeness is not a property that can be ascribed to any regularity in isolation from other uniformities. We must study the web of laws. Moreover, the Uniformity of Nature is just a complex fact and is nothing over and above all the separate uniformities, which exist with respect to simple phenomena. Therefore, according to Mill the question "What are the Laws of Nature?" must be stated thus:

> What are the fewest and simplest assumptions, which being granted, the whole existing order of nature would result? Another mode of stating it would be thus: What are the fewest general propositions from which all the uniformities, which exist in the universe, might be inferred. {Mill (1846) III:IV:1: 207}

Clearly, according to this view, the simplicity and the generality of the putative law statement, its role in explaining and predicting more complex regularities that would supervene on it and indeed its coherence with other regularities in the system will determine whether it is to be deemed a genuine law of nature. Ideally once the fundamental law(s) of nature (there may be only one)

are revealed, these laws alone should be sufficient for the deduction of all other regularities.

Therefore, the question "Is *X* a law?" is a misleading one to ask. We can only study any generalisation in the context of other generalisations. A generalisation is lawful if it plays a certain role in a deductive system namely if it can in conjunction with other generalisations *explain* and *predict* certain facts about the world.

Mill's ((1846) III: IV: 1: 206-207) own example is the rise of mercury in a barometer. We can deduce that the mercury rose or will rise in a barometer from the following three regularities:

> (7) Air has weight.
> (8) Pressure in a fluid is propagated equally in all directions
> (9) Pressure in one direction, not opposed by equal pressure in the contrary direction produces motion, which does not cease until equilibrium is restored.

Therefore, 7-9 are laws because taken together they consistently explain or predict the fact to be explained in the world (10):

(10) The rise of mercury in a torricellian tube (barometer).

Therefore, a law is any statement, which taken together with other statements in a theory, we can deduce the fact to be explained.

At a first glance, it would seem that the choice for laws becomes rather arbitrary on this account. It could be argued that what is deemed lawful rather than accidental is supplied by the designer of the deductive system and so it is epistemic, rather than ontological. For this reason, it could be argued that this

account also faces the challenges of subjectivity and relativity that faced both the naïve regularity theory and Goodman's account above.

However, the more sophisticated account does not accept just any old deductive system. Lewis does recognise this challenge to the sophisticated regularity theory. He consequently takes great pains to give his account an ontological component in so far as it is not coherence and generality alone that allows us to consider a generalisation a law, but it must also be made to fit with other truths to make a best possible system.

Lewis for example (1973:73) argues that there will be innumerable true deductive systems because true statements can be axiomatized in different ways. Some systems will be simpler than others and some will be stronger than others. Consequently, Lewis places the constraints of both *simplicity* and *strength* on the deductive system. It is important that there is a balance struck between these two constraints. Lewis states that a contingent generalisation is a law of nature if an only if it appears as a theorem (or axiom) in each of the true deductive systems that achieve a best combination of simplicity and strength.

If we were omniscient, according to Lewis, false systems would fall out, leaving the true axiomatic systems to compete in their balancing of simplicity and strength. Ideally, there would be just one axiomatic system (theory of everything) from which all else ought to follow. This would of course achieve the best balance of simplicity and strength.

Nevertheless, it is an open question whether several systemisations might compete in their representations of reality so that finding the *best* system may become difficult or impossible. Lewis's (1994: 232) reply to this is that if fortune favours us, that the best system will come out best and that if it does not, then lawhood might be merely a psychological matter. Therefore, it is possible that, in the end, which regularities are lawful rather than accidental turns out to be irreducibly epistemic, rather than ontological on his view.

4:3:1 The Regularity View and *Cp* Laws:

How would a commitment to the regularity theory influence our account of *ceteris paribus* laws? I wish to argue that accepting the semantic problem at face value and concluding that there can be no *ceteris paribus* laws presupposes the view that there can be no truthmakers for *cp* laws. Thus, the conclusion rests on the assumption that a regularity theory of laws is correct. We saw above that the Regularity Theory of laws is based on a Humean metaphysics, which denies any objective necessity in nature.

So, the law "All metals expand when heated" is true because there is some regularity in nature, according to which, when a metal is heated it does in fact expand. However, there is nothing in the nature of a metal that makes this necessarily the case and there is no modal commitment that makes it impossible that we may find a non-expanding metal. It can be put in the following way:

(11) It is a law that all *F*'s are *G*'s *iff* all instances of *F*'s are *G*'s.

This is the naïve or simple regularity view of laws. Clearly, the only way that an account of *cp* laws could be given in light of a regularity theory of laws is in terms of providing a semantic account of what *cp* means.

It could be formulated as follows:

(12) It is a law that all *F*'s are *G*'s *cp iff* all instances of *F*'s are *G*'s *cp*[5].

Therefore, on a simple regularity view it is essential that an account of the meaning of *cp* could be given in order to qualify the law so as to exclude those exceptional cases that will contravene the regularity. Since the law supervenes on the regularity, then the corresponding regularity must be supported if the law is to be upheld.

A more sophisticated regularity theory does not fare any better in relation to the semantic problem. As we saw earlier, the sophisticated regularity theory states that lawlikeness is not a property that can be ascribed to any regularity in isolation from other uniformities in nature. The sophisticated regularity view agrees with the simple regularity view that laws express regularities, but argues in addition that there is a further feature of lawlikeness, namely that in order to be a law of nature the statement must be a universally true statement in a deductive system of universally true statements, a system that strikes a balance between simplicity and strength (Mill (1846), Ramsey (1928), Lewis (1973)).

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[5] We saw in the last chapter that *cp* laws have *ipso facto* exceptions and so what the *cp* clause is doing is explicitly claiming that we do indeed know of instances where all things might fail to be equal; namely of exceptional cases where we have $F \& \neg G$. For this reason, the question regarding what distinguishes a *cp* law and an accidental generalisation is even more problematic that the general question of distinguishing between laws and accidents (where laws are understood as universally quantified conditionals.)

Tooley's (1977) chief objection to the regularity views of laws is the claim that there are no adequate truthmakers for law statements on that account. Tooley imagines genuine laws, which may remain "underived" in the best deductive system. For example, since it is at least plausible that there could be laws that are never in fact instantiated, then the claim that regularities are the truthmakers for laws is unwarranted.

This is exemplified in (Tooley (1977). He states:

> But what if [....] there could be underived laws dealing with the interaction of particles of types X and Y? Can one draw any conclusions from the assumption that such basic laws without any positive instances are possible – specifically conclusions about the truthmakers for laws? I would suggest that there are two very plausible conclusions. First *nonnomological* facts about particulars cannot serve as truthmakers for laws. [...] In the absence of positive instances, there is no basis for holding that one generalisation is a law, and the other not. (Tooley (1977): 669).

Tooley argues then that facts about particulars (e.g. regularities) cannot provide a satisfactory account of the truth conditions of uninstantiated laws.

I would argue that *cp* laws, like uninstantiated laws, are another special problem for the sophisticated regularity view. In order to be accommodated into this system a given *cp* law must be simple or add to its strength. Clearly, a vacuous statement will not be admitted into this system, in so far as both the regularity that it is supposed to describe, as well as exceptions to it, will be consistent with the vacuous statement. For example, take the following putative *cp* law:

(13) All *F*'s are *G*'s *cp*

Since according to the semantic objection (as addressed in the last chapter) this statement is strictly speaking vacuous then (13) can be read as (14):

(14) All *F*'s are *G*'s, unless they are not

Given that the regularities $F \,\&G$ and $F\&\neg G$ are consistent with this statement, both regularities and their exceptions can be deduced from the same law. Moreover, since the generalisation is clearly consistent with evidence both for and against the corresponding regularity it would not be deductively useful in any way. This is clear from the fact that admitting this type of generalisation into the web of laws would prevent any prediction with regard to further *F*'s.

If we take the semantic problem at face value then at worst *cp* laws would fare no better than accidents in the web of laws. In fact, there would be no sufficient reason to distinguish between the regularities expressed in putative *cp* laws and those expressed in purely accidental generalisations. Thus, the regularities expressed by *cp* statements should either be deducible from some further strict laws or be eliminable altogether.

However, it might be objected that to allow our semantic problem for *cp* laws such leverage presupposes a distinctly empiricist epistemology[6]. It involves an empiricist epistemology because it presumes that the modal facts (such as the claim commonly made laws that are well-confirmed statements of universal scope that support inductive inferences and so on) those very facts that are supposed to

[6] Woodward (1992) argues that this empiricist epistemology misleads accounts of laws of nature in general. It motivates not only regularity views of laws, but also somewhat surprisingly nomic necessitation views like those put forward by Dretske (1977), Armstrong (1967) and Tooley (1977). According to Woodward, there is a tension in the Nomic neccesitation view in that it is committed to two basically incompatible background commitments namely a Humean epistemology and a realist metaphysics.

distinguish law statements from accidental generalisations are in fact supplied by us[7]. This relies on the Humean claim that we cannot impugn any necessity to nature and that we can only provide a rationale for those facts that are directly verifiable.

Therefore, it would seem clear that in holding a regularity view of laws the semantic problem for *cp* laws ought to be taken seriously. It either entails that there are in fact no *cp* laws or that a deductive system with *cp* laws is merely a placeholder for some better axiomatic system that will achieve a better balance of simplicity and strength. Indeed, it would seem that grounding any scientific beliefs on *cp* laws could not be rationally justified. So, the burden of proof is on the regularity theorist to provide a semantic account.

The semantic problem that faces *cp* laws then is motivated by certain core semantic and epistemic commitments. In particular, it depends on the claim that we can only make meaningful claims about facts and regularities and the claims that the external world can provide no truthmakers for our law statements. Laws of nature *qua* regularities are the basic explanatory constituents of science because the laws supervene on the regularities. Specifically, regularity accounts cannot provide truthmakers for *cp* laws because the empiricist can only appeal to regularities and *cp* laws are not straightforward regularities.

[7] One of the chief problems with the regularity theory is the fact that both laws and accidents supervene on regularities. Since, both the laws and accidents are contingent, it might be claimed that the only distinction between them is found in our intuitions about the strength of lawful generalisations.

4:4 Nomic Necessitation

However, those who are loath to give up on *cp* laws may seek to save them from vacuity in another way; namely to provide a metaphysically substantial account of the truthmakers of *cp* sentences. This involves the rejection of an empiricist epistemology. In other words, the semantic problem is a result of a misleading semantics and epistemology in the first place and we should not draw any metaphysical conclusions from it[8].

So, a verificationist theory of meaning and an empiricist epistemology results in a Humean metaphysics[9], which expels unobserved real properties of the external world. Empiricist scepticism about the metaphysical status of powers, capacities, dispositions, or whatever is unwarranted and a richer ontology might provide us with the kind of truthmakers that will save *cp* laws from vacuity. At the very least, it is an open metaphysical question about whether a richer ontology might alleviate some of our semantic worries. Furthermore, it is a question that must be addressed before any conclusions might be made about *cp* laws.

One possibility for overcoming the *cp* semantic challenge is to say that there are real properties in the external world that can be considered as truthmakers for *cp* laws i.e. they can provide the content of *cp* laws. There are

[8] It is not necessary that something be directly verifiable in order to be meaningful. This rejects the verificationist criterion of meaningfulness.

[9] It is unclear whether if is in fact fair to attribute this metaphysical picture to Hume. Hume's claim is entirely sceptical it involves a claim about the possibility of a metaphysical worldview and the limits of human reasoning, rather than a clear-cut rejection of the possibility that there might be real noumena beyond the phenomena that we observe. Nevertheless, I will call the metaphysics "Humean" as is common in the literature.

several candidates for what these properties might be (universals, categorical properties, dispositional properties etc.)

We saw that Tooley's objection to the Regularity account of laws was that regularities could not provide the truthmakers for uninstantiated laws. In an attempt to solve this particular issue, Tooley argues that facts about universals provide the only plausible truthmakers for uninstantiated laws. Moreover, since facts about universals provide a positive way out of the problem of uninstantiated laws, Tooley suggests that facts about universals could be the truthmakers for all laws. From this premise, Tooley (1977) argues that laws are a relation of nomic necessitation between universals.

On a nomic necessitation view of laws, (Armstrong (1983), Dretske (1977), Tooley (1977))[10] the modal force that we consider laws to have, which in contrast we do not consider accidents to have, is a result of a relation of natural necessity in nature. Laws are taken to be higher order universals[11]; they are relations whose *relata* are first order-universals. So, for any law there is a relation of natural necessitation between two universals *F* & *G*, which can be formulated as follows:

(15) $N(F,G)$

The relation of necessitation binds *F*-ness and *G*-ness, so that when $F(N,G)$ holds so too does the corresponding regularity in the world (i.e. "All's *F*'s are *G*'s").

[10] The nomic necessitation view that laws of nature are relations between universals, was according to Armstrong (1983) : 85, reached independently by all three theorists in the 1970's. However, the idea is not a new one and could be traced back as far as Plato's *Phaedo* (102-107).
[11] This view involves a strong realism about universals.

So, the natural necessitation relation entails the universal conditional used to express the law by material implication:

(16) $(N(F,G)) \rightarrow (\forall x\,(Fx \rightarrow Gx))$

This is a material conditional rather than a bi-conditional, in order to leave open the possibility of regularities that are merely accidental. Nevertheless, this relation of natural necessitation is not one of logical necessity. This leaves open the possibility of worlds where the relation of natural necessitation may not hold. For this reason, it is a relation of *contingent* necessitation, even though in any particular world where the relation *N(F,G)* does hold, then it is the case that in that world "All *F*'s are *G*'s".

4:4:1 Nomic Necessitation and *Cp* Laws:

So how might this view accommodate putative *cp* laws? Take a putative *cp* Law like Farr's Law in Epidemiology, which states that:

(17) "All epidemics increase their incidence rates exponentially cp."

Let *F* stand for epidemics and let *G* represent the exponential increase in their incidence rates. Now, according to a nomic necessitation view of laws, Farr's law should be read as:

(18) *N cp (F ,G)*

In (18), *F* and *G* must both be universals, while the relation *N(F,G)* is a second order universal which expresses a relation between two universals "*ceteris paribus*". The regularities that hold in the world do not constitute the laws that hold in the world, but they are governed by the second-order relation *N* between

first order universals *F* & *G*. So, although the laws govern universal statements about regularities, universal statements about regularities need not all be laws.

The problem often cited against this view is that *N*, the relation of natural necessitation, is suspect. Armstrong ((1983): 86) cites his indebtedness to a correspondence he had with David Lewis on this very problem. He states:

> Two universals are linked in a certain way. The state of affairs is in turn linked to a corresponding uniformity, where this second linkage is an entailment. Is the entailment part of, actually contained in, the relation between the universals, so that the entailment has the form $(P\&Q)\rightarrow P$, with P = the uniformity? If so, the relation between the universals contains a surplus, Q, over and above the uniformity. What is this surplus, and how is it related to the uniformity? An alternative is that the entailment has the form $P\rightarrow (PvQ)$. (Armstrong (1983) : 86).

This problem of making the '*N*' relation less suspect becomes even more pertinent when applied to the case of *cp* laws, in so far as it is difficult to understand how a relation of natural necessitation '*N*' can admit exceptional cases and yet still remain distinguishable from mere accidental relations. Indeed, it would seem impossible for the nomic necessitation view to entail anything but strict laws; if it is taken that the *N* relation entails an exceptionless regularity. Even if we could read *F* and *G* as universals[12], the *cp* appendage to the *N*(*F*,*G*) relation makes it itself an implausible candidate to be a second-order universal, unless we can specify all of the natural contravening factors that may prevent N from holding between *F*,*G*.

Since there would be an indefinite (possibly infinite and "in principle" unknowable) list of qualifier cases it is unclear how we could ever determine

[12] It is unclear whether we should read the predicates in *cp* laws as being plausible candidates for universals.

whether *N(F,G)* was in fact a second-order universal. So, we are left with a case where we have universal properties, but the higher order relation between them cannot be grounded or maintained. It is unlikely that there is a relation of natural necessitation between *F* and *G* because *N(F,G) cp* does not express a universal. Therefore, the natural necessitation view then only accommodates strict (universally quantified) laws. Universals then are not plausible truthmakers for *cp* laws.

However, Armstrong (1978*b*) elucidates the '*N*' relation by committing to a different theory of universals. He envisages an instantiation requirement for all universals. Armstrong's instantiation requirement for laws entails that logically equivalent sentences do not necessarily express the same law. Therefore, *N(F,G)* may express a law, but *N(¬F, ¬G)* cannot, since *¬F* and *¬G* cannot be instantiated.

In contrast, for Tooley (1977 : 686), universals exist transcendentally[13]. Armstrong, *pace* Tooley makes universals exist only in their instances[14]. Laws, then since they are second order universals must have instances on Armstrong's account. Particular causal sequences are the instances of laws. Therefore, there is nothing spurious or unsubstantiated about positing the 'N' relation, according to Armstrong, since we can have knowledge of the 'N' relation by experiencing the particular causal relation that instantiates the law. Armstrong's theory of laws is therefore, corroborated by his broader theory of universals.

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[13] The difference between Armstrong and Tooley is essentially in their different accounts of universals. Armstrong as noted above favours an immanent realism about universals, whereas Tooley (1977) favours platonic realism about universals, where universals are transcendent.
[14] Armstrong consequently rejects uninstantiated laws.

At a first glance, it looks as though Armstrong's account could only accommodate strict laws. On Armstrong's account of laws, when *a*'s being *F* necessitates *a*'s being *G*, then it is because both *F* & *G* are universals and are such that *a*'s being *F* necessitates *a*'s being *G* independent of any other properties that *a* may have. In other words, the relation $N(F,G)$ entails the universal conditional $(\forall x\ (Fx \rightarrow Gx)$. On this reading of Armstrong's account, all laws would be strict laws, or as he dubs them 'iron laws'. (Armstrong (1983) : 147) He states:

> 'I shall call laws of the second sort 'iron' laws. They tell us that, given certain conditions, some further state of affairs is necessitated (or has a certain probability of being necessitated – note that an iron law can be irreducibly probabilistic) no matter what further conditions are added. If it is a case of necessitation [...], then the law issues an exceptionless uniformity. (Armstrong (1983) : 147)

Armstrong does however recognise that all laws may not be strict laws and discusses the possibility of universally qualified laws, which he calls 'oaken laws' (Armstrong (1983) : 148). He claims that it could be a nomic truth that, with an exception, *F*'s are *G*'s. In this case, there is only one single exception, which if present, will prevent an *F* being a *G*. Armstrong however, does admit that it is not contrary to reason that there may an indefinite number of such factors, lacking any specific commonality which may prevent an *F* from being a *G* in different situations. It is this characterisation that leads Armstrong to define oaken laws in the following way:

> 'It appears, then, that if $N(F,G)$ is an oaken law, then all that is entailed is that for all *x* where interfering factors are absent, if *x* is *F*, then *x* is *G*.' (Armstrong (1983) : 149)

According, to Armstrong the distinction between iron and oaken laws does not require a different analysis of laws in so far as the relation '*N*' of natural

necessitation is the same in both. Therefore, the difference between strict (iron) and *ceteris paribus* laws (oaken) is a matter of degree.

Armstrong's account assumes that the metaphysical '*N*' relation is sufficiently robust to make the possibility of exceptions relatively unproblematic. However, the semantic problem will recur in so far as the list of possible exceptions may be in principle infinite and open-ended. Therefore, a given qualified oaken laws i.e. (*N cp* (*F*,*G*) is tantamount to saying *N* {....} (*F*,*G*).

Moreover, this is more than a mere *semantic* problem. Armstrong has accepted that given an instantiation of a law, there are other factors that can interfere with that law holding and that these factors are such that we cannot in principle *know* them in advance of the law being instantiated. However, this is not merely an epistemic problem about our knowledge of these further factors, but rather the fact that the relations that we perceive in the world are irreducibly contingent.

It would appear then that once oaken laws are allowed on the scene the distinction between laws (both oaken and iron) and accidents becomes a matter of the degree of contingency of the relations of natural necessitation. However, our method of determining the degree of contingency of these relations is subject to these relations being instantiated (the instantiation requirement). The Humean sceptical problem then arises in so far as our intuition that some natural relations are more necessary than others may be just an unsubstantiated assumption.

Now on the two possibilities that we have considered thus far namely; the *Regularity theory* and the *Nomic Necessitation theory,* it is assumed that science

discovers laws that are general statements about categorical properties. On both these accounts the laws of nature are contingent, rather than metaphysically necessary. Importantly, this means that all the properties invoked in such laws have their causal and nomic powers non-essentially. There are possible worlds where these properties exist, where the laws of nature might have been different. So, this kind of analysis of laws involves the claim that there is nothing essential in the properties described in the laws of nature, which makes them the way that they are. In fact, it is the laws of nature that make the properties described in them, the way that they are.

For this reason, theorists such as Ellis (2002) and Mumford (2004) have argued that in fact both the regularity and the nomic necessitation views are committed to an empiricist metaphysics, where the laws are more metaphysically basic than the properties (or the properties of the kinds) referred to in those laws. It is the laws (or the regularities described by laws) about categorical properties, which make the properties behave in the way they do rather than any essential metaphysical facts about those same properties. Therefore, an account of laws of nature *tout court* will be sufficient for an explanation of the categorical properties. Clearly, if the laws of nature are understood to be contingent relations between categorical properties, then the semantic worry is a genuine one, in so far as there is no further metaphysically substantial truthmaker that can save *cp* laws of nature from vacuity.

4:5 The Dispositional View of Laws:

However, there is an alternative view as argued by Lipton (1995), Bird (2002) Ellis (2001)[15] that might be taken, which is to claim that law statements are descriptions of underlying stable dispositions. While all of these theorists agree, broadly speaking, that laws of nature describe stable dispositions, there is much disagreement on the precise relationship between the laws and the dispositions, more particularly centred on the role we give to the dispositions themselves. I will argue below that the precise nature of this relationship places constraints on the possibility of these dispositions acting as truthmakers in the particular case of *cp* laws. However, we must first analyse both what we mean by dispositions and why the view that dispositions are truthmakers for laws has such intuitive appeal.

What do we mean by dispositions? Dispositions are capacities, tendencies and propensities to certain types of occurrences. Dispositions are characterised by their stimulus and manifestation. For example, something that is flammable, will respond by igniting, following the presence of the stimulus (e.g. the striking of a match). Lewis (1997 : 143) argues for a conditional analysis of dispositions, which holds that the combination of the disposition and the stimulus always brings about the response. Bird (2004 : 2-3) formulates it in the following way, where D is the disposition, S is the stimulus, and M is the manifestation:

(19) $(Dx \textit{ iff } Sx \rightarrow Mx)$

So for example, for all flammable (their disposition) things, if they are set alight

[15] These views agree in so far as they hold that notions like dispositions, capacities and powers must play a role in an account of nomological relations. However, the views differ in what they consider this role to be and on how we are to characterise it.

(their stimulus) then they will burn (manifestation of the disposition). Moreover, the laws of nature that describe the dispositional properties are entailed by the conditional analysis of dispositions.

According to this *Dispositionalist* account of laws, the dispositional nature of laws is essential, in that it characterises an essential nature of the property *D*. So, for this reason, the conditional analysis of dispositions is necessary and the corresponding law that is entailed by it is also necessary. This account successfully avoids some of the problems facing the two contingentist theories of laws addressed earlier (i.e. *Regularity* Theory and *Nomic Necessitation* Theory).

On the one hand, one of the chief problems that faced the regularity theory of laws was that it was unclear how, on this view, we could draw the laws/accidents divide. However, the dispositional account provides a way of doing this, in that we can have accidental regularities that we might consider to be universal and yet, if they are not entailed by an essential dispositional property then they are not necessary and so we can deem them accidental. The dispositional account also avoids Tooley's (1977) objection to the regularity theory which claimed that categorical facts cannot act as truthmakers for laws, in that non-categorical dispositions can act as truthmakers for laws. Dispositions can act as truthmakers because a conditional generalisation (law) that describes an essential dispositional property of things is universally true, even if the disposition fails to manifest itself on a given occasion.

On the other hand, one of the chief problem that faced the nomic necessitation view of laws was that it had a second-order relation '*N*' between first-order universals which was difficult to cash out and might even be considered metaphysically suspect. However, the dispositionalist account does not face this kind of challenge in that the conditional is entailed by an essential property of the disposition and so it is metaphysically necessary.

Nevertheless, Martin (1994) has outlined some counterexamples to this conditional analysis. These counterexamples are the direct upshot of the possibility that objects can lose and indeed acquire different dispositional powers. One counterexample is the possibility of what he terms a "finkish" disposition. A disposition is finkish, if it is caused to cease by its own stimulus. For example, when a match is struck, the striking of the match makes it lose its disposition to re-ignite in the future. Lewis (1997) characterises finkish dispositions in the following way:

> Dispositions come and go, and we can cause them to come and go. Glassblowers learn to anneal a newly made joint so as to make it less fragile. Annoyances can make a man irascible; peace can soothe him again. [.....] Such a disposition, which would straight away vanish if put to the test, is called finkish. (Lewis (1997) : 144)

Another counterexample, as argued by Bird (2004: 262-263), is that a disposition is extrinsically linked to how it is satisfied in an environmental setting; so for example, a disposition might not be manifested because an antidote may be

present that prevents its manifestation[16]. These counterexamples present a problem for the straightforward conditional analysis of dispositions.

4:5:1 Dispositional View and *Cp* Laws

However, Bird (2004 : 7-10) argues that these counterexamples provide an insight into analysing putative *cp* laws. The idea goes (rather imprecisely) like this; if there are further real properties that are more basic that *ceteris paribus* laws, then these properties might provide the truthmakers for the conditional claims made in the corresponding *cp* laws. According to Bird (2004), the truth conditions for *cp* laws become unproblematic given the assumption that they describe underlying stable dispositions, given a conditional analysis of those dispositions and allowing for dispositions that may be finkish or have antidotes.

In particular, the counterexamples to the conditional analysis of dispositions are exceptional cases where "all else is not equal". So, the *ceteris paribus* appendage in the law literally adds the proviso that finks and antidotes *could* stop the disposition from manifesting when stimulated in certain circumstances. Consequently, the counterexamples to the conditional analysis of dispositions may provide plausible completers for *cp* laws. Therefore, the truth conditions for a given *cp l*aw can be stated as follows:

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[16] This is in fact a response to Lewis' (1997) reformed conditional analysis that attempts to avoid the counterexample of finkish dispositions. Something x is disposed at time t to give response r to stimulus s *iff*, for some intrinsic property B that x has at t, for some time t^1 after t, if x were to undergo s at t and retain B until t^1, s and x's having of B would jointly be an x-complete cause of x's giving response r. (Lewis (1997 : 157)

(20) (*(*$\forall x$(Dx $\rightarrow$ Mx) *cp)* is true *iff* ((Dx & Sx $\rightarrow$ Mx) & ($\neg$ finks and antidotes present))

An example will prove helpful here. Consider Farr's Law in Epidemiology which states that "*Ceteris Paribus*, all epidemics increase their incident rates exponentially". The *ceteris paribus* proviso is added on in order to account for cases where there can be a non-exponential spread in epidemics because of certain environmental factors (where the disposition to spread exponentially is ceased by an environmental antidote or by its own stimulus). For example, the modern epidemic of obesity in young children will not have the exponential bell-curve shape that we usually associate with epidemics. This is because no natural immunity can develop. Rather, an environmental intervention will be required such as imposing healthy school meals. We can see then that the exceptional cases are rather context-specific and also why it would be difficult to provide closure causes.

Under Bird's analysis of *cp* laws, we can understand this law as describing an underlying disposition. So, there is an essential dispositional property in all epidemics, which means that they have the "tendency" to spread exponentially. However, when they do not there is either an antidote (i.e. an environmental intervention) or the disposition is finkish (i.e. is stopped by its own stimulus). Therefore, the characterisation of the underlying disposition is supposed to explain both when a *cp* law successfully describes a disposition that does in fact manifest itself and also a disposition that does not in fact manifest itself i.e.

because certain finks or antidotes were present. The interesting upshot of this account is that it provides a semantic account of *cp* laws by the introduction of underlying dispositional truthmakers.

An object or kind possesses a dispositional property whether the disposition is manifested or not. So, even when an exception occurs and a given disposition is not manifested, the disposition is still possessed by the object. Consider a glass that possesses fragility. Were the glass to fall on a soft surface, (say for example a mattress) it would not break. Moreover, the appeal to finks and antidotes can explain the exceptional cases. In this case, the soft surface of the mattress is an antidote to the manifestation of the glass's fragility.

However, it is not a counterexample to the fragility of the glass, because the glass is still fragile, even though its fragility is not manifested on this occasion. So, it is still true to say that if the glass were dropped *ceteris paribus* it would break. Therefore, even though we have a case of $Dx \ \& \ \neg Mx$ (i.e. an exception) the law is still true because it describes the dispositional property possessed by the object even when it is not manifested. The *cp* law then is true of the underlying disposition even if the disposition is not or is never manifested[17]. So, the disposition provides a truthmaker for the *cp* law. Dispositionalism then is intuitively appealing in so far as it provides a way of coping with the *ceteris paribus* clauses in *cp* laws.

[17] Another advantage of this account is that it provides an account of uninstantiated (i.e. laws that have no positive instances) which was Tooley's (1977) main objection to the Regularity view and the chief motive for introducing universals as the truthmakers for laws. We have no problem, on the dispositional view of laws, in imagining that it might be true to make a conditional claim about a dispositional property of something, even if that dispositional property is never in fact manifested.

4:5:2 *Cp* Dispositions and Verification:

Now even though this account has intuitive appeal as a solution to our problem, the first problem that this account faces is the fact that we are making a metaphysical assumption in letting dispositions (at least understood as essential properties) into our ontology in the first place. Categorical properties are relatively unproblematic, in so far as the object possesses them when and only when they are manifested. Therefore, we have direct evidence for categorical properties since they are always manifested.

Dispositional properties, on the contrary, are thought to be possessed by the object even when they are not manifested; that is to say even when we have no directly verifiable evidence for their presence. Therefore, there is a problem with postulating dispositions as the truthmakers for *cp* laws, namely on what grounds can we reify dispositions as being stable underlying properties when they are sometimes not manifested (i.e. when at times we have no evidence for their presence)[18].

Dispositions, capacities and powers were eliminated from the language of science, after the onset of British empiricism, because it was thought that power concepts are too closely related to their effect, which consequently, makes it

[18] In fact, Armstrong, D.M. (2004a) argues that dispositions are identical to categorical states for this very reason. For Armstrong, capacities, powers and dispositions are not real properties in their own right. His powerless ontology reduces powers to categorical properties and their corresponding laws. Categorical properties are universals and laws are relations of nomic necessitation between universals.

difficult to test power claims. Cartwright (2002:427) exemplifies this point in the following:

> What makes heavy bodies fall? Gravity. What is gravity? That which makes heavy bodies fall{.....}Newton's second law seems to suffer the same difficulty. $F=ma$. What is it for a body of mass m to *be* subject to a total force of size F? A mass of size m is subject to force F *iff* its acceleration is a. (Cartwright (2002 : 427))

Therefore, there is a problem of circularity that faces the view the dispositional view of laws, namely that the only way we can give content to our dispositional terms is *via* the corresponding law.

This problem is even more significant in the case of putative *cp* laws. If dispositional terms require corresponding laws for content then how can dispositions save *cp* laws from vacuity? What does it mean to say that something is an epidemic? Well surely it means nothing more that the corresponding conditional i.e. 'If it is epidemic, then it will spread exponentially'. It would seem then that the dispositionalist view of laws is just making a metaphysical assumption that the disposition is more ontologically basic that the corresponding law. There is clearly a rather symbiotic relationship between laws and dispositions. Lipton (1999) recognises this problem. He states

> The problem for the occurrent view of *cp* laws is that we cannot give *cp* laws determinate content because we cannot specify that antecedent of the law; the problem for the dispositional view is that we cannot give the dispositional attribution determinate content because we cannot specify he antecedent of the corresponding conditional that would give the dispositional term its meaning. (Lipton (1999) 166-167)

So, a regularity theorist about laws can straightforwardly deny that the dispositionalist view has any force and ask how we can rationally justify our assumption that there are dispositional essential properties in the world, when our

only way of knowing about them is *via* the generalisations that we make i.e. by *cp* laws. So, the chief problem with the dispositional analysis of *cp* laws is that it is circular; *cp* laws need dispositions to be meaningful and in order for us to know about dispositions we need *cp* laws.

One response is to admit that dispositions remain a metaphysical assumption, but claim that nonetheless they can be legitimated by an inference to the best explanation called an *argument by display*[19]. {Ellis (2001), Lipton (1991)[20]}. On admitting that dispositions are a metaphysical assumption, this argument claims that the assumption is warranted because once they are introduced, they explain many of the things that we hoped the right metaphysics would explain[21]. Moreover they solve many of the problems that face a metaphysical account that relies on categorical properties or regularities. Ellis states his argument thus:

> You show your wares and invite people to buy them. If your system strikes your readers as being simpler, more coherent or more promising than any alternative for dealing with the recalcitrant difficulties of other systems, then this may be a good reason to buy it. (Ellis (2001): 262)[22]

So what does a dispositionalist analysis of *cp* laws "buy" us? As we have seen it provides a way of sidestepping our semantic worry and so gives us an account of *cp* laws. If this were the only thing purchased then clearly the entire

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[19] I follow Ellis (2001) in calling this form of metaphysical argument "an argument by display".
[20] Lipton (1999) admits that this challenge to the dispositionalist account hurts, however he argues that there is a way for the dispositionalist to respond. The first reason he provides is that this problem simply must have a solution, because it applies not only to *cp* laws but also to every dispositional attribution that we make in everyday life too. Secondly, Lipton thinks that this problem could be answered by some version of semantic externalism.
[21] Mumford (2004:119) rightly points out that this is a common form of argument in metaphysics. For example, David Lewis provides no positive argument for possible worlds other than 'give me all the worlds and I will give you all you want in return.'
[22] Ellis uses this argument to support his dispositional essentialism not to corroborate the more specific case of *cp* laws.

account would be question begging, in so far as we introduced dispositions in the first place to solve our semantic worry. However, Lipton (1991) has argued that in fact there are other factors that the dispositionalist account can explain which are left unclear in both of the competing regularity and nomic necessitation accounts.

Firstly, the dispositionalist account can explain why some *cp* laws are lawful even though some *cp* laws may not ever in fact be instantiated (because they describe stable dispositions that although they may not be manifested are still in fact possessed by the object regardless of whether they are manifested.)[23] Secondly, it can explain why *cp* laws typically fail to satisfy many of the requirements for laws that we saw at the beginning; namely support for counterfactuals, instance confirmability, and so on[24].

There are two further alternative possibilities that might support the introduction of dispositions. The first possibility is that it may be possible to provide some *a posteriori* empirical reasons that might corroborate our metaphysical assumption[25]. For example, science might provide us with evidence in testing that shows the invariance or stability of a disposition in controlled tests and therefore, allows us to introduce it as a theoretical entity (Woodward (2004)).

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[23] One problem that we saw the Regularity View of laws faced was that it could never give an account of uninstantiated laws, since there is no direct observational evidence for them.

[24] See Lipton (1999: 163-4) Firstly, it is clear that our counterfactual claims will not be supported because the disposition may not or may never be manifested, so the possibility of exceptions precludes the inference that if *x* possesses a disposition, then *x* would manifest it. Secondly, it is also clear why instance confirmability is not secures, because this account leaves open the possibility of laws with no instances (uninstaniated laws) and also exceptional cases directly contradict rather than confirm the law (categorically understood).

[25] Drewery (2004) has argued that an *a posteriori* argument is all we can hope for to corroborate our theory of dispositions.

The second possibility is to argue that dispositions are found as a result of idealisations in science. This view is motivated by Cartwright (1989) for example, where she argues that laws will describe capacities that hold in an idealised experimental setting. Cartwright (1999) argues that *cp* laws describe capacities when and only when there is a nomological machine present. A nomological machine is defined as "a fixed arrangement of components or factors with stable (enough) capacities that in the right sort of stable (enough) environment will, with repeated operation, give rise to the kind of regular behaviour that we represent in our scientific laws." (Cartwright (1999): 50). Now sometimes a nomological machine will be produced naturally (e.g. the rotation of the planets), but most of the time scientists in an experimental setting design the nomological machine, so that potentially disturbing factors are excluded.

According to this view, the *cp* law and the disposition it describes only hold so long as the nomological machine continues. If the factors were to radically change then we might get an exception to the law and the disposition may be eradicated. On this account, *cp* laws rarely literally apply to dispositions in the actual world. Unfortunately, the natural world is a rather messy place[26]. This is why designing idealized models, which approximate reality rather than make a perfect fit for it, makes up a significant part of much scientific research. On both these views, dispositions can be corroborated by literally doing the empirical work. So, an *a posteriori* methodology could provide a means of corroborating our metaphysical theory of dispositions.

[26] See Cartwright (1999) for the view that the world is dappled.

However, I wish to argue that this *a posteriori* argument entails a rather different account of dispositions, which significantly effects how we construe the relationship between dispositions and their corresponding laws. In particular, how this relationship is construed can radically alter whether we can claim that dispositions are genuine truthmakers for *cp* laws.

Broadly speaking, there are two competing characterisations of dispositions that emerge from the above accounts, each involving rather different accounts of the relationship between laws and dispositions. The first view is *Dispositional Essentialism*, (Ellis (2001), Bird (2004)), which holds that natural kinds possess their dispositional properties essentially. According to dispositional essentialism, a law just is the possession of an essential dispositional property by a natural kind. In other words, an account of laws as universally quantified conditionals is replaced by an account of laws as essential dispositional properties. The second view is *Variant Dispositionalism* (Cartwright (1989), (Woodward (1992)) which claims that the causal and nomic powers of dispositional properties are possessed non-essentially. On this view, it is the stability of the disposition against possible changes in an experimental setting that allows us to pick out our dispositional properties.

4:5:3 Dispositional Essentialism:

The strongest version of Dispositionalism is *Dispositional Essentialism*. Dispositional essentialism holds that dispositions are the truthmakers for laws

because all members of a natural kind have their dispositional (causal) powers essentially. What makes a conditional law statement true is the fact that dispositions are part of the real essence of natural kinds {Ellis (2001}. Laws expressed as universally quantified conditionals are replaced by a realist account of dispositional properties. Therefore, the traditional account of laws as universally quantified conditionals (e.g 'All *F*'s are *G*'s) is reduced to a metaphysical account of dispositions (*F*'s are disposed to *G)*. This view results in the claim that the laws of nature are metaphysically necessary. Laws are true then, on this account, because certain kinds of things have an essential structure. Dispositional essences provide the ontological ground for laws.

According to dispositional essentialism, dispositions possess their causal and nomic powers essentially. Dispositions are properties of natural kinds[27]. On this view, even when a disposition is not manifested it is still possessed essentially by the natural kind. Moreover, the disposition is ontologically prior to the corresponding law. For this reason, laws can be reduced to dispositions. Laws at most are merely statements, which describe dispositions.

This is the strongest sense in which a disposition could be a truthmaker for the *cp* law in so far as the dispositional property ascribed to the kind in our law statement is an *essential* power of the kind, which it possesses whether or not it is in fact manifested. So, it is not the possession of a disposition alone that acts as a truthmaker, but the fact that the disposition is an essential property. Now the

[27] One problem in applying this view to the case of *cp* laws is that many of the putative candidates for *cp* laws are in the special sciences and it is debatable whether the kind predicates of the special sciences refer to real natural kinds. I argued in Chapter two that natural kinds in the special sciences ought not to be construed in this way.

problem that faces this account is that the only way of knowing that the disposition is possessed essentially is in knowing that it is a property of a natural kind. But, the only way we know this is by the corresponding law.

However, this alone would not present a major problem for the dispositionalist account since it is merely an epistemic point about how we come to know about dispositions. The dispositionalists can admit a degree of epistemic ignorance about dispositions in the absence of corresponding law statements, and yet consistently be committed to the ontological reduction of the law to the disposition.

However, there is also, I contend, a metaphysical problem for the stronger variant of dispositionalism in that it construes dispositions in a fairly homogeneous light when candidates for dispositions are rather heterogeneous[28]. It is clear that there are not only different kinds of dispositions, but also that the behaviour of dispositional properties is variable. *Determinate* dispositions only have one determinate kind of manifestation[29]. Other *Determinable Dispositions* have more than one manifestation[30]. For other *Proportional* dispositions the degree or extent of the manifestation may be proportional to the degree or extent of the stimulus (e.g. the degree of solubility of salt is proportional to the volume of water[31]).

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[28] The view that there are different kinds of dispositions has been put forward before. Ryle (1949) distinguished between determinate and determinable dispositions. Huttemann (1998) distinguishes between continuously and discontinuously manifestable dispositions.
[29] Ryle (1949:117-118) called these dispositions determinate dispositions. Determinate dispositions suggest a proneness to one type of manifestation. (e.g. cigarette smoking)
[30] Ryle (1949:118) called these dispositions determinable dispositions (e.g. greedy)
[31] The example is Huttemann's (1998 : 131)

Some dispositions may never in fact be manifested. (e.g. The fragility of a glass that is never broken). Some *finkish* dispositions may be caused to cease by their own stimulus[32]. Moreover, dispositions can change their behaviour, so a determinate disposition may become a finkish one. The disposition may be prevented from manifestation by an *antidote* (e.g. poison may be disposed to cause death, but if an antidote is taken then the disposition will not manifest itself.[33]) Therefore, in different kinds of dispositions and in different kinds of initial conditions, a disposition might be always manifested, only sometimes manifested or may cease to manifest itself altogether.

4:6 Variant Dispositionalism:

Therefore, a view of dispositions where the causal and nomic powers of the disposition may vary is more consistent with the fact that there are heterogeneous kinds of dispositions. Call this view *Variant Dispositionalism.* With Variant Dispositionalism, since it is not essential that an object possess a certain property, it could just be a contingent fact about the physical system that it does in fact possess a certain dispositional property. Dispositions, on this view, might just be properties of objects or individuals. Moreover, this view is consistent with the claim that the laws of nature are contingent.

One problem then is whether a variable disposition that might be contingently possessed by a system is a plausible candidate to be a truthmaker? If

[32] See Martin (1994).
[33] The example is Bird's (2004)

dispositions are variable, then the semantic problem recurs. In *Variant Dispositionalism* a disposition can change or cease its manifestation, can vary the degree of its manifestation, may never manifest itself and so on. For this reason, the causal role of a given dispositional property is variable. Therefore, the conditional analysis of dispositions would only hold *ceteris paribus* where *cp* stipulates that the dispositional behaviour could vary. It would be formulated as follows:

(21) $(Dx \ \& \ Sx) \rightarrow Mx)\ cp$

The semantic problem with *ceteris paribus* laws was that the *cp* clause was indeterminate and perhaps, in principle incompletable. According to Bird's dispositional view of *cp* laws the semantic problem is no longer a genuine problem for analysis, once *cp* laws are understood as describing stable underlying dispositions. However, if dispositions are to be construed heterogeneously, then the reduction of the law to the disposition becomes more problematic, in that dispositions cannot be provided with a uniform and homogeneous analysis.

So, the truthmaker for the *cp* laws will be parasitic on an explanation of when and why the disposition might vary and when and why it will remain stable. Therefore, the real question of interest regarding this heterogeneous group of dispositions is the degree of stability of the disposition, not only that the disposition is present *tout court*.

If this is correct then any possibility for providing the truthmakers for *cp* laws, will be determined in scientific practice where the variability of dispositions can be examined, by designing a nomological machine or by doing randomised

controlled testing. However, on this account the question of what distinguishes a law from an accident becomes a matter of the degree of the invariance of the underlying disposition anyway. The only thing that distinguishes an accidental one from a putative lawful one is that the latter has a greater degree of stability. So, the traditional law/accident dichotomy, which involves a difference in kind, will be replaced by a continuous spectrum where the difference between the dispositions on the spectrum, is one of degree.

Cartwright (1999 : 59) in fact calls what I am calling variant dispositions here 'capacities'. Disposition terms as they are construed in essentialism are tied one-to one with law-like regularities. Capacities are, however, not tied to any single manifestation. In fact, the most important factor that a capacity possesses is its open-endedness, according to Cartwright. Laws lie literally lie about how reality is in that they construe dispositions homogeneously instead of heterogeneously. According, to Cartwright, when we focus on capacities rather than laws and when we make capacities open-ended and not linked to law-like regularities, then we get a clearer picture of the kind of reality a scientist is faced with in scientific practice.

Therefore, on this account capacities (open-ended dispositions) do all the work that we used to introduce laws to do. They support explanations and predictions (depending on their degree of stability). We can analyse their so-called 'modal force' in terms of the degree of stability of the dispositions. Testing becomes a matter of examining the capacities of a physical system, either across a range of possible initial conditions, or in an ideal experimental setting.

Since dispositions do all the work previously required of laws, it is unclear what laws would be introduced to do. It might be argued that law statements are still useful as heuristic devices that guide scientific practice. However, there is a stronger point to be made here; namely that on this account, the analysis of *cp* laws as the description of underlying homogeneous dispositions, excludes the less stringent, but realistic analysis of variable dispositions that can be provided in scientific practice.

The relationship between laws and dispositions on the dispositional essentialist view describe highly idealised situations. However, in so far as science is interested in providing the truth about reality, it should be interested also in those non-ideal cases, when laws fail to apply and when the behaviour of variable dispositions deviates from then norm. Consequently, a closer analysis of the relationship between laws and dispositions would suggest that philosophers of science are mistaken in thinking that the job of science is solely to discover laws. Interesting and successful science also discovers and aims to describe the complex open-endedness of variable dispositions in practice.

Therefore, what is interesting about *cp* laws is that they are pointing us in the direction of thinking about science in a fundamentally different way. Importantly, *cp* laws do not fit into the stringent theoretical criteria for laws on the *ideal* science model. The reason they do not is because they have *ipso facto* exceptions. However, what we discover in trying to account for these exceptions is that the exceptions to the generalisations are as important in the practice of

science as when the generalisation holds. Any account of science that explains away these exceptions would be doing an injustice to much of successful scientific practice.

So, a closer look at dispositions in scientific practice means that the relationship between variable dispositions and laws is far more complex than the dispositional account of laws allows for. Dispositions are a heterogeneous group that can vary and change in different contexts. Moreover, science is interested in how and why dispositions change and vary as well as how and why they sometimes remain unchanging and relatively stable. Our account of generalisations in the sciences ought to reflect this. For this reason, I contend that laws as they have been described in much of the philosophy of science literature are too uncompromising to represent how science is practiced.

4:7 Conclusion:

At the beginning of this chapter, I explained that different commitments about the modality of laws entail different possibilities for whether *cp* laws might have truthmakers. Broadly speaking, there are two possibilities that I have discussed which stem from an empiricist or realist metaphysics respectively. On the one hand, on an empiricist metaphysics, there is an insurmountable semantic problem that faces *cp* laws. On the other hand, on a realist metaphysics the only plausible candidates that might act as truthmakers are dispositions.

However, a closer analysis of the relationship between dispositions and laws reveals that a looser account is required if our theoretical account must be made to fit with the open-endedness and variability of dispositions in scientific practice. Therefore, even if dispositions did provide truthmakers for *cp* laws, I have argued that this account would not be consistent with scientific practice. Moreover, it would involve a metaphysical assumption about dispositions that cannot be corroborated. Therefore, the uniform analysis of all *cp* generalisations in science in terms of *cp* laws is misleading and ought to be rejected.

In the final chapter, I will examine the *cp* generalisations from a range of different scientific disciplines in order to provide an account of the methodologies of different sciences. In the end, I will claim that despite the negative claims about *cp* laws in the last two chapters, that realism about the special sciences can be corroborated by an account of how the *cp* generalisations in the special sciences differ from the *cp* generalisations in physics.

CHAPTER FIVE: THE JANUS-FACED *CETERIS PARIBUS* CONDITION:

5:1 Introduction:

Philosophers have pointed out in the past few decades that most law statements are in fact *ceteris paribus (henceforth cp),* since they cannot be analysed as strict exceptionless universal conditionals. Fodor (1991) has argued that most of the laws of the so-called special sciences (such as biology, psychology, geology and the medical sciences) only hold *cp*. Kincaid (1996) had argued that the laws of the social sciences (sociology, politics, economics and history) are also *cp* laws. Cartwright (1989) has argued that even the laws of physics are *cp* laws[1]. Indeed, the range of empirical generalisations from different scientific disciplines which all compete for *cp* law status are indeed far-reaching. In Chapters three and four, I provided distinct semantic, epistemic and metaphysical reasons for rejecting *cp* laws. In this chapter, I wish to argue that the uniform analysis of diverse *cp* generalisations *qua cp* laws is misleading in that it homogenises different kinds of generalisations so that important differences in content are not recognised.

As we saw in chapter 3, the standard objection to any construal of *ceteris paribus* laws is a semantic one; namely that we literally do not know what it is

[1] Cartwright (2002: 149) claims that in fact she is not an advocate of *ceteris paribus* laws if they are understood as laws that contain a vague clause that cannot be stated in a closed and precise form as they have been outlined in Earman et al. (2002) This disagreement corroborates my claim that in fact there are different notions of *ceteris paribus* laws being used for rather different purposes in the literature.

that the *cp* appendage is introduced to replace, and so it cannot be given a determinate meaning (Schiffer (1991)). The problem then is that "all else being equal" appears to be a catchall phrase for all the possible exceptional cases that might contravene the law, so, 'All *F*'s are *G*'s *cp*' could be read as 'All *F*'s are *G*'s unless they are not', or 'All *F*'s are *G*'s *iff* All F's are G's' or indeed as 'All *F*'s & {.....} are *G*'s' and thus, the statements that express *cp* laws are vacuous. The fact that there are *ipso facto* counterexamples to any given *cp* law means that there is no prior stable and statable way of expressing what *cp* means in advance of applying it in specific contexts. The *cp* appendage has no determinate content. This is the standard so-called vacuity objection to *cp* laws. In Chapter 4, I argued that there is no substantial metaphysical account of laws of nature that can save *cp* laws from the problem of vacuity either.

There is however, I contend another problem that faces *cp* laws, which I wish to call the *heterogeneity* problem. Candidates for *cp* laws function in very different ways in different disciplines, so that the content of these generalisations is rather dissimilar. The difficulty is that all of these generalisations are given a single analysis *qua cp* laws. I propose to examine the different uses of *cp* generalisations across different scientific disciplines, with the interesting consequence that this heterogeneous group of *cp* generalisations cannot be provided with a single truth-conditional analysis, without idealising away from important differences in the content of these generalisations.

In section **5:3**, I will examine different uses of the *cp* clause across different generalisations, which I call the *heterogeneity* problem. In Section **5:4**, I

distinguish between two different uses of the term *cp*, which matter for providing a semantic analysis: the *idealized* and *contextual cp* law. In Section **5:5**, I analyse how these two types of *cp* law face the semantic worry. In Section **5:6**, I argue that this distinction provides a new way of understanding the claim that it is "*cp* all the way down". In **5.8**, I argue that the disunity of the sciences is corroborated by the claim that in scientific practice, the *cp* generalisations of the special sciences function in an entirely different way to the *cp* generalisations of physics.

In Chapter 3, I proposed a dilemma argument against *cp* laws along the following lines: either it is *cp* "all the way down" (i.e. that even the laws of physics are *cp*), or it isn't. If it is, then many accounts of *cp* laws self-destruct; if it isn't then one's account of *cp* laws unattractively commits to reductionism. In this chapter, I will argue that the claim that it is "*cp* all the way down" has in fact been grossly misunderstood because of an equivocation of two distinct types of *cp* laws. Consequently, the claim that it is "*cp* all the way down" may not lead to reductionism and the subsequent rejection of *cp* laws.

5:2 The Meanings of *Ceteris Paribus*:

I have mentioned in earlier chapters that the Latin term *ceteris paribus* which originated in the 17th century can be interpreted in many ways. In English, the standard translation is as 'all other things being equal". However, it may also be translated as "all other things being constant", or "operating on the ideal assumption that nothing else changes" or "if nothing else interferes" or "all other things being right".

These differences in translation are interesting in that they underline the different uses and applications of *ceteris paribus*. Even in translating the phrase we see that *cp* is a rather vague catchall phrase. I contend that these differences warrant some closer investigation. In fact, a failure to significantly distinguish between different kinds of *cp* clauses may be the source of the semantic worries that *cp* laws are supposed to face. In particular, it is important to question whether differences in the application of the *ceteris paribus* clause in different generalisations and across different disciplines involve any real differences in meaning and content.

5:3 The Heterogeneity Problem:

A glimpse at some of the wide range of *cp* generalisations makes it clear that the *cp* generalisations that are candidates for *cp* laws are a wildly heterogeneous collection. Yet, in the literature, they are all treated the same; namely as universally quantified conditionals qualified by the *ceteris paribus* proviso[2].

The heterogeneity problem might be considered as a more general methodological problem with all treatments of laws of nature, in that a heterogeneous collection of generalisations in science are compressed into a uniform analysis of laws. We saw in earlier chapters, that much of the literature on laws has focused on the normative question regarding the features a generalisation *ought* to have to be considered a genuine law. Mitchell (1997 :

[2] The fact that such a diverse species of generalisations should not be given a singular analysis has been pointed out before (Schurz (2002), Woodward (2002)).

469) calls this approach to laws in the particular case of biology "the normative approach". She states:

> The normative approach is the most familiar one begins with a norm or *definition* of lawfulness and then each candidate generalization in biology is reviewed to see if the specified conditions are met. If yes, then there are laws in biology, if no, then there are not laws in biology. (Mitchell : 1997 : 469).

In other words, there is an assumption in the laws literature that one size fits all. A theoretical account of what laws *ought* to be is provided on the normative approach and then all generalisations are squeezed into this uniform analysis. The upshot is that any generalisation that does not fulfil these rigid criteria is deemed unlawful. I suggest that this methodology is wrong-headed. I particular, I propose that we move in the opposite direction, from an analysis of the various generalisations in science, to the more general question of why it is we consider these generalisations to be lawful.

Let's take a closer look at some of the *cp* generalisations that compete for law status. Some generalisations are specifically causal, that is to say that they isolate a sole cause as the factor that brings about an effect 'all else being equal'. In other words, the cause will bring about the effect, if certain other unspecified circumstances are present. Woodward's (2002 : 307) example from oncology is:

> (1) "Administration of chemotherapy drugs to human beings of tumour type *A* causes recovery *R cp*".

The *cp* clause here stipulates an indefinite set of factors the presence or absence of which will influence tumour remission, such as metabolism, age, general health and immunity, past medical history and so on. The *cp* in this generalisation is replacing two kinds of factors those that may interfere and impede the

generalisation and those that must be in place in order for the generalisation to hold. These further factors can and do influence tumour remission and impede or enhance the chances of recovery. So, *cp* here might be translated as "if nothing else interferes" and "if all else is right".

Still other generalisations considered as *ceteris paribus* laws in the literature describe some regularity or trend that holds in usual circumstances. For example, take Farr's law in epidemiology, which states that:

(2) Epidemics increase their incidence rates exponentially *cp*.

This law describes a trend exhibited in the spread of epidemics. It is non-causal in so far as it is not interested in how the epidemic is caused, but only in the tendency of epidemics to increase their incident rates exponentially. We can see that it is of a different kind to the generalisation in (1) above, since it is the regularity in the spread of epidemics that is of interest, rather than whatever causal factor (e.g. the disease pathogen) is responsible for the disease. There are however several environmental factors that might prevent an epidemic from increasing exponentially. For example, environmental features such as, whether there is a vaccination, whether infected patients are placed in quarantine, whether immunity is even possible and what level of human contact is required for the spread of the disease (e.g. if it airborne then it will spread rapidly) all influence whether or not a given epidemic will develop exponentially.[3] In this generalisation, the *cp* clause is used to indicate that there may be interfering contextual factors.

[3] See Cairns, A.J.G. (1990) 'Epidemics in heterogeneous populations II : non-exponetial incubation periods and variable infectiousness', *IMA Journal of Mathematics Applied in Medicine and Biology*, 7 : 219-230.

We can see then thus far that although the generalisations in (1) and (2) are of different kinds they do use the *cp* clause in analogous ways; namely to indicate the presence or absence of factors that *would* influence whether or not the generalisation holds in a given context. Therefore, there is no interesting difference in the content of their *cp* clauses.

The generalisations of the social sciences involve similar use of the *cp* proviso. Indeed, Kincaid (1996) has argued that these generalisations ought to be construed as *cp* laws too. For example, take the law of supply and demand in economics, which states that:

(3) "An increase in prices leads to a decrease in demand, *cp*."

Again, there are several factors that may intervene and consequently, that can prevent this from happening. Take for example a limited resource in a market economy. Imagine an example, like "An increase in the price of bananas leads to a decrease in the demand for them *cp*." However, there are again environmental factors that may contravene the law from holding. Recent research has shown that bananas may in fact go extinct in the next 10 years if scientists cannot develop new varieties that are resistant to blight. So, in fact, an increase in prices may not lead to a decrease in demand because the further factor (i.e. knowledge that bananas will go extinct in 10 years) will sustain demand for the product.

In this example, there is a similar appeal to the contextual factors that may impede or facilitate the generalisation, but those contextual factors that matter may be considered to be of a different kind. In this example, there is an appeal to the folk psychological human beliefs; norms and practices that may motivate

human actions and so interfere with the trend described by the generalisation (i.e. the belief that bananas may go extinct will lead people to desire them and so demand may increase despite the increase in prices).

In the social sciences, there is an appeal to human psychology and also to sociological norms and practices that influence human behaviour, whereas it is thought that the interfering factors in the natural sciences are natural features of the environment. For example, in oncology (1) above the age, immunity, general health etc. of the patient will influence whether or not the tumour goes into remission. However, folk psychological factors do not count as explanatory factors in explaining the effects of chemotherapy on tumour remission, since the patient's desires, beliefs and fears clearly cannot influence recovery. It might be argued that this marks a difference in kind between the generalisations that we find in the social sciences as opposed to those that we find in the natural sciences, which indicates that these generalisations should not compete for law status at all[4].

However, it is not clear whether this crude distinction between "natural" as opposed to "human"/"psychological" factors should be made. A thorough examination of the context or environmental setting where laws in the special sciences are applied will also take into account interfering factors of a psychological/sociological kind. Take Farr's law (2) above again, this law examines the spread of epidemics. However, a complete story of why an epidemic spreads or not will involve cashing out the complex causal relations between the

[4] Schutz (1954) argues for a difference in kind between the generalisations of the social sciences and those that we find in the natural sciences.

human agents where the disease propagates and spreads. Some of this story will involve a reference to human norms, practices, and folk psychology.

Equally, the specific spread of a pathogen at a particular historical time may also be the result of historical factors specific to that time in history. For example, one of the primary causes of the Spanish flu epidemic in 1918 was World War 1, where the disease pathogen spread rapidly in the trenches. Since human behaviour and interaction is notoriously hard to predict and explain then it is claimed that it would be impossible to decipher every possible exception to a given *cp* generalisation and so *cp* clauses replace an indeterminate collection of possible interfering factors.

Whether or not there is considered to be a difference in kind between the generalisations of the natural sciences as opposed to those we find in the social sciences, the important question for the purposes of our discussion is whether the *cp* proviso functions in a different way in these generalisations. In particular, is there a difference of kind in the way that the *cp* proviso functions in social science generalisations as opposed to those in the special sciences? There are two reasons why it might be argued that they are different; firstly as discussed above the "interfering factors" may include a reference to the practices, norms, beliefs and so on of human beings. Secondly, the interfering factors may be relative to a specific spatio-temporal context.

However, I would argue that neither of these two claims constitute a difference of kind in the content of *cp* claims. It can be argued that the *cp* proviso functions in the same way in these social science generalisations as it does in (1)

and (2) above. In all the examples discussed, thus far, from both the special sciences and the social sciences, the *cp* proviso indicates that there may be further *contextual factors* that will impede or facilitate the regularity described by the generalisation. In the social sciences, the task of cashing our precisely what these contextual factors are may be a more challenging one, than in some of the special sciences, since in the social sciences, these factors are ever changing and are often relative to specific spatio-temporal domains and individuals. For this reason, the difference between the *cp* clauses in both cases appears to be a difference in degree rather than of kind.

There are also examples from the natural sciences of cases where the contextual factors referred to do indeed refer to spatio-temporal domains. In fact, most of the generalisations that we encounter in biology are relative to the setting of a contingent spatio-temporal domain. For example, take Bergmann's Law in Biology, which describes the size differences in populations of species of animals in restricted climactic conditions. It states that:

> 4) For species of warm-blooded vertebrates, races living in cooler climates are larger than races living in warmer climates.

Again, there are exceptional cases where Bergmann's law will not apply. For example, a number of animals dig burrows, such as groundhogs, foxes, chipmunks, and moles. The body size of these animals does not alter in cooler climates, as they do not need the higher body mass-to-surface area ratio that favours heat retention. They adapt their environment to do the work for them, by building burrows. So, the law is contravened by an environmental adaptation or intervention. The *cp* clause in these kinds of generalisations is replacing

something indeterminate in advance of applying the generalisation to particular cases. The function of the *cp* clause then is to indicate that the context dictates whether the generalisation holds or whether there may be exceptional cases.

Other generalisations, describe a regularity that holds comparatively between two variables, where an increase (decrease) in one property *x* will lead to an increase (decrease) in another property *y*. An example is Gay-Lussac's gas law, which states that:

> (5) An increase of gas temperature leads to a proportional increase of gas volume *cp*.

This type of generalisation describes the relationship between two functional variables, once underlying variables remain constant. The *cp* clause in (5) ought to be read as "all else being constant". The "all else" in this phrase refers to all those other contextual factors that might impede the law from holding and whose absence will aid the generalisation. So, although again we have a generalisation that at face value works rather differently to those generalisations (1-4) discussed above, but where nevertheless, there is no interesting difference in the content of *cp* clause.

Other generalisations that are also taken to be *ceteris paribus* laws are those that delimit a certain describable cause or set of causes and assert that these will bring about an effect if the process occurs in an idealised setting. Therefore, the law makes a claim about the effects these causes would bring about in isolation or given the proviso that there are no other interfering causes. Take Ohm's law in electronic engineering, which states that:

(6) At a constant temperature, the ratio of the potential difference (voltage) across a resistor to the current flowing in the resistor is a constant.

Most conductors obey Ohm's law provided that their temperatures don't change. For most conductors, resistance is a rather stable property, largely unaffected by voltage or current. For this reason, we can regard the resistance of most circuit components as a constant. However, the law is only strictly speaking true for ideal resistors (where an ideal resistor is understood as one whose resistance does not vary with a change in voltage). This is because some conductors can exhibit non-ohmic conduction even if there is no change in temperature. (e.g. In some conductors, the ratio of the voltage across a resistor to the current flowing in the resistor is not always the same because it changes very slightly as the voltage is increased, even if temperature remains constant.)

In fact, ideal resistors are rarely (if ever) found in the real world. Ohm's Law is an idealisation and most real-life resistors only approximate the regularity described by Ohm's law. This is because in real life situations there are many other causal factors that might intervene to contravene the law from holding. Therefore, Ohm's law operates on the idealised assumption that causal processes occur in isolation. However, this is rarely true in practice, as real materials can have several causes operating at once. So, Ohm's law only states what *would* happen were the setting ideal. So, the *cp* clause in this kind of generalisation indicates that it would only hold in an ideal setting. Therefore, the content of the *cp* clause is rather different in this latter kind of *cp* generalisation. The function of the *cp* proviso in this kind of generalisation is to indicate that the generalisation is an idealisation.

A similar example is Snell's Law[5] in optics, which states that:

> (7) At an interface between dielectric media, there is a refracted ray in the second medium, lying in the plane of incidence making an angle θ_t with the normal such that: $Sin\ \theta/\theta_t = n_2/n_1\ cp.$

So why might we say that Snell's law requires a *cp* proviso? Snell's law is only true for isotropic media (i.e. where only one light wave is transmitted). The problem is that in most real life cases media are optically anisotropic (i.e. there are two waves transmitted.) So, it is true to say that Snell's law only describes what would happen were the setting ideal (i.e. in cases of isotropic media)[6].

Thus far, it is evident that the kinds of generalisations that are being given a single analysis in terms of *cp* laws are a rather heterogeneous group[7]. In this section, I have identified at least five different kinds of *cp* generalisations that involve different kinds of claims: causal, non-causal, functional, idealised and restricted spatio-temporal claims. It is important to point out that the generalisations competing for *cp* law status function quite differently. Nevertheless, the latter claim is only significant for our discussion, if the *cp* clause has a different content in these generalisations; namely if the *cp* clause is introduced to mean different things. I wish to claim that in fact there is an important difference in content to be considered.

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[5] The example is Cartwright's (1983 : 46-47).

[6] Another example is Boyle's Gas Law, which states that "For a fixed amount of gas and at a fixed temperature, the product of the pressure and volume for a gas is a constant." In reality, this law is only strictly true for infinitely low pressures. Standard pressures lead to a reasonable approximation for most gases, but it is an approximation nevertheless.

[7] The heterogeneity problem might be construed as a more general problem about the language we use in science. (Mumford (2004) : 129) points out that in fact there appears to be no consensus about why some statements in physics are considered to be laws and yet why other or not. For example, there are principles (e.g Heisenberg's Uncertainty Principle), rules (e.g. Hund's), equations (e.g. Einstein's $E = mc^2$) hypotheses (e.g. ergodic) and theorem's (e.g. Goldstone's). So, there is also a hetergoneous group of generalisations in physics that are segregated from genuine laws without thorough explanation of what constitutes the difference.

5:4 The *cp* proviso: Contextualisation or Idealisation.

I wish to argue that different uses of the term *cp* as it pertains to such diverse kinds of generalisation has led to an equivocation in terms of what *cp* is introduced to mean in different generalisations. Moreover, I contend that two of the standard uses of *cp* have quite a different content. It is a worthwhile task to pull apart the different semantic notions involved in the uses of *cp* in *cp* generalisations before we attempt to give one semantic analysis of *cp* laws and treat all kinds of generalisations homogeneously. If there really are important differences in the content of these claims then the semantic problem that has fuelled the literature on *cp* laws may be the upshot of a failure to make this distinction before attempting to provide a semantic analysis. I contend that there are two different conceptions of the *cp* proviso that matter in providing a semantic analysis.

5:4:1 *Contextual Cp* Laws

The first notion involves the idea that certain variables are either included, excluded or held constant in order for the law to hold *ceteris paribus.* Adding the *cp* clause (conceived in this way) specifically stipulates that the context in which the regularity of the putative law finds itself will affect whether or not it holds. Conceived in this way, we should think of *cp* generalisations as *contextual cp laws*. Put more formally, we might say that:

> (8) "All *F*'s are *G*'s *cp* is a *contextual cp* law just in case all *F*'s in a particular context *C* (C = the domain where certain variables are included, excluded or held constant) are G.

Take for example, Farr's law in epidemiology (2) above, which states that "Epidemics must increase their incidence rates exponentially *ceteris paribus*". In other words, Farr's law predicts that an epidemic will rise exponentially within a few months in a population and then decline within weeks or months and will have a bell curve shape. The rise reflects the exponential spread of the bacterium and the fall reflects the resulting natural immunity of survivors (Bregmann & Langmuir (1990)). An example of the spread of an epidemic exponentially is the influenza virus.

However, what the *cp* appendage refers to in this generalisation is highly context specific. There are many contextual factors that might effect whether or not the law will hold in a given situation. The spread of the Aids virus provides a good example. Here we have non-exponential growth and the context dictates when and where it either spreads or fails to. For example, Aids spreads more rapidly in communities of drug users where needles are shared. Importantly, Aids would spread exponentially 'all else being equal' that is to say if the correct contextual factors were present or absent. In examples like these, we can see that the further contextual situation where the virus propagates does count for scientific explanation. So, the *cp* appendage in Farr's law is introduced to leave open the empirical possibility of exceptional cases given the presence or absence of certain variables.

Another counterexample to the exponential spread of epidemics is the modern epidemic of smoking related illnesses. For example, modern epidemics of lung cancer, emphysema and other smoking related illnesses have risen steadily (non-exponentially) since the 1950s and have lasted now for over 50 years without evidence for immunity. It is now slowly coming down as smoking declines (Greenlee *et al.* (2000)). It did not spread randomly but was restricted to smokers and passive smokers who were in contact with toxins from cigarette smoking. Moreover, there was not an exponential rise and consequent decline but a steady linear increase since no natural immunity could develop. So, it does not have the usual bell curve exponential growth and subsequent decline that we associate with epidemics in accordance with Farr's law.

What is important in laws of this kind is that *cp* specifically stipulates that the generalisation has an interest in the further contextual variables (known or unknown), which contravene the law from holding. Conceiving of *cp* laws in the contextual way also accommodates causal generalisations like (1) above, which shows the effects of certain chemotherapy drugs on tumour remission. Again, we have a situation where whatever the context is (i.e. the patients past medical history and present clinical condition) it will count. It also accommodates spatio-temporally limited laws like (3) and (4) above, where the generalisation describes a contingent spatio-temporal domain (e.g. The price of a bananas and the corresponding supply/demand curve in the years 2000-2010, or warm-blooded vertebrates in cold climates). The fear of the extinction of bananas may lead to an increase in demand despite the increase in prices and so the law of supply and

demand (3) will be contravened. Some animals will adapt by manipulating their environment and so Bergmann's law (4) might be contravened[8].

5:4:2 *Idealised* Cp Laws:

However, there is a second conception of *cp* that I would argue involves a quite different claim. This involves what would happen were the regularity specified between the antecedent and consequent to happen in isolation. The *cp* proviso here is specifically stipulating the lack of context (or that the only context is the regularity described by the law). This conception should make us think of *cp* generalisations as *idealised cp* laws. This accommodates generalisations like (6) above which states that "At a constant temperature, the ratio of the potential difference across a resistor to the current flowing in the resistor is a constant." This law is true once it happens in isolation and given that there are no other causal factors that may make the resistor non-ideal (e.g. that there are no inductive effects[9]). Moreover, *cp* in this instance is specifically stipulating "in idealised circumstances" or "in an experimental setting". Idealised *cp* laws can be formulated in the following way:

> (9) "All *F*'s are *G*'s *cp*" is an *idealised cp* law just in case all *F*'s would be *G* if there were no other properties other than *F* & *G*[10].

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[8] In this kind of narrow scope generalisation the *cp* clause is also making the larger claim that the context can evolve slowly over time.

[9] The electric current in the wire may create a magnetic field and the field may influence the voltage, which may result in non-linear conduction.

[10] It is important for any semantic analysis that we get clear on what we take the *cp* proviso to mean; or moreover what we introduce the *cp* clause to do. Do we want to include *cp* generalisations conceived in both ways or only in one of these ways? Moreover, do these conceptions reflect a difference in the way these laws function in different sciences? I will return to these issues when I address how both conceptions face the second semantic problem.

Another example of an *idealised cp* law is Snell's Law in optics (7) above. This law is strictly speaking true in an ideal setting or where the media are isotropic. However, this is at the expense of how it applies in practice where most media are in fact anisotropic. The law states, in other words, what would happen in an idealised or laboratory setting, or indeed in some possible world where the setting were *ideal*. Ideal circumstances make simplification assumptions that screen off everything else, that is to say any possible variable other than those explicitly stated in the conditional are excluded. Importantly, the idealised *cp* law is only interested in the relationship between the variables expressed explicitly in the generalisation and how they operate in isolation.

The fact that there are some rather different notions of *cp* at work in different kinds of generalisations has been recognised before. Schurz (2002) made the distinction between what he termed *comparative cp* laws as opposed to *exclusive cp* laws. *Comparative cp* laws are those that make an assertion about functional properties, where an increase (decrease) in one property x will lead to an increase (decrease) in another property y (where generally the quantity of one property is a function of another property). Schurz (2002) argues that a comparative *cp* law "compares the states of two systems of an underlying application class, one state where the parameter f has been increased - and it requires both states to agree on all parameters which are causally independent from f." (Schurz (2002): 352) This accommodates generalisations like Lussac's gas law (5 above), where two properties are related in a proportional way. *Ceteris paribus* here means "all other things being equal or held constant".

In contrast, *exclusive cp* laws state that "a certain event or state-type (expressed by a single or complex predicate) will bring about another state or event–type expressed by a (possibly complex) predicate *Ax* leads to another state or event-type *Cx*, provided disturbing influences are absent" (Schurz (2002): 352). In other words, *ceteris paribus* ought to be understood as "in the absence of disturbing factors" or in terms of Joseph (1980) characterisation as '*ceteris absentibus*'. This is the most accepted construal of *cp* laws.

The confusion of exclusive and comparative kinds of *cp* law has, according to Schurz (2002), has come about because of some *cp* laws that combine both comparative and exclusive conceptions, which he calls *mixed cp* laws. Schurz uses the following example:

(10) An increase in demand leads to an increase of prices, *cp*.

The comparative conception of the latter law is the proportional relationship between demand and the increase in prices. So, not only must the two economies agree in remainder factors but also, there is a *ceteris absentibus* (or exclusion) understanding, in the sense that this will only hold true in the absence of other interfering factors, such as the demand for other competing goods etc.

While the distinctions between comparative, exclusion and mixed *cp* laws are important, in terms of how the generalisations referred to use the *cp* appendage, I wish to argue that because of the way Schurz classifies them, that the content of the *cp* condition would be the same and so they would require similar truth conditions. I agree that there are important distinctions to be made in what work the additional factors are doing in the law. So, we ought to distinguish

between laws where the presence of additional factors is required as opposed to those where the absence of additional factors is required and as opposed to laws where the additional factors must be present and kept constant. However, despite these distinctions, I maintain that they can be considered as examples of the same type, because they *all* stipulate that, whatever the further context is, that it is relevant to the generalisation. Therefore, broadly speaking the *cp* clause refers to the further context in *all* of these generalisations.

The *cp* clause is introduced to do the same work in these different generalisations, in so far as what the *cp* clause refers to is the variables (present, absent or held constant) that make up the context. What is important is that the law stipulates that there are further contextual variables that *do* count for whether or not these generalisations hold. The proviso then is indicating that we need to account for the context (whatever it might be) in scientific practice. For this reason, what the *cp* clause is doing is the same in all these types of generalisation, in so far as it stipulates a further context. All of these types of *cp* law involve the contextual conception of *cp* laws.

It might be argued that *idealised cp* laws, as I have described them, are similar to Schurz's exclusive *cp* laws. This is to claim that the only work an idealised *cp* law is doing is expressing "the absence of disturbing factors". However, I think that Schurz's conception of exclusive *cp* laws is misleading. He says that *exclusive cp* laws state that a certain event or state type (expressed by a single or complex predicate) will bring about another state or event type, provided other disturbing influences are absent. This construal specifically stipulates that

there are other variables (i.e. disturbing influences) that if they were to be present would contravene the law.

However, my construal of some *cp* laws as *idealised cp* laws is different. It makes a far more limited claim, namely that the generalisation would hold were it to happen in isolation. Importantly, it makes *no* claim about what would happen if the *cp* law were to be applied in a context. This is importantly different to the contextual conception because *cp* literally means contrary things in the two conceptions. *Cp* in contextual *cp* laws stipulates that the context will count, whereas the *cp* in the idealised *cp* laws stipulates that were there no context other than the one described explicitly in the generalisation then the generalisation would hold.

5:4:3 The Application of *Idealised Cp* laws:

Now one potential problem with *idealised cp* laws, as I have construed them, is that the notion of idealisation that I am incorporating is too nihilistic to capture even strong idealisations in science. In other words, is a setting ever so ideal that there are never any further properties (other than those specified in the generalisation)? A weaker idealisation claim might construe idealised *cp* laws in the following way:

> (11) "All *F*'s are *G*'s *cp*" is an idealised *cp* law just in case all *F*'s would be *G* if either there were no other properties than *F* & *G or* any further property *H* is ideal for *F* & *G*.

However, this weaker idealisation claim yields another possible objection to the distinction which asks why the idealised *cp* law cannot be specified as a particular kind of *contextual cp* law. Is (11) not just a *contextual cp* law where the context is ideal? Even in the stronger idealisation case (e.g. (9) above), it seems that my distinction is open to the same kind of objection; namely why should we not say that an *idealised cp* law just is a *contextual cp* law where we specify a context without other properties?

This makes it somewhat clear that the two notions are very closely related. Nevertheless, even if I were to allow a weaker notion of idealisation so that the distinction is no longer as stark, there is still an important distinction between these two cases in terms of how they are applied in scientific practice. This distinction can be seen in the following way; namely in terms of how the two types of laws apply in scientific practice, in particular with regard to the relevance of further contextual factors.

In fact, this point is key to understanding the distinction. My *idealized cp* laws are similar to Hempel's (1988) provisos. Hempel's discussion of provisos is couched in the old-fashioned "received view" of scientific theories, as has been rightly pointed out by Earman & Roberts (1999 : 442). According to the received view of theories, a given theory T is a set of sentences, which formulate the laws of the theory along with correspondence rules, which relate terms in the observational vocabulary with terms in the theoretical vocabulary. Traditionally, Hempel has been read as claiming that all of the sentences of a theory which are taken to express laws require a *cp* proviso and so it is "*cp* all the way down" to

physics[11]. So, all the laws of all our present theories in all of the sciences are *ceteris paribus*.

Earman & Roberts (1999) claim that Hempel did not mean to construe his provisos as *cp* laws and that indeed it is misleading to read Hempel as arguing that it is "*cp* all the way down". They claim that "Hempel makes it explicit that his provisos are clauses that must be attached to applications of the theory rather than to law statements" (Earman & Roberts (1999 : 444). Earman & Roberts distinguish between Hempel's notion of the proviso and the notion of the proviso evident in theorists like Lange (1993a) where the *cp* proviso replaces a long list of qualifer clauses without which the law would be strictly speaking false. We can see that there seem to be two distinct notions of *ceteris paribus* at work here.

In fact, I maintain that it is the confusion of Hempel's notion of the proviso with the *ceteris paribus* clause understood in Lange's sense that has led to the equivocation of putative *idealized* and *contextual cp* laws in the first place. Hempel (1988 : 29) clearly wishes to distinguish his notion of the proviso from the *ceteris paribus* clause understood in Lange's sense. He states:

> To say that provisos are just *cp* clauses is unhelpful, for the idea of *ceteris paribus* is itself vague and elusive. [.....] Provisos do not call for the equality of certain things but for the absence of disturbing factors.
> Provisos might rather be viewed as *assumptions of completeness*. The proviso required for a theoretical inference from one sentence S^1 to another S^2 asserts broadly speaking that in a given case no other factors other than those specified in S^1 are present that could affect the event described in effect S^2. (Hempel (1988) : 29)

[11] See Fodor (1991 : 21) and Giere (1999 : 90-91).

So what precisely does Hempel have in mind by his notion of the proviso? Firstly, Hempel motivates the notion that theories are always idealizations and that there is a problem in applying theoretical theories to reality in scientific practice. Hempel's (1988: 23) example is the application of the Newtonian Theory of Gravitation and of motion to a system of physical bodies (e.g. the planets in the solar system). In predicting changes in the state of a system from a previous state of the system at a time t_1, the force acting on any one of the planets will be the vector sum of the forces exerted on it by the other bodies in accordance with the law of gravitation. The force then will determine the velocity and position of the planet by the second law of motion ($f=ma$).

The "proviso", according to Hempel, is that the application of the theory presupposes that there are *no* other forces at play other than the forces of mutual gravitation. In other words, that the *total* forces is equal to the sum of the gravitational forces. The proviso precludes external gravitational forces, as well as frictional, electrical, or magnetic forces to which the bodies in the system might be subject. The notion of *ceteris paribus* that I advocate in the idealized *cp* law can be clarified by identifying it with Hempel's proviso. The *cp* provisos in *idealised* laws, given this reading, are just a particular case of the provisos that we have to add in applying the entire theory to reality. So, it is not so much that all laws are *ceteris paribus* (i.e. that it is *cp* all the way down) but that all laws are idealizations.

I would construe both the law of gravitation and the law of motion as idealised *cp* laws. So, why are these laws different to the contextual case you might ask? Couldn't we construe the law of motion as a *contextual cp* law, which

is different in different contexts, for example in a context where there are both gravitational and frictional forces? I claim not. The reason why this case is different to other contextual *cp* laws as discussed above (e.g. Bergmann's law and Farr's Law) is because the law of motion is supposed to take into account the *total* force that affects the physical object. So, a correct calculation of the velocity and position of the other object will involve the sum of *all* the forces. The *cp* proviso does not say that there are exceptional cases where the total force is *not* mass by acceleration (i.e. an exception to the law) but rather that when we apply the law there may be other forces, which also need to be calculated.

The *cp* proviso in an *idealised cp* law is an assumption of completeness where the only variables allowed in the context are those variables already outlined in the conditional law statement. The important point here is that we can specify the context, because the context is just an instance of the law, or putting it another way the context is ideal for the applicability of the law. So, there is *no* difference between the ideal specification of the law in advance of applying it and its actual application in isolation (e.g. an idealised setting). Moreover, when the law is applied in different contexts, there are no exceptions where the law does not hold, but rather where further calculations may need to be made in accordance with the law.

5:4:4 *Contextual Cp* Laws & Genuine Exceptional Cases:

Clearly, the idealisation case is very different to the contextual one, as I have construed it where the *cp* proviso is standing for the context that may contravene the law. However, different instances of the law can happen in very different contexts. Moreover, actual exceptions to the law can occur because of

interfering factors in the environment. Take, Bergmann's Law in biology, (i.e. (4) above) here we have an instance where something happens in the environmental context (namely some vertebrates build burrows) and the law does not apply because warm-blooded vertebrates literally are not bigger than their counterparts in warmer climates. Moreover, we are interested not just in instances where the law holds but indeed in contexts where it breaks down. The context dictates genuine exceptions to the law.

The reason why I call this a "*genuine exception*" to the law is because we have actual cases of warm-blooded vertebrates that are literally not bigger in cooler climates. It is similarly the case for other contextual *cp* laws that we have looked at. We can imagine genuine exceptional cases to Farr's Law where there is a non-exponential spread of epidemics (e.g. polio & aids). Similarly, in oncology, we can and unfortunately do, encounter patients who have chemotherapy tumour treatment and yet who fail to go into remission. These cases are genuine contextual exceptions to *contextual cp* laws where the context is constitutive of whether a generalisation will be applicable or not.

In contrast, there are no genuine exceptions to *idealised cp* laws. Rather, there are non-ideal cases where the law may not apply. However, *idealised cp* laws never claim to apply in all cases, but rather only in ideal ones. There are no genuine cases where Snell's law is false (i.e. where there are isotropic media whose light rays do not behave in the way that is indicated by the law). Snell's law does not apply to anisotropic media (where there are two transmitted waves) because the setting is non-ideal (there are two waves transmitted instead of one). Similarly, for Newton's second law of motion we never see actual cases where

force ≠ mass by acceleration, rather we have cases where the law does not apply because there are more forces than gravitational ones at play.

In summary then, we can see that the *cp* clause in *idealized cp* laws is performing an entirely different function to its role in *contextual cp* laws. In *idealised cp* laws, the *cp* clause plays the role of indicating that the relationship described in the law is an idealization. The law may not apply in other circumstances. In *idealised cp* laws the context is *ideal* and specifiable. Moreover, it is explicitly stated in the law. In fact, it is making a claim about what would happen were the setting to be ideal i.e. were there to be no further context other than the relationship described by the generalisation. In contrast, the *cp* clause in a *contextual cp* law is standing in for the context i.e the absence, presence or constancy of which may contravene the law. This context cannot be explicitly stated in advance of applying the law in particular contexts.

I have set down two notions of *ceteris paribus* that require a preliminary semantic analysis. I will now analyse how these two types of *cp* generalisations handle the semantic worry that I outlined in Chapter 3. It will be interesting to look at how the two conceptions of *cp* laws outlined above will fare when subjected to this semantic worry or indeed to analyse whether the semantic worry is the upshot of a failing in the literature to correctly distinguish between these two very different kinds of *cp* laws.

5:5 Contextual & Idealized *cp* laws & The Semantic Problem

The difference in the content of *idealised* and *contextual cp* laws is highlighted by the fact that the two conceptions should have rather different truth conditions since what *cp* means (or indeed what it replaces) is quite different in the two types of generalisation. So, the equivocation of these two conceptions of *cp* may be the source of the original semantic problem and separating out these two notions may prove useful in tackling that original semantic worry.

To reiterate, the original semantic worry that faces *cp* laws *tout court* is that any attempt to spell out their truth conditions and make the claims in *cp* laws determinate renders them tautologous and so vacuous. Moreover, it is thought that since, the content of *cp* clauses is indeterminate, elusive and vague then they are not good candidates for empirical laws. The suggestion then would be that the reason why straightforward truth conditions cannot be applied to *cp* laws is because there are different types of *cp* laws, which require different truth conditions. The aspiration is that on recognising equivocation(s) in the *cp* clause some room may be made to tackle our original semantic problem.

5:5:1 Contextual *Cp* Laws and the Semantic Problem:

Take *contextual cp* laws first and foremost. What makes the *contextual cp* law true *cp* are *a posteriori* facts about the variables given information about further variables that must be either included or excluded. We might know what these variables are in advance of applying the law to specific circumstances, in

which case the *cp* clause is in principle eliminable in favour of the variables that make all else equal. Equally, however, we may only know which variables count in a given context. Indeed, it may even remain indeterminate which factors might combine to contravene the law or to uphold it. Therefore, what the contextual *cp* law is stipulating is that it would hold (or that a cause or certain delimited set of causes will bring about an effect), if certain other variables, not specified in the generalisation are present or absent. These *cp* laws do not have the form of determinate generalisations, because what the *cp* clause replaces is highly context-dependent.

Moreover, any specification of the context must remain an approximation that is open-ended. Contexts can and do vary and so the *contextual cp* law must remain indeterminate in advance of its application to different contexts. Moreover, since contexts are so variable, the context that the *cp* clause replaces in one situation does not let us project to future occasions where we would imagine that the context would be the same. Contexts can and often do surprise us.

Ideally however, the *cp* proviso could be replaced by these variables in scientific practice. Importantly, the generalisation is interested in these variables, whether they are cited explicitly in the generalisation or not. So, allowing a certain degree of vagueness increases the chances of a more semantically rich description of *cp* laws in scientific practice. There may be no prior strict account of the truth conditions but once there is some methodology for semantically evaluating these claims in practice, then the use of contextual *cp* laws is methodologically justified. Lange (2002) has argued that there is no problem in

principle with proceeding in this way since there is tacit agreement in terms of what constitutes a counterexample to a putative *cp* law or not in scientific practice.

However, despite the aspiration that the larger semantic worry may be overcome there is a further methodological problem. Firstly, it is important to query whether the context is delineable in scientific practice to the degree that exceptional cases are not accommodated in an *ad hoc* way. The problem is that with contextual *cp* laws, the context must always play a part in testing the applicability of the law. Since, we cannot spell out the context in advance of applying the law, then the truth conditions of the law will only ever be spelled out in scientific practice.

In fact, it seems trivial to point out that contextual *cp* laws have no truth conditions in advance of applying them to any context, since by their very nature they always refer to a context. So, the worry that faces *contextual cp* laws is not really a semantic one, but a methodological one about whether the scientist can test the law in a non *ad hoc* way. Since, there is no prior statable version of *contextual cp* laws, then how can we be sure that the scientist will not explain away exceptional cases that ought to be understood as genuine anomalies, in order to retain the legitimacy of the law.

There is a further methodological difficulty that I claim would arise in any case, even if we could in fact find an answer to our methodological problem. Indeed, Woodward (2002) and Mitchell (2002) claim that *contextual cp*

generalisations ought not to be construed as laws at all. In fact, Mitchell (2002 : 339) claims that the laws metaphor is quite misleading, since it focuses only on when generalisations are successful and so apply, when in fact a proper analysis of these generalisations should also study the exceptional cases where the generalisations do not apply. She states:

> It is not particularly useful to say that laws are contingent or that they can be re-written as *ceteris paribus* generalisations, without detailing what kinds of conditions they depend upon and how that dependency works [.....] Providing a more adequate understanding of laws in the special sciences requires a better taxonomy of contingency so that we can articulate the several ways in which laws are not 'universal and exceptionless. (Mitchell (2002) : 339)

Both Mitchell and Woodward abandon talk of laws as traditionally conceived (i.e. as universal exceptionless conditionals). They deny the strict law/accidental dichotomy and argue that the difference between generalisations that are laws as opposed to those that are accidents is merely a matter of the degree of contingency involved. The task then is one of providing a methodology for testing the degree of contingency of scientific generalisations in different possible conditions/contexts rather than squeezing wildly heterogeneous generalisations into a "laws versus accidents" dichotomous framework.

Both theorists replace the dichotomous "laws vs accidents" framework with a methodology for testing the contingency of generalisations. For Woodward (2000), (2003) the way we do this is by testing the invariance of generalisations across possible changes. So, when testing the relationship between a variable X and a corresponding variable Y, we can test whether the independent variable X will stand in the same functional relationship to the dependent variable Y, across a

range of changing values for *X*. In the case of *contextual cp* generalisations, the relationship between *X* & *Y* will remain invariant across some, but not all changes in the value of *X*. There will not be any distinctions in kind to be made between laws (strict or *ceteris paribus*) and accidents, but rather there will be a range of generalisations, which differ in the degree of the contingency of their functional relationships.

It seems then that in providing a methodology for *contextual cp* laws in scientific practice and on a case-by-case basis we discover that *cp* contextual generalisations ought not to be treated as laws at all. The important point here is that *contextual cp* generalisations are only made meaningful when examined in scientific practice. But, when the methodological task of rendering them meaningful is performed they mean something quite different to what we started out with. They are no longer laws as traditionally conceived.

5:5:2 Idealised *Cp* Laws and the Semantic Problem:

Now let us consider *Idealised cp* Laws. Strictly speaking an idealisation law is true, even without the *cp* proviso. It looks promising that there is not any particular problem in providing the truth conditions for idealized *cp* laws. So take the statement of an idealised *Icp* law:

(12) *Icp* All *F*'s are *G*'s

The truth conditions for this law can be straightforwardly provided:

(13) (*Fx* and there are no other properties other than *F* & *G*) → *Gx*)

Take Boyle's Gas Law interpreted as a putative *idealised cp* law:

(14) *ICp* the product of the pressure and volume for a gas is a constant.

The truth conditions for (14) can be straightforwardly be provided:

(15) Given a gas with pressure x and volume y, and there are no other properties of the gas other that x and y and x and y don't vary, then the product of x and y is a constant.

We can provide a clear account of what the *ceteris paribus* clause in idealised *cp* laws means and so there is no particular semantic problem.

However, these *cp* laws are made true only at the cost of being inapplicable to most situations in reality, where complicated causal processes prevent the idealisation from holding. Idealised *cp* laws do not make claims about what will happen in the presence or absence of variables (known or unknown) rather they say what would happen if certain causal factors or processes operate in isolation. The *cp* clause does not replace some context-dependent variables rather it stipulates that there is no further context other than the relationship between the variables described in the law.

It may even be more accurate, given the latter description, to construe idealized *cp* laws as counterfactuals:

(16) Had F happened and had there been no other properties other that F & G, then G would have happened.

(17) Had there been a gas with temperature x and volume y, and had there been no other properties of the gas other than x and y, then the product of x and y would be a constant.

It looks as thought *idealized cp* laws do not present any specific semantic worry.

However, there is general problem about the semantics of counterfactual statements that may prove problematic for the counterfactual analysis of *idealised cp* laws. Standard truth-functional logic is not suitable for the semantics of counterfactual conditionals. The problem is that (16) and (17) above and in fact any counterfactual conditional as such will have a false antecedent. Take the antecedent of (17), 'Had there been a gas with temperature x and volume y, and had there been no other properties of the gas other than x and y'. Now the problem is that since the antecedent is not referring to any actual gas, the antecedent is false and this makes the consequent trivially true. So, analysing *idealised cp* laws as counterfactual conditionals render them trivially true. It would appear then that analysing *idealised cp* laws as counterfactuals will bring with it the more general semantic problems that face counterfactual conditionals.

There is also a further problem that faces these laws, which is a problem that faces theories in science in general: since all of our theories are idealisations how do we apply them to the messy chaotic world, where all else is rarely equal. We can provide *idealized cp* laws with truth conditions, only at the expense of them not applying to the natural world in a comprehensive way. The problem that then faces *idealized cp* laws is whether their status as laws is something that will remain or whether they are merely placeholders for some future strict laws of physics where the laws of some fundamental theory yield a perfect match to the facts about reality. However, this must remain speculative until such a theory presents itself.

Let me return to an earlier example to examine this claim further. Earlier we examined the Newtonian Law of gravitation as a putative *idealised cp* law. I argued that it is *idealised cp* law rather than a *contextual* one because there are no genuine exceptions to it. The *ceteris paribus* "proviso" is added on, so as to indicate that the application of the theory presupposes that there are *no* other forces at play other than the forces of mutual gravitation. The proviso precludes external gravitational forces, as well as frictional, electrical, or magnetic forces to which the bodies in the system might be subject. I argued that there are never cases where $F \neq ma$, but rather cases where applications of the law require a more complex calculation of the total force. Solely gravitational forces constitute the ideal setting for Newton's law. However, in reality the other forces (e.g. electrical, magnetic and frictional) will also need to be calculated in order to cash out what the *total* force of the law is.

Cartwright (1983 : 57) denies that the "further calculations" would in fact be as straightforward as I have claimed above. She makes a far stronger idealisation claim. She considers a case where the gravitational law would not yield the correct calculation because there are further electrical forces to be taken into account (i.e. where the gravitational law is applied in a context where there are also electrical forces). However, she claims that for bodies that are both massive and charged the law of universal gravitation in conjunction with Coulomb's law (the law that calculates the force between the charges) cannot merely be added together in a straightforward way to yield the total force. In actual fact, Coloumb's law submerges the gravitational one and so the resultant

force is very different to what would be calculated by a simple vectorial addition of the forces.

For this reason, she claims that formulating (what I call *idealised*) *ceteris paribus* laws in physics aren't that useful after all. When speaking of the problems that the law of gravitation faces when other forces are at play she states:

> But it is not a very useful law. One of the chief jobs of the law of gravity is to help explain the forces that objects experience in various complex circumstances. This law can explain in only very simple, or ideal circumstances. It can account for why the force is as it is when just gravity is at work; but it is of no help for cases in which both gravity and electricity matter. Once the *ceteris paribus* modifier has been attached the law of gravity is irrelevant to the complex and interesting situations. (Cartwright (1983: 58).

Although, it appears that we can provide *idealised cp* laws with a straightforward semantics, nonetheless, they face methodological difficulties when applied in scientific practice. *Idealised cp* laws as formulated above may be misleading in assessing actual real life cases where there is a composition of forces at work. It appears then that in assessing the application of the generalisation in scientific practice the *cp* law analysis leaves important factors out about how we use laws in science.

5:5:3 The Methodological Problem of Application:

These methodological considerations about the uses of both kinds of *cp* generalisations (*Idealised* and *Contextual*) in scientific practice are important because they show that even when we have made distinctions between the content of different kinds of *cp* law, analysing *cp* generalisations as *cp* laws (of different kinds) still leaves out important features of how these generalisations are used in

science. The *semantic* problem that *cp* laws faced was that these laws when read literally as descriptive sentences were in fact false. The further *methodological* problem that I am claiming they face is that they are also deemed false in the context of how we use them in science. So, even if it were possible to provide an account of the semantics of *cp* laws in a strict truth-conditional way, we would still face the further methodological worry about whether *cp* laws capture how these generalisations function in scientific practice.

5:6 "*CP* All the Way Down"

At the beginning of this chapter, I suggested that over the last few decades' philosophers have indicated that in fact all of the sciences have *cp* laws and so it is "*cp* all the way down". However, given the distinction that I have made in this chapter the claim that it is "*cp* all the way down" can be interpreted in a different way. We may have *cp* clauses in most or all of our present laws, but as we have seen this does not entail that they are the same kinds of laws (or generalisations) at all. So, despite the singular analysis qua *cp* laws that has been provided in the philosophy of science, these generalisations function very differently in different scientific disciplines. Furthermore, claiming that there are *cp* provisos in all scientific disciplines need not entail that all sciences were made equally.

For this reason, a subtler enquiry into how different sciences use their generalisations is needed. The distinctions between different types of

generalisations suggest that this should be done on a case-by-case basis. Moreover, this analysis suggests that the enquiry into *ceteris paribus* laws in the philosophy of science (and perhaps into laws more generally also) has been wrong-headed. Traditionally, philosophers have moved from an articulation of the criteria that laws of nature should have to analysing different generalisations in science and concluding on their law status from whether they match the criteria antecedently specified. I would urge that conversely, philosophers ought to examine how generalisations function in scientific practice and more general conclusions about what constitutes a law of nature; a scientific explanation and so on ought to be asked in terms of particular cases in scientific practice.

As theorists like Woodward (2002) and Mitchell (2002) have observed when we do observe science in practice we see that some of the criteria that we have imposed on science have been too stringent. In particular, the laws *vs* accidents dichotomy is too stringent to accommodate all the cases in between of different kinds of *cp* generalisations. Therefore, the particular case of *cp* laws is of significance not just as a particular problem in philosophy, but because a solution to it requires a different approach to analysis in the philosophy of science more generally.

5:7 The Disunity of the Special Sciences & *Cp* Generalisations:

It is, I think, significant that all of the *idealized cp* generalisations discussed in this chapter were laws of physics, whereas examples of the

contextual cp laws were laws of the special and social sciences. So, the distinction between *idealized* and *contextual cp* laws appears to underscore a fundamental difference in the methodology of physics as opposed to the rest of the sciences.

This distinction may provide a new way of thinking about the stratification of the sciences. *Pace* Fodor (1974) the disunity of the sciences is not corroborated because the special sciences have real *cp* non-eliminable laws, but rather because their *cp* contextual generalisations function in very different ways to their physics counterparts. So, the claim that the c*eteris paribus* condition is janus-faced may not only provide a new way of thinking about the semantic problem for *cp* laws, but also a different way of thinking about the disunity of the science. The disunity of the science ought to be understood as a claim about the different methodologies of different sciences.

CONCLUSION

I have argued that Special Science Realism as it has been construed by Fodor (1974), (1991), is informed and corrupted by an assumption that the special sciences must fulfil the same rigid criteria for successful science as physics. In Chapter One, I claimed that the scientific fundamentalist *ideal* is so pervasive that ironically, even though the disunity of the sciences presents a challenge to the unity of sciences view, theorists like Fodor are still coerced into analysing the special sciences in terms of the *ideal* criteria set down for future physics by the unificationists.

The chief constraint that I have focused on, in this thesis, is the claim that a science must have laws to be considered scientifically legitimate. Fodor (1974) avoids the charge of *Eliminativism* (Churchland (1989)) on the one hand and *Reductionism* (Kim (1993)) on the other, by arguing that the special sciences have real non-eliminable *ceteris paribus* laws. In Chapter 2, I argued that his realist account of the special sciences relies solely on an account of *ceteris paribus* laws because he motivates a weak construal of natural kinds.

However, I argued in Chapter 3, that *cp* laws face a serious semantic challenge; namely that we cannot provide non-vacuous truth conditions for *cp* laws. Furthermore, I claimed *pace* Fodor (1991) and Pietroski and Rey (1995) that any attempt to hedge *cp* laws in terms of the lower level laws and theories of physics simply defers the problem at hand in that it assumes that it is not "*cp* all

the way down". I argue that any attempt to provide the truth conditions by hedging *cp* laws faces a dilemma: either it is not "*cp* laws all the way down" and *cp* laws become placeholders for the real laws of physics, or it is "*cp* laws all the way down" and all laws are vacuous.

I also assessed weaker epistemic semantic accounts of *cp* laws, but I showed that these accounts led to a problem for testing *cp* laws. Lange (1993a) argues that *cp* claims are perfectly meaningful in so far as there is tacit agreement in terms of what constitutes a counterexample to any given *cp* law in scientific practice. However, I argued that this view faces an epistemic problem in that we cannot test these *cp* laws in a sufficiently rigorous enough way. Consequently, I argued that *cp* laws are faced with a pejorative dilemma which combines their *semantic* and *epistemic* problems: either *cp* laws can be completed prior to testing, but only *either* in a vacuous way or in such a way that they are made placeholders for the real laws of physics *or cp* laws can be completed within testing, but consequently, the authenticity of the scientific investigation cannot be preserved.

In Chapter 4, I argued however, that it would be premature to claim that there are no *cp* laws, based on these semantic and epistemic considerations alone. I examined another line of response which claimed that these semantic and epistemic challenges to *cp* laws are only fatal if one is committed to a certain view about laws more generally namely a regularity theory of laws. This is the claim that there are no ontological truthmakers for *cp* laws other than regularities.

I considered an alternative view, which claims that *cp* laws describe underlying dispositions that is the dispositionalist view of laws. According to

theorists like Bird (2004) and Lipton (1999), dispositions can provide truthmakers for *cp* laws. However, a closer analysis of the relationship between dispositions and laws reveals that a looser account is required, if our theoretical account must be made to fit with the open-endedness and variability of dispositions in scientific practice. I agree with Cartwright's (1989) claim that when we focus on variable dispositions rather than laws, we get a clearer picture of the kind of reality a scientist is faced with in scientific practice. It became clearer then that even if dispositions did provide truthmakers for *cp* laws this analysis would leave out important points about how these generalisations function in scientific practice.

Consequently, in Chapter 5, I argued that the singular analysis of *cp* generalisations *qua cp* laws is misleading. A closer examination of the diverse range of *cp* generalisations that are candidates for *cp* law status revealed that they are a wildly heterogeneous collection. I made a distinction between two kinds of *cp* law, the *idealised cp* and the *contextual cp* law, which I argued function in different ways so that the content of the *cp* clause is quite different. I argued that on the one hand, *idealised cp* laws can be provided with a straightforward semantics, but at the expense of their application to real life cases. I argued, on the other hand, that the semantics of *contextual cp* laws could only be provided in scientific practice and within a specific context. Therefore, the problem of *ceteris paribus* laws becomes a methodological problem of assessing how *cp* laws function in practice in different scientific disciplines, rather than a theoretical problem about whether they are meaningful.

In the end, the position I have defended is that the disunity of the sciences hypothesis is corroborated by the methodological claim that the *cp* generalisations

of the special sciences function in a different way to the *cp* generalisations in physics. I argue *pace* Cartwright that the claim that it is "*cp* all the way down" need not mean that all these sciences function in the same way. Moreover, I have defended Special Science Realism not solely because the special sciences have *ceteris paribus* laws, but because the special sciences have an altogether different scientific methodology to physics. The task for future research is to assess how the methodologies of different sciences differ in a more comprehensive way.

APPENDICES

Appendix I : The Derivation of Kepler's Laws from Newton's Laws

P1) Since, all planets move in ellipses (Kepler's 1st Law), they are continually accelerating.

P2) Since, the line connecting a planet to the sun sweeps out equal areas in equal amounts, (Kepler's 2nd Law), the force acting continuously on the planets (i.e. the reason for their continual acceleration) must be directed toward the Sun from the planet.

P3) From Newton's first law of motion (Every object in a state of uniform motion remains in that state of motion unless an external force is applied to it) and his second law of motion (The relationship between an objects mass m, its acceleration a and the applied force f is $F = ma$) it can be shown that the magnitude of the force must vary as one over the square of the distance between the planet and the Sun.

P4) Kepler's 3rd Law (The square of a planet's period is proportional to the cube of the planet's mean distance from the Sun) and Newton's 3rd Law of motion (For every action there is an equal and opposite reaction) imply that the force must be proportional to the product of the masses for the planet and the Sun.

C1: Thus, Kepler's laws and Newton's laws of motion taken together imply that the force which holds the planets in their orbits by continuously changing the planet's velocity so that it follows an elliptical path is (1) directed toward the Sun from the planet, (2) is proportional to the product of masses for the Sun and planet, and (3) is inversely proportional to the square of the planet-Sun separation. This is precisely the form of the gravitational force with the universal gravitational constant G as a constant, in other words Newton's Law of Universal Gravitation. ($F = G M_a M_b / r^2$). So, all of the laws in Kepler's theory of planetary motion are implied by (and so can be derived from) Newton's theory of gravity.

Appendix II: A variety of *ceteris paribus* generalisations from different sciences:

Biology:
Bergmann's Law:
Ceteris Paribus, for species of warm-blooded vertebrates, races living in cooler climates are larger than races living in warmer climates.

Von Baer's law:
Ceteris Paribus, those more general features that are common to all the members of a group of animals are developed in the embryo earlier than the more special features that distinguish the various members of the group.

Chemistry:
The Law of Definite Proportions:
Ceteris Paribus, any compound always contains the same elements in the same proportions.

Ecology:
Liebig's Law:
Ceteris Paribus, the distribution of a species will be controlled by that environmental factor for which the organism has the narrowest range of adaptability or control.

Economics:
Gresham's Law:
"*Ceteris Paribus*, good money (money that has little difference between its exchange value and its commodity value) drives bad money (money that has a substantial difference between its commodity value and its market value, where market value is lower than exchange value) out of circulation."

The Law of Supply and Demand:
"*Ceteris Paribus*, in a market economy, the forces of supply and demand generally push the price toward the level at which quantity supplied and quantity demanded are equal."

Electronic Engineering:
Ohm's Law:
Ceteris Paribus, at a constant temperature, the ratio of the potential difference (voltage) across a resistor to the current flowing in the resistor is a constant.

Epidemiology:
Farr's Law:
"*Ceteris Paribus*, epidemics increase their incidence rates exponentially"

Genetics:
Mendel's Law of Segregation:
"*Ceteris Paribus*, in Mendelian segregation, at a genetic locus, half of an organism's offspring inherit one of the alleles and the other half the other allele".

Mendel's Law of Dominence:
"*Ceteris Paribus*, in Mendelian segregation, where one allele is dominant, all of an organism's offspring inherit the dominant allele."

Geology:
"*Ceteris Paribus*, all meandering rivers erode their outside banks."

The Law of Superposition:
"*Ceteris Paribus,* in a sequence of sedimentary rocks, the deeper layers are older than the shallower layers."

Immunology:
Behring's Law:
"*Ceteris Paribus*, the blood and serum of an immunized person, when transferred to another subject, will render the latter immune."

Intentional Psychology:
Ceteris paribus frustration leads to aggression.

Linguistics:
Pitres' law
"*Ceteris Paribus*, in acquired aphasia with a multilingual patient, recovery comes first in the language most used just before the injury, whether or not it is the patient's mother tongue."

Metereology:
Buy's Ballot's Law:
"*Ceteris Paribus*, wind travels counter clockwise around low-pressure zones in the Northern Hemisphere."

Neurophsyiology:
Jackson's law:
"*Ceteris Paribus*, the nerve functions that are latest developed are the first to be lost when the brain is damaged by disease."

Optics:
Snell's Law:
Ceteris Paribus, at an interface between dielectric media, there is a refracted ray in the second medium, lying in the plane of incidence making an angle θ_t with the normal such that: $Sin\ \theta/\theta_t = n_2/n_1\ cp.$

Physics:
Newton's 2nd Law of Motion:
Ceteris Paribus, force equals mass times acceleration.

Boyle's Gas Law:
Ceteris Paribus, the product of the pressure and volume for a gas is a constant

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