Introduction

In this thesis, I will argue for the existence of scientific law in geology. In Part I, I will discuss the notion of scientific law in general. Should we side with a strict normative interpretation of law in which only those statements that cover universal, exceptionless – even necessary – relations qualify? If we choose this option and support a normative definition of scientific law as universal and exceptionless, we must be ready to defend the notion that such laws even exist. While we cannot, of course, evaluate all the laws currently used in the practice of science, a close look at a few paradigmatic laws will be taken and will show this notion to be unsupportable. Specifically, I will demonstrate that even some of the most paradigmatic laws of physics require at least some qualification in the form of a *ceteris paribus* clause. I will conclude that there are no laws that meet the criteria embodied in the traditional conception of scientific law as universal and exceptionless.

Having decided that there simply are no laws in this traditional, normative sense, we are left with two options if we wish to continue the practice of science: either we (1) practice science without the concept of natural law, or we (2) modify our conception of scientific law to fit the work-a-day reality of the practicing scientist as it is manifested in the lab and in the field. These issues will be discussed in Part II. Concerning the first option, I will look at two prominent philosophers, Ronald Giere and Nancy Cartwright, who claim to argue for the practice of science without laws. I will show that Giere is not, in fact, arguing for the practice of science without laws, but is arguing semantics instead. I will show that while Cartwright is actually arguing for science without laws, she offers no tenable substitution.

Others' failures to provide a coherent philosophical account of how science might be conducted without laws do not mean that such a thing is impossible. A brief look at the way science is actually practiced in the lab and in the field, however, will show that the idea is misguided from its inception. Having reached the conclusion that science cannot be practiced without laws, I will be left with option (2) and will argue for a departure from the traditional view of scientific law and the adoption of a pragmatic approach to natural law in the sciences. Of course, one philosopher's pragmatism is another's flight of fancy, but I hope to convince the reader that my approach is pragmatic in the ordinary sense of that word vis-à-vis the actual practice of science.

In Part III, I will evaluate several of the general principles used by geologists and see if they measure up to the pragmatic criteria I had established in Part II. These rock-ordering principles (as they are sometimes called) will include original horizontality, superposition, and cross-cutting relations. I will conclude that these principles *do* meet the pragmatic criteria established at the end of Part II and are as legitimate as any laws used in the actual practice of biology, chemistry, or physics.

Part I: Ceteris Paribus Laws

I will address two issues in this section: what counts as a *ceteris paribus* clause?, and are there *any* laws of nature (even in physics) that do not require at least some qualification? As to the first question, I will argue that many laws that are said to not have *ceteris paribus* clauses in fact do have them and the only defense against their having *ceteris paribus* clauses is dismissal by fiat. As to the second question, I will look at some laws (such as Newton's gravitation) thought to be classically universal and absolutely free of qualifiers. I hope to convince the reader that even these laws are not so clearly free of qualifiers as is often supposed.

What Counts as a *Ceteris Paribus* Clause?

I should make clear at the very beginning what I mean when I say *ceteris paribus*. Frequently, the phrase is translated simply as "other things being equal". Jerry Fodor, for instance, in his paper *You Can Fool Some of The People All of the Time, Everything Else Being Equal; Hedged Laws and Psychological Explanations*, says of what he calls *nonstrict* laws: "they must be "ceteris paribus" or "all else equal" laws." (Fodor 1991, 21) In his 1991 paper Ceteris Paribus *Laws*, Stephen Schiffer says, "A ceteris paribus law, if there are any, might tell us that F events cause G events ceteris paribus, or, to confine the expression to a single language, that F events cause G events *all other things being equal*." (Schiffer 1991, 1) This translation is certainly true to the original Latin.

But when philosophers of science use the phrase, it seems to me they mean something more specific. In the introductory editorial to their compilation Ceteris Paribus *Laws*, John Earman, Clark Glymour, and Sandra Mitchell cite John Cairnes's *The Character and Logical Method of Political Economy*. Cairnes states that, when discussing economics, *ceteris paribus* means “only in the absence of disturbing causes”. (Earman et al. 2002, 1) It is this formulation, I feel, that best captures what the philosophers I have read mean when they use the phrase. I intend, for the purposes of this paper, for the phrase to mean precisely this unless otherwise noted.

Now that I have made clear what I mean by *ceteris paribus* (hereafter CP), it will be helpful to see what sorts of conditionals fall under the purview of this definition. To begin, I will discuss an interesting issue raised by Cartwright which, at first, will seem to have little to do with CP clauses. Why this issue is pertinent will become clear. In her book *How the Laws of Physics Lie*, Cartwright quotes Richard Feynman: “The Law of Gravitation is that two bodies exert a force between each other which varies inversely as the square of the distance between them, and varies directly as the products of their masses.” (Cartwright 1983, 57) Cartwright says, concerning this quote, that “It is not true that for *any* two bodies the force between them is given by the law of gravitation.” (Cartwright 1983, 57) She is correct in this assertion, but only trivially so. To my knowledge, no working physicist (or indeed, bright high school student) would ever make this claim. It is well understood by people who study such matters that the law of universal gravitation does not purport to give the *total* force between two bodies.

Perhaps it would be helpful to see what various sources have to say about Newton’s Law of Universal Gravitation (hereafter UG). The text I used to teach high school physics says the following: “If particles have masses m_1 and m_2 and are separated by the distance r, the magnitude of the *gravitational* force is …”. (Serway 1995, 193) *The Cartoon Guide to Physics*, by Art Huffman, UCLA physics lecturer, says: “The *gravitational* force between masses M and m is proportional to the product of the masses and inversely proportion to the square of the distance r between them.” (Gonick 1991, 27) Finally, I quote Wikipedia: “Isaac Newton's theory of universal gravitation is a physical law describing the *gravitational* attraction between bodies with mass." In each case, the emphasis is mine, but the point is made. One need not refer to particularly sophisticated sources to discover that the equation governing UG does not purport to give the *force* between any two bodies; it purports to give the *gravitational* force between any two bodies. In claiming that Newton's Law is somehow deficient because it fails to account for other possible forces, Cartwright attacks the fluffiest of straw men.

Cartwright admits that her complaint is answerable by an "obvious rejoinder". (Cartwright 1983, 57) She says that she could be accused of stating the law in shorthand form and thereby omitting an implicit CP clause. To fix the problem as she perceives it, she offers the following:

> "*If* there are no other forces other than gravitational forces at work, then the two bodies exert a force between each other which varies inversely as the square of the distance between them, and varies directly as the product of their masses." (Cartwright 1983, 58)

Clearly, this is a CP clause in the sense that it accounts for "disturbing causes", and it *would* solve the "problem". But is it necessary? What if there were a step that obviates the CP clause? Rather than write the equation governing UG like this,

$$F = (Gm_1m_2)/r^2$$

I could write it like this:

$$F_g = (Gm_1m_2)/r^2$$

Is inserting the subscripted g in the equation making use of a CP clause? No, it is not. The subscripted "g" is not in any way accounting for extraneous factors or "disturbing causes". The presence of gravity in this scenario cannot reasonably be considered as disturbing cause; gravity is what the scenario is *about*.

However, there is – to borrow Cartwright's appropriate phrase – an obvious rejoinder. One could claim that UG is about objects *with mass*, and that this stipulation – mass – is a CP clause. After all, objects *without* mass would not be governed by UG. While it is true that massless objects are not governed by UG, this rejoinder misses because of its faulty premise. UG gravitation is not about *objects* at all, with or without mass. This is simply incorrect. UG is about point *masses*, so the *objects* involved do not need to be qualified in any way because they do not play a role in the law's description. They could be anything at all or even nonexistent; it is the masses that matter.

Coulomb's Law and Cartwright's evaluation of it present a similar scenario. The equation for Coulomb's Law should look familiar:

$$F_E = (Kq_1q_2)/r^2$$

The obvious rejoinder in this case is also familiar. The objects involved here must be charged objects, and this stipulation of charge being necessary to the object's obeying Coulomb's Law is a CP clause. This objection fails for the same reason. Coulomb's Law is not about objects; it is about charges.

Clearly, what is or is not a CP clause must involve some gray area, and reasonable people can disagree about where the boundaries of this gray area lie. After all, one could argue that simply putting the word *gravitational* in front of the word *force* when discussing UG is a CP clause. I disagree. It is unclear to me how one can even discuss a scientific law without at least acknowledging what the law is about, that is, its *domain*. While it seems clear to me that one must at least state the law's domain, discussing the entities it governs is admittedly more problematic. When asserting that Newton's UG governs only *point* masses, one could claim that that the word *point* is an attempt to account for a disturbing cause (e.g. an irregular shape) and is therefore a CP clause. Here again, I would disagree. The notion that the masses must be qualified as point masses – or, indeed, qualified in any way – is an artifact of the mistaken idea that UG is about *objects* with mass. As has been said, this is not the case. We need not qualify the masses in that way, because a mass is just and only that: a mass. It is not point, or oblong, or irregular or anything else. At some point, in order to further the conversation, a philosopher must insist. To that end, *I will not consider a statement of a law's domain or a single aspect statement of the entities it governs as CP clause.* Any additional attempt at qualification – either spatial or temporal – either by scientist or philosopher – will be considered an attempt to account for a disturbing cause and is therefore a CP clause.

Are There Any Non-CP Laws?

We have discussed my views of what a CP clause is not. Obviously, the next step is to discuss what a CP clause is. Fortunately, the gray area we have discussed will present little difficulty in the furtherance of my argument insofar as the CP clauses I will present as examples are well clear of it.

In their paper "Ceteris Paribus Lost", Earman, Roberts, and Smith discuss the law of thermal expansion. Their intention is to defend this law against accusations by Marc Lange that it is a CP law. Lange states that the law of thermal expansion is CP because in order to state the law, one would have 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 by a rigid material that will not yield as the bar is heated, and so on."(Lange 1993, 234) Earman et al. claim that this is not a CP law because:

> If one helps oneself to technical terms from physics, the condition [that would render the law rigorously true] 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, 8)

This assertion is incorrect. Stating that there are "no external boundary stresses" on the bar is an attempt to account for a "disturbing cause". As such, it is an *archetypical* CP clause. The fact that the conditional is stated in the "technical terms from physics" is irrelevant. The phrase "no external boundary stresses" is no less an attempt to account for a disturbing cause than the phrase "as long as it ain't wrapped up good an' tight with bailin' twine" would be. The subject-specific appropriateness of the language has nothing to do with it. Nor does, contrary to Earman's claim, the *ease* with which the qualifier is stated. A qualification consisting of fourteen words is no more a CP clause than a qualification consisting of only the thirteen words actually used. If Earman considers ease a pertinent criterion, he should provide some sort of standard by which to distinguish the *easy* from the *difficult*. Finally, they are mistaken in the technical sense. The conditional they add would not make the law strictly true in any event. Metals bars follow the law of thermal expansion until they approach their melting point. It is true of crystalline solids (rather than amorphous solids, such as glasses and polymers) that they tend to have a well-defined melting point. But it is not *perfectly* defined. There is a narrow range of temperatures – only a fraction of a degree – when the law of thermal expansion does not apply. So we should add to "no external boundary stresses" the following condition: *over this particular range of temperatures*. Not to mention that the bar must be *chemically pure*. Further, the bar must be heated *absolutely uniformly*. (Any uneven heating of the bar would cause it to bend slightly, affecting the way the bar expands; it would be slightly longer on one side than the other. To heat the bar absolutely uniformly is, by the way, precluded by relativity.) Finally, what a *bar* is must be defined. Is a piece of metal that is only twice as long as it is wide still a proper bar? A *cube* of steel behaves differently than a long (say fifty times longer than wide) bar of steel. Would Earman et al. consider these CP clauses, or are they too easily stated in too appropriate a language?

This nearly page-long list of necessary qualifiers falls well outside the gray area we have discussed. This law is not even in the wildest approximation universal or exceptionless. Those, however, who support the idea of the existence of universal, exceptionless – that is, non-CP – laws would at this point raise a very legitimate objection. The "law" of thermal expansion provides much too easy a mark for the universal law skeptic. To address this objection – and perhaps make a more philosophically noteworthy point – we should look closely at a grand, paradigmatic law. In *Ceteris Paribus Lost* (already discussed) Earman, Roberts, and Smith do precisely this. Earman et al. take a close look at Newton's Universal Gravitation. In this subsection, I will begin with a close look at their close look and find it fundamentally misguided. I will provide my own analysis of UG and find that Earman et al. are mistaken: UG is a qualified – and therefore CP – law.

Earman et al. set out to defend UG from the accusation that it is a CP law and go awry from the very beginning. When discussing the application of UG to our solar system, Earman states that this can be done by assuming that the solar system is closed. With this assumption, one can:

> "… derive an equation of evolution type (or coupled set of them) that describes the motion of the planets given this situation. The application will be valid provided that no other significant non-planetary masses are present and provided that no significant non-gravitational forces are acting on the planets." (Earman et al. 2002, 9)

I will dispense with the technical issues before entertaining the more philosophical notions. Claiming that a differential equation (of any type) can be developed to describe this situation is incorrect. Since there is no exact solution to the equations governing the gravitational interaction between more than two bodies, the best than can be hoped for is a set of equations with approximate (numerical method) solutions that will *approximately* describe the behavior of this nine (or depending on one's view of Pluto, ten) body system.

In any event, by my count there are at least three attempts to account for disturbing causes in this brief quotation: "under this assumption", and two uses of the word "provided". Well, that is what a CP clause *is*. It is a set of words that makes clear what things must be "assumed" and "provided" in order for a law to be "true". (It should be noted here that even positing the assumption in question – that the Solar System is closed – is an immediate and complete surrender to the notion that UG does not apply to the Solar System in any but the engineering sense. The notion that the Solar System is closed is obviously incorrect; it is no more correct than to "assume the cow is a sphere".) Earman claims that it is wrong to assume that "Newton's laws have implicit CP clauses" (Earman et al. 2002, 9) merely because the second proviso in the quoted material has "… a kind of open-ended characteristic reminiscent of a CP clauses." (Earman et al. 2002, 9) He explains:

> "For in the first place, the condition for the validity of application can be stated in precise and closed form: the magnitude of the non-gravitational forces must be small enough in comparison with the gravitational force that the theory implies the neglect of the non-gravitational forces does not affect the desired degree of accuracy of the predictions of the planetary orbits." (Earman et al. 2002, 9)

This attempt to relieve UG of its CP burden is simply disastrous. A truly universal, exceptionless law by definition needs no statement describing its "condition for the validity of … application". A universal, exceptionless law by definition applies in *all* times and in *all* places without qualification of any sort. That is what the words "universal" and "exceptionless" *mean*. Second, I am troubled by Earman's use of the word "precise" when it is followed so closely by the phrase "small enough". "Small enough" is not a "precise" term; it is a relative term. How many centimeters (or what percentage – and to how many decimal places – of the objects in question) qualifies as "small enough"? Third, Newton's *theory* (to echo Earman's italics) of UG (as Earman admits) says absolutely nothing about extraneous forces in this scenario. (Earman et al. 2002, 9) If so, how can the theory *imply* anything about them as he seems to think? Forth, Earman's criterion that the conditional is not a CP clause if it can be stated with *ease* – his word – seems to have gone astray here. The conditional in this case requires forty-four words to state. (As I have pointed out already, the *ease* with which a conditional is stated has nothing to do with whether or not it is a CP clause. Still, if it is a criterion Earman thinks is valuable, it should be adhered to at least by him.) Finally, this is all wrong in the rough philosophical sense. Any time a discussion of the work of scientists incorporates concerns over the "desired degree of accuracy", one has begun to discuss an engineering matter. "[T]he desired degree of accuracy" has nothing to do with whether or not UG is a CP law, and Earman et al. must know this insofar as they admit that "the conditions for the provisos are conditions for the *validity of the application*, not conditions *for the truth of the law statements of the theory*." (Earman et al. 2002, 8) Earman is correct here – the things they have been discussing *are* about the application and *not* the capital T truth of the theory. One wonders why they commit so much text to a matter that they know ahead of time is irrelevant. Earman et al. have failed to relieve UG of its CP burden primarily because they have said very little about UG proper having spent so much time discussing an engineering matter. But that failure does not in itself constitute an argument that such clauses exist vis-à-vis UG. In the remainder of this sub-section, I will provide such an argument.

If we intend to look closely at UG with the intent of exposing its qualified nature, it is incumbent on us to ensure that we have its statement correct. To that end, we will start at the very beginning, with the *Philosophiae Naturalis Principia Mathematica*. In doing so, we discover that Newton himself reveals his concern over justifying conditions before even the preface of the section entitled Proposition VIII, Theorem VIII is finished. From the *Principia*:

> "In two spheres mutually gravitating each toward the other, if the matter in all places on all sides round about and equidistant from the centers is similar, the weight of either sphere toward the other will be reciprocally as the square of the distance between their centers." (Newton 1687)

While this statement of UG is not the neat, tidy equation to which we are accustomed, it is clear that it represents the same entities (for "weight", read "force") that are represented in the equation with which we are so familiar and which has been discussed above. And yet there is also much more. This quotation contains perhaps the most important word that any statement of scientific law can contain in light of the notion of *ceteris paribus* clauses: *if*. And the *if* in question is non-trivial. Many regard the UG equation unqualified because we consider the "r" in the denominator of the argument to be the distance between the "centres" of the masses in question and feel no need to say so. Yet, as Newton himself correctly points out with his ever-important *if*, his UG is only applicable to two "spheres" (never mind rotating oblong asteroids) that are perfectly uniform in density (never mind two rotating oblong asteroids that are made of real substances). No such perfect instantiation can occur, even theoretically.

There are only two ways that UG could be ever perfectly instantiated: with *true* point masses, or with perfectly rigid, perfectly uniform extended spheres. True point masses are difficult to come by. Electrons have been described as point masses, but the same theoretical framework which lends the electron a potential existence as a point mass also robs it of being in any particular place x at any particular time t and therefore makes it very difficult to measure the "r" between these two proper point masses that we need for a valid instantiation of UG. Special Relativity precludes the existence of perfectly rigid bodies (the existence of such entities would make super-luminal communication trivial). And while an imperfectly rigid body might be perfectly spherical in an otherwise empty universe, it would not be perfectly spherical in a universe with two bodies. The sides of the bodies that face each other would be under a greater gravitational force than the sides of the bodies facing away from each other and the spheres would therefore be distorted away from perfect sphericity. This renders the second option untenable as well. It may be the case that UG can never be perfectly realized; any instance of it use must carry a CP clause when it is applied to what actually is the case.

Should the reader find the foregoing unconvincing, this should settle the matter: in the first femtoseconds of the universe's existence, there was no matter. The variables in the numerator of the UG equation's argument literally had nothing to which they could be applied. To say that UG is true in all times and in all places is simply incorrect. At best, UG applies to the universe only when matter is present. This stipulation is a CP clause. Should the reader *still* be unconvinced, there is the simple fact available to all bright high-school physics students that UG is known " … in the light of relativity theory not to be strictly speaking true." (Kline 1986, 33) UG's strict falsity in light of relativity is not a vague philosophical matter: it is manifest at the engineering level. Relativity simply makes a more accurate calculation of the precession of the aphelion of Mercury's orbit. One wonders why philosophers would bother to defend the notion that UG needs no qualifying statements when we know as a matter of fact that it is not even true. Simply stated, UG applies only when non-relativistic masses and velocities are considered. This stipulation, too, is a CP clause.

The preceding is not an attempt to devalue the law of Universal Gravitation. UG is a profound intellectual achievement from a profoundly gifted thinker. It has much to say about the universe and is eminently useful in the practical sense. Nor is UG intended to be singled out for special criticism. Similar objections could be made to other laws of physics held in similar esteem. For many of these esteemed laws, the disqualifying characteristics are trivially easy to state. As William Barr points out in his paper "A Pragmatic Analysis of Idealization in Physics", Boyle's Law is true only if the molecules involved are "…perfectly elastic and spherical, possess equal masses and volumes, have negligible size, and exert no forces on each other except during collisions…" (Barr 1974, 48) None of these things, of course, is true.

Barr also points out the qualifications necessary to make Galileo's law of falling bodies applicable to the real world, but misses the much more interesting point that *even if* the friction is zero and no other extraneous forces are acting and the world is perfect, Galileo's law of falling bodies is false. Galileo's law of falling bodies states that "the distance that x falls divided by the square of the time it takes x to fall equals a constant". (Barr 1974, 49) This notion is based on Galileo's best efforts at timing and seemed to have been supported by the later development of Newton's laws of motion and his universal gravitation. It is simple: any object near the Earth's surface is attracted to the center of the Earth with a force proportional to the product of the object's and the Earth's masses divided by the distance between the center of the object and the center of the Earth, squared. In equation form, we have the by now very familiar UG:

$$F_g = (Gm_1m_2)/r^2$$

Also from Newton, the acceleration of the object is given by F = ma. Substituting in the ma for the F_g in the UG expression gives us:

$$ma = (Gm_1m_2)/r^2$$

Since m on the left and m_1 on the right are the same, we get:

$$a = (Gm_2)/r^2$$

which is Galileo's law of falling bodies in equation form. But it's wrong for a very simple reason: the r in the denominator on the right side of the equation is *not constant*. As an object falls, it gets closer to the center of the Earth, the r gets smaller, and the *acceleration increases as time elapses*. This effect, by the way, was not measurable in Galileo's or even Newton's time, but is trivial to measure today. One needs only a modestly expensive scale and a middling skyscraper. But even if one were to render this law ideally true by expressing it as a first order differential equation, it would still be false in the light of relativity: as masses approach each other under gravitational influence, their stored potential energy is changed, and this change would show as changes in the bodies' masses. So, in reality, not only is the *r* not constant, neither is the *m*. We now have a very messy equation, indeed. Further, and even more simply, bodies do not fall to the Earth in any event; small bodies and the Earth both fall to their *common center of gravity*. It is true that the Earth would move less than an atom's width in this situation, but in that small distance is all the space necessary to separate the true from the merely useful.

Other so-called universal and exceptionless laws require even less analysis: General

Relativity breaks down near the primordial singularity and therefore is *not* true in all times and in all places; it explains much of course, but when it comes to the most important picosecond in the universe's history – the first – it has nothing at all to say. While no physical laws seem more staid and reliable than those of thermodynamics, they may be the most violated of all laws. Depending on one's interpretation, the laws of thermodynamics are violated literally millions of times every second in every cubic centimeter of space throughout the universe. The fact that, given time, the energy accounts are "settled" at the macro level is irrelevant to the discussion at hand. The laws of thermodynamics simply do not apply in every instance and in every framework. Its necessary macro-level instantiation is a CP qualifier.

As Sandra D. Mitchell points out in her 2000 paper "Dimensions of Scientific Law", *universal* and *exceptionless* are simply impractical demarcation criteria. She is correct to say that when "…one looks to the actual products of scientific practice, one is hard pressed to find examples that fit [this] ideal image". (Mitchell 2000, 249) I would go one further and say that these ideal laws simply do not exist. Simply put, all natural laws are CP laws. But even if I am mistaken, and a very few of these ideal laws do exist, they are certainly an insufficient scaffolding upon which to base the practice of science. This situation, of course, presents a problem: How do we compensate for the loss of the traditional scientific law? While surprisingly little has been written on these issues concerning geology, it has been covered extensively regarding biology. In Part II, I hope to borrow from the progress that has been made in the philosophy of biology. I will attempt to persuade the reader to accept two ideas: (1) we simply cannot proceed in the practice of science without those entities which have hitherto been known as laws, and, (2) if so, we must take a more pragmatic approach to the very notion of natural law.

Part II: Natural Law, a Pragmatic Approach

Some philosophers of science have argued that we should conduct the practice of science without laws. (Giere, 1999, Cartwright 1999) In the first section of Part II, I hope to convince the reader that this is a mistake. Their arguments, it seems to me seem, go wrong in one of two ways. (1) They are not, in fact, arguing that we should conduct the practice of science without laws, and are actually claiming quite a different thing. Their arguments have a great deal to do with nomenclature and very little to do with the *practice* of science and still less to do with the practice of science without what have until now been called natural laws, or laws of science, or scientific law, etc. Or (2) they *are* arguing to conduct the practice of science without laws but provide a woefully inadequate replacement. I will take a close look at an example of each sort of argument and find that the result is unconvincing. In the second section of Part II, I will show that the notion of practicing science without scientific law – using specific, real world instances – is fundamentally misguided. It is an empty philosophical notion that simply does not map onto the actual world in which scientists live and work. In the third section, I will argue that abandoning the too-rigid criteria of the normative conception of natural law as universal and exceptionless need not mean that we must abandon the notion of natural law altogether. I intend to show that a pragmatic approach to scientific law based on the way these laws are actually *used by scientists* is the sensible approach.

Proposals to Practice Science without Laws

In Chapter Five of his book *Science Without Laws*, Ronald N. Giere states that he will advance "…some general reasons for skepticism regarding the role of supposed laws of nature in science" (Giere 1999, 85) and also that he will:

> “…outline an alternative interpretative framework which provides a way of *understanding* the practice of science without attributing to that practice the production or use of laws of nature *as typically understood by contemporary philosophers of science*.” (Giere 1999, 85) (emphasis added)

The added emphasis in the quotation above is important. It is one thing to say that the practice of science can continue without the notion of scientific law “*as typically understood*,” but it is quite another to say that the practice of science can continue without any notion of scientific law. I contend that Giere is not arguing that we can practice science without those “expressions” (his word) that we have until now known as scientific law. Rather, he is arguing simply that we re-evaluate the nature of the natural laws themselves, and in this he is correct. Periodic reevaluation of dearly held notions is important lest they assume the status of dogma. But I claim that his argument, in fact, is much less provocative than the title of his book would suggest and that even if his ideas were adopted globally and immediately it would not change significantly what it is that scientists do in the lab and in the field. It would simply change the words they use.

Giere asserts that – when considering “claims that are typically cited as ‘laws of nature’” – close inspection reveals that they “…are neither universal nor necessary – they are not even true.” (Giere 1999, 90) Again, he is correct. But while seemingly comfortable with the idea that “natural laws” are strictly speaking not true, Giere is concerned that the problem is not taken more seriously by the philosophical community. Specifically, Giere is worried that many philosophers of science – for example Coffa and Hempel – are too at ease with the notion of the strict falsity of natural law insofar as they view natural not as universal statements, but as statements including an ‘implicit proviso’”. (Giere 1999, 91) As Giere puts it: “… Hempel’s account is that purported statements of laws of nature of the form “All bodies, …, etc.” are to be interpreted as really of the form “All bodies, …, etc., with the proviso that …”. (Giere 1999, 91) Giere’s “…objection to this interpretation is that it is impossible to fill in the proviso so as to make the resulting statement true without rendering it vacuous.” (Giere 1999, 91) I agree with

Giere, but would go even further. Not only is it not possible to provide an account of, say, Newton's Laws of Motion with suitable provisos that would render them true without being vacuous, I contend that no proviso can make them *true* in any way, vacuous or not. If enough provisos were added to make them true, they would no longer be recognizable as Newton's Laws; they would look more like relativity. But while Giere's point is correct, it is also irrelevant if one is discussing, as he claims to be doing, the actual field and laboratory *practice* of science. Whether these laws are true or not – or can be rendered true or not – does not much affect the way lab-coated scientists *use* them.

When referring to Newton's equations, he states simply that "[e]veryone uses these equations" and then goes on to say that the issue is not whether or not to use these equation, but rather "how to interpret them." (Giere 1999, 92) This is incorrect. Whether or not we use these equations, and will continue to do so, is *precisely* the issue. How to interpret them (the equations, laws) has very little to do with how scientists use them or whether or not the practice of science can be conducted without those cognitive entities that we refer to – rightly or wrongly – as "laws". Giere makes a point of referring to Newton's equations as just that, equations, since the use of the word *laws* implies that these equations "have empirical meaning and that there is an implicit universal qualifier out front." (Giere 1999, 92) Implicit to whom? Certainly *I* do not take it to mean that the simple attribution of the word *law* imbues a sentence, relation, equation, or regularity with the force of a universal qualifier (should such a thing even be said to exist). This concern may have been warranted much more in Newton's day when the appellation "law" did (by some accounts) carry that sort of weight for most people who thought carefully about such issues. Today, however, there are far too many people who question this notion for it to be a given that any particular individual will hold this view. Giere's concern about using the word "laws" simply highlights the point I made in the introduction to this chapter about nomenclature.

He is not arguing here that we practice science without laws. Rather, his concern is largely semantic, and this largely semantic concern is elaborated in the next section of his chapter.

Referring to Newton's and Schrodinger's equations, Giere says (and I agree) that they "…seem to capture something fundamental about the structure of the world." (Giere 1999, 94) The problem, he says, is to "capture this aspect of these fundamental equations without lapsing back into the language of universal laws." (Giere 1999, 94) Giere proposes that we refer to Newton's Laws of Motion (or, as Giere would have it, Newton's Equations of Motion) as, rather, Newton's Principles of Motion. And thus are we are led to a *third* way to which we can *refer* to what had before been Newton's Laws of Motion without discussing even briefly how they are used, not used, or even need or need not be used, in the practice of science. Giere says that we "can make use of the linguistic variation" (Giere 1999, 94) of *referring* to laws as principles and thus we have at last come (at least peripherally) to the issue of how these entities (whatever they are called) might be *used* by practicing scientists. Giere suggests that:

> "Principles … should be understood as rules devised by humans to be used in building models to represent specific aspects of the natural world. Thus Newton's principles of mechanics are to be *thought of* as rules for the construction of models to represent mechanical systems, from comets to pendulums." (Giere 1999, 94) (emphasis added)

Here again, Giere seems to be talking about how Newton's principles should be *thought of* rather than discussing how they should be *used* in the practice of science. Still, if we were so bold as to substitute "used" in place of "thought of" on Giere's behalf, we would have a perfectly practical outline of the way Newton's laws should be used by the scientist in actual places like NASA, the Jet Propulsion Laboratory, and the Keck Observatory. I suspect that working scientists at these sorts of places tend to be curious and intelligent people who frequently consider abstruse topics like the nature of scientific law. But the scenario Giere proposes in which Newton's principles are used to construct models to represent mechanical systems from comets to pendulums would sound like centuries-old news to them. This is exactly the sort of activity that has been engaging the practicing scientist since Galileo first constructed a wooden ramp or used his heartbeat to ascertain the period of a swinging chandelier. This is one of the things that scientists *do*.

It may surprise the reader to know that I find Giere's argument perfectly sensible. Words are important, and it may be the case that the word "law" carries too much epistemological (even ontological?) baggage, and its use in the philosophy of science should be reevaluated. His argument is not wrong; it is simply not germane to the title of his book and in fact does essentially nothing to suggest scientists should not use laws. Rather, it seems to me, he is arguing that we continue to use those things we had called laws, but that we refer to them – even think of them – as something else. This is an entirely different matter, and he may be right in that regard. But his failure to consider the subject implied by his provocatively titled book leaves us still with a very important issue unaddressed. Perhaps a different philosopher of science will provide a more pertinent argument for the abandoning of natural law in the practice of science.

Much has been said about Cartwright's intention to do away with the notion of natural law in the practice of science. Mitchell, referencing Cartwright's 1999 book *The Dappled World: A Study of the Boundaries of Science* says that "…Cartwright's strategy has been to reject the need for laws in science … and replace them with talk of capacities and nomological machines." (Mitchell 2000, 250) When reading Cartwright, it is easy to assume that she intends for the practice of science to be conducted without laws. Cartwright says that we need:

> "…claims about capacities to understand nomological machines and cannot make do with laws, in the necessary regular association sense of 'law'. I shall look at two prominent places where we can see why we need capacities *instead of laws*." (Cartwright 1999, 64) (emphasis added)

A close look at this quotation raises important questions. When Cartwright says that we cannot "make do with laws", does she mean that we cannot make do with laws in the practice of science? Or does she mean that we cannot make do with laws as a method of understanding nomological machines?, as the sentence suggests. If the latter, then not very much has been said. By

Cartwright's own admission, a nomological machine is "…after all, only a philosophical concept". (Cartwright 1999, 57) Since this mere philosophical concept is of her own devising, it seems reasonable that she is best fit to determine what things best make it understood. But even if she is questioning the necessity – or even effectiveness – of natural law in the practice of science, she is questioning this necessity only insofar as law is construed in the "necessary regular association sense". Questioning the usefulness of the concept of *natural law* in the practice of science and questioning the usefulness of the concept of natural law in the *"necessary regular association sense"* in the practice of science are different things entirely. I disagree with the former completely, and yet the latter provides the foundation for this paper's thesis.

Other questions this passage raises, of course, are: What is a "nomological machine"?, and what is a "capacity"? According to Cartwright, nomological machines are "…very special arrangements, properly shielded, repeatedly started up, and [that run] without a hitch" (Cartwright 1997, S292) and these nomological machines are what it takes to "…to get a law of nature." (Cartwright 1999, 49) (Of course, Cartwright also says that – regarding probabilistic laws – that nomological machines are "chance set-up[s]", seemingly the exact opposite of the previous description. I suppose a nomological machine can be whatever one needs in whatever circumstance.) (Cartwright 1997, S293) I will for the sake of argument momentarily stipulate to Cartwright's nomological machine, and that will lead to "capacities". Of capacities, Cartwright says that

> "… our understanding of [nomological machines] depends on knowledge of capacities, not knowledge of laws. Is there much, after all, in the difference? I think so, because when we refuse to reconstruct our knowledge as knowledge of capacities, we deny much of what we know and we turn many of our best inventions into pure guesses." (Cartwright 1999, 59)

(How this refusal to transform our knowledge into knowledge of capacities renders our best inventions as *pure guesswork* is never explained as far as I could tell. In any case, I suspect that Newton, Einstein, Salk, Crick, and many others would be surprised to hear it.) I must admit a slight confusion on my part concerning the relationship between laws, capacities, and nomological machines. Cartwright claims that we need capacities, not laws, in order to understand nomological machines, but also that it is the job of nomological machines to create laws of nature. Well, if laws of nature are the things that nomological machines create, and nomological machines operate on capacities, then it is clear that by definition of the various entities that capacities – rather than laws – are needed to understand nomological machines and by extension laws. As I said earlier, nomological machines and capacities are Cartwright's ideas and it is she who should best decide how they are to be understood. As I also said earlier, regarding the actual practice of science, not much has been said. Still, if capacities are important, and nomological machines are important, then the things created – natural laws – by the in-tandem functioning of capacities and nomological machines must also be important. So it seems that we need capacities, nomological machines, *and* laws.

But while I am unclear, perhaps, on the various roles these various entities play in Cartwright's philosophy of science, I am quite sure that the argument she makes for replacing laws with capacities in the actual practice of science is wrong. I will look at a passage in *The Dappled World* in which Cartwright intends to justify the use of capacities instead of laws. Her justification – irrespective of what role she intends for them to play in the practice of science – is based entirely on a simple misunderstanding, not unlike that which we had seen when analyzing her writing earlier. Not for the first time in this paper, I will show that Cartwright would have fared better had she simply paid more attention to the proper formulation of a law.

Cartwright claims that Newton's principle of gravity and Coulomb's law can in conjunction with F = ma be used to "… explain the trajectory of a charged body." (Cartwright 1999, 65) She claims that for this to be effective, we must suppose three things:

> First, that there is nothing that inhibits either object from exerting both its Coulomb and its gravitational force on the other; second, no other forces are exerted on either body; and third, everything that happens to the bodies that can affect their motions can be represented as a force. Notice that these caveats all have to do with capacities and their exercise. Nothing must inhibit either the charges or the gravitational masses from exercising their capacities. (Cartwright 1999, 67)

There are many errors in this brief passage; I will look at them one by one. The first sentence is simply technically confused. Cartwright worries that there must be nothing that prevents either object from exerting its forces on another object. Well, she need not worry about this insofar as *no such thing is possible*. There is nothing that can prevent any object from exerting its gravitational force on another object. There is no such thing as a gravity shield. There is nothing that can shield an object from a Coulomb force. Of course, any given charged object might be attracted by a charged object in one direction and that attraction be cancelled by the presence of a charged object that provides a balancing force the other way. This idea is accounted for perfectly (as we will see in the next paragraph) in F = ma. But while this balancing charge is possible, that is not the same thing *at all* as providing some sort of method to shield one object from another's gravitational pull.

Her second worry that no other forces are exerted on the bodies is legitimate only if it is assumed that the problem is about only gravity and Coulomb forces. This stipulation shows a lack of understanding about the nature of the equation F = ma. In this equation, the F is actually F_{net}, as many not particularly sophisticated sources will reveal. Other forces are not a problem if they are simply accounted for. I fail to understand the confusion here. Cartwright is herself talking about two distinct forces. It is not a problem for F = ma that a third, or fourth, or fifth force be introduced. She considers a body that is being acted on by gravity and Coulomb forces, and this is indeed a proper domain of F = ma. The rendering of the equation in this circumstance is simple:

$$F_{gravity} + F_{Coulomb} = ma$$

Let us – to address Cartwright's concern – introduce other forces. Suppose we introduce the fact that not only is this body massive and charged, it is also magnetic and currently under the influence of an expanding spring. Cartwright seems to think that this would invalidate F = ma for this situation. This is a mistake. The rendering of F = ma for this situation is only slightly more complex:

$$F_{gravity} + F_{Coulomb} + F_{magnetic} + F_{spring} = ma$$

Some might say that there is more than one force here, so F = ma does not strictly apply. That cannot be the case even for Cartwright; it is she who introduces multiple forces into the scenario. That would be wrong in any event. The above equation could be rendered just as easily (and often is in high school texts) as:

$$F_{gravity + Coulomb + magnetic + spring} = ma$$

Cartwright is correct to say that one can use F = ma to analyze the motion of a charged and massive body only if no other forces are present, but only trivially so. If one is discussing a charged and massive body, one has good reason to believe that there are no other forces present. Otherwise one would be discussing a charged, massive, magnetic, sticky, etc. body.

Her third concern is also technically confused. Cartwright says the situation is valid only if everything that happens to the bodies that can affect their motions can be represented as a force. I am not aware of anything that can affect a body's motion that is *not a force*. The idea that a thing that exists can affect the acceleration of a body with mass and not *be* a force is incoherent.

Cartwright's notion of capacities seems to be rooted somehow in the idea that F = ma is a valid understanding of the world only where *motion* is concerned. She says:

> "… the relevant vocabulary of occurrent or measurable properties in this case is the vocabulary of *motions* – positions, speed, accelerations, directions and the like. But there is nothing in this vocabulary that we can say about what masses do to one other." (Cartwright 1999, 65) (emphasis added)

This may or may not be true, but even if it is true, it is true in an unimportant way. One can always provide a list of words based on a particular criterion and claim that it does not apply to a given situation. What is crucial is that the criterion be valid. Here, Cartwright's distinguishing criterion is based on a mistake. The notion that F = ma applies only to situations where motion is concerned is false. I suspect that the practitioners of an entire branch of physics called *statics* would be very surprised to hear Cartwright's argument. I am sure that all the world's architects, bridge builders, and crane designers would also be very surprised. Any time a structure that must support a given load is designed, or an attempt is made to stabilize a charged particle in an electric field, or the spring constant of a spring in a high school physics class is determined, F = ma is used. It simply the case that the situation is intentionally contrived so the *a* in F = ma is equal to zero. When the *a* from F = ma is equal to zero, that means that the beam is supporting the load so that it does not *accelerate* to the ground below, or the electron does not *accelerate* out of the carefully balanced electric field, or the mass in question is hanging statically (that is, the $F_{net} = 0$, $- F_{spring} = W_{mass}$, and so the *acceleration* = 0) at the end of a spring. In each of these cases, Cartwright is shown to be mistaken. The idea that these words she has listed do not apply to static situations is simply wrong. A static situation is *by definition* one in which the velocity and acceleration are equal to zero. There is no other way to define the word in its physical sense. This vocabulary is completely coherent and eminently useful.

This mistaken notion that things must somehow move in order for F = ma to be applicable as a law instead of a capacity stems, I suspect, from a confusion of the notions of *impeding* a force and *balancing* a force. As I stated earlier, nothing can impede a fundamental force of nature. Nothing can interfere with its ability to interact with the universe. Cartwright is confusing this notion with the notion that an object's *motion* can be impeded. In this Cartwright is correct: it is possible to impede the *motion* of an object. But the only way to impede the motion of an object is not to prevent a fundamental force of nature from being manifested, but to provide a balancing force. Cartwright says that F = ma is a true regularity only if the caveat that is added that it "*operates unimpeded*." (Cartwright 1999, 71) Until Cartwright can produce a gravity shield, or a Coulomb shield, or a strong or weak nuclear shield, it is simply a fact that forces will *always* operate unimpeded. It is just the case that they may be balanced by other equally unimpeded forces.

Again, there is nothing in F = ma requiring anything to *be in motion*. Her concern with the idea that F = ma is necessarily about motion leads to her failure to consider the fact that charged or massive bodies can exhibit their *capacities* without anything having to be in motion in a particular framework. Cartwright thinks of gravity, charge, etc. as having the capacity to cause *motion*, and it is motion – and only motion – in this situation to her that are "…permanently occurrent or directly measurable properties". (Cartwright 1999, 65) As I said earlier, Cartwright is wrong to claim that the vocabulary she has picked does not apply to static situations, but even if she were correct, it would not mean that such a vocabulary could not be developed. If one understands that the physics embodied in the equation F = ma can manifest itself in static situations, one can add several very useful words to her list: flexion (loaded steel truss), warping (capacitor plates), sag (catenary wires), extension (springs), etc. In all these instances, properties that are "permanently occurrent or directly measurable" are manifested in absolutely unambiguous ways. One can also observe a *lack* of motion.

Whatever the philosophical merits of Cartwright's capacities and nomological machines, they simply do not provide us with a reason to stop using those things we have heretofore called *laws* in the daily practice of science. Cartwright says that;

> "[t]here is no one fact of the matter about what occurrent properties obtain when masses interact. But that does not mean that there is no one thing we can say. 'Masses attract each other'. That is what we say, that is what we test, in thousands of different ways; and that is what we use to understand the motions of objects in an endless variety of circumstances." (Cartwright 1999, 65)

It is very true that there is no one fact of the matter about what happens when masses interact, but I do not recall any philosopher or scientist who ever said there was. And whether or not "masses attract each other" depends entirely (as I'm sure Cartwright would be the first to point out) on whether or not these masses also carry a charge (or are magnetic) and to what extent this charge overwhelms their mutual gravitational attraction. It is odd to me that Cartwright who seems to regard UG (and other such laws) with such suspicion seems to regard "masses attract each other" as a somehow better formulation. In any event, she is wrong. *Capacities* such as "masses attract each other" will never replace natural law in the actual practice of science. It is not "masses attract each other" that we use to describe the "motions of objects"; it is Newton's Laws of Motion, Universal Gravitation, Coulombs Law, F = ma, and various relativistic and quantum mechanical *laws*. And it is emphatically not "masses attract each other" that we "test", as Cartwright claims. People do not need science and its laws to test such a proposition. One can simply drop an apple or play with a balloon rubbed on a child's head. These simple activities will show that sometimes – even reliably in many different circumstances – "masses attract each other". How would a scientist test "masses attract each other"? Since the meters and timers and force gauges, etc. used in such a "test" would need no numbers on them ("masses attract each other" is purely qualitative), one wonders what they would look like. Cartwright's suggestion has nothing to do with those things we have known as laws. Just ask an actual scientist at CERN how far she would get in a day's work with a supercomputer, a particle detector, a 12 billion dollar accelerator, and "masses attract each other".

A Day in the Actual Life of an Actual Scientist

This actual scientist at CERN (or The Mayo Clinic or The Human Genome Project) I mentioned is important. I find it odd that after having read literally hundreds of pages of philosophical papers on the subject of natural law in the sciences, I have read essentially nothing about what it is this woefully neglected person actually *does*. The philosophical literature about natural law is perhaps very sophisticated, but, in the words of Ernst Mayer, it all seems to have “little relevance for the working biologist.” (Mayr 1985, 37) Indeed, it seems to have little relevance for any working scientist of any stripe. Why do philosophers of science spend so little time discussing actual scientists and what it is they actually *do*? And by “what it is they actually do”, I do not mean the broad sweeps of historical narrative – even when they pertain particularly to a Galileo, a Newton, or an Einstein. I mean, instead: what is it that the actual scientist does between putting on her lab coat at 9:00 a.m. and her lunch break in the lounge? I suspect that philosophers discuss the issue so little because what philosophers do – as Mayer said – has so little to do with what scientists do. (This, of course, is our fault and is not beyond remedy.) That *anyone* is even seriously considering the practice of science without laws is a glaring example of this phenomenon. Of course, very few philosophers *are* actually proposing to do science without laws. Giere certainly is not. Rather, philosophers like Giere seem to me to be avoiding what James Woodward refers to as “substantive issues” and are instead enmeshed in “terminological disputes … about how permissively we should use the word ‘law.’” (Woodward 2001, 4) (As I have said before, these disputes may or may not be important, but they have nothing to do with actually abandoning the notion of scientific law.) Of the literature I have read, only Cartwright offers an actual candidate for an entity that could replace laws. We have seen how untenable that suggestion was. Cartwright’s suggestion would not get our scientist to her lunch break, and explaining why requires much less philosophical sophistication than we have seen so far.

What is it that a scientist actually does in the course of a workday? I will not say that they do “science”; considering the disputative environment of the philosophy of science, that would be to say essentially nothing. Rather, we should ask: what are a few of the *actual tasks* they perform, and what tools will they use before 5:00 o'clock? Imagining a particular environment might be helpful. The Large Hadron Collider (LHC) is due to come online this year (2008). One of the primary objectives for the Collider is to find the Higgs boson. What is it that the CERN scientists will actually *do* in order to make this discovery once the machinery has been set in motion? They will *not* type "masses attract each other" into their computers and wait for the result. It is rather more complicated than that. The Higgs boson will have properties. These properties vary according to what version of the standard model you adhere to. Some instances of the standard model even call for more than one Higgs boson – sometimes as many as five. But whatever properties the Higgs manifests, how will the scientists be able to say with some certainty that they have found (or perhaps failed to find) the Higgs boson? They will do it using the *laws* of physics.

One property of particles, for instance, is charge. Charges of elementary particles are positive, negative, or neutral. Scientists can distinguish among these because – for a given magnetic field – a moving positively charged particle will be deflected in one direction, a moving negatively charged particle will be deflected in the other direction, and a moving neutral particle will not be deflected. These are actual facts, known to be true because they are observed, countless times, by people who are actually *doing* things. These observed, factual phenomena are represented directly in the laws of physics, and without even touching mathematics, we already have three times the information provided by "masses attract each other". And it gets, of course, much better. Another property of particles is mass. Scientists will use the *laws* of physics to account for any given particle's behavior owing to its mass as it interacts with the LHC's detectors. The fact that the particle may have a certain charge and a certain velocity in a given magnetic field will allow the scientists to determine the force exerted on the particle. Using $F = ma$, the scientists can combine all this information to determine the particle's mass. The relevant equation is in any good freshman-level physics text. It looks like this:

$$F = qvB \sin \Theta$$

where **F** is the force on the charged particle, **q** is the magnitude of the charge, **v** is the velocity of the particle, and **Θ** is the angle (relative to the magnetic flux lines) at which the particle enters the field. Using **F = ma** and substituting for **F** in the above yields this:

$$ma = qvB \sin \Theta$$

and we have, without leaving the realm of the freshman college physics student, rendered "masses attract each other" *provincially insignificant*.

Of course, the scientists at CERN will be using much more sophisticated versions of these equations that account for all sorts of technical and scientific factors. For example, relativistic adjustments must be made to account for these particles insofar as they will be traveling near the speed of light. Other complications understandable only to experts in the field will no doubt be handled. And, of course, these physicists will not be punching numbers into handheld calculators at the end of each data run. High-speed computers will analyze terabytes of data that would take all the world's scientists millennia to do by hand. But these scientists *will* take a personal look at candidate traces that the computer selects from the billions of events recorded in the detectors. These scientists will look at deflection angles, radii of turns, originating and annihilated particles, etc. These are the things they will actually *do*, and not a single one of them will question whether or not they should be using the laws of physics to inform their investigations. Not a single one of them will think – since they are refusing to frame their knowledge in the light of "capacities", as Cartwright urges – that what they are doing is "pure guesswork", as Cartwright suggests. Not a single one of them will note that "masses attract each other". "Masses attract each other" is useless to these scientists. What would they *do* with it?

The same argument could be made for other practical endeavors that occupy the time of working scientists. Working chemists are going to use the conservation of mass and the atomic theory of matter to account for leftover reactants, to verify the expected densities of new chemicals, or perhaps to explain why this particular product *smells* so bad. Nothing any chemist does in any laboratory would make the slightest bit of sense without these two laws. Working biologists are going to expect that 25 percent of offspring (on average) are going to have a mono-genetic recessive trait if each parent is known to carry one copy of that gene. And so they will make marks on clipboards and enter things into laptops and are going to know that if the expected 25 percent are not present in the next generation, something is up. I will insist on asking the question again: without conservation of mass, the atomic theory of matter, Mendel's law, etc. what would working scientists actually *do*? How would they occupy their time?

The same argument applies to the engineers and technologist who make the instruments and machinery that scientists use (from billion-dollar atom smashers to hundred-dollar scales) in order to make new discoveries that the next generation of engineers and technologist and scientists will use. When an electrical engineer is deciding which resistor to put in series with an MRI machine's indicator lights so as not to burn them up, he is not going to use some "capacity" such as "different materials affect the way electrons flow in various ways" (to render my own Cartwright-esque version of a *capacity* relating to electricity). He is not going to use Ohm's Nomological Machine, either. He is going to use Ohm's Law. When a scientist is deciding which spring to put in torsion meter, he will not use a "capacity". He will use Hooke's Law. Scientist, engineers, and technologist actually *do* things in the course of a day, and they do things using laws. It is my insistence on this perhaps too obvious *fact* that will lead to my conclusions in the next section.

The Pragmatic Approach

So far, I have addressed two issues: (1) I have argued that there are – as yet, anyway – *no* laws that conform to the strict normative idea of a law as universal and exceptionless. All laws must be qualified to some extent in a spatial, temporal, or spatio-temporal way. And (2) that the idea of conducting the actual practice of science in its quotidian manifestation without those things we have hitherto called the *laws* of science is problematic at best. It would leave the scientists with very little to actually *do*, and very little to say about the little they had done. We are led logically to this conclusion if we want to continue with science: since we cannot do science without those things we have called laws, and since the most prevalent notion of what counts as a scientific law is simply wrong in that its adherents can produce no such entity, we must accommodate the situation by modifying our notion of scientific law. This conclusion raises the obvious question: How do we modify our conception of scientific law? I favor a pragmatic approach. As I said earlier in this thesis, very little has been written pertaining to the notion of natural law in geology. On the other hand, a great deal has been written on the subject of natural law in the biological sciences, and I hope to use the progress that has been made in this area to explore the possibilities of natural law for geology. But before we adapt these pragmatic ideas to geology, we should take a close look at them in their own environment, biology.

I will look first at Sandra Mitchell's argument, presented in her 1997 paper "Pragmatic Laws", for whether or not there are laws in biology. Since I favor her approach in a general way and will use a specific criterion she suggests, it is appropriate to review her position. Mitchell claims there are "three strategies for pursuing this question: a normative, a paradigmatic, and a pragmatic approach." (Mitchell 1997, S469) She asserts that the normative and paradigmatic approaches are inadequate to the task of explaining the role of scientific law in the biological sciences insofar as they fail to examine the *use* of these entities in biology. Only the pragmatic approach "… focuses on the *role* of laws in science, and queries biological generalizations to see whether and to what degree they function in that role." (Mitchell 1997, S469) Mitchell is right that the normative and paradigmatic strategies are insufficient to determine whether or not biology has laws. To see why, each approach will be discussed briefly in its turn.

I will review Mitchell's assessment of the normative strategy first. According to Mitchell, the normative strategy simply takes a definitional account of scientific law and compares various biological generalizations to this normative account to see if they meet the criteria. Since this normative account usually entails "universal generalizations" (Mitchell 2000 258), no generalizations of the biological sort measure up, and therefore biology is not considered to have laws. There is a problem with this demarcation criterion for lawfulness, though, and it has been much discussed in this paper already. It need not be covered again in detail here, but a quick reminder might be helpful. If one posits "universality" as a criterion for lawfulness, then *no* generalizations (even in physics) will measure up. *Universality* – insofar as it is an absolute term – can be used only to question the status of all laws, not just those in biology. Universal/not universal is an uninformative false dichotomy in that there are no examples of the former.

Another aspect of the normative tradition, according to Mitchell, is that laws must be non-contingent. Beatty, for instance, insists that "[a]ll generalizations about the living world are just mathematical, physical, or chemical generalizations … or are distinctly biological, in which case they describe *contingent* outcomes of evolution." (Beatty 1995, 47) (emphasis added) and that "there are no laws of biology. For whatever 'laws' are, they are supposed to be more than just contingently true." (Beatty 1995, 46) Beatty is right in that the "laws" of biology, should we decide there are any, are contingent on the evolutionary process. It is not necessary, after all, that Mendel's law of gamete separation be what it is. In fact, the mere presence of life in the universe may be wholly an accident of this particular planet at this particular time. We have no evidence to the contrary. Stephen J. Gould has said famously that evolutionary history is like a video tape that – if played over again from the beginning – would have a different outcome every time. If so, and there is no reason to believe that it is not so, then Gould and Beatty are right to claim that any particular feature of life on Earth (e.g. mammals have hair) is historically contingent. These things are all no doubt true and *entirely beside the point* if one's goal is to find a demarcation criterion to distinguish the scientifically lawful from the scientifically lawless. Beatty's assignation of contingency to biology is not wrong; it is simply far too limited. Every aspect of the universe in which we live is historically contingent. It may have *all* been some other way. The mere existence of matter is – for all the evidence to the contrary – a matter of historical contingency. A very slight asymmetry between the production of matter and anti-matter in the very early universe accounts for all that we see today: stars and planets, and genes and chromosomes. It might have been that there was perfect symmetry between matter and anti-matter in the early universe and, if so, there would be no galaxies or stars, let alone beetles or gametes. Just as with the video tape of evolution on Earth, we have no idea whether or not the Big Bang, were it to be "played over from the beginning", would come out the same way.

On the other hand, it might be the case that there simply is no other way our universe could be structured. If this were discovered to be a fact, there would at long last be a valid distinction between some laws and others that is more than just a matter of degree: historical contingency. But since no such fact is in evidence, the simple presence of matter in this universe may very well be no more than an accident. If so, then UG and Newton's Laws of Motion must also be historically contingent. Should the adherents to non-contingency as a criterion for lawfulness be willing to admit of degrees, I would agree that the presence of matter in the universe is less contingent than the presence of the ruby throated humming bird, but I would insist that it *was* a matter of degree, not kind. Should they, however, insist on absoluteness, they would be rendering contingency a moot point. No aspect of the universe can be conclusively shown to be necessary. Like the concern over the CP clause, the contingency thesis is not wrong; it is simply not – as yet – a distinguishing criterion. It is, rather, another uninformative false dichotomy.

While the universal/not universal and the contingent/not contingent false dichotomies are distinct, they have a common ancestor. According to Mitchell the "…notion of natural necessity was fashioned from the cloth of logic." (Mitchell 1997, S470) She says that:

> "Logical necessity is an all-or-nothing affair. Either a statement's truth follows necessarily from the truth of a set of statements, or in virtue of its form, without exception and all times, or it does not. The security of expectation warranted by logical necessity may be comforting, but that security does not get carried along when expropriating the notion of necessity to the natural world." (Mitchell 1997, S470)

Mitchell is right. It would be great if we could pick out neat and orderly laws with simple equations that apply universally to the world, but that does not seem to be the way it works. Some scientific generalizations apply widely, some only in narrowly defined situations, and none everywhere in all circumstances. Only by abandoning the normativist tradition can we come to appreciate the way laws actually work in science and "… move out of the dichotomous space inherited from logical definitions into a continuous domain of kinds and degrees of contingency that may be exhibited by scientific generalizations." (Mitchell 1997, S470)

Mitchell's second analysis is of what she calls the paradigmatic approach. Using this method, we simply gather together laws we consider paradigmatic – those that fit best what we think the ideal law of science is – and see how the generalizations in biology compare. The first problem Mitchell highlights is that "lumping together all the exemplar laws of physics undifferentiated" (Mitchell 1997, S475) is not helpful. To assume that all laws of physics meet equally well *whatever* criteria one chooses to judge by is untenable. For instance, Boyle's Law falls far short of being as widely applicable as general relativity. Second, Mitchell points out that:

"while taking physical laws as paradigmatic and comparing them with biological generalizations is a useful enterprise, it leaves open the philosophical question of what a law of nature is." (Mitchell 1997, S475) As I said, Mitchell is correct to raise these concerns. This approach works only if one can show that the laws of physics used for comparison *themselves* meet the paradigmatic ideal of a scientific law. To compare the laws of biology to the laws of physics is fruitless because *even the laws of physics* fail to be paradigmatic exemplars of scientific law as that concept is commonly understood. This enterprise would be more useful in proving that the laws of physics are *not* laws than it would be to prove that the laws of biology *are* laws.

Finally, Mitchell presents her choice for the most productive approach to understand the nature of law in the biological sciences: the pragmatic approach. She says, "Taking a pragmatic approach to scientific laws replaces a definitional norm and multiple exemplars with an account of the *use* of scientific laws. How do they function in experiment, in education, or in engineering?" (Mitchell 1997, S475) This emphasis of the word *use* in the quotation is Mitchell's, and it is exactly the word I would have chosen to emphasize had she not already done so. I will argue in Part III that it is the *use* of the generalizations I will discuss that warrants their status as laws. But to make this argument compelling, it is essential to demonstrate that these generalizations are indeed *useful*. Mitchell's approach for defending the usefulness of a given generalization is to provide a list of pertinent criteria by which they can be judged. I like Mitchell's method of constructing a straight-forward list to evaluate the usefulness of a given generalization, but I find her particular list unsatisfying. I will assemble a list of my own that I feel provides simpler criteria, and therefore more starkly demonstrates the usefulness of these geologic laws. One criterion will come from Mitchell's list, another is of my own devising, and a third will be borrowed from Jim Woodward and is discussed below.

In his 2001 "Law and Explanation in Biology: Invariance is the Kind of Stability that

Matters", Jim Woodward claims that it is *invariance* – not adherence to the normative conception of natural law – that best speaks for a generalization's status as law. As I understand it, Woodward's notion of invariance is as follows: How well does a given generalization hold up when the initial conditions for its instantiation vary? He uses Hooke's Law to explain. Hooke's Law states that for a given spring, a restoring force will be produced by the spring that is proportional to the spring's deformation from its relaxed state and in the opposite direction of the force that causes the deformation. It is a familiar equation:

$$F_{spring} = -kX$$

where k is a constant that varies with the qualities of the spring in question, and X is the amount of compression or extension. Every high school physics student knows this equation, and the good ones know that it applies only under limited conditions. For example, if the spring's temperature deviates significantly from room temperature, or if the spring is stretched outside its elastic range, the relationship breaks down. Woodward considers the explanatory power of this relation as "rather shallow" in that the relation breaks down with relatively trivial alterations of the initial conditions. In other words, the relation fails to answer a "[wide] range of what-if-things-had-been-different questions about the spring" (Woodward 2001, 11) Perhaps it is true that the explanatory power of Hooke's Law is limited, but I am more concerned here – as I indicated in the discussion on Mitchell's 1997 – about how *useful* a given generalization is. Hooke's Law works very well in the engineering sense, which, since one infrequently finds well-formed springs in nature, is all that should be asked of it. While Hooke's Law says nothing about *why* a spring behaves the way it does, it makes quite useful predictions about *how* a particular spring will behave even before it is even manufactured. It is not that I question Woodward's emphasis on explanatory power; there are various legitimate ways to approach the issue of natural law. It is simply that I have chosen for the purpose of this thesis to emphasize the *usefulness* of a generalization. But while I have chosen usefulness over explanatory power as a criterion for judging scientific generalizations, I will adopt Woodward's choice of *invariance* as a pertinent tool for evaluating usefulness.

Here at the end of Part II, I will do no more than to present the criteria I have chosen for evaluating the usefulness of scientific generalizations. I will not argue for their pertinence here. I feel their best defense is to see them applied to the individual generalizations. The list is:

Invariance: *A wide range of initial conditions leads reliably to the law's instantiation.* What would have to have been different for a given law not to obtain? We will see that these geological laws are quite impervious to variation over a wide range of initial circumstances and therefore apply very widely (after Woodward).

Degree of Accuracy: *The law in question must make accurate predictions.* Of course, what is or is not accurate is highly debatable, but most people would agree that physics is the paradigmatic science when the accuracy of laws is in question. We will look at one geologic law typically considered "just a rule of thumb" and see that, when correctly applied, it is as accurate as at least one paradigmatic law of physics already discussed (after Mitchell). (I should point out that the laws I will discuss concern mostly structural geology and infrequently admit of accuracy assessment in that they are either/or types of situations. That said, other laws of geology, and indeed scientific laws in general, vary widely in their usefulness depending on how accurate they are.)

Reliability: *It must be highly unlikely that a law that appears to have been instantiated has in fact not.* It is frequently claimed that the "laws" of geology are riddled with exceptions. For some geologic laws, this accusation carries *some* weight. But there are some laws used in geology that would require a highly contrived, fantastic set of circumstances for an exception to obtain. This fact makes these laws highly reliable.

A note of explanation here will be helpful. It may seem at first glance that *invariance* and *reliability* are similar criteria. They are not. With high invariance, a wide range of initial circumstances leads to the instantiation of a given generalization. With low invariance, a wide range of initial circumstances leads only infrequently to the instantiation of a given generalization. Woodward's example of Hooke's Law is a good example of low invariance – even trivial changes in the initial conditions render the law inaccurate, limiting it usefulness to contrived, engineering settings. The geologic generalizations I will discuss show high invariance, as will be seen.

I will use the notion of *reliability*, on the other hand, in a very specific way for the purposes of this paper. To check the reliability of some laws is reasonably straight forward. To check UG for reliability, for instance, one need simply release a ball bearing from one's grip a thousand times and note all the instances in which it fails to fall to the floor (always being careful to check for disturbing causes, such as the upstairs neighbor's recent purchase of a very strong magnet). One soon begins to suspect (*pace* Popper) that something reliable has been discovered. To check geologic generalizations, though, one cannot establish a set of initial circumstances and wait a million years to see if the expected results obtain. One certainly cannot do this thousands of times. Rather, one must analyze a given geologic environment, establish expectations based on geologic generalizations, and check those expectations against other evidence. A given geologic generalization is *reliable*, then, if it consistently makes predictions that are borne out by evidence from other geologic laws and the laws from other sciences such as physics, chemistry, and even biology.

Finally, I will not evaluate each geologic law with each criterion; some laws I will discuss simply do not admit of the aspects involved in the particular criterion. For instance, Cross-cutting Relations does not seem to be analyzable in the sense that it either is or is not accurate to so many decimal places. Original Horizontality, as another example, cannot be analyzed *directly* for reliability insofar as it makes claims about situations that by definition no longer exist. Since I do not adhere to the “universal, exceptionless” paradigm for scientific law, I feel it is not necessary that a given law be all things to all scientists. Like Mitchell, I view scientific laws as existing on a spectrum of usefulness rather than as an either/or proposition. Not only will a given law vary in its applicability and usefulness in an overall sense, it will adhere more or less closely to any given criterion.

But a (necessarily) small sample of geological laws will be scrutinized using those criteria I have suggested that are relevant to it. In Part III, I hope to convince the reader that these geologic generalizations are not only useful but indeed indispensable, that they are (when correctly applied) highly reliable, and that by virtue of their qualities, they are as deserving of the appellation *law* as any used in the other sciences.

Part III: Geologic Laws

In Part I, I argued that the common conception of scientific law as universal and exceptionless is misguided in that there are no such laws. My argument against the existence of universal laws was that all laws require some qualification in the form of a *ceteris paribus* clause. In Part II, I argued that the idea of practicing laws without science is untenable, and that those who claim to argue for this position fail – usually – in one of two ways: either they are (1) not, in fact, arguing for science without laws but are discussing semantics instead, or (2) they *are* arguing for science without laws but offer no viable alternative. In the second section of Part II, I argued that the notion of science without laws is fundamentally misguided and is made conspicuously so by a brief look at what it is that scientists actually *do*.

In Part III, I will present three geological principles that appeared in Nicolaus Steno's 1669 *De solido intra solidum naturaliter contento dissertationis prodromus* (The prodromus to a dissertation concerning a solid naturally contained within a solid), referred to hereafter as the *Prodromus*. They are: original horizontality, cross-cutting relations, and superposition. I will explain the general idea behind these principles (each a study in simplicity), a small amount of freshman geology necessary to understand them, and will then discuss their use in the actual practice of geology. I will show that they are much more than "rules of thumb" and that nothing any geologist does would make any sense whatsoever without them and similar generalizations. Using the criteria I established at the end of Part II, I will argue for the wide-ranging effectiveness of these generalizations. It is this usefulness, I will claim, that gives warrant for calling these geologic generalizations "laws".

Original Horizontality

Nearly everyone has noticed that rocks are frequently formed in layers. These layers (the geologic term is *strata* or the singular *stratum*) may be paper thin or meters thick. They may be barely distinguishable one from the other, or they may be strikingly different. But whether straight or curvy, vertical, horizontal, or somewhere in between, these often conspicuous structures raise an obvious question: why do rocks form in layers? There is no one answer, of course, but with the vast majority of rocks that form in these parallel bands there is a common factor: water.

Rocks are eroded (that is, broken into tiny bits) by various processes: freeze and thaw cycles, the action of plant roots, wind-driven sand, etc. The grains that result from these processes are carried along with the running water of streams and rivers, or rain water as it flows downhill. As long as the water is flowing quickly enough, the grains will continue to be propelled along. Once the water slows, however, it no longer has enough energy to transport the grains, and the grains will settle out of the water and be deposited on whatever substrate is already present. In this way, layers are accumulated. Adjacent layers may be very different from one another because the source of the eroded bits may change over time: from sandstone to siltstone, or from siltstone to igneous rock. It may take hundreds of years to accumulate a single centimeter of sediment, or a catastrophic event (such as the breaking of a natural dam) may deposit meters of sediment in literally minutes.

There are also other ways layers of rock can accumulate. Tiny sea creatures with shells die and settle to the bottom of the ocean, and their carcasses accumulate to form sometimes kilometers-thick strata. Such was the phenomenon responsible for the white cliffs of Dover. Changes in water temperature can cause dissolved minerals to spontaneously precipitate, and in this way many limestones are formed. Algae can form beautiful localized layers of rock that are responsible for structures called stromatolites. The list goes on and on, and this brief primer is very simplified, of course. A geologist can spend an entire productive career studying a single aspect of any one of these phenomena. Still, this primer is in its broad outlines accurate: water carries suspended bits of matter and for various reasons these bits fall from the water and accumulate to form layers that eventually become (roughly) parallel bands of rock.

But while the sources and results of this process are many and varied, at least one statement that is generally true can be made: when these layers first formed, they were more or less parallel to the horizon. This is the principle of original horizontality. As Steno says in the *Prodromus*, "…strata either perpendicular to the horizon or inclined toward it were at one time parallel to the horizon." (Steno 1669) The careful reader will have noticed that my presentation of original horizontality (hereafter, OH) contained two qualifiers: "generally" and "more or less". What do I mean by "generally", and "more or less"? Actually, I mean by them different things. I will cover what I mean by "more or less" first.

Since this thesis has at times been a deliberate effort at nit-picking paradigmatic laws, it is only fair that I subject my candidates for laws to the same rigorous treatment. First, or course, one must be clear by what is meant by the term "horizontal". We think of horizontal beds as being flat, but of course they are not really. Anything thing on the surface of the Earth that is truly flat is either the result of a deliberate engineering effort or a very unlikely accident. The curvature of the Earth is non-trivial – sometimes even in the engineering sense. (The towers of the Golden Gate Bridge, for instance, are about an inch further apart at the top than at the bottom due to the Earth's curvature.) Some strata are laterally continuous for hundreds of miles. Were they truly flat – that is, a geometrically straight line – they would stick out of the ground by miles. So we must mean something else when we say that beds are essentially horizontal. Since the Earth *is* curved, perhaps we mean more closely that these beds make up arcs of what would be nested concentric circles were these beds globally continuous. That might be closer to the truth, but we all know that the Earth is not a perfect sphere – its distortion away from sphericity due to its equatorial bulge far exceeds the height of any mountain. So this suggestion cannot be exactly right, either.

My point is that precisely defining the notion of horizontality is problematic. One should not simply assert the right to claim that the beds are more or less horizontal simply because it is often used colloquially. Is there any way to quantify the accuracy of a claim to horizontality – to put some numbers on it? There is, in fact. I will compare the accuracy of the claim of horizontality to the accuracy of a paradigmatic law of physics much discussed already in this paper: universal gravitation. I will show that the claim to original horizontality is at least as accurate as the predictions made by UG, and that using the phrase "more or less" is more generous to OH's critics than to OH itself.

What do I mean by "generally"? I demonstrated in Part I that all laws of nature are CP laws. OH is certainly no exception. There are, indeed, strata that are deposited at an incline. Lateral accretions surfaces, for instance, are formed at the bends of rivers as sediment is deposited on the slope of the river bed as the bend migrates transversely. As a result, these deposits are conspicuously *not* horizontal when they are formed. Alluvial fan deposits (the sediment that drops out of a river as it slows to enter a still body of water) are frequently tilted to a small number of degrees. Sand dunes are often preserved with their sometimes steeply sloping sides beautifully intact. There are other exceptions, but they all have one thing in common: they are nearly always extremely localized, occurring usually over a distance of meters, sometimes tens of meters, and unusually hundreds of meters. But they are *never* geographically extensive. Since I am a believer in CP laws (insofar as there is no other kind), I propose, with the help of Steno and in italics, my CP clause for OH: "*Geographically extensive* strata either perpendicular to the horizon or inclined toward it were at one time parallel to the horizon". Of course, one could debate the meaning of "geographically extensive", but it would be to little avail. The CP clause "geographically extensive" is very generous (like the phrase "more or less" above) to OH's critics. Typically, beds much smaller than those that cover entire states or, indeed, entire regions such as the Midwest proper (such beds exist), will be deposited horizontally. My CP clause for OH is very forgiving. But even with this very conservative and forgiving CP clause, it is still the case that the vast majority of the sedimentary rock coverage on the Earth's surface falls under OH's purview. This is what I mean by "generally". I will now analyze OH using the criteria I established in at the end of Part II.

OH stands up well under my first criterion, *invariance*. As was made clear in the brief primer I presented, many phenomena can cause the accumulation of sedimentary beds. Among these phenomena are mechanisms that are quite distinct one from the other. These processes can be the result of life or can be entirely abiotic. They can be the result of hydrodynamics or physical chemistry. Strata can accumulate in seconds or millennia. The material that makes up each bed – whether deconsolidated igneous rock, metamorphic rock, or sedimentary rock of any variety – has virtually no effect on whether or not the beds will form horizontally. Horizontal beds can form at the bottom of any still or slow moving body of water: swamp, estuary, lake, or ocean. Under the vast majority of the many and varied conditions under which sediment will fall from the water column, it will form horizontal beds. This geologic generalization answers very well Woodward's what-if-things-had-been-different question. Under a wide range of initial conditions, the result is very often the same: horizontal deposition of laterally extensive strata.

As was mentioned earlier, OH presents an excellent opportunity to speak for the accuracy of a geologic generalization. We will look at OH's accuracy by comparing it to that of another law much discussed in this paper: universal gravitation. This raises the question, of course, as to how one assesses the accuracy of UG. It is known that, strictly speaking, UG is false. It is also known that general relativity provides a more accurate method of calculating some phenomena for which UG fails to account. Relativity correctly (to the best of our ability to measure) accounts for the precession of the aphelion of Mercury's orbit. We can therefore assume that relativity has it just right, and use this assumption along with UG's predicted value to assess UG's accuracy.

Mercury, like all the planets, has an elliptical orbit. Not only is the orbit elliptical, but the ellipse *itself* rotates around the Sun over time. This phenomenon is referred to as the "precession of the aphelion." (The aphelion is one end of the ellipse – that is, when Mercury is farthest from the Sun.) The aphelion precesses for different reasons including the influence of the other planets and the imperfect sphericity of the Sun. When using UG, the calculated value for Mercury's aphelion precession is 5557 arcseconds/century. (In other words, it takes the ellipse of Mercury's orbit 233.22 years to complete one revolution.) This figure is known by observation to be incorrect. The actual figure is almost exactly 5600 arcseconds/century. General relativity accounts for the discrepancy. So we can use these numbers – 5557 arcseconds/century (off by 43) and 5600 arcseconds/century (accurate within measurement capabilities) – to calculate a percentage accuracy for UG's prediction of observable phenomena:

(5557/5600) x 100 = 99.2 % accurate

This means, then, that UG's prediction of observed, verifiable phenomena is off by .8 %. That seems pretty good. I would be willing to admit UG into the cannon of useful laws based on these numbers. (NASA has, of course, granted a *de facto* acceptance of UG no matter what we think of its accuracy.) The question, then, is this: Does OH display this same sort of accuracy? It depends on the usage. If one is using OH outside it domain – that is, trying to account for the original gradient of small, localized beds – then no. Used in this way, it can be off by an order of magnitude. But if OH is used only where it is known to be reliably applicable – large strata covering geographic regions – then we will see that it is very accurate. To demonstrate, we should discuss an actual formation.

The Morrison Formation (MF) is famous among geologists, both professional and amateur, as a particularly fertile source of fossils. It extends in a north/south direction from central Arizona to southern Canada. Conservatively, its north/south range is at least 1000 miles (depending, of course, on the exact direction of measurement). Remember, the claim that OH makes is this: when the MF was deposited, it was deposited horizontally. How accurate is this claim? One way to judge the accuracy of this claim would be to see what the result would be if the claim were *off* by a certain amount. I will define the worst case scenario and let that be an error of 100%. Worst case, of course, would be for the MF to have been deposited vertically, or 90^o off – that would be a 100 % error. If I use, instead, the error of .8 % that I derived for UG, I get the MF tilted at:

$$\mathbf{(.8/100) \times 90^o = .72^o}$$

I will assume then, that the MF is not horizontal but is in fact tilted at .72 degrees. What would be the result of such a tilt? After all, .72 degrees does not seem like very much. But a little high school math will show that if the MF *were* tilted at .72 degrees, the results would be *rather* noticeable:

$$\mathbf{\sin(.72^o) = x/1000\ miles}$$

$$\mathbf{x = (1000\ miles)\sin(.72^o)}$$

$$\mathbf{x \approx 12.5\ miles}$$

To state in plain language, if the OH claim for the MF were off by the amount that UG is known to be off for Mercury’s aphelion precession, the northern end of the MF would stick out of the ground by about 12.5 miles. This equals 66,300 feet, or well over *twice* the height of Mt. Everest. This is, of course, not the case. To say that OH is just a rough rule of thumb is simply incorrect.

It is – when properly applied – more accurate than UG. It seems that OH holds up well under the scrutiny of my *degree of accuracy* criterion.

I will turn now to the *reliability* of OH. If used outside its domain of geographically extensive strata, OH is the most unreliable of the geologic laws I will cover. As has been discussed, many geologic phenomena – such as lateral accretion surfaces, sand dunes, etc. – result in deposition of local strata inclined many degrees to the horizon. It is these localized phenomena that cause many to feel that OH is riddled with exceptions and is therefore unreliable. But while it is easy to see why OH is frequently – and mistakenly – considered unreliable, defending its reliability is more conceptually difficult.

OH, *per se*, is difficult to analyze for reliability. Unlike the other geologic laws I will discuss, OH by definition argues for a physical situation that no longer exists. Since we cannot go back in time to verify that this particular bed we are observing – which is tilted at, say, 45^{o} – was *originally* (i.e. no longer) horizontal, one must verify the original horizontality of a bed by secondary means. The only way to accomplish this task is to demonstrate that this particular section of bed under scrutiny is part of a larger, geographically extensive, bed. Since there simply is no mechanism for depositing geographically extensive beds at a steep angle, this demonstrated correlation would be very good evidence that this particular section of bed was, in fact, deposited more or less horizontally.

To understand what evidence a geologist uses to say that a particular section of local strata is indeed part of geographically extensive group of strata requires another short primer. As has been said, geologic strata can cover very wide ranges, geographically speaking. Entire states, even the big Western ones, can be underlain by a single group of beds. Of course, it is not as if the beds are everywhere visible and as flat as state-sized parking lots. A given bed, such as the MF, in different areas, will be alternately buried deeply under mountains, exposed only in hillsides, cover wide areas on the ground's surface, or be eroded entirely away, lost forever to the geologic record. How, then, lacking a neat, orderly picture to look at, can a geologist be reasonably sure that this group of strata in New Mexico is the same as another group of strata in Montana over a thousand mile away? They do it by correlating characteristics. When observing the strata in New Mexico, they discover that Bed No.1 has fossils of type *X*, is *Y* meters thick, has mineralogical make-up *Z*, and a radiometric dating of *Q*. When they look in Montana, they find a similar bed that also has fossils of type *X*, is *Y* meters thick, has mineralogical make-up *Z*, and a radiometric dating of *Q*. Further, when looking at the beds in New Mexico, they find that Bed No.2 (just above Bed No.1) has fossils of type *A*, is *B* meters thick, has mineralogical make-up *C*, and a radiometric dating of *R*. So naturally, they go to Montana and find, over what they thought might have been the same bed as Bed No.1 in New Mexico, a bed that has fossils of type *A*, is *B* meters thick, has mineralogical make-up *C*, and a radiometric dating of *R*, and begin to be confident that they in fact have found another outcropping of Bed No.2, the same Bed No.2 they found in New Mexico. This is, of course, an idealized picture of the procedure. Things are more complicated in the real world of the working geologist, but this general procedure is carried out for dozens or hundreds of beds and the results for many outcroppings in many areas are carefully coordinated. One sees that for two sets of carefully observed strata to *accidentally* have all these many characteristics – same fossils, same minerals, same thicknesses, same radiometric dates, and same position relative to other similar beds – indistinguishably realized in unrelated beds would be wildly improbable. Since the reliability of OH is directly related to the reliability of strata correlation, finding that a bed originally thought to be geographically extensive and deposited horizontally was actually a localized bed deposited at a steep angle is unlikely. OH is a very reliable law.

Superposition

In the Prodromus, Steno states that: "…at the time when any given stratum was being formed, all the matter resting upon it was fluid, and, therefore, at the time when the lower stratum was being formed, none of the upper strata existed." While this language may seem a bit baroque to modern ears, the idea it conveys is simple. When a stratum is being formed, it is a fact that it must have something to form *upon*. If so, strata must necessarily be younger as they rise in elevation. This is a simple statement of the law of superposition.

Stated this concisely, with no qualification, the law of superposition bears little weight. If one considers the discussion on original horizontality in the previous section, the first problem with superposition becomes clear. When looking at strata that have been tilted 90^{o} to the horizon, one would have no way of knowing if the younger strata were to the right or the left. More problematic still is the fact that strata may be completely overturned so that they have regained horizontality. (This situation is atypical, but by no means unheard of.) In this case, the stratagraphic evidence is not only ambiguous, but is indeed misleading: the youngest strata would be on the *bottom* of this particular group. Like all scientific laws, superposition must be stated with a CP clause: *Younger strata are found above older strata when the strata are considered in their original orientation.* With the addition of this CP clause, superposition becomes much more reliable. Still, there are problems with even this qualified statement of superposition. There are geologic situations – not particularly contrived – that would be contrary to superposition as it is stated above.

Consider the situation in which a cave is slowly filled with sediment from an opening in the cave's ceiling. Ten million years ago, we have the empty cave, seen here in cross section in this highly schematic representation:

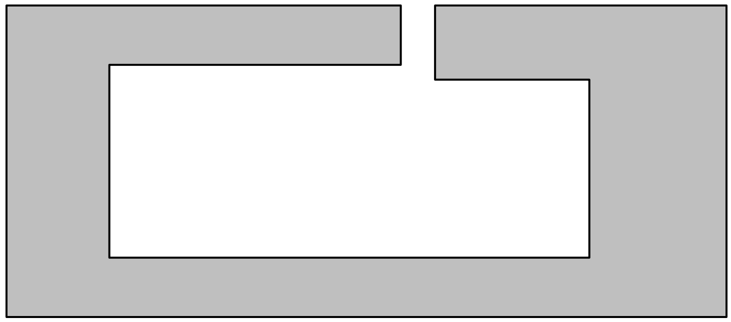

Figure 1

Over time, sediment is washed into the cave, and layer of consolidated rock forms on the cave's floor. This leaves us with this picture:

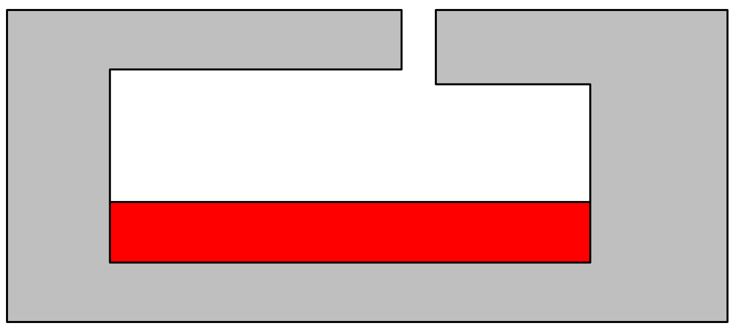

Figure 2

This process continues over geologic time and eventually results in a cave that is filled with sedimentary rock from various sources. In cross section, it would look like this:

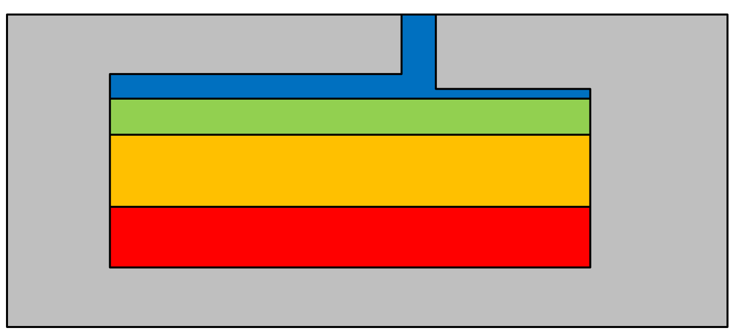

Figure 3

If a geologist were lucky enough to see this entire geologic situation in profile, it would be straight forward enough. As is almost always the case, though, the geologist here gets only a partial picture to observe. The entire cross section is not observable; only a small section has been exposed by erosion, and the geologist sees only this exposed sliver of the cave profile:

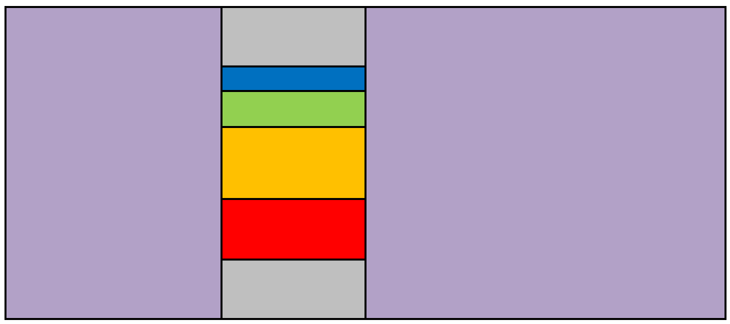

Figure 4

The problem should be immediately obvious. Any geologists interpreting this exposed cross section would have good reason to believe – due to superposition – that the gray layer at the top of the exposed sequence is younger than the blue layer immediately beneath it. This is not the case, however, and this situation has exposed another weakness with superposition: superposition is unreliable when used to interpret small areas or poorly exposed sequences. An additional CP clause will prove helpful and will yield the final rendering of the law of superposition: *Younger geographically extensive strata are found above older geographically extensive strata when these strata are considered in their original orientation*. The legitimacy of this well qualified law of superposition will be discussed at the end of this section. For now, I will discuss superposition's invariance and reliability as stated.

The statement of superposition as stated above is highly invariant. For much the same reasons discussed in the section on original horizontality, superposition is manifested in a wide range of geologic situations. No matter the source of the sediment or the manner in which it is deposited or consolidated, superposition holds for extensive strata in their original orientation.

Indeed, superposition holds well even for geologic phenomena outside sedimentary rock. For instance, sequences of volcanic flows – locally extensive, in their original orientation – can be reliably time ordered as younger above, older below.

Superposition as stated above is very reliable. Again, as with OH, it would require a highly contrived set of circumstances for an apparent instantiation of superposition to be shown false. For geographically extensive strata in the original orientation to be deposited out of the older-rising-to-younger time sequence would require a fantastic scenario. It would look, perhaps, something like the following sequence of events:

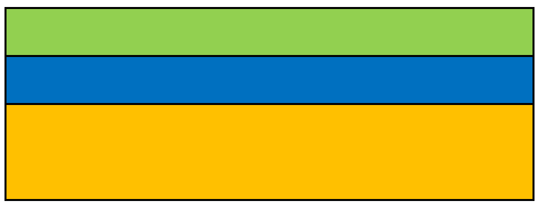

Figure 5

This geologic situation in Figure 5 is exactly as it appears: the gold layer is the oldest and the green layer is the youngest. Over time however, the blue layer is eroded away in its entirety:

Figure 6

and then is replaced by another layer of sediment that is deposited between the gold and green layers, yielding the situation seen in Figure 7:

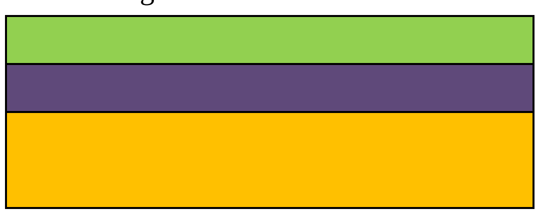

Figure 7

In Figure 7, the purple layer is actually younger than the green layer, seemingly contrary to superposition. This situation is possible (as has been shown previously in this section), but it possible only for localized phenomena. Suppose Figure 7 represented (out of vertical scale, of course) dozens or even hundreds of miles of laterally extensive strata. If the blue layer of Figure 5 were eroded completely away, how would the green layer of Figure 5 be mechanically supported? How would the sediment composing the purple layer of Figure 7 come to occupy the void? It might be the case that the blue layer of Figure 5 is intermittently eroded, leaving mechanical support for the green layer, like this:

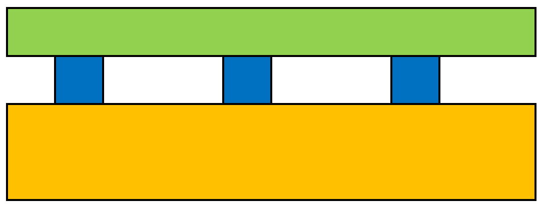

Figure 8

After this rather particular sort of erosion occurred, the purple sediment could be deposited around the bits of blue sediment, leaving this situation:

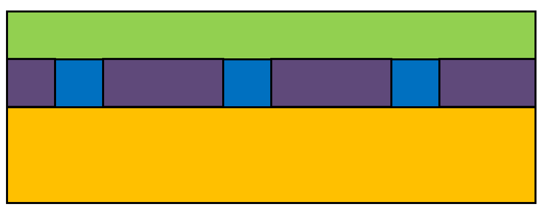

Figure 9

And if these peculiar layers of strata were eroded in just the right way, it might reveal a picture like the one in Figure 7. It should be made clear that these sorts of misleading scenarios are presented to geologists frequently, but they are nearly always on a small scale: meters or tens of meters. Limestones, for instance, are frequently eroded haphazardly and the voids are then filled with sediment from other sources. However, it would be physically impossible for entire layers of sediment to erode from beneath overlying layers and leave uninterrupted, geographically extensive voids intact. But if an underlying layer were erode spottily, and the intermittent voids were filled, it would be wildly improbable that any large exposure of these strata would not reveal evidence of the former intervening layer. Considering the confluence of unlikely events necessary for a misleading instantiation of superposition as I have stated it, superposition can be regarded as highly reliable. But this assertion raises the question: Is it philosophically tenable to pose the law of superposition in such a qualified manner? Has the law been rendered vacuous by the two CP clauses attached to it? *Yes*, I think, to the first question and *No* to the second.

As to the first question, I have shown that the notion of completely unqualified scientific law is problematic at best. Even exemplar laws in the natural sciences do not bear unqualified scrutiny. All scientific laws, as far as I have been able to discover, must have at least one CP clause. To draw the line at just one seems, to me, an arbitrary distinction. So then, must scientists allow two CP clauses, or three? The notion of multiple CP clauses is not contrary to my thesis. I have argued in this paper, al a Mitchell, that scientific laws exist on a continuum of accuracy, reliability, invariance, contingency, or other attributes by which other philosophers wish to judge scientific law. One CP clause leaves us with a law like relativity. Fifty CP clauses leaves with a law that is probably more suspect. There is no reason to assume that these are either/or situations. As I have shown, these sorts of dichotomies in the discussions of natural law are usually false.

As to the second question, I do not think superposition has been rendered vacuous by the two CP clauses attached to it. Boyle’s law, after all, has at least six CP clauses (discussed earlier in the paper) and it is never referred to as Boyle’s *rule of thumb*. The CP clauses I have proposed for Superposition are fewer than those necessary for the applications of Boyle’s law and, unlike those for Boyle’s law, are not necessarily counter factual. Boyle’s law (like UG) can literally never be instantiated. With superposition, on the other hand, it is at least *possible* to satisfy the conditionals.

Cross-cutting Relations

Rock formations relate to each other in particular ways. Unlike original horizontality, which speaks mostly to the relationship a given stratum had (when it was deposited) to the physical context of the *Earth*, cross-cutting relations (hereafter, CR) speak in particular about the relations that rock formations have to *each other*. Specifically, CR provides a powerful tool for the geologist to determine which of two geologic features came first. This is known as relative (as opposed to absolute) dating. These relationships cannot tell the geologist how old, exactly, a given feature is, but it can tell the geologist if a particular geologic feature is younger or older than other features in the same context.

It would be very difficult to adequately portray this technique without illustrations. I will provide a series of idealized illustrations that become increasingly complex as the explanation progresses. With only a few illustrations, we will have a simplified but reasonably accurate picture of the way this idea works. *This is very important*: initially, we will assume that the common interpretation that working geologists would give these scenarios is accurate if only to facilitate the explanation process. Later, we will entertain the notion that these interpretations might be mistaken and evaluate how likely that would be. Let us start with basic strata. Sedimentary rocks are deposited in layers that frequently look basically like the schematic below. The different colors represent different rock types:

Figure 1

We cannot tell which layer is oldest. As we saw in the superposition section, the entire bed may be overturned, putting the youngest rocks on the bottom, where we would expect the oldest to be. But there is one thing about which we can be very sure: the age of rock layer 2 is somewhere between the age of rock layer 1 and rock layer 3. It may be older than 1 and younger than 3 or vice versa, but one of these two situations is almost certainly the case. The same can be said of the relation between rock layers 2, 3, and 4: layer 3 is of middling age when compare to layers 2 and 4. Already, we can begin to form a very sketchy history of the area, but we have yet to enter the domain where CR applies. When we look more closely at the area, a few meters away to the left, we find another feature:

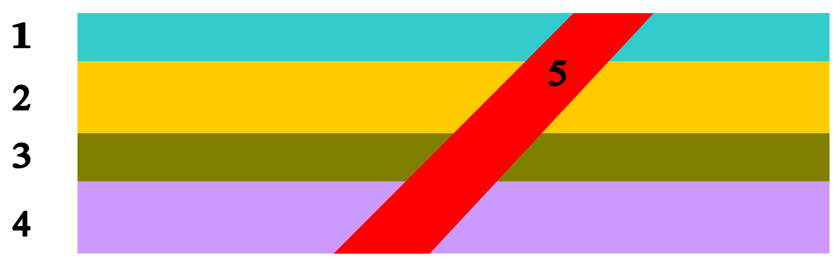

Figure 2

Feature 5 is what geologists call an *intrusion*. That is, magma from deep underground rose through a crack in the rock layers and hardened in the position in which we now see it. Again, we cannot be certain exactly what happened in this area millions of years ago, but now we have another bit of information about which we can be almost certain: feature 5 came after features 1, 2, 3, and 4. What makes a geologist think that this is so? Feature 5 is a single feature that cuts all the other features shown. How could feature 5 have cut the other features unless the other features had already existed? This is the basic idea behind CR. Two more examples – further complications of the scenario we have established – will help clarify the idea.

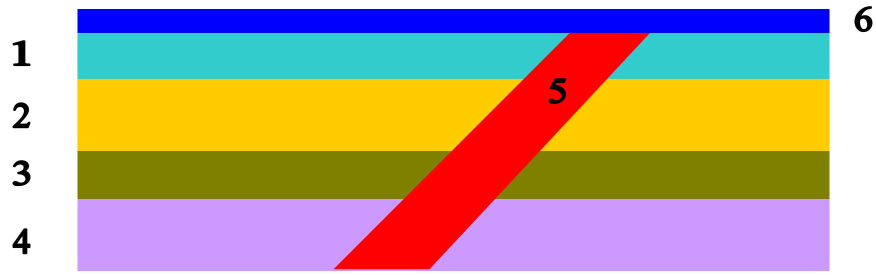

Figure 3

We have now introduced Feature 6. We can be very sure that 6 is younger than all the others. Why? It seems clear that 6 has cut 5. (Most probably, the surface between layer 5 and layer 6 is an erosion surface. The top of the strata we are seeing was eroded away, along with the intrusion, feature 5, and a new layer of sediment, layer 6, was deposited on the fresh surface. The relative age dating still holds for this explanation.) And since a feature cannot be cut unless it already exists, 6 must have come along after 5. If that is true, then 6 also appeared after 1, 2, 3, and 4, which have already been shown to be younger than 5. I will introduce one final feature to conclude our introduction to cross-cutting relations: feature 7.

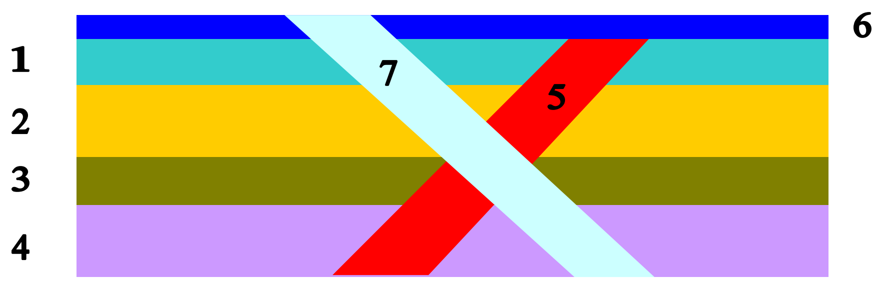

Figure 4

Feature 7 must be the youngest of them all because it cuts all the features that are present. Again, this is the basic idea of CR. As Steno says in the Prodromus, “If a body or discontinuity cuts across a stratum, it must have formed after that stratum." (Steno 1669) Today, Steno's idea of CR has expanded to include not only strata, but is considered valid when any geologic feature cuts another geologic feature. Of course, the scenario I have presented is idealized. An actual geologist would feel lucky indeed if he ran across a situation so simple and clear. As I said in the section on OH, a geologist can spend many years studying a single area in trying to work out the details of its history. Still, this representation is in its broad outlines accurate. Let us look at CR under the light of the criteria I have chosen.

My invariance criterion applies very well to CR for the same reasons it applied very well to OH; a wide range of initial conditions will result in similar instantiations of the law. In the brief primer I gave, I showed mostly igneous types of intrusions interrupting sedimentary strata. CR covers far more territory. All manner of geologic features such as intrusions, fault lines, erosion surfaces, precipitated crystalline dikes, bolide impact scars, channel scours, and many others, can cross all manner of *other* geologic features. The availability of many types of "cutters" and equally many types of "cuttees" leads to literally hundreds of absolutely distinct sets of initial conditions that result in instantiations of cross-cutting relations.

CR is extremely reliable. Like OH, the best way to argue for the reliability of CR is to examine how unlikely it would be for an apparent instantiation of the law to in fact not be an instantiation. To accomplish this, I will construct the type of scenario required to provide a misleading apparent instantiation and will show how wildly unlikely it would be. I will start with an illustration and the standard geologic interpretation.

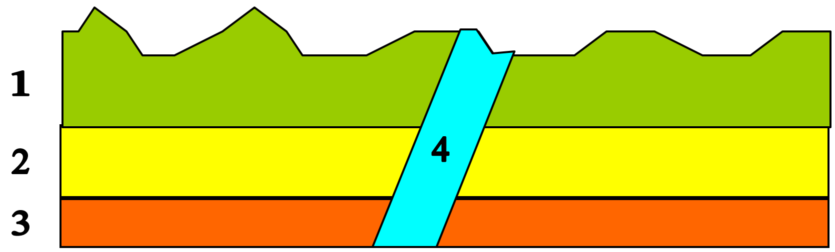

Figure 5

Bed 1 is a siltstone layer about 3 feet thick, Bed 2 is a mudstone layer 2 feet thick, Bed 3 is a sandstone layer 1 foot thick, and Feature 4 is a quartz vein two inches thick. (Obviously, not to scale.) The standard geologic interpretation of this scenario would be that Feature 4 was younger than either Bed 1, Bed 2, or Bed 3 because it cuts across them all. This assumption entails at least one further assumption: the beds on either side of Feature 4 that appear to be the same bed are in fact the same bed. We discover that in this case, though, we have been misled by CR. What would have had to happen for this to be the case? There are, of course, many scenarios that could lead to the physical situation pictured in Figure 5. Otherwise, CR would not have a high invariance. But we can explore one possible history for the misleading physical manifestation of Figure 5, and this will perhaps convince the reader that such scenarios are fantastically unlikely. Ten million years ago, this was the situation:

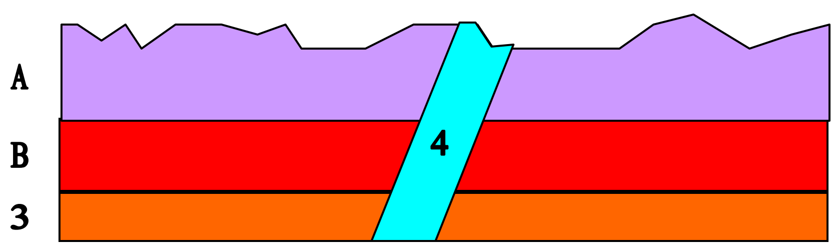

Figure 6

Unbeknownst to the field geologist studying this area, Beds A and B, not Beds 1 and 2, were *actually* present at the time Feature 4 was realized. Then, over time, Bed A and Bed B were eroded away leaving this situation:

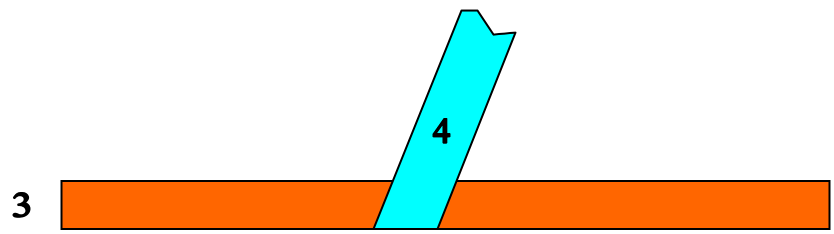

Figure 7

The reader should realize that this is a two-dimensional drawing of a three-dimensional situation. The quartz vein seen here (Feature 4) actually extends into and out of the page, making something physically similar to a tilted, two-inch-thick wall separating the left and right halves of Bed 3. After the erosion of Beds A and B, Bed 2 would have to be deposited on both sides of Feature 4, resulting in this:

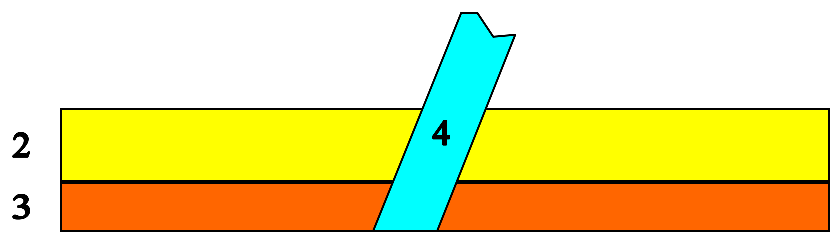

Figure 8

Finally, Bed 1 would then have to be deposited on both sides of Feature 4 over Bed 2, resulting in this configuration, the one that misled the field geologist in the first place:

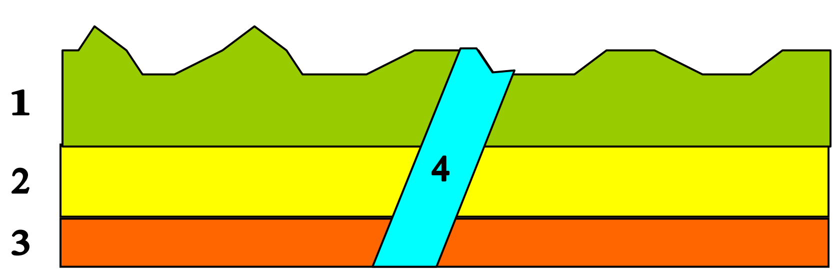

Figure 9

Following the scenario I have established, Beds 1 and 2 are actually *younger* than Feature 4, and CR has been shown to be misleading. I concede the point immediately that this scenario is possible. It is not precluded by logic or spatial geometry or the Earth's history. But when arguing for a law's *reliability*, one need not argue that it is impossible that it not be realized, only that it is highly improbable that it not be realized. The point to be demonstrated here, then, is that this scenario I have proposed is highly unlikely.

The first highly unlikely aspect of this scenario is that Beds 1 and 2 could be eroded completely away and yet the quartz vein (Feature 4) would have remained. There is nothing unlikely in geology about differential erosion – it is responsible for the appearance of much of the Earth's surface. But that entire large, lateral beds are eroded away leaving a relatively fragile two-inch-thick quartz vein intact is highly unlikely. Further, it is the case with rocks that they are – to a greater or lesser extent – week in tension (thus the need for closely spaced posts or arches in stone architecture). A two-inch-thick vein of any type of rock would not survive exposure – jutting into the air – long enough for Beds 1 and 2 to be deposited around it. On the other hand, if Bed 1 was quickly deposited around Feature 4, it would have to be a violent geologic event, and Feature 4 would have needed the mechanical wherewithal to withstand the forces associated with violent geologic events. Further, if Beds 1 and 2 are distinct beds – the case in this scenario – then that means that Bed 1 would have required time to consolidate and lithify before Bed 2 was deposited. This would require that Feature 4 withstand not only the mechanical stresses of a violent geologic event, but also unsupported exposure over literally geologic time.

There are other considerations. CR is supported in its various contexts by other geologic evidence. When a quartz vein is established in already existing beds, there is mineralogical evidence for this occurrence. Frequently, bits of the extant beds are deposited in the quartz vein as the action of the water flowing through the crack (a necessary occurrence for the formation of the quartz vein) erodes them. If the quartz vein was there first, this could not be the case. Absolute dating techniques can frequently allow for independent dating of the various bits of the geologic scene. These dates could belie the unexpected scenario. If Feature 4 were an igneous intrusion, Beds 1 and 2 would have what is referred to as “baked surfaces”. This is the case where exposure to high temperatures causes metamorphosis in the already established beds. This could not happen if the beds came after the intrusion. Any one of these occurrences is unlikely enough, but for all these various geologic events to happen in a single setting – all seemingly conspiring to produce a false instantiation of CR – is fantastically unlikely. CR is very reliable.

Conclusion

My aim in this thesis has been to argue for the existence of natural laws in geology. The most frequent objection to the notion that geology has natural laws is that the generalizations used in geology have exceptions. While this objection is true, it fails to serve as a distinction between the laws of geology and the laws of any other science. As I demonstrated in the first section, even the most respected laws of physics do not apply in all times, in all places, or in all contexts. It requires little scientific sophistication to demonstrate that entities unhesitatingly referred to as laws often require several stipulations to be “true”. Boyle’s Law, for instance, requires half a dozen qualifiers to be useful even in the engineering sense. Other paradigmatic laws of physics also require stipulation to a greater or lesser extent. Universal gravitation, Galileo’s law of falling bodies, conservation of energy, and even relativity require *ceteris paribus* clauses. I have shown that these laws are simply not universal and exceptionless in the absolute sense.

I do not intend the phrase “universal and exceptionless” to be taken in a manner that is too easily disqualified. Clearly, it is not reasonable to expect Hooke’s law to apply to gamete distribution, Bose-Einstein condensates, or original horizontality. It is obviously not universal in that sense. Hooke’s law applies only to springs – I would even allow that Hooke’s law applies only to “well-formed” springs – and should be expected to have little else to say about the world. It is my contention, though, that even once the domain of “well-formed springs” is established, *ceteris paribus* clauses must still be considered. Hooke’s law applies only when “well-formed springs” are operating within their elastic limits, are not mechanically fatigued, and are well away from both their melting points and their reference transition temperatures. Still, I have never intended to imply that the laws of nature apply only haphazardly to the world. Once a law’s domain has been established, and the necessary CP clauses attached, it can apply universally to phenomena *within these criteria*. Hooke's law may apply only to the small world of well-formed springs, but it applies to *all* such springs on any given day.

I stand by the assertion that the lack of unqualified universality is not a distinguishing criterion between the laws of physics and those of geology, biology, and other sciences. They all require qualification. And yet this thesis opens the opportunity for the exploration – a thesis in itself – of a criterion that perhaps *is* a distinction between the laws of physics and those of other sciences. Many philosophers of science have, in my view, painted themselves into an odd philosophical corner. Philosophers such as Earman, Roberts, and Smith hold in high esteem – and consider universal and exceptionless – laws that we know for a fact are *not even true*. Indeed, I have shown that at least one of these laws (UG) could never be instantiated even in ideal circumstances. And yet these same philosophers tend to dismiss as laws generalizations (such as cross-cutting relations) that not only *can* be true, but *usually* are. But to these philosophers' credit, physical laws such as the always-untrue UG have one quality that laws such as the rarely-untrue cross-cutting relations lack. They are always untrue, but they are always untrue in *exactly the same way*. They seem utterly reliable in their falsity. On the other hand, laws such as cross cutting relations are rarely untrue, but may be untrue in various ways. I find it remarkable that so little (to my knowledge) has been written on this aspect of natural law.

The concept of natural law simply cannot be explained by the dichotomous universal/not-universal evaluation in that there are no examples of the former. The issue is much more complex than that. We are left, then, with the options of (1) simply doing away with the notion of natural law in the practice of science or (2) modifying our conception of natural law. The former option is untenable. Cartwright's suggestion, for instance, that we replace natural laws with "capacities" is simply not practicable in the strict sense of that word. As I demonstrated in the second section of Part II, the replacement of natural laws with capacities would leave actual scientists in actual laboratory settings very little to do. I will insist on asking the non-rhetorical question again: what would a scientist at CERN actually *do* with the statement "masses attract each other". What tasks – specifics, please – could she *perform* with such a statement? It is simply not useful to a scientist. The quotidian act of removing a calculator from a breast pocket – never mind the presence of high-speed computers – renders all such talk vacuous. It is true that this objection applies only to mathematical laws, but the laws Cartwright offers for replacement with capacities are all mathematical. It is possible that Cartwright might formulate a useful "capacity" to replace a non-mathematical law like, say, superposition. Until she offers such a suggestion, however, we must be content to evaluate the usefulness of those capacities she has rendered. It would be unfair to speculate on her behalf.

Giere, for his part and despite titular claims to the contrary, fails to even argue the position that we should do away with natural law. Rather, his position is that we continue to *use* (his word) those things that we have hitherto – rightly or wrongly – called natural laws, but we henceforth refrain from referring to them as such. Giere raises an important issue. As I have said, words are important, and all manner of breathless hand waving might be avoided by a change in nomenclature. But while his argument may be right, it does nothing to support the notion that we should – or even could – do science without laws. Simply changing the way laws are referred to and then suggesting that we are no longer using laws is not philosophically robust.

We are left with the second option: modify our conception of natural law. If I have accomplished little else in this thesis, I hope to have convinced the reader that the dichotomous universal/not universal system for evaluating natural laws is not useful. The need for a more productive evaluative procedure seems clear to me. Since natural laws seem reluctant to conform to the neat and orderly criteria many philosophers (but fewer scientists, it seems to me) desire, we must adopt a system that accounts for the marginally messy reality with which we are confronted.

Any such system must admit of degrees of law-likeness, and I (after Mitchell) have argued for such a system. I have chosen the criteria of accuracy, reliability, and invariance (or wide applicability). Whether or not these criteria are philosophically profound is for each reader to decide, but it seems difficult to argue against a scientific knowledge structure built on natural laws that are reliable, accurate, and widely useful. Further, and perhaps even more importantly, these criteria readily admit of degrees but do not rely on the improbable concept of perfection in the natural world. Many things are unreliable, many things are very reliable, but very little is perfectly reliable. With these flexible criteria, scientists are free to place candidates for natural law on a continuum of law-likeness that suits the sciences as they are actually practiced. Hooke's law would be a small – but eminently useful – little law, very reliable in its domain. On the other end of the spectrum, relativity would have far ranging consequences and would help to explain just about everything concerned with the large-scale structure of the universe.

If we accept the evaluative procedure for which I have argued, the generalizations used in geology appear in a much more favorable light as candidates for natural laws. Geologic laws such as original horizontality, cross-cutting relations, and superposition are frequently dismissed as just rules of thumb. I have shown that this is simply not the case. Once the proper *ceteris paribus* clauses are attached, these laws are shown to be very reliable, widely applicable, and – insofar as the notion applies – accurate. For instance, some argue that original horizontality is only a rule of thumb because many geologic phenomena produce inclined depositional surfaces. It is true that geologic processes frequently produce inclined strata, but this fact does render original horizontality an unreliable rule of thumb if we simply attach a clear, brief (as would please Earman, et al.) *ceteris paribus* clause to the strata under discussion: geographically extensive. This simple addition to the statement provided by Steno hundreds of years ago renders original horizontality reliable in the extreme. And yet even though I have shown that original horizontality and the other geologic generalizations discussed here apply very broadly and reliably to the world, I make no claims that they apply perfectly to the world. No natural laws do. Only when we accept the imperfection of all the natural laws so far discovered can the generalizations of geology, biology, and the other sciences take their place alongside the generalizations of physics.

References

Barr, William F., (1974) “A Pragmatic Analysis of Idealizations in Physics” *Philosophy of Science*, Vol. 41, No. 1 pp. 48-64.

Beatty, John., (1995) "The Evolutionary Contingency Thesis." In Gereon, Wolters and James G. Lennox (eds.), *Concepts, Theories, and Rationality in the Biological Sciences*, The Second Pittsburgh-Konstanz Colloquium in the Philosophy of Science. Pittsburgh: University of Pittsburgh Press. Reprinted in E. Sober (ed.). In press. *Conceptual Issues in Evolutionary Biology*. Cambridge: MIT Press.

Cartwright, Nancy (1983) *How the Laws of Physics Lie*, Clarendon Press, Oxford, Oxford University Press, New York.

Cartwright, Nancy, (1997) “Models: The Blueprints for Laws” *Philosophy of Science*, Vol. 64, Supplement. Proceedings of the 1996 Biennial Meetings of the Philosophy of Science Association. Part II: Symposia Papers (Dec., 1997), pp. S292-S303.

Cartwright, Nancy, (1999) *The Dappled World: A Study of the Boundaries of Science*, The Press Syndicate of the University of Cambridge, Cambridge, U.K.

Earman, John, C. Glymour, S. Mitchell (2002) *Ceteris Paribus Laws*, Kluwer Academic Publishers, Dordrecht, The Netherlands.

Earman, John, C. Glymour, S. Mitchell (2002) “Ceteris Paribus Lost” in *Ceteris Paribus Laws*, Kluwer Academic Publishers, Dordrecht, The Netherlands.

Fodor, Jerry (1991) “You Can Fool Some of The People All of The Time, Everything Else Being Equal; Hedged Laws and Psychological Explanations” *Mind*, New Series, Vol. 100, No. 1 pp. 19-34.

Giere, Ronald N., (1999) *Science Without Laws*, The University of Chicago Press, Chicago.

Gonick, Larry, A. Huffman (1991) *The Cartoon Guide to Physics*, HarperPerennial.

Kline, David, C. Matheson, (1986) “How the Laws of Physics Don't Even Fib” *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association*, Vol. 1986, Volume One: Contributed Paper, pp. 33-41.

Lange, Marc (1993) “Natural Laws and the Problem of Provisos” Erkenntnis 38, 233 – 248.

Mayr, Ernst, (1985) *The Growth of Biological Thought: Diversity, Evolution, and Inheritance*, Harvard University Press, Cambridge.

Mitchell, Sandra D. (1997) "Pragmatic Laws" *Philosophy of Science*, Vol. 64, Supplement. Proceedings of the 1996 Biennial Meetings of the Philosophy of Science Association. Part II: Symposia Papers pp. S468-S479.

Mitchell, Sandra D., (2000) "Dimensions of Scientific Law" *Philosophy of Science*, Vol. 67, No. 2 pp. 242-265.

Newton, Isaac, (1687) *Philosophiae Naturalis Principia Mathematica.*

Pearson, Norman (1886) *Mind*, Vol. 11, No. 44 (Oct., 1886), pp. 563 – 569.

Schiffer, Stephen (1991) "Ceteris Paribus Laws" *Mind*, New Series, Vol. 100, No. 1 pp. 1-17.

Serway, Raymond A., J.S. Faughn (1995) *College Physics, Fourth Edition*, Harcourt Brace College Publishers.

Steno, Nicolas (1669) *De solido intra solidum naturaliter contento dissertationis prodromus.*

Woodward, Jim, (2001) "Law and Explanation in Biology: Invariance Is the Kind of Stability That Matters" *Philosophy of Science*, Vol. 68, No. 1 pp. 1-20.