THESIS

A PLURALISTIC ACCOUNT OF SCIENTIFIC EXPLANATION AND REDUCTION: LESSONS FROM HISTORY AND MULTISCALE MODELING

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ABSTRACT

A PLURALISTIC ACCOUNT OF SCIENTIFIC EXPLANATION AND REDUCTION: LESSONS FROM HISTORY AND MULTISCALE MODELING

This thesis analyzes the history of scientific explanation and theory reduction in the 20th century and offers a pluralist account of both explanation and reduction. The lessons from assuming nomological, causal, and multiscale approaches across a variety of disciplines push back against a monistic, theory-first account of explanation that ignores scientific practice. Instead, I argue that looking at examples from contemporary scientific practice demonstrates the need for a pluralistic model of scientific explanation with an incorporation of a reinterpreted conception of reduction. The pluralistic approach to both scientific explanation and reduction avoid the problems of historical accounts, accords with scientific practice and is sensitive to the epistemic constraints around explaining physical phenomena.

DEDICATION

To my wife Anna, whose love and support I could not do without. To my daughter Sophia, who entered this world when this project was ongoing and who teaches me new depths of love everyday.

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DEFINITION OF TERMS

Mechanism(s) – Physical entity or entities that are involved (generally thought causally) in bringing about a physical phenomenon or phenomena. (Salmon 1984, Craver 2007)

Ontic Conception – When applied to scientific explanation, posits that explanations are non-representational facts that exist in the real world, not arguments, models or other representational figures. (Salmon 1984)

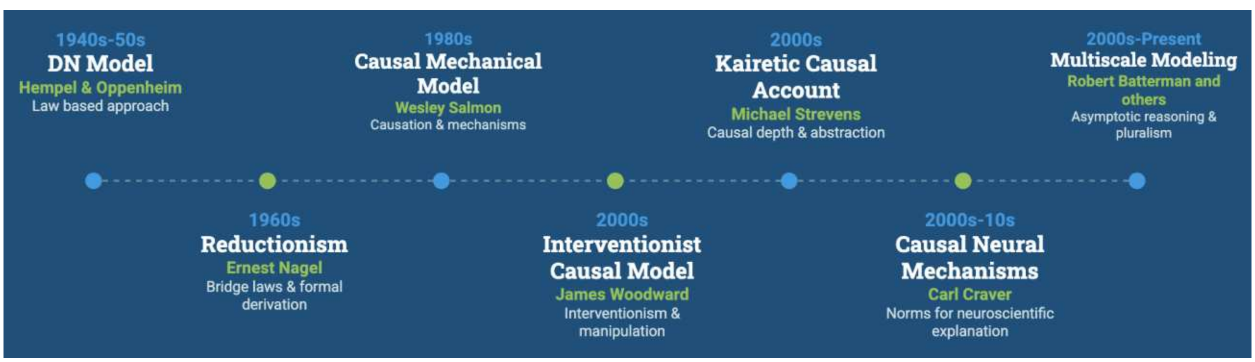
Epistemic Conception – When applied to scientific explanation, posits that explanations are representational content, such as models or arguments, that can bring about understanding, knowledge, or some other epistemic good. (Salmon 1984)

Explanatory Monism – Position that argues for a singular, optimal approach or structure to scientific explanation, usually in service of reducing all scientific explanations to a single "fundamental" framework. (Batterman 2002, Rice 2024)

Explanatory Pluralism – Position that argues for multiple approaches or structures to scientific explanation, typically in service of recognizing the most appropriate explanatory frameworks for a given context. (Batterman 2002, Rice 2024)

Chapter 1: Introduction

The ability to explain something represents a cognitive achievement that has been cashed out as understanding or knowledge about why something occurred. Scientifically explaining a phenomenon is a way to characterize the features and behavior of entities in a manner that enables scientists to further understand, model, or at least capture epistemically salient details. The scientific understanding gleaned from satisfactory explanation may allow scientists to meet a variety of desiderata, namely control and prediction of phenomena but also to ultimately provide psychologically satisfactory responses to *why* questions. There have been many attempts to specify the form of scientific explanation that offers the best means to the aforementioned epistemic insights throughout the history of philosophy, science, and philosophy of science more specifically. In this thesis, I argue for a pluralistic account of explanation and reduction that avoids committing to a single necessary form of either, and that is informed by lessons from contemporary scientific practice.



(Figure 1) A history of scientific explanation and reduction timeline

Though many accounts of explaining natural phenomena can be traced back to Aristotle and even earlier, for the purposes of this thesis the first account examined in the "Monistic

Approaches to Scientific Explanation and Reduction" section is Carl Hempel's Deductive-Nomological (DN) model presented in the middle of the 20th century (Hempel 1965). Restricting the scope of the accounts considered will enable a more fine-grained analysis that can more easily move through the numerous alternative accounts offered since Hempel and include contemporary models. The deductive form that Hempel and Oppenheim used to characterize scientific explanation is intuitively appealing due to the uniform and simple logical structure but is historically noted to be fraught with problems and ultimately unsatisfactory as a candidate. Although the DN model fails to successfully characterize the form of scientific explanation, the subsequent discussions it generated from the terms used to give a DN explanation, the numerous counterexamples created, and the excitement around the possibility of theoretically reducing science to fundamental physics, all confirmed the outsized impact the model had on philosophy of science in the 20th century.

Theories of explanation also inform philosophical accounts of explanatory reduction. Expanding the project of a successful explanatory framework could allow for unifying science across vast domains (psychology, economics, physics etc.) through theory reduction. For example, Ernest Nagel built on Hempel's deductive approach in his theory reduction framework to argue that a reducing theory can operate on a reduced theory to collapse top level theories to more fundamental physical theories (Nagel 1961). Nagel borrows from Hempel's nomological approach and postulates bridge laws that act to translate terms in reduced theories into terms of the reducing theory. The goal of reducing theories is to eventually have a smaller fundamental set of physical theories (or theory) that explain everything from the social sciences to the physical sciences. Successful theory reduction aims at goals of unifying science and connecting the vast array of macroscopic theories to a smaller, more fundamental set of theories. Further, this thesis will look at examples from contemporary scientific practice to analyze why such a project is also not practically feasible or the most desirable epistemic aim.

The failure of the DN model as the sole model of scientific explanation opens the possibility for other explanatory framework proposals to potentially fill the gap. The first alternative considered are the various causal models that argue for causation being more optimal explanans (premises) for explanation in science. I will analyze Wesley Salmon's Causal-Mechanical account, Jim Woodward's causal model, Michael Streven's Kairetic model and Carl Craver's mechanical account to demonstrate the variety of causal models and show why each are not viable individual candidates. Salmon's account is chronologically first and shares with the other causal accounts a counterfactual notion of causation through his introduction of "mark transmission" (Salmon 1984). Salmon's Causal Mechanical model argues that causal processes coalesce to transmit marks or changes in entities that persist through spatiotemporal locations. The most notable difference between the CM model and the two exclusively counterfactual views is Salmon's inclusion of the concept of mechanism. Woodward, seeking to pare down the requirements of a causal explanation, focuses solely on causality as the main component of explanation. Woodward's notion of causality is based on "manipulability", where explaining a phenomenon involves determining the possibility of a process occurring given various counterfactual manipulations being used on the potential causal processes. Similarly, Strevens' Kairetic model focuses on "difference-makers" in causal relationships, where the causal difference-making factors are an abstraction away from the physical causal processes. This abstraction then pares down causal factors further to only the causes that had to occur for an effect to occur, any other causal relationships are deemed unnecessary for explanation (Strevens 2008). Craver's mechanical model is similar to Salmon's in that it utilizes notions of causality and mechanism. Craver uses examples from neuroscience to demonstrate that etiological causation is concerned with antecedent causes but physio-chemical mechanisms in the brain demonstrate the need for constitutive causation where the focus is on the structures involved and their role in producing neural phenomena (Craver 2007). The variety of approaches in these models demonstrates their strength as a group of causal approaches in meeting the needs of various disciplines and contexts. Though since they define causality in novel ways, their individual definitions of causality are by-in-large incompatible and thus, highlight the difficulties in utilizing causality as a singular, explanatory desiderata.

Since causal formulation of scientific explanation does successfully avoid many of the problems that plague the DN model, I look at the potential for this approach to allow for reduction. If causal relations are sufficient for furnishing explanations, then it's worth analyzing their potential for unifying scientific domains through theory reduction. I primarily utilize Strevens' Kairetic model and his commitment to a strong reduction of higher-level phenomena to lower-level, "fundamental" processes and explanations. This model of reduction provides an alternative candidate to Nagel's problematic DN based reduction, even though Strevens argues that causal explanations naturally lead to the possibility of reduction (Craver disagrees with this approach), there are problems that cast doubt on this conclusion. Notably I will show how examples in biology, neuroscience, and geomorphology resist a causal explanation as the sole source of scientific insight. These examples will demonstrate that although causation does seem to be generally a better candidate for scientific explanations but if it possible to furnish non-causal explanations that provide novel insight then it challenges not only the potential for complete reduction but also the monist approach to an explanatory framework (Bokulich 2021). I argue that the cases presented in this thesis do indeed demonstrate the need for a pluralistic framework that can incorporate causation but also leaves room for other approaches to explanation and reduction.

In contrast with these approaches that begin from a unified theory of explanation, in "Lessons from Multiscale Modeling" section, I analyze specific examples from scientific practice that show how attempting to explain phenomena that operate across multiple scales requires explanations that are tailored for individual scale sensitivities. Studying examples as varied as vertebrate limb development (Batterman & Green 2017), economic recession modeling (Jhun 2019) and crack propagation in nanoscale materials (Bursten 2018) not only will demonstrate the ubiquity of multiscale phenomena but also the need for explanations that operate at individual scales. The imposition of mathematical boundary conditions in models such as verterbrate limb development modeling will show how decisions made to allow for modeling of complex phenomena limit the ability of scientists to offer an explanation that operates across all relevant scales. Further, I will use Jennifer Jhun's argument from the use of equilibrium assumptions in economics to demonstrate the importance of maintaining macroscale explanations and the novel insight they can provide (Jhun 2019). The final example that I will analyze is Eric Winsberg's attempt to establish a novel interscale relationship that he refers to as "handshaking" by using examples from nanoscale crack propagation (Winsberg 2006). If Winsberg's handshaking relationship works, then it could be a candidate for a new form of reduction where scales can be connected in a way that offers microscale explanations of macroscale phenomena. However, I will agree with Julia Bursten that the handshaking method doesn't work in the way that Winsberg argues it does, notably because of the differences between incompatible assumptions in continuum mechanics and quantum mechanics (Bursten 2018).

Since monist approaches to scientific explanation are not adequate to meet all of the epistemic needs of researchers and proposed reductive methods have not been successful, I argue in the "A Pluralistic Account of Scientific Explanation and Reduction” section, that a pluralist approach to both explanation and reduction is the only viable candidate to accomplish these ends. This pluralist approach acknowledges the utility in various monist accounts of explanation, such as the role causal-mechanical explanations can have in providing neuroscientific insight and identifying microscale mechanisms that can connect to specific macroscale phenomena (Batterman 2002). Though at the same time the pluralist approach to explanation considers the limits of monist accounts such as the role mathematical limits (Batterman 2002), multiscale challenges (Rice 2025), and the epistemic limits of human investigators (Potochnik 2017). Acknowledging the utility of the variety of approaches found in scientific practice is key to motivating a pluralist approach and the insights gained from deprioritizing a classical reductive approach highlight the need for a similar reinterpretation of reduction. Rather than trying to force a single reductive schema on scientific practice, the role of reduction can be reinterpreted to be various mathematical modeling techniques that allow for translation between various scales. This reinterpretation of reduction combined with the pluralistic explanatory approach establishes a more robust and pragmatic framework for scientific explanation, modeling, and reduction. Ultimately this pluralistic approach to both explanation and reduction prioritizes context-dependent epistemic goals over scientific unificatory aims.

Chapter 2: Monistic Approaches to Scientific Explanation and Reduction

Section 2.1 Hempel & Explanation

The first model of scientific explanation introduced that established much of the terminology and conceptual framework of the work in the 20th century is Carl Hempel's Deductive-Nomological model (Hempel & Oppenheim 1948). The DN model requires four components of an explanation: 1. The argument must take a deductive form, 2. One of the premises of the deductive argument must include a law of nature, 3. The premises must contain empirical content, and 4. The premises must be true (Salmon 1984 pg. 12). Within the deductive form of the argument a conclusion statement must be made that sufficiently "describes the phenomenon needing to be explained." (H&O 1948) This conclusion statement is referred to as the *explanandum*, whereas the premises that seek to explain the explanandum are referred to as the *explanans*. Within the explanans there must be a general law referenced that covers the phenomena in question. The deductive form of the argument is supposed to provide a logical case for the explanation at hand, while the inclusion of a law of nature looks to subsume the explanandum's occurrence under a scientific theory that is widely accepted. This requirement to include a law of nature is referred to as the covering law model. [1]

Hempel uses John Dewey's observation of soap bubbles forming in glass tumblers on a plate to illustrate the DN model. Hempel notes that Dewey explains the bubbles growth and recession as follows:

> "In transferring a tumbler to the plate, cool air is caught in it; this air is gradually warmed by the glass, which initially has the temperature of the hot suds . The warming of the air is accompanied by an increase in its pressure, which in turn produces an expansion of the soap film between the plate and the rim. Gradually, the glass cools off, and so does the air inside, with the result that the soap bubbles recede." (Pg. 685)

Hempel argues that this explanation is successful because it references "certain explanatory facts... 1. particular facts and 2. uniformities expressed by general laws" (pg. 685). This demonstrates that a successful scientific explanation, for Hempel, includes the facts about what is occurring in a particular case combined with the scientific laws that subsume the facts under a more general pattern. The facts of the bubbles appearance, expansion and recession with the temperature of the air are the facts of this example, while the laws of heat exchange and elastic behavior of bubbles are the relevant scientific laws. Together, these form the necessary explanatory facts to create a satisfactory DN explanation of the soap bubble behavior.

The reference to a law of nature comes with a few caveats and requires further clarification. The 1948 Hempel and Oppenheimer makes the distinction between law-sentences and lawlike-sentences. Lawlike-sentences are defined as having 1. Universal form, 2. Unlimited scope, 3. They don't contain designations to particular objects, and 4. They contain only qualitative predicates (Salmon 1984). The first two requirements fulfill typical expectations of laws and lawlike sentences that they operate without restriction over the entire universe – i.e. they are exceptionless. The third requirement further specifies that in establishing a universal form and unlimited scope for a lawlike-sentence the sentence cannot single out a particular objects or objects to operate over. The fourth requirement employs the notion of qualitative predicates which are predicates that only reference features inherent to an object, rather than dispositional qualities that are features that react or respond to the external world (the color of an object vs the fragility of an object for example). Given this criteria, an example of an acceptable law of nature is the law of universal gravitation which is stated as "every particle attracts every other particle in the universe with a force that is proportional to the product of their masses and inversely proportional to the square of the distance between their centers of mass." (Moebs 2016, pg. 199) This formulation applies across the universe without limit and does not specify a specific location, particle or object, satisfying the requirements pertaining to form and scope. A law or lawlike sentence proposed along the lines of “All Colorado Jays can mimic voices and sounds”, although possibly true, specifies a particular species in a particular location and therefore would not qualify as an acceptable lawlike-sentence for Hempel. These examples serve to illustrate the distinction between laws and lawlike sentences on side and “accidental generalizations” on the other (Salmon 1984). The concern with avoiding accidental generalizations is due to their lack of explanatory power. It may be true that all Colorado Jays can mimic voices, but the scope is so severely limited because of the inclusion of location and species related information that it would not explain much compared to a law of universal form. The Jay example may serve a different purpose within the biological sciences, such as that of description of the species but does not serve to provide an explanation within a DN framework.

Another distinction that the Hempel and Oppenheim DN model makes is the difference between fundamental and derived laws. A fundamental law is “purely universal and true” and a derived law is “essentially, but not purely, universal and is deducible from some set of fundamental laws”. A fundamental lawlike-sentence is a purely universal sentence that does not require deduction from any other law and can be used in a DN explanation. The DN model requires specification of not only the terms (i.e. qualitative predications) but also the form of the sentences within the explanans, since the model hinges on inference from one proposition to another. Another interesting aspect of Hempel and Oppenheim’s formal explication of the DN model is the terms “law” or “lawlike” never appear and instead the term “theory” is used. Though for purposes of understanding the model the terms can be understood as near synonymous, the primary distinction being that theories can incorporate existential or universal quantifiers, where laws can only reference universal quantifiers. This gives theories the room within the model to mimic law-like universality while also allowing theories to address more specific phenomena, for example proposing a new process that explains certain species-specific characteristics.

All of these specifications and distinctions come together in a formal account of the DN model as such: "<T,C> is an explanans of E (singular sentence) iff 1.) <T,C> is a potential explanans of E and 2.) T is a theory and C is true." (Bangu 2011, pg.21) T representing a theory that adheres to lawlike-sentence requirements. C represents the empirical content that may be the initial conditions of a particular observational case but also the potential deduction(s) made from meshing the initial conditions with the theory referenced. E then is the explanandum that is the conclusion of the deductive argument and as such the phenomenon intended to be explained. An example could then be: Observed phenomenon in question: When I put a metal pot over a fire it gets very hot. 1.) A cold metal pot was held over a hot flame. 2.) Metals are a good conductor of heat 3.) If a metal pot is held over a hot flame, then it conducts heat very rapidly and becomes hot itself. 4.) Therefore: When I put a metal pot over a fire it gets very hot. The phenomenon observed is the explanandum and thus the conclusion, proposition 4. The initial conditions are contained within premise 1. The theory is represented by premise 2 that serves to setup the deduction made in premise 3 when combined with the initial conditions from premise 1. Then the explanandum is supposed to supported by the deductive argument and therefore explained according to the model. With a model of explanation in hand, it is now possible to explore what possibilities exist for potentially unifying or consolidating theories.

Section 2.2 Nagel & Reduction

The concept of reduction has also played a large role in the philosophical literature of the 20th century. Reduction has its roots in the metaphysical reduction arguments that have been a part of philosophy since at least the Cartesian debates about dualism and physicalism. This sort of reductionist approach can be applied to a vast array of philosophical theories that propose all objects reduce to physical entities (physicalism), mind (idealism), and even more granular notions such as abstract objects reducing to facts about objects (nominalism). These approaches often, but not always, include widespread metaphysical or ontological reductions that seem to allow, at least in theory, a complete reduction of reality to a uniform sort of material. This assumption of monism – that there is a single type of simplest substance out of which all material objects are made – has appealed to philosophers of science, often themselves materialists, to further attempt reductions of scientific endeavors to more "fundamental" theories or even languages. Rudolf Carnap, operating within the linguistic turn of philosophy in the early 20th century, described the process of reduction as a type of "translation" between different theories or fields of the sciences (Carnap 1934). This translation, according to Carnap, could potentially unify all knowledge into a common language. This later became known as the "unity of science" hypothesis. Although the unificationist program is largely seen as infeasible, reductionism continued to play large role in various schools of thought within philosophy of mind. The reductionist program to reduce folk psychological concepts to neuroscientific explanations is one of the more prominent examples and arguably still an active project, though it has its fair share of prominent critics as well.

It is worth noting that although theoretical and ontological reduction have historically been the primary meaning of the term "reduction", there is a more temporal and practical notion of reduction, that is reduction as theory succession. This understanding of reduction sees it moving through time as the scientific theories are succeeded by newer and presumably more explanatory and useful (for scientists testing and theorizing within them) for the particular field. This type of reduction is generally referred to as diachronic reduction since the "newer" theory succeeds the older as science "progresses" through time. The goal, then, is to derive the old theory from the principles of the new theory to show how the previous can be "recovered". Although this type of reduction is present in the philosophical literature (Kitcher 1953) and brings with it interesting questions about scientific progress, theory succession and operational paradigms; this thesis will hereby use the term "reduction" to refer to the synchronic, intertheoretical notion of reduction discussed above.

Around the same time that Hempel was developing the Deductive-Nomological model, Ernest Nagel was developing his notion of reduction that would have a large influence on the subsequent reduction discussions. Nagel argued that intertheoretical reductions, where one theory could be reduced to another, were matters of logical deduction typically via bridge laws that connect the predicates of one theory with the predicates of another theory (Nagel 1949). This deductive approach assumes that reduction is a relationship between theories that are themselves explanations of particular phenomena. The goal of connecting theories that span a wide range of explanatory domains to unify science was the main impetus for Nagel to pursue the project of theory reduction. Connecting theories, particularly logically, would provide a stronger connection between scientific domains but also justification for realist conceptions of science. The formula for reducing one theory involves having the reduced theory as the conclusion of the argument and the reducing theory, with the relevant auxiliary assumptions, as the premises (Palacios 2025). Nagel further notes two distinct kinds of reductions that can take place, homogenous reductions where the terms present in the reduced theory are already present in the reducing theory and as such allow for a straightforward theoretical reduction. The second kind are heterogeneous reductions where the terms from the reduced theory are not present in the reducing theory and so require "bridge laws" to be utilized which connect the terms across the two theories. Because these predicates feature in the laws of each of theories, connecting them shows how the laws (and thereby explanations) of one theory can be translated into the laws (and thereby explanations) of another theory. Nagel adds that the bridge laws should constitute scientific hypotheses themselves, and this can stand as evidence that they are non-trivial. Once the bridge laws are in place the reduction can proceed deductively in the same way as homogenous reductions. When the reduction is completed, whether homogenous or inhomogeneous, the laws of the reduced theory are supposed to be derivable from the reducing theory. Nagel writes "A reduction is effected when the experimental laws of the secondary science (and if it has an adequate theory, its theory as well) are shown to be the logical consequences of the theoretical assumptions (inclusive of the coordinating definitions) of the primary science." (Nagel 1961 pg. 352)

Nagel heavily relies on Hempel's approach to scientific explanation to establish the possibility of scientific theory reduction. The assumptions that theories require laws to explain phenomena, and that explanations are arguments that can be deductively operated on, both mirror the DN model. The postulation of bridge laws further demonstrates the reliance on a nomological approach, as Nagel's own conception of reduction is law based. The entire process of term translation (in heterogenous reductions) and the introduction of bridge laws is based around conceiving of covering laws as satisfactory for explanation but also connecting theories. Further, the whole concept of theory reduction seems like an attempt to push the possibilities of deduction within scientific explanation. Utilizing the deductive approach taken by Hempel, Nagel theorizes that beyond just deducing phenomena from laws, it is possible to deductively reduce theories to more “fundamental” theories.

An example brandished by Nagel and often referenced in the literature is the reductive explanation of the Thermodynamic ideal gas (Boyle-Charles) law within Statistical Mechanics (Bangu 2011). The ideal gas law, $p\mathrm{V}=n\mathrm{RT}$, relates the pressure, volume and temperature (with a specified amount of substance and the ideal gas constant) of a hypothesized ideal gas. This law that is associated with thermodynamics, provides a good approximation of the behavior of gases in many situations. A reduction of the ideal gas law within statistical mechanics is commonly understood to provide a more “fundamental” understanding of the behavior of gases across a broader spectrum of contexts. Thermodynamics is seen as a more macroscopic approach that provides phenomenological laws that achieve explanation through a more pronounced use of idealization, whereas statistical mechanics is understood to provide a more microscopic and mathematical explanation that allows higher applicability and connectivity across other contemporary fields of physics. In accordance with this more precise approach, a model in statistical mechanics assumes a fixed number of molecules for a particular volume of gas, whereas a model in thermodynamics merely posits a bulk amount of molecules. Understanding the approach of either model regarding the ontological composition (in this case the particles involved in the gas) is taken to be the first step in this Nagelian reduction, referred to as the ontological identification assumption (Bangu 2011). Utilizing this assumption with other assumptions about how the molecules behave within the gas, idealizing a gas “...in which the particles interact so weakly that their eigenstates are effectively those of isolated particles” (Hamilton 1999), the second step is the derivation of the law. This derived lawlike statement is now within the context of Newtonian mechanics but also a statistical mechanical framework, the equation becomes, L_{SM}: PV = 2E/3 (Bangu 2011). This equation as a lawlike statement is now the counterpart to the ideal gas law equation, $pV=nRT$. Since this reduction is an example of a heterogenous reduction, the final step in the reduction is to add the bridge law, which allows us to translate the terms from one equation to the other. In this case since the pressure and volume values are already in the same relationship on the left side of each equation, the bridge law will equate the respective right sides, BL: 2E/3 = kT (Bangu 2011). With this bridge law established, once it is plugged into our original lawlike statement from statistical mechanics (L_{SM}), the ideal gas law equation can be seen (Bangu 2011).

Section 2.3 Objections to Nagel & Hempel

The primary objection underlying Nagel's approach is not just that bridge laws encounter many problems (they do) but that the whole reductionist project relies on an outdated framework, namely the Deductive-Nomological model of explanation. Although the problems have notoriously been dissected in the literature, it is worth acknowledging a few merits of utilizing the DN model to formulate a reduction model. The DN model which is subsumed under a Covering Law model of using laws and antecedent conditions with empirical content to arrive at logical conclusions about scientific phenomena (Craver 2007, Hempel and Oppenheim 1965). The use of deduction as the logical maneuver is truth preserving and as long as the empirical content is true (one of the DN requirements), the appropriate auxiliary assumptions are available and the formula followed, the truth of the conclusion is guaranteed. Nagel utilizes this approach to attempt truth preservation across reduction, as the theoretical terms being translated are both supposed to follow similar requirements while also deductively showing that the reduced theory (which presumably has successfully predicted and explained phenomena) follows from the reducing theory (Palacios 2025). This logical operation then lends credence to the reducing theory as the truth is preserved across the terms and bridge laws (if necessary).

Although this syntactical approach seems to ensure truth preserving theoretical translation, numerous issues with the DN model of explanation including the previously mentioned inferential conception of scientific explanation, the DN account of a scientific law of nature, the covering law conception of scientific explanation, the explanation/prediction symmetry thesis, the role of causality in scientific explanation, the supposed literal truth of the explanans, and the possibility of quasi-formal models of explanation all challenge its feasibility. The assumption of mathematical and scientific equations, theories and models as purely syntactical entities simplifies the terms for such a reduction to occur but also narrows the scope of scientific practice as well. For example, there are large parts of biology that involve little mathematics (e.g. there is little math in Darwin's Origin) and laws are likewise relatively rare in biology (Beatty 1995). All these objections have developed through the decades following the development and proliferation of the DN model within the literature on scientific explanation. It is beyond the scope of this paper to evaluate every potential problem with the DN model but there are a few worth taking a closer look at to understand how fundamentally flawed the approach is.

One of these problems worth exploring is Hempel and Oppenheim's requirement of "purely qualitative predicates" within the DN model. This requirement that was included to limit predicates to features inherent within the object, rather than features that could respond to the world was challenged famously by Nelson Goodman in his book *Fact, Fiction, and Forecast* as part of his "New Riddle of Induction" (Goodman 1955). In it Goodman argues against any account of purely qualitative predicates by creating the predicates "grue" and "bleen" (Goodman

1955). “The predicate ‘grue’ applies to all things examined before t just in case they are green but to other things just in case they are blue.” (Goodman 1955, pg. 74). “Bleen” applies to things examined before t if they are blue and other things in case they are green (Goodman 1955, pg. 79). The new problem of induction then occurs when the attempt is made to consider objects in the future such as emeralds, the question being whether they should be considered green or grue. It then seems that not only are the terms “grue” and “bleen” time-dependent but so are blue and green and as such cannot qualify as purely qualitative. By providing a formula and examples for challenging the possibility of qualitative predicates Goodman is generally seen to have successfully shown that “...qualitativeness is an entirely relative matter.” (Goodman 1947) As such the requirement that all predicates utilized within the DN model should be purely qualitative seems untenable. The failure of the concept of qualitative predicates leaves the DN model open to problems related to the terms being dependent on time or other factors and as such potentially not universal in the way that Hempel and Oppenheim wanted. This would potentially effect the outcome of the deductive argument as the conclusion would not be as universal or general as the DN model advertised.

In part because of the large presence of the DN approach, many counterexamples were produced to vividly exhibit some of the other problems with the method and assumptions made by Hempel and Oppenheim. One of the more straightforward examples is the eclipse explanation thought experiment. The explanation of a lunar eclipse seems successful if the prior relative positions of the earth, sun and moon are known in conjunction with the laws of celestial mechanics. Though one could also “explain” the same eclipse by working backwards and using the relative positions of the earth, sun and moon after the eclipse has occurred (in conjunction with the same laws of celestial mechanics). Though both approaches could fit within the DN model of explanation, the former explanation seems satisfactory while the latter does not. This counterexample highlights the lack of temporal restraints on a DN explanation and although Hempel acknowledges this, does not find it necessary to add to the model.

Perhaps an even more famous counterexample to the DN model is Bromberger's flagpole example. Beyond a lack of temporal restraints, there is also an issue relating to the role of causality within an explanation. Although Hempel acknowledges that "The type of explanation which has been considered here so far is often referred to as causal explanation." (H&O 1948 250), the direction of causality is not given enough attention and leads to problems with explaining certain phenomena. Bromberger reveals this problem by asking the reader to consider a flagpole on flat ground under a shining sun that is casting a shadow on the ground below. Given the height of the flagpole and the position of the sun, along with the law of rectilinear propagation of light, a DN explanation can be given for the length of the shadow on the ground. So far so good but consider that given the length of the shadow rather than the height of the flagpole and the position of the sun, a DN explanation could be formulated for the height of the flagpole. Though it seems backwards to "explain" the height of the flagpole from the length of the shadow, an "explanation" can be constructed that adheres to all of the DN model requirements that does not seem to follow our understanding of what causal direction explanations should follow. This lack of nuance regarding causality within scientific explanations points to more problems with the DN model and leaves open questions about the kind of explanations the model is offering. This problem of directionality also raises problems for Nagel's account of reduction. Since Nagel's account leans heavily on the DN model, it also does not consider directionality in reduction. For example, similar concerns could be raised for why reduction must happen from thermodynamical theories to statistical mechanical theories. If the explanations offered by thermodynamics satisfy the DN model of explanation and are utilized by scientists to answer why questions, then why should statistical mechanics take precedence in the reductive chain. Since Nagel doesn't offer any compelling arguments for the directionality of reduction pointing to smaller things being primary, it becomes clear that the notion of more "fundamental" reducing theories is more of an assumption than a conclusion.

Another counterexample that raises challenges to the explanation/prediction symmetry thesis of the DN model is the moon and the tides. Consider that prior to Newton correlating the effects of the moon's gravity on the motion of the tides that mariners were aware of the connection, though without the knowledge of the mechanism of the force of gravity. If the mariners were to furnish an explanation they might say that the connection between the moons position and phase and the rising of the tides was due to God's providence in setting the moon in the sky (or something to that effect). The initial conditions of the moon and tides, with the law of God's providence would allow them to establish an explanandum of the subsequent rising of the tides in accordance with the DN model. Although the DN explanation would also serve as a retrodiction for the behavior of the tides in the past, it could also serve as a prediction of the rising of the tides in the future. Since Hempel and Oppenheim did not include temporal constraints on DN explanations this gives rise to the prediction/explanation symmetry thesis, as demonstrated by the tides example. Thus, this counterexample shows not only that laws can be constructed according to DN requirements that are scientifically spurious, but also that the lack of temporal restraints allows predictions to be made without explaining the phenomenon in question (the very purported goal of the DN model).

A final counterexample to consider concerns the lack of explanatory relevance that is possible within a seemingly satisfactory DN explanation. This example imagines a biological male, John Jones, that has consumed his wife's birth control pills every day for a year and has not become pregnant. In fact, any biological male that takes women's birth control pills does not become pregnant, thus establishing a law that advises any males looking to avoid pregnancy to take them. Although this example establishes a DN explanation that "explains" the fact of these men not getting pregnant it clearly misses the explanatory relevant feature that biological males are incapable of becoming pregnant, regardless of taking birth control pills or not. This lack of explanatory relevance leaves the DN model open to criticism about irrelevant explanations and leaves questions about any proposed DN explanation and whether it is actually providing much insight at all.

Section 2.4 Causal approaches to explanation and reduction

These numerous objections and counterexamples eventually led to new approaches to explicating an account of scientific explanation. In response to objections about the DN model's lack of consideration of causal directionality, such as in Bromberger's flagpole example, models of explanation that replaced concerns with laws (or lawlike statements) and instead emphasized causal considerations began to emerge. The DN model was designed to incorporate Humean concerns about causal regularity by referencing laws as the acknowledgment of theoretically significant causal regularity. The causal theories that subsequently developed in response to the failures of the DN model were still sympathetic to a similar Humean notion of causal regularity but also incorporated a less demanding notion of causality to offer explanations across a broader range of scientific practice.

It is worth noting that within the causal framework of scientific explanation there are two prominent approaches, namely (purely) causal models and causal models that incorporate the concept of mechanism. Representing the latter type of causal explanation is the appropriately named Causal Mechanical model offered by Wesley Salmon. Salmon acknowledged the importance of causal factors but also included the concept of mechanism that grounded the causal conditions within loci of causal production. Salmon's view evolved over time but consistently involved causal processes serving as the explanans of a scientific explanation. The CM model is generally understood best in its differences with the DN model or "Received View" as Salmon referred to it (Salmon 1984). He categorizes the DN model as an epistemic model that characterizes explanations as arguments, whereas his CM model is categorized as an ontic model that approaches explanations as being built out of physical entities that are in the world and causal in nature. Salmon argued that approaching explanations as deductive arguments led to the issues of asymmetry and lack of relevance that appear most distinctly in the famous counterexamples. The asymmetry seems to mirror causal asymmetry as the lack of bidirectional explanation (flagpole example) acts like the cause to effect (not effect to cause) relation and as such is a piece of evidence in favor of the CM approach. The lack of relevance that leads to the possible creation of spurious DN explanations is solved by incorporating relevance conditions that stem from Salmon's older Statistical Relevance model, but that have similar effects to Woodward's and Strevens' approaches to include only causally relevant factors into explanations (similarly avoiding the Jon Jones counterexample). The causally relevant factors that compose the mechanisms responsible for explanation also depend on the counterfactual notion of "what if things were different" that are present in other causal models (Woodward's most notably). Although Salmon was wary of using counterfactuals stemming from his development of the Statistical Relevance model, it worked within the CM model to allow explanations that are possible in principle, even if we can't explore the mechanisms directly (Beginning of the universe etc.). This allows for a large scope of possible explanations since it incorporates counterfactual causal factors while maintaining a commitment to finding mechanisms responsible for phenomena occurring.

One notable presentation of the CM model that highlights both the emphasis on causality and the use of mechanism as the nexus of the causal factors is the notion of *mark* transmission. Salmon argues that distinguishing between causal processes and "pseudoprocesses" involves considering the possibility for said process to transmit a mark that persists across spatio-temporal locations. An example would be a dent in an automobile, the dent is considered a mark because it changes the structure of the fender of the car and the dent persists across time and space as the car moves from one location to another. The dent is a mark that involves the counterfactual consideration of how the car fender would be like if the dent never occurred. This counterfactual consideration is possible because of the antecedent conditions that coalesced into the causal process of dent transmission, such as a baseball being struck by a bat and following a trajectory (subject to gravity and air resistance etc.) that led to the ball striking the car fender and compressing the bodywork to form the dent. Salmon also notes that causal processes can interact with each other, termed causal interaction, that can leave marks on all bodies involved and/or alter the causal process as a result of the interaction. The paradigmatic example of causal interaction being two billiard balls that are in motion striking each other and subsequently changing their respective trajectories.

A causal model that doesn't incorporate a notion of mechanism can be found in the work of James Woodward, notably in his book *Making Things Happen*. Woodward's conception of explanation is based on "manipulability" (Woodward 2004, pg. 9) of factors related to the phenomenon intended to be explained. This notion of manipulability is a counterfactual one, as in many cases of manipulation it is impossible due to temporal constraints (explaining phenomenon in the past), limits of current technology (explaining cosmologically scaled phenomena) or ethical considerations of research (editing the human genome). In other words, Woodward focuses on manipulations that are *logically possible* even if they are not actually possible due to other constraints. Woodward also makes a distinction between explanation and description, arguing that his approach is concerned entirely with causal factors and as such classificatory or descriptive practices that do not utilize manipulation or intervention do not count as explanatorily relevant. In this causal theory of explanation, modal manipulability doesn't require referencing laws or attempts to establish new laws or make lawlike statements, rather one just needs "information about invariant relationships, and one can identify invariant relationships even in cases in which one doesn't know laws, cannot trace spatiotemporally continuous processes, or unify and systematize." (Woodward 2004, pg. 10) Rather than referencing laws or underlying causal processes, in this model, c is the cause of e, when the following counterfactual is true: If c would not have occurred due to an intervention (manipulation), all other variables held constant, e would not have occurred. By tracing the causal relationship between particular processes, counterfactual information is ascertained that explains the phenomenon in question. Woodward uses an example from economics to further demonstrate why a causal account that utilizes manipulability is superior to accounts like Salmon's (1984) causal mechanical account. The example involves explaining (via supply and demand) a rise in the price of oranges due to a freeze that occurs where they're grown. Woodward argues that his account would identify the relevant factors to the price hike and note that if the freeze does not occur then the price does not change. If an explanation of the price hike is given within a causal mechanical framework such as Salmon's that require tracking spatiotemporal factors that account for a given phenomenon, then there are too many irrelevant features that are included. Woodward notes

> "Underlying the behavior of these systems are individual spatiotemporally continuous causal processes and interactions in Salmon's sense: there are myriad individual transactions in which money in some form is exchanged for physical goods, all of which involve transfers of matter or energy; there is exchange of information about intentions or commitments to buy or sell at various prices... However, it also seems plain that producing a full description of these processes (supposing, for the sake of argument, that it was possible to do this) will produce little or no insight into why these systems behave as they do." (Woodward 2004, pg. 355)

The numerous causal factors that Salmon's account would incorporate to explain this result would lead to an explanation that would seem to be overdetermined, whereas Woodward's account offers a singular causal account that is much more parsimonious and seemingly complete. The lack of understanding or insight gained from taking into account all of the factors present in the Causal Mechanical framework demonstrates the advantage of a causal account based on intervention.

Another prominent causal model of explanation is the Kairetic model as presented by Michael Strevens in his book *Depth* (Strevens 2009). Strevens acknowledges that views such as Woodward's are compatible with his in terms of utilizing causation as the primary explanatory factor, but that models like Woodward's focus too much on the detailed causal story rather than the specific difference-making factors of the causal story. Strevens thus requires causal factors to structure scientific explanations (like Woodward) but insists that only the factors that if removed would prevent the effect from occurring at all are explanatorily relevant. Strevens refers to this account as a "two-factor account" that has the causal relation which includes all of the causally relevant features and the explanatory relation which is the difference-making factors that are relevant for the act of explaining the phenomenon. The difference-makers serve as an epistemic "abstraction" from the causal components and the emphasis on paring down the causal factors to only necessary difference makers serves as the demarcation between the Kairetic approach and other causal models of explanation. Appropriately the name of the model is taken from the Greek word “kairos”, translated as a “decisive point” or critical moment. The decisive point for Strevens is the difference-making causal components that lead to the phenomenon in question. Strevens’ uses the example of Rasputin’s murder to illustrate his two-factor account and how it differs from other causal approaches. The example uses an account of Rasputin’s murder that is presented as follows “Rasputin was invited to visit Yusupov’s home and, once there, was given poisoned wine and tea cakes. When he did not die, the frantic Yusupov shot him. Rasputin collapsed but was able to run out into the courtyard, where Purishkevich shot him again. The conspirators then bound him and threw him through a hole in the ice into the Neva River, where he finally died by drowning.” (Strevens 2009, pg. 45) The causal story of Rasputin’s demise is complicated since it might be argued that the poisoning, shooting and drowning all had hands in his death. Strevens argues to the contrary that the Kairetic account successfully explains the death by considering all of the causal factors in the causal relation but narrowing the causal factor to the drowning as the sole cause of death in the explanatory relation. The two factor approach gives Strevens room to include the causal factors that are intuitively involved in a phenomenon while the explanatory factor allows for concise, accurate causal explanations.

Both Woodward’s interventionist causal model and Streven’s Kairetic model represent approaches to scientific explanation that avoid many of the pitfalls faced by the DN model. Bromberger’s flagpole example that allowed for DN explanations that used a shadow to explain the height of the flagpole would not present a problem for either of these approaches. When the causal factors are considered instead of appealing to laws, it becomes obvious that the shadow is not a causally relevant factor to the height of the flagpole. Woodward could argue that if an intervention occurs such that the shadow is no longer present (e.g. blocking the sun with a shade), the flagpole would still be the same height as before and as such it is apparent that the shadow has no causal bearing on the height. Strevens similarly could use Woodward's causal approach to arrive at much the same conclusion and make the point that the shadow does not serve as a difference-maker to the height of the flagpole. Similarly, an example like Jon Jones taking his wife's birth control would also fail to trouble the causal approaches, as the problems with explanatory relevance could be teased out via intervention or appeals to a lack of difference-making. The overall approach of a causal model also serves to increase the scope of possible explanations in different scientific fields. Although the DN model's reliance on laws was supposed to provide guardrails to ensure that explanations were consistent with the scientific corpus more broadly, it became apparent that this approach was largely irrelevant in certain fields such as the biology and other special sciences. In areas of science such as physics it may seem obvious to reference laws to explain certain phenomena but the study of say, the human brain, seems less amenable to such an approach. Rather than establishing laws of anatomical phenomena and using these laws or lawlike statements to explain other biological features or psychological phenomena, explanations reference causal features to connect and explain.

Carl Craver makes a similar but notably distinct argument in his book, *Explaining The Brain* (Craver 2007). Craver advocates his mechanical account of explanation that like Woodward and Strevens utilize causality while also, like Salmon, considers the role of "mechanism" in explaining neuroscientific components and phenomena. In contrasting his view with the aforementioned causal accounts Craver writes, "Salmon, and most of the others who have paid attention to causal explanation, have focused exclusively on *etiological* causal explanation, that is, explanation of some event by its antecedent causes (as when a virus causes the flu). The variety of explanation that I am interested in is *constitutive* (or componential) causal-mechanical explanation: the explanation of a phenomenon, such as the opening of a Ca^{2+} channel, by the organization of component entities and activities." (Craver 2007, pg. 8) In distinguishing between etiological and constitutive causation, Craver demarcates his theory with its use of mechanistic componential (constitutive) considerations and those of which antecedent causal conditions are primary (etiological). The justification for the proposed shift to mechanism from not only Covering-Law models but previous causal approaches, comes from analyses of Neuroscientific practice and methodology. Craver uses the term mechanism in a different sense than Salmon, noting that "... mechanism... is a set of entities and activities organized such that they exhibit the phenomenon to be explained." (Craver 2007) and as such demonstrate a key component in the types of explanations that neuroscience furnishes. The use of mechanism outlines the way that neuroscientific explanations can be partial or (constitutively) causal in ways that violate the requirements of the DN model. The scientific study of the brain doesn't seem to lend itself to other notions of causation either, since etiological causation requires specifying prior events that occurred and led to a phenomenon occurring subsequently. The type of causation that neuroscience seems to demand, according to Craver, does not follow this pattern and instead demonstrates how the structures of the biological components and the physical mechanisms involved cause certain phenomena in a way that is not necessarily temporarily linear (though most tend to be). Craver goes on to argue that this indicates that an etiological account of causal explanation is not sufficient to explain neuroscientific phenomena and as such should be adapted to his account of constitutive causation. It is also worth noting that although Craver's emphasis on constitutive causation could lend itself to a reductive interpretation, Craver pushes back against this as will be discussed later in this chapter.

All of the presented causal approaches to scientific explanation, whether mechanistic in nature or not, avoid many of the pitfalls of the DN model. Since attempts at Nagelian reduction inherit the problems with DN explanations and as such are not feasible, utilizing a causal model of explanation provides a novel perspective to view the relations between supposed higher-level and lower-level phenomena. Although there are multiple approaches to considering reduction using causal models of explanation, Michael Strevens provides a strong candidate for analysis. Strevens (2009) argues that taking scientific explanations to be causal allows for explanatory reduction to occur from higher-level phenomena to fundamental causal explanations in physics. Strevens writes "some philosophers suspect that the thesis is in fact false: there are irreducible high-level causal relations that are quite capable of providing understanding of the phenomena that they relate. Given what we now know, these suspicions are, I believe, extravagant: there simply are no causal relations of which we are aware that cannot be attributed to lower-level interaction and, ultimately, to the causal influence of fundamental particle on fundamental particle." (Strevens 2009, pg. 82) If explanation bottoms out at causal relations that are difference-making in the particular context, then these causal relations can be reduced to causal relations at the lowest level of explanation, namely fundamental physics. This reduction model has the difference-making explanations furnished by referencing fundamental particles as the bedrock that all other scientific explanations can be collapsed down to. Similar to Salmon's CM model, the Kairetic reductive approach assumes an ontic approach that considers the difference-making components of explanations to be features of the physical world and not merely epistemic or communicative tools.

Although Strevens' Kairetic model uses causal relations to establish a reductive hierarchy of fundamental and less-fundamental science, not all causal approaches are committed to such a hierarchical framework. Craver notably argues that neuroscience provides a strong counterexample to Nagel or Strevens' explanatory reduction, providing a causal explanatory model that trades reduction for intertheoretical collaboration. Regarding his alternative approach, Craver argues that "...the unity of neuroscience is effected when researchers collaborate to build multilevel mechanistic explanations. Different fields approach a mechanism from different perspectives using different techniques. Their findings place constraints on the mechanism. It is not the case that theories at one level are reduced to theories at another. Rather, different fields add constraints that shape the space of possible mechanisms for a phenomenon." (Craver 2007) Craver doesn't think reduction is the vertically stacked picture provided by classical reductive (such as Nagel) or causal reductive approaches where higher-level phenomena such as learning or memory are translated or more fundamentally explained by purely molecular phenomena. To illustrate this, Craver investigates the reductive story presented about long-term potentiation or LTP and finds that "They did not bridge levels, create equipotent images, form bridge laws, or establish ontological reductive links relating the items of two domains. Instead, they combined different techniques to investigate different aspects of a mechanism at the same level." (Craver 2007, pg. 236) What appears to be reduction across multiple levels of physical entities is actually a mesh of independent fields and techniques coming together to investigate a phenomenon and providing causal explanations that provide "provide different constraints on the same mechanism and yield a more complete image of the mechanism's organization than the fields can provide individually." (Craver 2007, pg. 236) It's the integration of these independent fields that furnish the best explanations according to Craver and as such represent a resistance to a reductive approach since each provide explanatory value to the mechanism that explains the phenomena in question. Rather than hierarchical collapsing to lower levels, this picture involves lateral structures that each contribute to a larger explanatory mosaic. While Strevens' reductive approach works to pare down the macroscale explanations to difference-making fundamental explanations at lower levels, the Craver approach views explanatory inflation as an advantage.

Section 2.5 Objections to the Causal Approach

The causal approach to explanation and reduction (or intertheoretical work) does seem to present a more widely applicable model than the DN model for explaining scientific phenomena, though the various views are not without their own potential problems. Notoriously Hume presented challenges to causation as a feature of the world and argued that it is a judgment made by observers of a constant connection between cause and effect (Hume). Although there have been many responses and supposed solutions to this objection, it does point at the potential complications arising from relying on causation as the foundation of scientific explanation. Consider the reduction of mental phenomena to neurochemical processes in the brain. Although it seems much progress has been made in translating certain features of the mind to mechanisms in the brain, there are fundamental features of causality that present potential problems. The most notable is that when reference is made to cause and effect, generally the cause is understood to occur at some time, T1, where the subsequent effect is then said to occur at T2. Normally this is an unremarkable feature of causality but when applied to mental to physical reduction reveals a temporal disconnection between the two levels of phenomena. When the neurochemical processes that make up the biological mechanism responsible for producing the mental states occur they are discrete physical occurrences, while the mental states that result are experienced in a unified and continuous stream of consciousness. The continuous nature of mental phenomena challenges the physical reduction of mental states if causality is understood to be the fundamental framework for explanation.

Though some problems of etiological causal approaches might be avoided by embracing Craver's constitutive notion of causation, there are still considerable hurdles to overcome. The most notable of which is an objection to constitutive causation providing complete explanations for biological phenomena. John Dupre argues that genes are a paradigmatic case where the constitutive causal components do not fully explain the mechanism involved in gene expression. Dupre notes, "In the early days of molecular genetics, and nicely encapsulated in the central dogma, it was supposed that genes were discrete DNA sequences that specified precisely the amino acid sequence for a particular gene product. Subsequently, however, things have proved far more complicated." (Dupre 2005, pg. 41) Rather than being simple DNA sequences, Dupre further notes that genes are "not typically unbroken" and have coding sequences broken up by non-coding sequences (Dupre 2005, pg. 41). Further complicating the issue is when the DNA sequences are transcribed into RNA, they undergo further excision and result in different kinds of RNA products. These RNA produces also require further editing before they are translated into amino acids and as such are very different from the DNA sequences that started the process. Once the full chain of events is established, it becomes obvious that a gene is not simply a mechanism that contains nucleotides that produces RNA and subsequent proteins. Rather molecular genetics revealed that genes and the genome as a whole are a complex web of bidirectional causal mechanisms and Dupre also notes biologists disagree whether the molecular gene is even a coherent concept. Dupre further argues that attempts to isolate the role genes play in a larger biological system are mistaken. The biological system that genes are expressed in play just as large a part (if not bigger) in how certain sequences are read and expressed and as such need to be included in the explanations offered. This would further push back against a causal account of genetics, since the mechanisms operating within the causal nexus of the gene would not fully account for the biological phenomena downstream. Whether Dupre's entire thesis is accepted or not, the gene is a concept that has furnished numerous explanations of biological structures and phenomena, though it is apparent that utilizing a constitutive causal conception of explanation is not sufficient for explanation across molecular genetics, thus not providing the universal model for scientific explanations.

Another issue with a purely causal or causal mechanical approach to explanation is the limited ability to cover all cases of scientific explanation. Alisa Bokulich argues that there have been two assumptions at play when attempting to argue against a causal account of explanation, "First, that finding an example of a non-causal explanation requires finding something like an uncaused event, and, second, that causal and non-causal explanations of a phenomenon are incompatible." (Bokulich 2021, pg. 141) Challenging these assumptions involves acknowledging the successes of causal explanations while also allowing for non-causal types of explanations that may bring novel understanding to the phenomenon being studied. This pluralist approach is demonstrated by the sand ripple example that Bokulich utilizes to show how although there is a causal explanation of a phenomenon, it doesn't rule out the potential value of a non-causal explanation to complement and provide unique insights. Bokulich notes that Robert Anderson provided an explanation for sand ripple patterns in sand via a mathematical model that while including idealizations, analyzed and explained how the bouncing of sand grains into other sand grains, reptation, explains how these patterns form across deserts. This model, although mathematical and idealized, is characterized as a causal explanation by Bokulich, "... because the mathematics and model explanation are still a straightforward and direct representation of the relevant fundamental causal processes, causal mechanisms, and causal entities that we know to be operating in that domain. An incomplete, idealized, and statistical representation of a causal process is still a representation of a causal process." (Bokulich 2021, pg. 153) Although this explanation explains the phenomenon of sand ripples via causal processes, twelve years later the "defect dynamics" (Bokulich 2021, pg. 150) model was proposed by Brad Werner and Gary Kocurek, that "introduces a new 'pseudo-onotology' at the more abstract level of bedform structures, and makes them the dynamical variables through which the system evolves and the phenomenon is explained." (Bokulich 2021, pg. 154) This explanation does not utilize the causal processes directly but instead references geometric idealizations of bedforms that have ripple crests and deformations of the crests. Using the models established by ignoring the causal processes and focusing on the bedform idealizations, the researchers were able to still explain ripple formations and their models matched observations in various contexts. It is possible that an argument could be made that their explanation still leans on causal processes that form the ripple crests, but this argument is challenged with the fact that the defect dynamics model not only explains sand ripples but underwater sand ripples as well. The sand ripples formed at the bottom of the ocean are not subject to the same causal processes that form wind blown sand ripples, fluid flow shear stress vs reptation. The ability of the non-causal explanation to explain across a larger set of natural contexts demonstrates the advantage of an explanatory pluralist approach that Bokulich advocates.

An explanatory pluralist approach may not rule out causal explanations being the primary form of scientific explanations, but there are further considerations that may complicate the causal approach taken by Strevens, Woodward, and Craver. Angela Potochnik, although a causal account advocate, notes a distinction between ontic and communicative senses of explanation that distinguish ontological causal mechanisms (as emphasized by the aforementioned philosophers) and explanations as a feature of human communicative priorities (Potochnik 2017, pg. 59). These priorities include the connection to scientific understanding that provides epistemic value beyond simply identifying causal features responsible for phenomena occurring. A purely causal explanation may not provide understanding that is gained from certain model considerations or learning from exemplars (Lipton 1991) and this consideration points at a monist account of causal explanation not being the singular desiderata of studying scientific phenomena. Beyond the concerns of potentially expanding explanation to include understanding, Potochnik also raises concerns about the priority of representations in causal approaches to scientific explanations. A notable example she raises is within Strevens' kairetic account where the primary "dependency relation" is causal and requires determining the difference-makers that are the most fundamental to explaining the phenomenon. The difference-makers as acknowledged by Strevens' are an "epistemic abstraction" and as such are not ontological features, thus are dependent on representational choices that serve a purpose for human communication. If the features that are necessary and sufficient for a kairetic account are beholden to human communicative goals, then it opens the possibility for other accounts of explanation providing explanatory value. This further complicates Strevens' attempts to establish a reductive chain based on the difference making causal factors. If the key causal factors are epistemic in nature rather than ontic, then reducing theories may at best serve epistemic interests without saying anything further about the "reduced" phenomena under investigation. Although this pushes back against Strevens' notion of reduction, it only opens the door for strictly causal accounts not being sufficient for explanation, whereas Potochnik may have provided an even stronger argument against causal-mechanical accounts of explanation. Both Salmon and Craver's accounts of explanation advocate for locating physical mechanisms to explain phenomena and as such are vulnerable to the sort of representational critique offered by Potochnik. The representational decisions by researchers may prioritize certain features of physical components that allow for modeling phenomena of interest under practical constraints. Though potential distortions and/or idealizations utilized during the modeling process may make establishing a clear causal relationship between physical mechanisms and the phenomenon being modeled difficult or at times impossible. This further complicates an attempt to use causal mechanical accounts as a basis for reducing theories as well, since if the causal relationship between a mechanism and phenomenon are not firmly established it's difficult to understand how that relationship can be further understood at a lower level.

Chapter 3: Lessons from Multiscale Modeling

The previous accounts of explanation fail to capture various kinds of scientific phenomena satisfactorily because they all share the same approach of attempting to fit a theoretically predetermined model of explanation onto a vast array of phenomena resulting from varied scientific practices across different fields and across a wide range of scales. Another issue is that the explanatory relations identified by these accounts rarely hold between features across wide ranges of scale. Hempel's nomological approach assumes laws are the appropriate explanans for explanations, while Woodward, Strevens, Salmon and Craver assume various notions of causation work as the theoretical framework for explanation. All of these models fail in certain cases of explaining multiscale phenomenon because they assume their explanatory framework will generalize to explanations of higher-level regularities by appeals to properties of the lower-level components of the system. The problem is that features of systems at these 'higher' levels often fail to be explainable in terms of laws or causes that connect them with the constituents of the system. As a result, law-based or causal approaches to explanation will systematically struggle to capture the explanations of these multiscale phenomena.

In contrast with this theory-of-explanation first approach, I propose that the method of establishing an account of explanation should instead be reverse-engineered by looking at scientific practice across various domains and building the theory from the observations of practice. In particular, in order to get the various relationships between scales or theories right, we need to look at how scientific practice actually goes about explaining these multiscale phenomena. This problem of direction of fit in the relationship between theory and practice is the reason these previous models of explanation have not been successful in explaining, unifying or reducing in all circumstances. In order to remedy this, I here adopt the approach of building from in-practice multiscale modeling explanations and then using this account to draw lessons concerning explanation, modeling, and reduction more generally.

Section 3.1 Modeling Multiscale Limb Development

The first example from scientific practice that can provide insight into the kind of challenges inherent in explanatory modeling concerns limb development in vertebrates (Batterman 2002). The modeling of the development of limbs creates challenges in connecting microscale phenomena to macroscale phenomena since both play an ineliminable explanatory role. To fully understand the context of the interaction across scales and the roles of boundary conditions, it's worth considering an example provided by Robert Batterman of modeling the harmonic structure of a violin's string. To solve the wave equation to get the relevant harmonic information from the string, it is necessary to set the distance the string spans from the bridge to the nut as a fixed mathematical boundary condition. (Batterman 2002, pg. 7) Though once the distance is fixed, Batterman notes "Unfortunately... if such strict conditions on the string's endpoints were actually imposed on the violin, it would make it impossible for the vibrations in the string to be amplified by the sound box of the violin and we would be unable to hear the instrument. Modeling the behavior of the vibrations that get amplified via the sound box requires that one completely shift scales and focuses on the molecular and sub-molecular interactions between the string and the bridge." (Batterman 2002, pg. 8) The imposition of boundary conditions on the model at the macroscale, where the string is stretched across the body of the violin, creates problems for explaining the microscale interactions of the vibrating air and the soundbox. Similarly, the explanation of limb development incorporates mathematical boundary conditions on cells in biological models of morphological signal gradients (Batterman 2002, pg.

8). At the larger continuum scale, the signal gradients are used to map out the cell development that leads to limb growth and it is necessary to set the cells or cell layers as fixed in space and time to feasibly model the growth properly (Batterman 2002, pg. 8). Batterman further notes that "Ignoring the microscopic processes of cell-cell interactions and subcellular mechanisms can, however, be problematic in contexts where a small number of elements have a large impact on a system. In such cases, upper-scale models must be combined with lower-scale discrete models (e.g., Langevin equations) that capture individual (microscopic) details such as kinetic rates of particular proteins." (Batterman 2002, pg. 8) So although the boundary conditions are required to make the macroscale phenomena tractably modellable, this requires that additional microscale models are included in the explanation because the effect of microscopic processes is ineliminable to fully understanding limb development. Denying the reductionist approach in this patchwork of models Batterman adds "... connecting the models does not involve a process of reduction of one model to a more fundamental one. The models combined in multi-scale modeling projects like the ones examined are explanatorily independent but epistemologically interdependent (Potochnik 2009, p. 19)." (Batterman 2002, pg. 20) The use of mesoscales that bridge the gap between the micro and macro, allow for explanations that operate at each scale while also being epistemologically interdependent because they rely on information that is represented in the other model. (Batterman 2002, pg. 19) The necessity of boundary conditions and multiscale modeling look much different from a purely causal or nomological model of explanation and introduce representational considerations that challenge either monistic approach. The multiscale modeling approach in this context does not explicitly reference a scientific law to explain limb development, nor does it allow for fundamental causal difference-makers to be determined because the models rely on mathematical boundary conditions that subsequently require multiple models to sufficiently explain the phenomena in question. Further, the boundary conditions also limit the possibility of reduction because they represent artificially imposed limitations on the ability of microscale explanations or models, understood in a reductionist framework to be more fundamental, to autonomously explain the phenomena in question without reference to macroscale models.

Section 3.2 Modeling with equilibrium assumptions

Another example of explanatory challenges across scales is found in economics, highlighting that challenges to monist accounts of explanation are present in a variety of scientific fields. Jennifer Jhun (Jhun 2019) acknowledges the role that mesoscales play in scientific practice by referencing Batterman's example of a steel beam (Batterman 2013). She notes that Batterman has correctly highlighted the difficulties in using a reductive approach to explain crucial properties of the beam, arguing the lesson from Batterman is that

> "Knowing all the details about the microstructure of the steel beam won't help us at all. Looking at just the macro-scale won't do it either; a steel beam looks fairly homogeneous from that perspective, too. Even in this fairly mundane case, it turns out that structural features crucial to steel's ability to withstand shocks are located neither at the micro- nor the macro-scale, but at meso-scales in between. Giving rise to these features are dislocations, structural imperfections, 'non-equilibrium defects' that contain trapped energy even though the overall structure of the material they appear in may be at equilibrium." (Jhun 2019, pg. 466)

Even though the details of a steel beam's microscale structure can be instructive in some contexts (nanoscale crack formation), it is not explanatory as it pertains to macroscale behaviors such as elasticity. The material goes into equilibrium based on scale-indexed features such as grains that stop dislocations in the crystal structures of steel, found in the mesoscales (Jhun 2019, pg. 466). This makes modeling behavior such as steel elasticity scale sensitive, since the assumptions about what causes the material to deform or not, are only relevant to the scale that they are utilized in. The mathematical equilibrium assumptions that operate to balance out competing forces present in the model only work at specific scales and as such require multiple models to adequately explain the phenomenon. The dependence of modeling this phenomenon on scale sensitive equilibrium assumptions also pushes back against a reductive approach that assumes models can be collapsed into lower scale models. As we have seen the equilibrium assumptions would vary across these models at different scales and as such would not allow for reduction in the manner that has been presented so far. Subsequently, Jhun argues that similar difficulties in successfully employing bottom-up reductive explanations exist within the field of economics. Jhun identifies the use of equilibrium assumptions in economic models as a practice that similarly helps enable scale sensitive explanations and limits of the possibility of reduction, describing the assumptions as idealizing strategies that "... help us characterize the system of interest by helping us delineate causal relationships between salient, scale-dependent variables." (Jhun 2019, pg. 470) Jhun uses the example of a firm considering the effects of downsizing by modeling the effects of a shock to the particular economy on the equilibrium price of their good (Jhun 2019, pg. 464). By considering the effects on the particular economy and holding the larger economy static, the model allows for analyzing the impact to the firm's goods without being computationally intractable. These equilibrium assumptions thus are only relevant for the smaller scale model and are not relevant for analyzing the economy at large. Jhun compares this practice of equilibrium price modeling to the Carnot cycle in thermodynamics, where a heat engine is modeled without energy dissipation in the form of heat loss through friction. This idealized approach allows one to "... see the pure causal relation between pressure and volume—and only pressure and volume" (Jhun 2019, pg. 463). By idealizing the economic effects at large and the loss of heat through friction, these models can allow scientists to explore the particular variables and relationships that they are interested in. Although Jhun points to the causal relationships in the economic and thermodynamic models as the point of interest, neither are the kind of causal relationships that are required by a purely causal account of explanation. The idealization that occurs in both models doesn't reflect conditions in the world in the way that ontic causal explanations seek to capture, but they still offer researchers insight into understanding economies and engines respectively. Furthermore, the variables of interest operate in such a way to be "sensitive to scale" (Jhun 2019, pg. 467), so that establishing a reductive hierarchy of causality across scales may be impossible. Jhun uses the example of generalizing from the law of consumer demand that states an increase in price of a good will decrease demand for it. Although this seems to work for describing an individual, on the macroscale it doesn't seem to hold, Jhun notes "The 1970's Sonneschein–Mantel–Debreu (SMD) theorem is the formalization of this difficulty. It states that the rationality assumptions that apply to individual demand functions do not carry over to the aggregate, so that familiar utility maximization assumptions on individuals do not lead to any testable, interesting properties of the population as a whole." (Jhun 2019, pg. 467) Instead of using microscale relationships to explain macroscale behavior, Jhun argues that the macroscale properties should be integrated into the explanation as well. The multiscale approach to explanation solves the problem of integrating both microscale insights with macroscale insights and allows for a fuller picture of the dynamic systems being studied. Similarly to the role equilibrium assumptions play in allowing for multiscale explanation while resisting reduction in the steel beam example, the role they play in economic modeling resists reducing the macroscale economic behavior to theories about individual behavior. Within both material science and economics, when phenomena require multiscale explanations, the possibility of classical reduction is not only lowered but not epistemically desirable. The assumptions made that enable modeling the phenomena are not compatible across scales, accomplishing the goal of understanding is more valuable to researchers than being able to establish a hierarchy of scale indexed explanations.

Section 3.3 Modeling crack formation in nanoscale materials

A final example from scientific practice that can be instructive for establishing an explanatory framework is the study of crack formation and propagation in nanoscale materials. The study of this phenomenon highlights the necessity for a multiscale approach to explanation, though the manner in which the separate scales operate with respect to each other requires different types of relationships across different scales.

Eric Winsberg (2006) advocates one of these novel relationships by introducing the concept of handshaking across different scales of analysis. Winsberg notes that studying crack formation and propagation in nanoscale materials requires “Modelers... to use these multiscale methods— the coupling together of different levels of description—because each individual theoretical framework is inadequate on its own at the scale in question.” (Winsberg 2006, pg. 584) The three different scale frameworks are Continuum Mechanics at the largest scale, Quantum Dynamics at the smallest scale and Molecular Dynamics at the mesoscale between CM and QD. CM works well at studying crack propagation at larger scales and “... provides a good description of the mechanics of macroscopic solids close to equilibrium. But the theory breaks down under certain conditions.” (Winsberg 2006, pg. 584) Winsberg makes the point that when the system is far from equilibrium, it becomes apparent that a different framework is required. The assumptions that enable CM to work require assuming homogeneity at the smallest scales but effectively modeling solids further away from equilibrium requires including the heterogenous details involved. This is the same problem that arose for utilizing equilibrium assumptions across different scales in economics as well. Since a new framework is needed, this is where MD is introduced since it does a better job at modeling the material. Winsberg notes that MD is "The ideal theoretical framework for studying the dynamics of solids far from equilibrium", specifying that "This is the level at which thermal fluctuations and material decohesions are most naturally described" (Winsberg 2006, pg. 584). Since MD is constrained due to computational intensity, it is generally only used for small areas of about "107–108 molecules" (Winsberg 2006, pg. 584), where the material is far from equilibrium, while CM can be used to model the rest of the material that is closer to equilibrium. This allows for a multiscale modeling approach that saves scientists from considerable computational investment while modeling the area of interest with a more accurate framework. Winsberg though notes a further problem that "When cracks propagate through a solid, this process involves the breaking of chemical bonds. But the breaking of bonds involves the fundamental electronic structure of atomic interaction. So methods from MD (which use a classical model of the energetic interaction between atoms) are unreliable right near the tip of a propagating crack." (Winsberg 2006, pg. 585) At this atomic scale, it becomes necessary to use QD to properly analyze the phenomenon of crack propagation, with the downside of even further computational expense than MD. Though similar to the relationship between CM and MD, the use of QD can be limited to the necessary scale of ~250 atoms in size where the tip of the crack is propagating. Outside of this extremely small area the methods of MD are sufficient for modeling and then CM can be used to model the rest of the material. Winsberg argues that this tripart modeling of nanoscale crack propagation is inherently multiscale because of the three different frameworks used to model the phenomenon, all of which are necessary, while none are individually sufficient to account for the behavior. Beyond being inherently multiscale, Winsberg argues that this particular case demonstrates the models are "parallel multiscale" (Winsberg 2006, pg. 586). The relevant difference between “serial multiscale" and parallel multiscale is that in a serial simulation the data is aggregated and translated to be passed up to a higher scale framework, while in parallel “What seems to be required... is an approach that simulates each region simultaneously... and then allows each modeling domain to continuously pass relevant information back and forth between regions.” (Winsberg 2006, pg. 586) This more dynamic picture of multiscale modeling still treats each scale framework as necessary features of modeling phenomena like crack propagation without serial approaches to data aggregation and translation for communication between the respective frameworks. The key for avoiding serial aggregation and translation according to Winsberg is that the theories overlap and thus form “‘handshaking regions’ (that)... are governed by ‘handshaking algorithms.’” (Winsberg 2006, pg. 586) This new understanding of the relationship between scales is orthogonal to the orthodox view of multiscale modeling and “to the extent that the literature in philosophy of science about levels of description has focused on whether and how one level is reducible to another, it has implicitly assumed that the only interesting possible relationships are logical ones” (Winsberg 2006, pg. 591), instead by taking the parallel approach “What this suggests is that there can be a substantial physics of interlevel interaction; a physics which is guided, but by no means determined by either the theories at each level or the mereology of their respective entities.” (Winsberg 2006, pg. 592) Since the different scales and models are interactive they do not stand in a reductive or emergent relationship but are instead mediated by the handshaking relation that allows dynamic communication between them. This novel relationship could establish a new way of conceiving multiscale modeling that allows for more seamless modeling of phenomena that span multiple scales. This approach pushes back against the previously mentioned multiscale models that insist on boundary conditions or microscale and macroscale incompatibility providing ineliminable barriers to seamless holistic model integration.

Winsberg's interpretation of nanoscale crack formation and propagation within a parallel multiscale handshaking framework, although appealing for its novel approach to model integration, is not without its critics. Most notably Julia Bursten (2018) argues that Winsberg's account "does not attend to the different ways in which scales may be 'sewn together.'" (Bursten 2018, pg. 2) Bursten points out the main problem with Winsberg's Handshaking framework is that it does not sufficiently take into account the differences between the respective handshakes at different scales. Particularly, the handshake that is supposed to occur between the mesoscale MD and QD models do not reference similar conceptual entities and therefore maintain their autonomy, as opposed to Winsberg's more dynamic and continuous interpretation. Bursten notes that while in the case of CM and MD's handshake, "The physical and computational details of the models in turn constrain the handshaking strategies available" (Bursten 2018, pg. 2) The assumptions that are utilized to make CM and MD's handshake work such as continuous deformability of the mesh make the options for handshaking limited, though in other handshaking cases these assumptions are not even a possibility. The handshake between MD and QD (referred to as TB) does not work in the same way due to the fact that although CM and MD operate under classical mechanics assumptions, QD uses Quantum Mechanical assumptions that require different conceptual machinery. The handshake uses fictionalized entities that are created only for this particular model interaction so that communication between the models can occur, "The MD/TB handshake is constructed from the electronic interactions of a carefully-defined set of... fictional 'atoms,' called 'silogens,' are assigned some silicon-like properties and some hydrogen-like properties." (Bursten 2018, pg. 6) Bursten notes that the properties assigned to silogens are not found in silicon atoms and thus make silogens useful fictions. The Quantum scale models individually do not require the use of the silogen entities to effectively model the quantum scale crack propagation but are required to make the handshake with the mesoscale model work. This transformation of the QM model to make the handshake work is much different than the smoother integration possible between the continuum mechanics models and demonstrates the relative autonomy of the lower scale model. Bursten sums up the difference between the two handshakes by noting, "... by lumping the relations between Broughton's models into one category of theoretical activity, Winsberg overlooks the intricate system of physical, computational, and pragmatic constraints upon the individual handshake algorithms." (Bursten 2018, pg. 7) She further notes that the constraints on the algorithms force Winsberg to utilize different strategies to make the handshakes work. The variety of strategies then "generate different representational content, as well as different ways of rationalizing both the permissibility and effectiveness of modeling the crack propagation behavior in this multiscale model." (Bursten 2018, pg. 7) What the difference in the handshakes demonstrates is that the supposed novel model integration that seemed to be possible from the largest scale down to the smallest is not actually a viable representation of the modeling techniques used. Although this doesn't rule out using different approaches to allow models to communicate across scales it again demonstrates the difficulty in overcoming the relative autonomy present at each scale and the demands on modelers to approach each respective scale with the tools that best suit the phenomena to be explained. Instead of simplifying the models at different scales by connecting them through handshakes, Winsberg's proposal ends up demonstrating the wide variety of interscale relations that are required to allow these models to communicate. This expansion of relations further complicates the reductive picture since each model boundary requires unique mathematical modeling techniques to allow for intermodel communication.

Chapter 4: A Pluralistic Account of Scientific Explanation and Reduction

Section 4.1 Pluralistic Account of Explanation

These three examples from scientific practice demonstrate the particular difficulties with a monist approach to scientific explanation, since all three require unique conceptual machinery for their respective explanations, namely boundary conditions, equilibrium reasoning, and scale specific handshakes. The attempt to commit scientific explanation to a monistic framework that requires only one fundamental type of relationship between scales, e.g. nomological, causal, etc. is bound to run into problems when confronting the variety of explanatory methodologies employed in scientific practice. The multiscale nature of each of the examples shows how even if we accept a multiscale modeling solution, there are limits to the cross scale integration. The need for continuum scale models that aggregate and idealize certain smaller scale features and microscale models that focus on smaller, more detailed features of larger phenomena within scientific practice indicates that a more pluralistic account is needed to account for the numerous modeling approaches and intermodel communication complexities. This pluralistic approach can then incorporate numerous explanatory desiderata that arise depending on the context of the phenomenon being studied. In some cases, this might be mechanistic structures in the brain that explain particular biochemical signaling, which corresponds with a causal mechanical account of explanation advocated by Craver and Salmon. In other cases, this might be subsuming phenomena under general laws of physics that resemble a nomological framework. Though in other cases it will be necessary to utilize multiple models across different scales that require novel intermodel relations to adequately explain a phenomenon such as steel elasticity, which leads to a multiscale modeling framework that can (but not necessarily) incorporate nomological or causal accounts but also others as well. This pluralistic approach certainly does not bring the theoretical simplicity that a monistic, theory-first approach does, but it also successfully accounts for the variety of scientific explanatory practices and does so without requiring manipulating explanations to fit a predetermined formula. If the commitment is made to a unitary account of explanation, say based in causation, then essential features like boundary conditions, equilibrium reasoning and handshakes between frameworks will be missed and thus leave multiscale scientific explanation methodologically impoverished. Instead of using (largely a priori) metaphysical assumptions about what explanation should be like, the structure of scientific explanations should be driven by the (a posteriori) epistemological goals of practicing scientists in a specific context.

If explanation is understood to be answering why questions about phenomena while being sensitive to context and being guided by the epistemological ends of the scientists, then it is unnecessary to force the form of explanations to fit a predetermined reductive schema. The deflation of the ontological assumptions for explanations should make the methodological inflation easier to accept. Multiple types of relationships between the *models* and *theories* at different scales need not commit us to multiple ontological relationships between entities at different scales. The variety of conditions, assumptions and epistemic aims in this pluralist model of explanation makes the unification of explanation through theoretical reduction untenable. This fact does not necessitate denying the possibility of ontological reduction in principle, but it does deprioritize metaphysical reasoning in favor of practical considerations of scientific practice. Rather the epistemic pluralism advocated for is compatible with either an ontological monist or pluralist position. Though the possibility of ontological reduction is still in principle compatible with this framework of explanation, the epistemic goals of practicing scientists provide clearer guidance for the structure of explanation than focusing on a goal of reduction.

This independence from metaphysical implications is worth noting since some multiscale modeling advocates argue that the multiple scale framework does indicate that reality itself seems to be multiscale (Bursten 2018, Batterman 2002). For example, Bursten writes that "Multiscale modeling itself is an outgrowth of the multiscale nature of the physical world." (Bursten 2018, pg. 8) I contend, however, that this assumption that multiscale models can provide insight into the fundamental nature of reality seems unnecessary and flawed. Unnecessary, in the sense that the failure of monist accounts, lessons from scientific practice, and epistemically driven approaches provide enough arguments for utilizing a pluralistic model of explanation that incorporates multiscale considerations. There doesn't need to be a metaphysical argument given to prop up the pluralist account, especially considering the advantages demonstrated by eschewing failed theory-first metaphysical frameworks for explanation. Instead, it can be acknowledged that a variety of ontological assumptions can be compatible with this account and that any further speculation as to the nature of reality can be explored outside of the scientific context. Further, the bidirectional assumption seems flawed in the sense that the models that are fashioned within a multiscale context are explicitly created to meet human epistemic aims, answering why questions as they interest scientists. These aims are not necessarily truth-apt in a deeper metaphysical sense, especially considering the use of idealizations and approximations in the models. This, however, raises a different concern: if my pluralist account is taken to be epistemically driven and not conducive to metaphysical inferences about the nature of reality, then what role does reduction play in the account?

Section 4.2 Pluralistic Account of Reduction

Rather than having a large role in describing the nature of reality, I propose that reduction ought to be reinterpreted as a methodological research strategy in the tradition of Imre Lakatos (1968). This reinterpretation allows for various reductive practices to be a toolkit available to scientists without the reductive research program serving any larger ontologically unifying goal. Specifically, within a multiscale modeling context, this collection of research strategies can function as a subset of modeling strategies that allow for communication across scale-dependent frameworks while respecting scale autonomy. Instead of attempting classical theory reduction this reinterpretation of reduction incorporates the inherently incompatible nature of frameworks utilized across different scales and aims for communicating the information to allow for *partial* model integration for purposes of explanation. This deflationary version of reduction is similar to Nagelian reduction in bridging between the respective theories across scales but notably without the commitment to full theoretical reduction or the need for bridge laws. Instead of translating theoretical entities and models completely to lower scale theories (i.e. Thermodynamics to Statistical Mechanics), reduction becomes model integration where unnecessary model details that contribute to computational complexity are subject to mathematical simplification procedures that allow for lower scale frameworks to converge on areas of interest with larger scale models. There are many reductive modeling procedures that allow for multiscale phenomena to be analyzed across scales such as the Reduced Model Multiscale Method or R3M, Multiscale Finite Element Method or MsFEM, and the Variational Multiscale Method or VMS (Yvonnet & He 2007). All of these techniques use various mathematical methods to aggregate smaller scale models into computationally feasible larger scale models that are still sensitive to scale-dependent features. This typically involves taking a Representative Volume Element or

RVE, of the microscale location of interest and mathematically integrating the information into an approximated value for the features of interest. Then the values from the integrated RVEs are homogenized into a larger scale model until equilibrium is achieved. These techniques are used at the discretion of the researchers studying a particular phenomenon that spans different scales. This type of epistemically driven mathematical model reduction is a far cry from the large-scale theoretical reduction of whole theories proposed by Nagel. Though this interpretation of reduction does not rule out the possibility of Nagelian reduction, it is much more relevant to contemporary scientific practice, particularly in multiscale contexts.

The pluralist approach advocated for in this thesis can account for reductive practices beyond multiscale modeling contexts by recognizing context specific cases where reduction can occur. Taking the previous example of the multiscale nature of nanoscale crack propagation, the Quantum scale framework is a partial reduction of the macroscale phenomenon but because of the incompatibility of Quantum systems and continuum mechanics can’t be said to be a wholesale theory reduction. Though the nanoscale theories give more insight into particular features of crack propagation, they do not fully integrate with the macroscale frameworks, notably tracing a crack involves macroscale geometry that is not computationally feasible at microscales. The best scientists can do is to take either extremely small representative samples of an area to apply nanoscale techniques or inversely, approximate and generalize mathematically to scale up the microscale models. Though these strategies can allow for partial reduction of macroscale phenomena to nanoscale or microscale frameworks, in this case the very tip of crack propagation, it fails to account for the phenomena completely in terms of the smaller scale theory/model. This notion of partial reduction, like the mathematical modeling methods mentioned above, is acceptable within the pluralistic framework advocated for here. Though it’s worth noting that even beyond a partial reduction that occurs across a framework chasm such as between QD and CM, a reduction of a particular theory to another is acceptable within the pluralistic approach. If there is an instance where researchers realize that a particular macroscale theory is entirely explained by a microscale theory and can be wholly reduced, then there is no metaphysical principle that stands in the way. Though it is difficult to fully understand this reduction without seeing it as a theory replacement that may begin a push towards questioning the underlying framework that supported the reduced macroscale theory. Regardless of exactly how this reduction could occur, the lessons from current scientific practice currently indicate that reductions in the form of mathematical modeling methods seems to be the form that reduction takes.

Avoiding making metaphysical commitments such as the world being multiscale because multiscale modeling is effective, not only allows a pluralistic framework to avoid the mistakes of theory-prioritized approaches to explanation but it may also afford philosophical insights about epistemic limits of human investigators. The autonomy of scales indicates that our frameworks for investigating the world are not unified in a way that affords fundamental reduction. It seems too strong of a claim to argue that the world is fundamentally structured in a way that demands scale autonomy or reduction, but rather seems more plausibly to demonstrate the epistemic goals of human investigators are at odds with ontological reduction and that the scientific investigation of the world is limited by human cognitive and computational abilities (Wimsatt 2007). The first claim is a descriptive one that can be seen in the vast array of examples found in contemporary scientific practice, including the ones examined in this paper. The second claim is more contentious than the former but also less metaphysically demanding than the argument for a world that mirrors our best multiscale models. The success of scientific theories to help humanity understand the world is a credit to the power of human cognition and knowledge building, though the fundamental incongruities present between the most powerful theories, such as general relativity and Quantum mechanics, speaks to the difficulties of modeling the world in a unified manner. Rather than making claims about the structure of reality, it seems more likely the reasons why models in science tend to be unsusceptible to fundamental reduction is the difficulties in mathematically unifying models that would require, for starters, an entire reconceiving of fundamental physics (Batterman 2021). Granting the possibility of a unified theory of everything for human investigators still leaves open questions about the computational possibilities of modeling such a framework. The constraints on computational power, algorithmic efficiency and mathematical representation seem to present a very real challenge to the possibility of utilizing the theory in a way to suit the needs of researchers in other fields. Reducing the complexity of the brain to a microscale framework seems to be computationally infeasible as a primary example.

Section 4.3 Concluding Remarks

This thesis has presented a story about the various attempts to formalize the structure and goals of scientific explanation. As noted, the previous attempts to capture what explanatory reduction should look like all fail because of the monist nature of forcing an abstract theory of explanation onto all of scientific practice. The particular models chosen in this paper serve to show the modern foundational theories, nomological and causal, while also highlighting various attempts to modify or build from where these models failed, causal-mechanical and Kairetic. These various models serve as building blocks to the pluralist account that is advocated for here. The particular examples from physics, neuroscience, economics etc. all were chosen to highlight the vast epistemic goals that scientists are trying to meet when building explanations. The examples from scientific practice highlight the contemporary work on multiscale modeling and how trying to mold explanations to fit monistic reductive goals is a mistaken project that instead should encourage philosophers of science to consider the autonomy of scales and how epistemic, mathematical and practical considerations operate across each respective scale. The lessons from multiscale modeling further indicate the need for a plurality of explanatory forms in science, that serve the epistemic needs of the scientists in their particular contexts. Further, the presence of incompatible frameworks that are nevertheless ineliminable across a variety of contexts shows the epistemic limitations of human investigators and provides another reason to avoid reductive goals and that a plurality of explanatory forms is not indicative of failure but of the variety of successes in science.

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