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The landscape of integrative pluralism

(El paisaje del pluralismo integrador)

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ABSTRACT: In this essay, I revisit and extend my arguments for a view of science that is pluralistic, perspectival and pragmatist. I attempt to resolve mismatches between metaphysical assumptions, epistemological desiderata, and scientific practice. I consider long-held views about unity of science and reductionism, emergent properties and physicalism, exceptionless necessity in explanatory laws, and in the justification for realism. My solutions appeal to the partiality of representation, the perspectivism of theories and data, and the interactive co-construction of warranted claims for realism.

KEYWORDS: pluralism, realism, emergence, scientific laws, perspectivism, pragmatism, complexity.

RESUMEN: *En este ensayo, reviso y amplío mis argumentos a favor de una visión de la ciencia que sea pluralista, perspectivista y pragmatista. Intento resolver los desajustes entre los supuestos metafísicos, los desiderata epistemológicos y la práctica científica. Considero opiniones largamente defendidas sobre la unidad de la ciencia y el reduccionismo, las propiedades emergentes y el fisicalismo, la necesidad sin excepciones en las leyes explicativas, y en la justificación del realismo. Mis soluciones apelan a la parcialidad de la representación, al perspectivismo de las teorías y los datos, y a la co-construcción interactiva de las afirmaciones justificadas del realismo.*

PALABRAS CLAVE: pluralismo, realismo, emergencia, leyes científicas, perspectivismo, pragmatismo, complejidad.

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Introduction

The methodology that I have embraced in my research in the philosophy of science, as have many others, attempts to answer philosophical questions in epistemology and metaphysics by appeal to scientific reasoning and practice. This requires a continual dialogue between the normative, philosophical analyses and accurate, thick[1] descriptions of science. In employing this strategy, I have been struck by the mismatch between philosophical views of what scientific knowledge should look like, and the assumptions that are implemented in scientific practice. In particular, some long-accepted views about reductionism, unity of science, explanation by laws and causes, and realism do not find a direct application to the multiple models targeting different levels of organization and abstraction that scientists craft to account for robust, evolving, complex systems. In my attempts to resolve the tensions that arise, I have developed and defended a view of science that is pluralist, perspectival and pragmatist. I call this view integrative pluralism.

In this essay, drawn from my 2021 Raimundus Lullius Lectures, I will invite the reader to travel through the landscape of integrative pluralism, crossing and recrossing the borders between metaphysics, epistemology and scientific practice (Table 1). I aim to shed light on answers to the following questions. How does and should the metaphysical commitment to physicalism shape the relationship between explanations in physics and biology? How does and should the complexity of phenomena revise our understanding of scientific laws? How does and should the epistemic structure of representational models direct scientific practices? What kind of scientific realism is possible from a perspectival and pragmatist stance?

Table 1. Landscape of Integrative Pluralism and Perspectivism

Metaphysical Ontology	Epistemology	Scientific Practice
Compositional Materialism/Physicalism	All scientific representational models are *partial* – “leave things out”	Islands of local unifications
	All scientific representational models are *perspectival* – encode what is “left in”	Moments of partial reductions
Real phenomena have causal effects	Models can be more or less *accurate* – how well they accommodate the accepted empirical data	Persistence of model/experimental/explanatory pluralism
	Models can be more or less *adequate* – how well they serve pragmatic aims.	
Gibsonian affordances	Real reference is co-constructed by *interaction* of conceptual framework, theories and experience	Ongoing iterative integration in crafting a coherent network of beliefs

[1] Thick in the sense of Geertz (1973).

My early work on the success of functional explanation in evolutionary biology and its failure in cultural anthropology, and how functional explanation is related to the then growing number of scientific models of the division of labor in social insects, led me to explore the multiple sources of pluralism across and within scientific disciplines (Mitchell, 1987, 1989, 1992, 1993, 1997, Page and Mitchell, 1991, 1998). I confronted the fact of increasing varieties of scientific explanation, and asked what was the nature and value of this explanatory pluralism? Not all cases of pluralism are the result of competitive theories, vying to win the race to be the one, true one. Some forms of pluralism are based on compatible analyses and explanations, targeting related but not identical aspects of a phenomenon or representing causal features by different degrees of abstraction. I have focused on articulating, defending and extending a philosophical account of compatible scientific pluralism. The locus of my analyses has been in the sciences of multi-level, multi-causal, robust complex phenomena typical of biological and social organization, which provide a rich plurality of models to examine. In these investigations, I discovered new challenges to previously predominant homogeneous accounts of causation, modeling and experimentation.

In the first part of this essay, I confront the mismatches of reductionist and unificationist epistemology and some versions of physicalist metaphysics with the results, methods, and assumptions of complexity sciences. In particular, I will revisit and extend my arguments defending the capabilities of integrative pluralism to account for the consequences of *what we know*, i.e. the many scientific explanations of complex system behavior, for epistemological and metaphysical accounts of emergence and for scientific laws.

In the second part, I will explore implications of *how we know* what we know, i.e. the pluralism of perspectivism and the contextualization of pragmatism, for warranting claims about realism. Scientific knowledge is produced by human beings collectively engaging in conceptual and causal interactions with various parts of nature. The results of scientific activities necessarily reflect human capabilities and values. I argue that perspectivism of models and experiments explains how integrative scientific practices can license inferences beyond the data to what we can count as real phenomena. Adopting a pragmatist stance transforms metaphysical questions of realism from searching for what is absolute, eternal and independent of us with its accompanying strategies of analytically removing the human stain from scientific knowledge, into epistemic and methodological questions of how we provisionally justify claims that this is real and that is not. Investigating the pragmatist, perspectival character of scientific practice illuminates a constellation of assumptions involved in warranting coherent assertions of realism. My approach recommends integrative pluralism (Mitchell, 2000, 2003, 2009; Mitchell & Dietrich, 2006; Bertolaso & Mitchell, 2017; Mitchell & Gronenborn, 2017) and a pragmatist variety of perspectivism (Mitchell, 2020a, 2020b, 2023). In the first part of this lecture, I will trace the path from scientific complexity to epistemic pluralism, and in the second part from representational perspectivism to pragmatist realism.

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PART 1
From Scientific Complexity to Epistemic Pluralism

1. Introduction to Part 1

Complexity in general, and biological complexity in particular, has presented new challenges to philosophical assumptions about the character of explanation and causation as well the nature of investigations that yield empirical support for scientific knowledge (Mitchell, 2009). It is undeniable that complex structures and behaviors populate the natural world. Some complex structures, like multicellular organisms have evolved from relatively simpler ancestors, over millions of years (Maynard Smith & Szathmary, 1997). Other complex structures develop from relatively simpler components over just a lifetime, for example in the differentiation from a single fertilized cell to the nearly 200 different cell types composing a human body. Still, further complex behaviors, like the robust responsiveness to internal and external changes of e. coli in realizing chemotaxis (Braillard, 2010; Alon *et al.,* 1999), are the result of network structured feedback and feedforward dynamics. There have been significant developments in the scientific understanding of complexity, some made possible by new technologies and computational resources. The philosophical image of science needs to be extended to accommodate these developments. Are there emergent phenomena? In what relationships do the plurality of scientific models at multiple levels of organization and abstraction stand to each other? What is it about the patterns of phenomena, or the laws of nature, that support explanation, prediction and intervention? (Mitchell, 2009). A standard strategy for understanding complex systems is to decompose them into component parts and model the detached components in isolation, by experimentally shielding or randomizing non-target conditions to silence the influence of confounding factors. Then, the complex structure may be reconstituted by combining the component causal contributions. This is the logic of many experimental practices, including knock-out experiments designed to elicit the causal function of single genes and Newton's vector addition of the joint causal impact of multiple individual forces like gravity and friction on the motion of a body.

The strategy of decomposition, and reconstitution only works under certain conditions (Bechtel & Richardson, 2010). The adaptive, evolved, and robustly reorganizing complex systems that have been the focus of my research often escape analysis by this strategy. The causal impact of a component part of a complex structure can be radically local and context specific, not globally invariant. The causal contribution to a system of one component might be re-assigned to a different part, in the face of a loss of the first, as in the case of neural reassignment in the human brain during puberty, or in response to traumatic injury. A similar pattern occurs in many biological structures. As I will argue, dynamically robust systems challenge static representational mappings and linear causation, inducing us to modify and expand narrow or strict accounts of legitimate scientific knowledge. In this effort, I have defended a view of science that embraces diversities that respect the different kinds of structures that populate our universe and the necessarily partial, perspectival models that science proposes to represent their behavior.

From the 16th century onward, science has subscribed to an assumption of materialism, later to be called physicalism, that the universe is composed of material objects and properties whose behavior is the result of objective or natural causal and constitutive relations.

More complex natural structures are built from physical components – a view that can be called compositional materialism (Dupré, 1988). Adhering to this assumption clearly has implications for scientific practice. Scientific explanations and descriptions are constrained, they cannot appeal to the subjective, the non-material, or the miraculous.[2] An additional constitutive assumption of science is that there is "one objective world", which all the different sciences individually and jointly investigate. Thus, the different theories, laws, models, explanations that are accepted to explain one feature of nature, if represented by propositions, must not contradict those accounting for other features of nature occupying the same, one, world (Mitchell, 2003; see also Shaw, 2016).

What should epistemically follow from these two assumptions? I contend that inferences to reductionist and unificationist requirements for the relationship among the plurality of models and explanations do not follow from the ontological assumptions of physicalism and the one-world thesis. Compositional materialism does not entail that all descriptions, explanations or theories are reducible to descriptions, explanations or theories in physics. In short, physicalism should not be confused with physics-ism. Whether the phenomenon is the Higgs boson, an ensemble of protein conformations, a genetic regulatory network or a social institution, phenomena available for scientific investigation are assumed to be material and subject to material causal interactions. I agree. But the scientific representations of phenomena are a different matter. Descriptions, mathematical models, or simulations of phenomena are both partial and perspectival, involving abstraction and idealization of target phenomena. They do not map one-to-one on to the entirety of the undescribed targets in the world. Indeed, producing a representationally complete model which includes features at all scales, and all strengths of causal influence would not facilitate explanation and prediction. It would constitute something more like a duplicate of the target phenomenon leaving us not much better off than engaging directly with the very system we are trying to understand. No account of causal or constitutive explanation requires an account of every describable feature of a system in every possible degree of precision. To serve the goals of science, representations need to capture only those causal features that are relevant or salient for what we want to understand or do. All useful scientific representations are partial.

That means that representations in physics, whether propositional, mathematical, structural or graphical, are partial as are those in biology or chemistry or economics. There is no difference in that respect that privileges models in physics. The constraint put upon scientific models by the constitutive assumption of materialism pertains to all scientific models equally, no matter what their subject matter or level or abstraction or which compositional level they target.

The partiality of representation blocks the inference from compositional materialism to reductionism. There is no guarantee that different partial descriptions will be translatable into each other. What one model leaves out can be, and often is, the focus of what another model represents. Take the case of a protein folding into its functional conformation. Physics models this process in terms of thermodynamic properties of the different structures, idealizing the properties of water as homogeneous in the folding environment (while

[2] See Daniel Stoljar (2022) for a lucid account of the complications of holding and justifying this assumption.

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temperature and pressure are variables in the model). Chemical models include more realistic accounts of water to describe the role of the hydrophobic/hydrophilic dynamics of bonding while at the same time abstracting away the presence of other molecules that would be found in the cells where proteins fold. Biological models explicitly represent these other molecules to account for the impact of within-cell interactions, including heat-shock chaperon molecules or cell wall molecules, to describe protein folding. These different ways of looking at the same phenomenon may be compatible, and mutually informative. They all certainly endorse compositional materialism, but they are not easily translated to reduce one to another. Different models sometimes tell us different things. They do not tell us the same things in different ways (Mitchell, 2020).

There are lessons to draw about scientific practice from this view of ontology and epistemology. We should expect to find a plurality of models, representations, and explanations when we look at actual scientific practice. There will be islands of local unification and moments of partial reductions, but not the global unity in Steven Weinberg's dream of "the convergence of explanations down to simpler and simpler principles (that) will eventually come to an end in a final theory" (Weinberg, 1993). Integrative pluralism is neither anti-reductive nor anti-unification; both are conceived as categorizations of effective methods or strategies for generating of scientific knowledge. But it does oppose an ideological (and as I have argued, unjustified) adherence to either reduction*ism* or unification*ism* as the one true path to scientific understanding.

In what follows I will navigate through the philosophical landscape depicted in Table 1 by moving back and forth between scientific practice, epistemology and ontology, questioning how they influence each other, how they depend on each other and what the assumptions are made in each domain to make sense of scientific knowledge of complex systems.

2. *What is complexity?*

There is no agreed upon definition of what counts as complexity. But there does seem to be agreement about some of its features. These include *dynamical complexity* in changes in state over space and time, *compositional complexity* of the relations between the multiple parts that constitute a more complex structure, and a type of *context dependency or contingency* that attaches to the stability of the structures and behaviors of complex systems.

First, dynamical complexity exhibits a trajectory of state changes represented by non-linear chaotic functions. This type of complexity is exhibited in *Dictystilium discodium*, the slime mold, that lives in two radically different states, undergoing the equivalent of a phase transition (Kessen, 2001). As single celled amoebae they feast on bacteria and replicate asexually. However, under conditions of starvation, individual amoeba signal to each other and join together to form a multicellular slug, as the cells reconfigure, changing their shape and function to form stalks, which produce bulbs, called fruiting bodies, that send out single-celled spores that start the lifecycle again. Second, compositional complexity (Simon, 1963; Wimsatt, 2007) is illustrated by a human brain where the parts are not arranged like a pile of interchangeable sand particles, but rather display functional and structural localization. Of course, the discreteness of decomposition is unsettled in both neuroscience and philosophy of neuroscience. Third, contingency is a type of complexity illustrated by the path dependence and chancy evolutionary processes responsible for the structures that actually populate our planet. The causal rules describing evolved systems existing at a given time, have been disrupted and failed to describe descendent arrays of organisms. Examples can be found in extinction events. The dominant theory of the extinction of dinosaurs at the end of the Cretaceous Period appeals to the impact of a meteor, a chance event relative to the more orderly unfolding of evolution by natural selection.[3] As Gould has argued, evolutionary contingency implies that you cannot "roll the tape back" and get the same progression of change when you replay life's tape (Gould, 1989, see also Beatty, 2006). Complexity in this sense identifies the temporal and spatial diversity of the domain of evolved, adapted organisms which limits the generality and persistence of the causal rules explaining why organisms behave the way they do. In what follows, I will show how scientific studies of these types of complex phenomena challenge ontological claims about the reality of emergent phenomena, and epistemological claims about the necessity of scientific laws.

3. *Emergence: the mismatch*

Emergence can involve compositional, dynamic complexity, or contingent path dependence. Higher-level structures are built from lower-level components, but are they "the sum of the parts"? And what are the relationships between descriptions of a system at different levels of organization? Appeal to emergent properties in science and philosophy of science has waxed and waned in recent history. In the 19th century British Emergentists identified the existence of emergent properties of a structure, an ontological claim, with whatever could not be predicted or explained by properties and interactions of the lower-level constituent parts, an epistemological claim. For example, Mill (1843) claimed that the fluidity of water could not be explained by features of hydrogen and oxygen. Emergent features on this account are novel and cannot be predicted from laws about the properties and interactions of constituent parts. For emergentists, laws about emergent properties would be known only from observations or regularities at the higher-level, a methodological strategy. Thus, if any property could be reduced, explained, predicted from the lower-level laws, they would no longer be deemed emergent. Scientists proceeded to do exactly that with many of the properties that were standard bearers for 19th century emergentists. As a result, there was very little use of the term "emergence" in science from the 1920s until the 1960's (Corning, 2002). In this period, science itself appeared to ring the death knell for emergence. Philosophers developed accounts of why this was correct both in ontological and in epistemic terms. J.J.C. Smart defined reduction in ontological terms, an entity x reduces to an entity y only if x does not exist 'over and above' y (Smart, 1959). Others gave accounts in terms of explanations, famously Nagel (1961) outlined how bridge-principles operated in the translation of higher-level to lower-level theories, such that the content of the former could be recovered in the latter. "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

[3] See A. Chiarenza, A. *et al.* (2020) for a recent account of the evidential support for various causal accounts of this major extinction event.

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the logical consequences of the theoretical assumptions (inclusive of the coordinating definitions) of the primary science." (Nagel, 1961, p. 352). Then something changed. Talk of emergence was revived in science in the 1970's, when interest in complexity and complex systems was on the rise leading to the inauguration of complexity science (Mitchell, 2012; Corning, 2002). By 2019, scientists using both ontological and epistemological concepts of emergence are "commonplace". As a neuroscientists Turkheimer *et al.* put it (2019)

> The concept of "emergence" has become commonplace in the modelling of complex systems, both natural and man-made; a functional property" emerges" from a system when it *cannot be readily explained* by the properties of the system's sub-units. A bewildering array of adaptive and sophisticated behaviours can be observed from large ensembles of elementary agents such as ant colonies, bird flocks or by the interactions of elementary material units such as molecules or weather elements. Ultimately, emergence has been adopted as *the ontological support* of a number of attempts to model brain function. (My emphasis, p. 3)

Emergence for both philosophy and science is identified with three key features: emergent higher-level properties are novel, unpredictable and, in the strongest form, engage in downward causation (McLaughlin, 1992; Bedau & Humphries, 2008) While many philosophical analyses continue to dismiss the reality or relevance of emergence, scientists, especially since the middle of the 20th century have embraced emergence as an important feature of complex phenomena. How do we resolve this mismatch? Are the scientists misguided? Are the philosophers mistaken?

Scientists identify emergence with cases where the whole is not the simple sum of the separate parts. On this understanding, emergent properties and behaviors may still be understandable by appeal to their component properties and behaviors, but only with the addition of the knowledge of how these parts interact. Self-organization has been proposed as one account of how complex higher-level properties emerge from the interactions of individual component part behaviors, but not by simple summation nor by encoding a teleological "goal" into the rules governing those individual behaviors. (see Camazine *et al.,* 2001). Numerous higher-level patterns have been analyzed in terms of self-organization, from the clustering of cell types in fetal development (Keynes and Stern, 1988) to multi-species fish schools (Papadopoulou *et al.,* 2023) to the murmuration. i.e. adaptive patterns of large scale bird flocks (Couzin & Krause, 2003).

> It is usually not possible to predict how the interactions among a large number of components within a system result in population-level properties. Such systems often exhibit a recursive, non-linear relationship between the individual behavior and collective ('higher-order') properties generated by these interactions; the individual interactions create a larger-scale structure, which influences the behavior of individuals, which changes the higher-order structure, and so on. (Couzin & Krause 2003, p. 2)

A higher-level property, flocking, affords predator avoidance which has causal influence on the survival and reproduction of the individual birds in the flock. This case appears to display all the features that define emergence: unpredictability, novelty and downward causation. Yet some philosophers, like Jaegwon Kim (1999) claim that "higher-level properties can serve as causes in downward causal relations only if they are reducible to lower-level properties. The paradox is that if they are so reducible, they are not really "higher-level" any longer" (Kim, 1999, p. 33). For Kim emergent properties with downward causation are "apparently absurd" yet that is exactly what is described in the scientific examples of self-organization. What accounts for the mismatch of philosophical views like Kim's with scientific practice? By presenting a detailed analysis of Kim's reasoning, we can identify and locate the assumptions logically compelling his conclusions.

Kim (1999) followed in the tradition of defining emergence epistemically as that-which-cannot-be-reduced. Earlier accounts of reduction, like Nagel's (Nagel 1961) had required articulated theories of properties at both the higher and lower levels, and a method of translating vocabularies via bridge principles to determine if the lower level theories logically entailed the higher level theories. However, many of the sciences that target compositionally complex phenomena lacked the kind of theoretical structures required. Kim introduced a new account of reduction in terms of functional replacement that avoids some of the challenges of the older view. Kim replaces translation and logical entailment between claims at different levels by functional identity. His argument appeals to ontological physicalism as grounding all real causes to undermine any significant possibility for emergent properties to exist and play explanatory roles in science. Kim argued that if emergent properties did exist, they would be causally inert, since all the causal work is done by the lower-level physical constituents of a complex structure, not by properties of the higher-level.

My analysis of Kim's philosophical argument against emergence below explains the role of central assumptions in his reasoning that are shared by many who reject the attribution of emergence to the kinds of structures that scientists commonly denote as emergent (Kim, 2006; see O'Conner, 2021 and Bishop, Silberstein & Pexton, 2022 for other responses to Kim). In what follows I suggest Kim's philosophical methodology prevents his account from representing just those dynamic features of complex systems upon which scientific attribution of emergence rests.

Kim's argument is based on an analytical strategy of functional redescription of higher-level properties. Consider a higher-level property *p* that stands for something like being in pain (Kim's example) or the large-scale structure of a flock of birds (the scientific example). One first gives it a functional re-description: *p* is a state of the system that is caused by X and in turn causes Y. Where X and Y are terms of the lower-level vocabulary, i.e. physical causes and physical consequences for the case of pain. This is a kind of functional behaviorism, where we do not need to know what is going on inside the system, just the physical input-output pair that is coexistent with instances described in the higher-level vocabulary. For Kim, pain (higher-level 'emergent' description) is redescribed as that which is caused by tissue damage (material description) and causes groans and winces (material description). Then Kim invokes the assumption: "every material object has a unique, complete micro structural description." Thus, if one can translate a higher order property into a functional material description, and every material object has a micro-structural description, then distinct higher-level objects are thereby reduced to the micro-structural level. This solution to emergent objects and properties further assumes that because a *description* is in the lower-level micro-structural vocabulary (and metaphysically everything is physical) we can causally *explain* higher order properties just by appealing to causal interactions of objects represented in the lower-level redescription. And furthermore, any downward causal efficacy attributed to the higher-level property disappears with the translation. There are two problems with this strategy.

First, the assumption that compositional materialism or physicalism (metaphysical ontology) upon which all science is built, entails that there will always be a unique and complete *representation* of a simple or complex physical object (epistemology). As I have argued above, the unescapable partiality of *representation* disrupts the inference. There is no unique, complete mapping of a natural (physical) object into any language. Kim's assumption suffers from the conflation of physicalism with physics-ism. The representational completeness assumption of Kim's strategy is unjustified.

Second, there is an important difference between representing a property attributed to an object "she is in pain", or "the birds are in a flock" and understanding how the property arises and is maintained in the object. The physical functional pair <nerve damage, winces and groans> black box the internal causal dynamics into whatever it is that satisfies the input-output function and calls that pain. Indeed, in the case of pain, if one were to flesh out the details of nerve damage from sensitivity at the site of trauma to the spinal cord via the peripheral nervous system to the higher brain through the central nervous system in the ascending pathway followed by descending signals to motor responses that would issue in winces and groans, there would be temporal misalignment between the beginning and end of the occurrence of pain and various stages of signaling that are inside the black box of functionally redescribed pain.[4] Because there are different kinds of pain which have different mechanisms, and different symptoms, there would also be different ways in which pain could fail to occur depending on the location of the disruption to the pathways which are the micro-physical descriptions of nerve damage and winces and groans. Minimally, this is a case of multiple realizability by which the "unique" requirement of the unique and complete description assumption is no longer going to align the functional account of pain with the various microstructural accounts of physical, chemical, and electrical pathways. Alignment is further challenged when we consider cases of phantom limb pain or psychosomatic pain. Multiple realizability is one means of defense against reductionism given that causal explanations are more than local, token descriptions and the categories, taxonomies and concepts of higher-level models and lower-level models are not inter-translatable, not even within the functional redescription strategy.

The functional redescription strategy provides another insight into the mismatch between philosophical and scientific approaches to emergence. Kim presents us a static mapping of higher-level properties to lower-level properties. This approach makes invisible how the higher-level properties arise and are maintained. Not all system level properties are emergent, some are merely aggregative (Wimsatt, 1997) or resultant like the total mass of a protein assembly which is just the sum of the masses of the individual proteins (Regenmortel, 2004). What determines if a complex structure, behavior, or property is scientifically emergent is the type of causal processes which produce and maintain it. These processes and the constitutive and compositional components do not violate physicalism. On the scientific account, the causal structure of dynamic processes replaces a static mapping function between properties in defining emergence. Self-organization dynamics of positive and negative feedback, for example, generates and stabilizes a property at a higher-level, a property not possessed by any of the lower-level components and not the simple sum of its parts. For example, Melanie Mitchell defines complex systems as "a system

[4] I thank Jim Bogen for pointing this out to me.

that exhibits nontrivial emergent and self-organizing behaviors. The central question of the sciences of complexity is *how* this emergent self-organized behavior comes about" (my emphasis, M. Mitchell, 2009, p. 13). Within a dynamic interpretation of emergence, downward causation can be non-problematically characterized in a way that cannot be done in a static snapshot of what is going on at a single moment or by higher-level to lower-level mapping in a series of snapshot moments. Downward causation is not mysterious on the dynamic account. In the flocking behavior described above, the large-scale structure of the flock not only has novel properties that are not possessed by the individual birds, but it confers fitness benefits on those individuals by providing predator protection by means of its density and volume. The flock can be modeled by self-organization as constituted by individual birds each following simple rules of collision avoidance, orientation alignment and attraction (Couzin and Krause, 2003). Different parameter values e.g. the number of individual birds, and different environmental conditions e.g. varying wind speed, influence the shape of the flock and its stability. Some emergent features of a high-level structure can be explained by self-organization dynamics in this way. This might be understood as a kind of weak emergence, since it permits explanation of higher-level behavior by appeal to the behavior of component parts of the system.[5]

In addition, the higher-level structure itself can influence the rules followed by the individuals causing them to encode different features which then influence the higher-level structures and back down again. This is downward causation, the signature feature of strong emergence. Individual birds are adjusting their distance from each other by following simple rules of collision avoidance, changes in flock configurations partially determine how those rules are expressed by each bird, and, thus, by self-organization, a flock structure emerges. In addition, changes in the behavior of individual birds are caused by being in a flock, the new behaviors of the individual birds then interactively change features of the flock which in turn causes changes in the individuals and so on. Importantly, individual learning and social learning in birds that may be enabled or enhanced by flocking can influence not just the output behaviors, but the internal rules followed by the individuals (Slagsvold & Wiebe, 2011).

There are many other examples of dynamic downward causation, that appeal to both self-organization models and also network structures (not the properties of the objects occupying the nodes of the network) to explain robust biological systems. Robustness is the property of a higher-level complex structure by which the system level values (system temperature, chemotaxis, genetic regulation) are stably maintained in the face of internal and external perturbation in uncertain environments (Edelmen & Gally, 2001; Greenspan, 2001). Kitano (2004) identified robustness as a fundamental feature of complex biological systems, which, in part, led to the development and spread of systems biology. Examples of the complex dynamics of robustness are widespread including neuronal reassignment in the brain that can be induced by traumatic injury, genetic expression that can be triggered by social conditions in honeybees, cellular factors that can be radically altered by an organism's location in a social hierarchy in fish (Stelling *et al.,* 2004; Robinson *et al.,* 2005; Fernald & Maruska, 2012; Sinha *et al.,* 2020; Aerts *et al.,* 2016).

[5] See O'Conner 1994 & Bedau 1997 for seminal discussions of weak and strong emergence. Bedau identifies the difference with the absence (weak) or presence (strong) of downward causation.

I have argued that by moving through the philosophical landscape of metaphysical assumptions, epistemological desiderata and scientific practices we can locate the sources of divergent views about emergence. I explicated how Kim's bottom-up-only view of causality was taken as a consequence of physicalism, and that he then mistakenly inferred that there would thus be unique and complete microphysical representations of natural, physical phenomena. Translation of one description (higher-level) into another (microphysical) collapses the causal dynamics of self-organization and downward causation, the hallmarks of scientific emergence, into some kind of "self-causation" which Kim calls "absurd" (Kim 1999, p. 28) Kim's argument suffers from the confusion between physicalism and "physics-ism", discussed above. Not only is emergence possible for physicalists, the current scientific appeal to emergence displays how it designates a unique class of physically causal and constitutive structures. Scientific emergence does not require a rejection of physicalism in the metaphysics part of the landscape, though it does reject a reductive physics-ism as a necessary epistemic consequence. The dynamic systems approach has spurred new research programs in the development of self-organization modeling and robust system analysis to explain both what emergence is and how it operates in a diversity of types of complex systems. Reductive strategies do provide explanations for much, but not all of the phenomena that populate our world. Thus, a plurality of models at different levels of organization, higher-level, lower-level, middle level, et cetera, targeting the contribution of different individual causal factors, downward causes, upward causes, genetic factors, biochemical factors, social factors are required. These partial representations and targeted investigations can then be integrated to provide more accurate explanations and predictions of what science observes. Emergent properties are part of the ontological furniture of the universe.

In the next section I will consider another type of mismatch, this one between the third type of complexity, context dependency or contingency, and the requirement that causal explanations must use strict laws.

4. Scientific Laws

I argue that contingent complexity evident in evolved biological systems, reshapes the landscape of the kinds of dependencies that, should be counted as epistemic resources for causal explanations, namely as scientific laws. The traditional philosophical account of laws, in part inherited from the Logical Positivists, restricts lawfulness to exceptionless, naturally necessary, universal truths. But the causal relations in and generalizations about evolved and historically situated complex structures do not appear to have the resources to meet those strict requirements. Yet, there is the language of laws in biology and in social science. There are Mendel's Laws, and the Law of Supply and Demand, for example. But, if strict causal laws are required for explanatory and predictive inferences, and the so-called special sciences do not have such laws, what are we to say about the kind of scientific knowledge available for complex structures and behavior in those sciences? Cartwright (1983) famously argues that even the laws of physics lie, that their abstractions and idealizations, which discount interfering causes in the physical world, leave them unable to explain the goings-on in the world we want to understand. So maybe science doesn't need laws to have explanation based on strict laws in physics. In addition to idealization, biology fails to have explanatory laws, due to the contingency of the evolved, robust complex phenomena it studies.

Some philosophers of biology agree that biology is lawless. While accepting the strict definition of scientific laws, and agreeing that evolutionary biology does not have laws, they tried to recover some notion of non-lawful explanation instead (Beatty, 1995; Brandon, 1997). Others, (Sober, 1997) instead attempted to find a way to logically transform contingent, contextual generalizations into the form of strict laws, to defend a view that biology has laws. I was confronted with these options for resolving the mismatch between strict laws and biological knowledge when I served as a respondent to papers by Beatty, Brandon and Sober at the 1976 meeting of the Philosophy of Science Association. It was then that I suggested and later elaborated a different solution (Mitchell, 1997, 2000, 2003, 2009). I suggested it was the strict account of laws that needed to be revised. Instead of defining laws as universal, exceptionless true generalizations (with natural necessity) tuned to a covering-law account of explanation and prediction by logical inference, I suggested we look instead at the functions of laws and see what kinds of knowledge claims, besides strict laws, could perform those functions. These I called pragmatic laws. The recognition of the contingent complexities in evolutionary biology and other complex systems, on my account, induces revisions in what counts as scientific knowledge itself. Pragmatic laws, functionally defined, do not need some of the features of strict laws. Stable, non-universal, dependence or invariance is enough. I now turn to the details of this argument.

Scientific laws are general claims of the relationships, typically causal, that structure the behaviors of simple and complex material objects. They are not logically necessary but discovered to be true in our world in so far as they are confirmed by observation and experiment. Logically, things could have been different. The strict law concept specifies that laws hold universally, for all space and time, without exception. Employing the philosophical strategy of logical empiricism, where scientific reasoning and practices were modeled in terms of logical relationships (explanation and prediction in terms of deductive and statistical inferences, intervention in terms of instantiation, theories in terms of axiomatized assumptions plus inference rules, etc.) laws are typically represented, in the simplest form, as $(x)\ (Px \rightarrow Qx)$. For all objects and properties, for all space and time, if the antecedent of the generalization is satisfied, the consequent will be satisfied, at least *ceteris paribus*. However, this model of laws was not sufficient to exclude universally true generalizations that seemed to be true merely "by accident". The canonical example of the difference is the comparison between two generalizations that each satisfy the universally true, exceptionless conditions. All gold spheres are less than a mile in diameter. All uranium-235 spheres are less than a mile in diameter (Goodman, 1947). Both are in fact true, there are no exceptions and yet it seems to most people that the first could have occurred if there was sufficient gold in the universe in the right place, while the second one is not possible. Uranium-235, assuming normal density, reaches critical mass at approximately 123 pounds which would form a sphere with a diameter of just under 7 inches. Nuclear fission would occur and a chain reaction begun that would detonate the uranium. That there is not sufficient gold to compose such a large sphere, is deemed accidental, but that there is no sphere of uranium-235 is considered not accidental, it has to do with the laws of nuclear interactions. To make this distinction another requirement was added for strict laws. The universally true relations must meet the condition of natural necessity.

My argument starts from a different place (Mitchell, 1997, 2000, 2003, 2009). Rather than presenting an obligate definition of what a law is, strictly speaking, and then looking at whether the knowledge claims being generated by the different sciences satisfy it, I recommend instead adopting a functional account of law. Following this epistemological approach determines what laws do epistemically and investigates what kinds of epistemic objects, models, generalizations, theories, etc. produced by actual scientific practices are adequate to the tasks. It turns out that on my analysis that there is pluralism in the types of knowledge claims that can serve our epistemic needs. Rather than restrict science to using some, possibly empty, class of statements with the strongest possible inferential power to explain, predict, and permit successful interventions, I have chosen to call all the generalizations that can achieve those epistemic tasks pragmatic laws. By expanding the conceptual breadth of laws, I suggest we can better understand the differences between the laws of physics and the laws of biology. It is not a dichotomous difference between lawfulness and lawlessness, but rather a difference in degree.

4.1. Pragmatic Laws and Contingent Generalizations

The intuitive difference between natural necessity and contingently accidental truths encodes a dichotomous structure mirroring the difference between logically necessary truths and contingent truths. While there is no consensus on what it is that makes logically necessary truths necessary (Gómez-Torrente, 2022), it is generally held that a one feature of such truths is formal (If A, then A) such that any way one interprets A will always produce a true statement. The form of contingent truths (If A, then B) do not "guarantee" their truth for any interpretation of A and B. It is contingent on the truth assignment for A and for B whether or not the statement "If A then B" is true or not. Form explains why necessary truths are true. In standard truth-functional logic, propositions are either true or not true and some propositions, by virtue of their form alone, cannot be false, hence are necessarily true. No scientific claims about what relations structure the facts of our world are logically necessary; they are all logically contingent. The argument for adding natural necessity to the definition of scientific laws in order to distinguish among logically contingent natural truths displays the same dichotomous structure. Law-like scientific generalizations when true, universal and exceptionless are either naturally necessary or accidentally, contingently true. This creates a two-box classification. The philosophical debates about how nature makes some truths necessary and hence laws and others not are legion, from positing universals or dispositions in the metaphysical column of the philosophical landscape, to identifying pragmatic or conventional grounds on the organization of scientific knowledge in the epistemic column (Ott and Patton, 2018; Carroll, 2020). They all preserve this two-box framework so some general truth "$E = mc^2$" is a strict law that is necessarily true or it is not a law at all, because it is not necessary, but contingent or not exceptionless. Notoriously, nearly all of all the confirmed and accepted generalizations in biology fall into the "not a law" box, along with "All the coins in Goodman's pocket are copper" (Mitchell, 2000). I will not engage with all the various definitions of scientific law here, but challenge the dichotomous structure of the definitional approach.

I suggest we replace what's been presented as a dichotomy with a continuum. That logical truths are either necessary or contingent might be a useful distinction in a system of logic, where necessity depends on the form of the proposition, and contingent truths, logically, depend on facts about the world. But how contingent, scientific truths depend on our world, I believe, varies continuously, not dichotomously. The distinction of necessary and contingent logical truths being in two boxes may have shaped the assumption that natural truths also come into two boxes. But I maintain that is too limited a conceptual framework to apply to natural truths. I have argued that one axis of variation among scientific truths is with respect to degrees of stability (Mitchell, 2000). That is, the *relationship* described by a scientific law, the causal or constitutive dependence among the variables represented in the law (e.g. the dependence of Q on P in (x) (Px → Qx)), though true, is typically not universally, exceptionlessly true. Rather the holding of the lawful relationship is itself contingent in the logical sense for all scientific generalizations about our world are contingent *on* contextual features of the world that make it true. Those contextual features are what confer the variations in stability, from the most stable constitutive dependencies, "laws" of the universe allegedly fixed in the first three minutes after the big bang 13.8 billion years ago, to the "laws" of biology, like Mendel's law of the 50:50 ratio of parental gene segregation in sexual reproduction - a structure that arose about 2 billion years ago, to more ephemeral, less stable "laws" of social organization like the Law of Supply and Demand in economies, which arguably could not have occurred before the evolution of *homo sapiens* some 300,000 years ago. Even the most stable physical dependencies could have been different, they are not logically necessary. Some contextual changes introduce exceptions or transformations in the lawful relationship even in physics. This seems to be the case for natural phenomena produced by cooperative effects between light and matter in super-and subradience, or in cavity quantum electrodynamics. These phenomena "are a product of cooperative effects, i.e., they cannot be understood by sole consideration of the individual constituents as they arise from the interplay among them" (Reitz, Sommer & Genes, 2022). The exceptional conditions under which the "laws" of physics fail to apply may be extremely rare, but possible. Mendel's law of segregation is contingent *on* less stable conditions, given the evolutionary contingency of body plans. Thus, the "laws" of gene segregation are made true under a set of complex, local, less probable contextual conditions than more fundamental physical laws. Exceptions to Mendel's laws are expected and explained by several mechanisms that are responsible for segregation distortion (Ubeda, 2006).

Scientific general truths are laws, on my account if they let us predict, explain and intervene in nature. General claims are detached from the specific instances of observation and experiment, the original set of data that exhibit the structural relations represented in the law, to explain other situations or predict future situations. Strict laws, (x) (Px → Qx), if we had them, would permit the most efficient way to transfer knowledge from some particular observed instances, to other instances, since it would guarantee that the very same relations between P and Q hold universally, exceptionlessly and necessarily. But less-than-strict laws can also be used for explanation, prediction and intervention. The practices for achieving these goals, however, require more than just the law to be instantiated, it requires information about the contexts in which the relation was discovered and confirmed, i.e. the other facts about the world upon which the stability of the relation contingently depends. If there are no, or few, naturally necessary laws, the epistemic practices of explanation and prediction will need to attend to the varying degrees of stability, the varying roles of local contexts in which the lawful relations may hold, to determine if they support the relation or not. The practices of science in will therefore need to discover and use information about those contexts for the stable, lawful relationships to effectively explain, predict and permit intervention. This is not a difference between a general truth being a law or being an accident, it is a difference in the degrees of stability that support lawful dependencies and in the ways in which laws can be used (Cartwright, 1979, 1997,1999, 2019, 2022).[6]

4.2. Lawfulness and Stability

A number of philosophers have also introduced notions of stability, resilience, robustness, and invariance to accommodate the explanatory and predictive value of less-than-strict laws. Marc Lange and Jim Woodward, for example, have proposed non-universalist stability-based accounts of lawfulness (Lange, 2000, 2009; Woodward 2013, 2018). And while there are clear similarities with my analysis of laws,[7] I will suggest that locating these claims in the philosophical landscape will allow us to identify important differences. Lange, Woodward and I make different assumptions about ontology and epistemology which have implications for scientific practice. I share with Lange and Woodward both attention to the relationship between what we take a law of nature to be and the associated scientific practices that appeal to laws, as well as the view that causal dependencies, or physical necessities, or invariances are in some cases matters of degree. Yet both Lange and Woodward develop their accounts in order to preserve a categorical difference between lawful general truths and accidental general truths, Lange by appeal to logical differences among sets of accepted truths, and Woodward by means of positing a threshold in the continuum of invariances.

Lange anchors his view in the metaphysical assumption of physical necessity and derives what epistemically and practically should follow. Necessity, for him, is predicated of general claims, which determine what must occur and supports the truth of counterfactuals (though he admits there are problems providing an account of their truth value). Physical necessity, he claims, is somewhere between logically necessity and accidental truth. Physically necessary laws hold independently of our knowledge, and thus are metaphysically basic, they govern what facts can occur, and what empirical claims must be true. Lange is a necessitarian about laws. The epistemology of Lange's laws, how we know what is necessary and what is accidental, is in part, driven by intuitions like those invoked in the examples of gold and uranium. Lange's account of the epistemology of laws is also, importantly, a function of his innovative account of the logical relationships among sets of claims. Lange introduced the idea of nomic preservation to distinguish between lawful truths and accidental ones, "*m* is a law if and only if *m* would still have held under any counterfactual (or subjunctive) supposition *p* that is logically consistent with all of the laws (taken together)" (2009, p. 13). The stability of the truths under varying counterfactual contexts, if things had been different in this way or that way, is the mark of natural necessity. Lange carefully considers a host of objections to and explications of his account of laws. But for my

[6] My views have been greatly influenced by Nancy Cartwright. Here is a sample of the citations of her pivotal work on laws (Cartwright 1980, 1983, 1994, 1997, 2016). Her focus initially was on abstraction and idealization, while mine was on contingency. However, we have both arrived at similar disunified pictures of nature and science.

[7] See Woodward (2018, footnote 4).

limited purposes here, it is sufficient to describe his assumptions and his strategies to elicit what role stability plays in his vision of laws. "The laws' stability turns out to account not only for the sharp distinction between laws and accidents, but also for the fact that the laws would still have been *laws* had *q* been the case, for any *q* that is logically consistent with the laws" (2009, p. 44). For Lange, stability is a metaphysical feature of the physically necessary governing laws which form counterfactually stable sets of true statements which we can epistemically access. His modification of the strict view is to allow that laws are not exceptionless truths simpliciter, but rather exceptionless relative to salient contexts of relevant alternatives.

Woodward introduces a different account of stability. On his account invariance under intervention distinguishes between causal and merely accidental generalizations. "...a central feature of laws is that they describe relationships that will (or would) continue to hold over some substantial range of different ICs (initial conditions), as well as other conditions" (Woodward, 2013, pp. 60-61). The other conditions under which invariant stability of laws would hold are changes in background conditions (insensitivity of laws), as well as changes in the presence or absence of other contributing causes (modularity of laws). Woodward does not posit natural necessity as a fundamental metaphysical truth that makes the facts line up in the invariant ways he describes. But on this account, the source of the modalities of what is possible and what is necessary is embedded in his account of causal invariance. Here, Woodward shares with Lange an essential role for counterfactuals. Instead of Lange's focus on the deductive inferences among statements, Woodward's strategy embraces causal graphs of networks of variables to represent the modality of the patterns of influence under the introduction of ideal interventions (Woodward, 2003). Actual interventions in the form of controlled experiments are a practice that can elicit relevant information about causality, and, under certain assumptions, statistical correlations can too. If two variables stand in a cause-and-effect relationship, then counterfactually, if an intervention had been introduced to set the value of the causal variable, the corresponding change in the effect variable would have been produced. Counterfactual invariance is not universally exceptionless, it is not necessary for all possible values the causal variables can take, nor is it insensitive to all changes in background contexts, or constitutions of causes. Laws hold, causes have specifiable effects described by a scientific generalization. But they can have exceptions, the causal relations can break outside that ranges of invariance.

Woodward appeals to scientific practice to explicate his notion of invariant stability required for lawfulness. "One way in which this stability feature of laws manifests itself in scientific practice is in scientists' willingness to combine the same law with many different ICs and to then use the law to calculate what would happen under these different conditions, a procedure that obviously presupposes that the law will continue to hold under this range of ICs (and typically a range of background conditions as well)" (2013, p. 61). Non-explanatory so-called accidents, however, do not carry that expectation, especially when it is easy to experimentally observe their more ephemeral connection breaking down. For Woodward, a necessary but insufficient condition for lawfulness is that there be at least some test interventions under which the causal relationship is invariant. Pure accidents will fail this condition. Above this minimum Woodward endorses a hierarchy of increasing invariance and introduces a qualitative threshold above which we classify the generalizations as laws (for example, those in fundamental physics) and below which they are not laws, but nevertheless causal generalizations (like those in the special sciences). Only accidents are not causal. Given this, Woodward proposes a classification hierarchy among laws, causal generalizations and pure (non-causal) accidents. For Woodward, it appears that laws occupy only an honorific position, not a difference in kind from other causal generalizations. If this is correct, then on my pragmatic or functional analysis of laws, Woodward's laws plus causal generalizations coincide with my attribution of laws to the entire causal continuum of stability, differing only in when we are willing to ascribe the name 'law".[8] For Lange, laws have an additional attribute, namely physical necessity, a modal feature identified by the stability of truths in nested sets of governing laws.

There is another important difference. For Woodward and Lange, the modal content of laws references not just what actually occurs but what counterfactually *would have* occurred, for Woodward, or *must have* occurred for Lange. This is a component of Woodward's appeal to imagined productive counterfactual arrows in a causal graph under ideal intervention and for Lange in the truth functional relations among sets of counterfactual claims. It is clear that Lange's view of physical necessity is a metaphysics-first account. Metaphysical necessity is what makes some truths laws and other true claims merely accidental. For Woodward "invariance-based accounts provide a naturalistic, scientifically respectable and non-mysterious treatment of what non-violability and physical necessity amount to: this just amounts to the claim that within the domain of invariance of a law there are no initial and background conditions that might be realized—nothing that might be done by nature or an experimenter—under which the law will fail to hold" (2018, p. 160). There is no more metaphysical truth-maker for Woodward than the invariance itself. But what kind of invariant stability is at work here?[9]

Both Woodward's and my account of laws eschew thick metaphysical truth-makers. Yet Woodward identifies the modality of laws with counterfactual invariance. In this sense, he is a counterfactualist, invariances explain why what happens does happen, because what happened had to happen, since if things had been different within an appropriate range of interventions, events still would have occurred as described by the law.

However, for adapted, evolved, robust complex systems, the appropriate range of interventions may not be unique nor singular, which introduces problems for any counterfactualist account. In contrast, I have defended an "actualist" account. On my view of laws, what shapes the sphere of invariance has to do not with what doesn't happen, but what does.[10] Namely, the actual conditions that occur, here and now, but maybe not there and then, stabilize the relations represented in a causal law. Consider the existence of sexually reproducing organisms and the process of gamete production in the absence of meiotic drive that support the fifty-fifty gene segregation invariance described in Mendel's Law. Sexual reproduction, the mixing of genetic material from different individuals is not necessary for reproduction, and in fact raises a paradox for adaptation accounts since it has been argued

[8] See Woodward (2013, 2018) for a discussion of how all three stable invariance accounts of laws of Lange, Mitchell and Woodward differ from Best Systems Accounts.

[9] See also the papers by Mitchell and Woodward in Earman *et al.,* 2003.

[10] My view is like Cartwright's "nomological machines" account of actual relations in nature. These are "a fixed (enough) arrangement of components, or factors, with stable (enough) capacities that in the right sort of stable (enough) environment will, with repeated operation, give rise to the kind of regular behavior that we represent in our scientific laws" (1999, p 50).

that there is a two-fold fitness cost to females of sexual reproduction over asexual reproduction (Maynard Smith, 1978).[11] In evolution of life on earth, sex arose in eukaryotes about 2 billion years ago and is currently ubiquitous, but why and how sex evolved and continues to occur is still contested. To reason counterfactually about whether Mendel's Law of Segregation *is* a law, we would have to identify which conditions could change that would preserve the 50:50 ratio of gene segregation. Some have suggested there are more than twenty different hypotheses for which conditions led to the evolution and maintenance of sexual reproduction. There may well be, at different moments in its evolution, different conditions required for the invariance described by Mendel's law. To rely on counterfactual reasoning in either Lange's or Woodward's view of laws, seems to introduce a circularity. Counterfactual reasoning does not, by itself, give us empirical evidence for specifying what is the appropriate range of invariance under which the lawful relationship will be stable. What then is the role, of counterfactuals for lawfulness? Counterfactuals reflect how humans reason about causal invariance (Byrne, 2016). They presuppose some range of invariance for known causal relations, and then draw inferences about what would have occurred, had other things happened that in fact did not. But stable conditions, and the causal relations that are supported by them, are not just in our thoughts. What conditions make causal relations invariant are the actual conditions in the world that support them, whether those conditions evolve over time, or are contextually local. Reasoning hypothetically about possible ranges of stable invariance can allow scientists to design experiments that could then provide empirical evidence of those conditions, but it is the actual conditions that ground actual invariance.[12]

In Part 1, I have considered a variety of tracks through the landscape of integrative pluralism. I have argued that scientific discoveries about of robust, self-organizing, reorganizing dynamic behaviors of complex systems have implications for how we should think about what kinds of things occupy our universe. In light of these results, the appeal to emergence as real and explanatory is made scientifically coherent. This reframes both the epistemology of reductionism and the metaphysics of physicalism. The kinds of explanations that are available to evolutionary biology, to explain why what there is does what it does, require an account of contingency that does not satisfy the universal, exceptionless and naturally necessary requirements for strict laws. But biology of adapted, complex systems does provide explanations, predictions, and the ability to intervene in nature. I have argued that what needs to shift, is the account of laws, not the practices of science. What I call pragmatic laws may require more information than just the formal invariance relations to be applied, since one needs to know under which conditions those relations hold, but they can still serve epistemic and pragmatic purposes. In Part 2, I will travel through the landscape of integrative pluralism in other directions. What and how do human abil-

[11] In asexual reproduction, the offspring have 100% of the genes of the single parent. In sexual reproduction, the offspring has 50% of the genes from each parent.

[12] There is some indication that Woodward is sympathetic to my approach. In the conclusion of Woodward's (2018, 179) he considers "the scientific project of explaining why various relationships we find in nature are invariant to the extent that they are. The goal of such explanations is to explain why certain kinds of variations in the values of certain variables do not matter for why the relationships hold—why the relationships are stable across variations in those variables."

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ities and experience shape scientific practice and the epistemic and metaphysical claims warranted by science?

PART 2
From Representational Perspectivism to Pragmatic Realism

5. *Introduction to part 2*

I have argued that actual relationships of varying degrees of stability support the contingent laws required for explanation, prediction, and intervention. These patterns of dependence are represented in scientific laws, theories and models. Scientific accounts of nature are judged by their fit with well-established theories and background knowledge in addition to the empirical results of observation and experiments. Actual dependencies are the target of scientific claims, yet their representation by abstract mathematics, semantically rich natural languages, or pictures and graphs do not provide a perfect "mirror of nature". What are the means by which scientists justify a claim that some property or relation or entity is real? The framework within which I investigate this question is pluralist, perspectival, and pragmatist.

Questions about realism and anti-realism in philosophy of science range from issues about what is metaphysically out there independent of us (Kant's unknowable *noumena*, see Stang, 2022) to literalism about the reference of every term in our currently accepted (or ultimately true) scientific theories (see Van Fraassen, 1980, ch 2). On my view, a claim for realism is an empirical question internal to a conceptual framework, comparable to Carnap's linguistic framework (Carnap, 1950). A conceptual framework contains assumptions licensing causal claims and theoretical inference, as well as what can count as evidence. Following the pragmatist tradition of Dewey (1905, 1941) and Sellars (1962) I take realism to be about warranted assertibility.[13] If you are looking for a more robust metaphysical defense of realism, the arguments I present will not address your concerns. I am interested only in the structure of scientific justification internal to different philosophical, theoretical, and experimental practices and the metaphysical projections they license.[14] My aim is to make explicit the logic and assumptions involved in scientifically justifying a claim that a phenomenon is real.

5.1. Convergence and Divergence of Empirical Data

The role of convergent experimental data models supporting claims for realism are well known. If different experimental practices which causally engage different features of an entity and appeal to different laws and assumptions generate the same observable outcome about a property or properties of the hypothesized entity, then a compelling explanation is

[13] Sellars (1962, p 75), "to have good reasons for holding a theory is ipso facto to have good reasons for holding that the entities postulated by the theory exist."

[14] Here again we might appeal to Sellars. As he says, "In the dimension of describing and explaining the world, science is the measure of all things, of what is that it is, and of what is not that it is not" (1956, p. 303).

that there is a real, robust, objective cause for the convergence. The "very remarkable agreement" of Perrin's thirteen experiments to determine Avogadro's Number is an iconic example of how empirical evidence of observables support claims about the reality of unobservables.[15]

> Our wonder is aroused at the very remarkable agreement found between values derived from the consideration of such widely different phenomena. Seeing that not only is the same magnitude obtained by each method when the conditions under which it is applied are varied as much as possible, but that the numbers thus established also agree among themselves, without discrepancy, for all the methods employed, the real existence of the molecule is given a probability bordering on certainty." (Perrin, 1923, pp. 215-16)

Experimental convergence is sometimes described as a type of robustness. Wimsatt adopts this language in connecting epistemic practices with ontological conclusions

> All the variants and uses of robustness have a common theme in the distinguishing of the real from the illusory; the reliable from the unreliable; the objective from the subjective; the object of focus from artifacts of perspective; and, in general, that which is regarded as ontologically and epistemologically trustworthy and valuable from that which is unreliable, ungeneralizable, worthless, and fleeting. (Wimsatt, 1981/2007)

Eronen (2015) further develops Wimsatt's view explicating robustness in terms of the multiple accessibility for "what are we justified in holding to be real now, as limited beings, based on the current state of science."[16] Philosophical analyses endorsing convergence/robustness as grounds for realism vary, from Salmon's (1984) invoking common causes in the background conditions of experimental practices, to no miracle arguments, to Kuorikoski & Marchionni's (2016) appeal to error reduction. Philosophers generally agree that convergence of multiple experimental results methodologically provides stronger confirmation for claims about reality than a single experimental confirmation. Divergence of multiple experimental results, on the other hand, is taken to be a symptom of either the failure of reliability of one or more of the experiments, or the un-reality of the phenomenon.

I will defend a view that might seem counterintuitive. I will argue that a pluralist, perspectival, pragmatist framework explains how divergent experimental data can provide more accurate models of phenomena and support for realism. The first is a story about perspectivism, the second a story about how the interdependence of philosophical, theoretical and experimental assumptions justifies claims of realism. I will start with perspectivism and end with realism.

[15] There are many interpretations of Perrin's logic and the warrant of his convergent experiments on the reality of atoms. See Cartwright, 1980; Salmon, 2005; Van Fraassen, 2009; Psillos, 2011; Hudson, 2020, and Chen & J. Hricko, 2023 for a sampling of views.

[16] Eronen (2015) distinguishes derivational robustness, which marks the invariance of an inferred result from multiple models with different assumptions, from multiple accessibility, which marks the invariance or robustness of the result of a common cause in a variety of experimental methods. See Dethier (2022) for arguments for the similarity of the two types of inference.

5.2. Realism about phenomena, experiments about data

Bogen and Woodward (1988) introduced a much-used distinction between data and phenomena to clarify the relationships between what theories or models describe and the results of experiments that are used to provide credence to the claims about nature made by those theories. On their account, scientific theories predict and explain facts about phenomena, like the melting point of lead, the 3-dimensional structure of a protein, or the chemotaxis behavior of e. coli. However, for Bogen and Woodward, data acquired observationally or experimentally, while serving as evidence for the existence of phenomena, cannot be predicted or systematically explained by theories. This is because data are "idiosyncratic to particular experimental contexts, and typically cannot occur outside of those contexts" (p 317). Data provide a record of what is locally observed whereas phenomena have stable, repeatable characteristics that may be detectable by means of a variety of different (local, idiosyncratic) procedures that yield different kinds of data. Phenomena for them are typically entities or structures that are not directly observable.

Another way we might say this is that phenomena are what we posit as the source of signals detected in an observation or experiment through its causal interactions with detecting devices. The results of that causal interaction are represented as data. If the experimental detection devices and procedures are *reliable* then, from measurements of the produced signals, we can construct a model from the data that can be used as evidence to confirm or disconfirm theoretical claims about the stable, non-idiosyncratic, typically non-observable phenomena.

On my view, both the data that represents the effects of experimental causal interaction with a phenomenon and the representational theory predicting the phenomenon are partial and perspectival. In what follows I will show how model and experimental perspectivism structure the warrant of claims for realism. This new kind of warrant for realism, provides us with a better picture of the roles that a modified version of realism plays in scientific practices. This requires a reconceptualization of the sources and grounds for realism, what I am calling pragmatist realism, or the realism of affordances.

6. *Perspectivism*

In Part 1, I argued that the partiality of representation blocked the inference from compositional physicalism, a metaphysical assumption, to reductionism, an epistemic methodology. To be useful, scientific representations must leave out some features of nature to provide explanations and predictions. The flip side of partiality is that by leaving some features out, every method, model, and representation "selects" features to be included. This "selection" reflects a perspective, either explicitly, by identifying the features relevant to the goal of investigation, or implicitly, by the constraints imposed by the form of the representation. Furthermore, partiality and perspectivism entail model pluralism. A single model does not deliver a complete, maximally precise representation of a given phenomenon. What it leaves out could be, and often is, represented by other perspectival models. If the features that are left out in one model but included in another are causally independent, partitionable into distinct sub-features, or neatly mereologically nested, then the multiple models might be simply combined to form a single, more complete model of the phenomenon. These situations permit islands of local unification. If the features left out in one model but included in another do not have any of the properties listed above, then unifying them by simple composition is not an appropriate strategy. In these cases, a more complete understanding of the phenomena can only be produced using strategies for *integrating* rather than *combining* the partial models. I explore features of integration that differ from those of simple combination of models in examples below.

6.1. Visual perception and experimental perspectivism

My version of perspectivism owes much to Giere's seminal work (2006a). In what follows, I will extend his appeal to different visual systems to exemplify features of perspectival models. As Giere suggests, an informative analogy can be drawn between visual perception and experimental or instrumental perspectives, illustrating how interaction with the very same phenomenon (by senses or instruments) can accurately generate divergent data. I will extend the analogy to the relationships between multiple sensory modalities and multiple experimental methods to illustrate integrative features of scientific practice, applying it to the case of experimental protein structure determination.

The visual analogy can be drawn between bees and humans which are both trichromatic; that is, they each have three photoreceptors within their visual systems by which color perception is constructed. However, humans base color combinations on red, blue, and green wavelengths, while bees base all their colors on ultraviolet, blue, and green wavelengths. The same flower from the perspective of a bee and the perspective of a human looks very different (Giere, 2006a, 2006b). Bees cannot see red, humans can, but flowers display ultraviolet color patterns that bees detect, but humans cannot. When humans see a uniformly yellow evening primrose, for example, the bee sees a white edged flower with a distinctive dark bull's eye in the center (which helps them locate the pollen and nectar that are found there). It is the same identical flower made of the same material in the same environment, but the visual apparatus of a bee and the visual apparatus of a human access different visual signals that are afforded by the same flower. There is one phenomenon, the flower, and one task, representing its color, but two diverging visual models each partial, some wave lengths characterizing the flower color are not represented in either, and perspectival, what is represented in one is not in the other, and vice versa. Which one is correct? The answer is both.

This, I suggest, is similar to what happens in the causal, experimental interactions designed to detect the atomic-level structure of proteins. The model of the very same protein inferred from the data generated in the experiments, does not "look" the same in an X-ray Crystallography experiment as in a Nuclear Magnetic Resonance experiment. The inferred models can diverge. In fact, a comparison between the two experimental protocols on 109 nearly identical proteins, "showed that the structures are, on average, surprisingly dissimilar" (Sikic *et al.,* 2010, p. 92). Like the two representations of the flower by bee and human, the two representations of the protein by X-ray and NMR detections each produce only partial and perspectival data. They differ in their experimental set-ups, the subset of features used to characterize molecular structure and, in this case, use different physical theories to infer the data model from the results of the source-to-signal causal processes of the experiments.[17] When the two experimental models of atomic structure of the same protein

[17] For more details, see Mitchell (2020).

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diverge, which one is correct? If we understand protein structure models as propositional descriptions of the actual protein in the world, then the divergent models make contradictory claims, and we must conclude that at most one of the representational models is correct, the true proposition. Perspectivism allows a different resolution to divergence, one that more accurately captures scientific practice.

Within the perspective of X-ray experimentation, what is detected are interactions between electron clouds in the protein molecule when it is bombarded with X-rays, while the NMR perspective detects nuclear magnetic changes produced by varying the magnetic field in their environment. Each has a partial perspective on the atomic structure of the protein. X-ray experiments allow an inference to the relative positions of atoms, and NMR experiments permit inference to the relative distance between atoms. From their different measurements, they can generate divergent 3-dimensional predictions of structure of the same protein. They produce scientific models of the same phenomenon accessed, in one case, as structures of electron clouds of atoms scattering X-rays vs. in the other case, as structures of nuclei in atoms in magnetically varying environment. They use different parts of physical theory and different methods of interacting with their samples. When the two experimental protocols are operating as they should and, yet, deliver different predictions, the answer to the question, which one is correct?, just as in the case of color vision of bees and humans, is both. Each experimental modality correctly detects the features it targets and correctly represents the protein from that perspective. To be sure, not all experimental divergence is like this. Sometimes, one of inferred models is incorrect, the experiment is unreliable, or the underlying theoretical assumptions are wrong. But perspectivism not only illuminates cases where both models are correct, it offers a further insight into how divergent models can, through integration, produce more accurate results than the "sum" of the partial accounts.

6.2. Multi-model sensory integration and experimental data integration.

I suggest we extend Giere's color vision analogy to experimental model perspectivism by appeal to another feature of sensory perception, namely, multi-model sensory integration. Our five senses—sight, sound, taste, hearing and touch—permit us to acquire information about nature. These different modalities detect different aspects of a given phenomenon. Here again, there are differences in perspective and partiality of data that depend on the types of signals accessible by the different sensory apparatus. Consider sight and sound. What information can we acquire visually? Reflectance, saturation, color, light reflection and refraction. Simply put, the human eye has a cornea and a lens that focuses light onto the retina. The retina includes millions of light sensitive cells - rods and cones. When light hits the rods and cones it is converted into an electrical signal that is relayed to the brain's visual cortex via the optic nerve. What about sound? What auditory information do we acquire? Now it is sound waves, not light, that is detectable. The basilar membrane in the inner ear detects frequencies of sound waves by vibration. Different frequencies activate different groups of neurons on this membrane. In addition to detecting what tone is being emitted by the target source, hearing also can locate the source of the sound by using the difference in loudness and timing between the two ears. As the eye and ear illustrate, the same organism can have multiple apparatuses to detect different features or different aspects of one phenomenon. The auditory and visual models of the relative location of an entity, for example, can diverge as a result of the differences in the source-to-signal causal mechanisms producing data using the two different detecting instruments. Each set of data is partial, and perspectival, getting at some features but not others, and getting at them by different "experimental" protocols each with its own range of precision.

For most tasks where we detect something in the external world, we employ multiple senses. An object affords the production of different, but accurate, representations through these different sensory modalities. The individual senses may have optimal usefulness in different circumstances, thus in some contexts dominance or deference would be appropriately attached to one of the senses, dismissing the input from the other. For example, in extremely low light, or visually cluttered environments auditory signal might be more accurate, in noisy (literally) environments, visual signal might be more accurate. If you are a birdwatcher, like me, you will have experienced this difference.

Not surprisingly, using two senses is often better than just using one. Experiments have shown that neural processes of integrating multiple modalities, like vision and audition, produces an increase in the likelihood of detecting and identifying events or objects over a single modality. What might be surprising is that the degree to which multi-sensory integration is better; it is super-additive. Multi-modal integration in the brain has been studied at both the neuronal level and by experiments on task completion: to quote two scientists who have studied the phenomena "the integrated product reveals more about the nature of the external event and does so faster and better than would be predicted from the sum of its individual contributors." (Stein & Stanford, 2008, p. 255). So not only does using different perspectival senses provide more information than using a single modality, the cross-modal interaction of stimuli can lead to multisensory integration which yields a nonlinear, amplified neuronal response and more than additive reduction of time to task completion. Using compatible input from two senses can generate more accurate models of external phenomenon than their "sum."

I suggest something like this occurs in the scientific practice of "joint refinement" of X-ray and NMR experimental models of proteins. Joint refinement is a method of mutual error correction that identifies and accommodates the protocol-specific systematic biases of the individual experimental approaches. A joint refinement procedure investigates the compatibility of different data generated by each of the two methods, and when possible, attributes differences to the instrumental biases. For example, proteins have to be crystallized for X-ray experiments. Therefore, they are in a different physical state from those in NMR experiments which are kept in liquid solutions and that difference can account for some of the divergence in the predicted structures. In the fixed, crystallized protein, the same signal is received by atoms in elements that are on one sheet of a protein as from atoms on elements that are on distinct sheets. In NMR those sheets move independently and thus a discrimination between atoms on one instead of two sheets is possible. X-ray diffraction is primarily sensitive to the overall shape of the molecule, whereas NMR is mostly sensitive to the atomic detail (Schirò *et al.,* 2020). Other sources of divergence between the two protein models include different degrees of error in data retrieval, and in the ranges of uncertainty in the inferential algorithms used by the two experimental protocols (Carlon *et al.,* 2016). When the known systematic biases have been resolved, then the refined data from the two experiments can be compared to see whether there are regions of overlap. The two instrumental perspectives each produce an ensemble of structures that are consistent with their individual data. If there is no overlap in the NMR and X-ray ensembles of data supported structures, then they would be determined to be incompatible. The "joint" conclusion would be that an accurate picture of the structure could be either within the NMR predictions or the X-ray predictions or neither but not both. In such cases, underdetermination is increased with their joint contribution, thus making the result less accurate.

However, if there is overlap, then the data from each is refined to include only the overlapped structures which will be smaller than the original sets from each initial experiment. This reduces the underdetermination set and hence integrating both experiments allows the prediction of a protein structure that is more accurate than what could be obtained by either method alone. The blind spots of X-ray crystallography and NMR cannot be removed, but when they are system relative, then they can be exposed by the mutual analysis of joint refinement. In the words of one protein scientist: "…joint structural refinement using both NMR and X-ray data provides a method to obtain a more reliable structural model, which may disclose additional relevant information on its functional mechanisms" (Carlon *et al.,* 2016:1601).

Just as in the case of multimodal sensory integration, there is no third perspective, no view from nowhere of the objective color of the flower or of the objective 3-D structure of the protein from which to judge the accuracy of either sensory modalities or X-ray and NMR perspectives. What scientists have in order to judge the accuracy of any representative model are the measured data from observations from whatever detecting devices we have. When you have data from only one perspective, that is all that can be used to provide justification for a predictive structure of a protein. When you have data from a plurality of perspectives, that can provide stronger justification for a predictive structure. Joint refinement is a form of integration that is pluralism preserving and metaphysically modest. There is no reduction of one modality or experimental protocol to the other, you cannot teach the ear to see nor the eye to hear. Both are required to gain the benefits of the integrative pluralism of our multi-modal sensory experiences of the world. So too with X-ray and NMR perspectives on the molecular structures of proteins.

I have argued that integrating compatible divergent data can give us more accurate representations of nature. And that benefit accrues only when each of the experimental protocols reliably detect the target phenomenon. What more is needed to justify a claim that the phenomena causally engaged in the experiments is real? Answering this question requires a more complicated journey through the landscape of integrative pluralism. To detect the locations *en route* to realism, I will consider the example of emergent phenomena. Are emergent phenomena real?

7. *Realism*[18]

The content in a claim of realism spans a spectrum from minimal to maximal. Minimally, entity realism posits entities independent of human detection and conceptualization which serve as the causal source of experimentally detected signals. Maximally, realism commits to phenomena having the precise features and relations described in our best theories. Struc-

[18] Portions of this section are published in Mitchell (2023).

tural realism is closer to the maximal end of the spectrum committed to the real being the relations described in our best theories. These degrees of thinness and thickness of commitment are related to two fundamentalist ways of warranting metaphysical claims, a bottom-up approach and a top-down approach.[19] I will argue for a pragmatist, interactionist approach that requires both top-down and bottom-up reasoning. What we say there *is* and what we say it *does*, is justified by the ongoing interactions among representative models, causal experience and experiment, and constitutive conceptual frameworks used in reaching a fallible convergence on what is real (Mitchell, 2023).[20] I will illustrate the integration of top-down and bottom-up strategies in identifying and describing an emergent, robust phenomenon.

The bottom-up approach takes causation as the foundation of positing what's real. Unobservable phenomena are taken to be the cause of experimental data and thus the interactions and results of empirical practices provide the required metaphysical warrant. Defenders of this view include Ian Hacking (1983) and Nancy Cartwright (2007). Hacking, in discussing an experiment he observed at a Stanford laboratory where electrons and positrons were sprayed, one after the other, onto a superconducting metal sphere, famously claimed that "if you can spray them, then they are real" (1983, p. 24). Cartwright suggests that what are real are causal capacities in the world, identifying capacities as the source of stable causal laws that we can infer from causal interactions. When a particular experiment yields specific measurements, for Cartwright, the explanation for these measurements posits entities in the world that have the capacity to generate those signals, similar to Bogen and Woodward's stable, non-idiosyncratic phenomena. Entity realism is inferred bottom-up from causal manipulations. Data are evidence for the existence of phenomena.

A fundamentalist top-down view recognizes that unobservable phenomena are the referents of abstract explanatory theories. Structural realists read what is real off of the formal relations represented in scientific models of such theories. Using this strategy, the best confirmed theories are the source of warranting claims about what is real. On this top-down approach what science discovers about nature are not the entities which have causal capacities but rather the structural relations that explain patterns in our observations. The structures described by the mathematical relations in our best theories are taken to be isomorphic or otherwise similar to what is real. John Worrall (1989) defends this type of structural realism when he claims that "On the structural realist view what Newton really discovered are the relationships between phenomena expressed in the mathematical equations of his theory". Current philosophical debates between entity realists like Cartwright (1983) and structural realists like French & Ladyman (2010) exemplify the bottom-up versus top-down fundamentalist dichotomy.

I suggest we break out from the dichotomous choices on offer. I maintain that we need a non-fundamentalist, pragmatist option that jointly uses both strategies for warranting the real. What we are warranted in claiming about what is real are not just structures and not just entities, rather it involves the integration of both human interventions and conceptualizations. Neither of the two fundamental strategies alone will capture the judgments re-

[19] See Chakravartty (2021) for a defense of a tight-rope happy medium between these two options. See Massimi (2022) for a detailed defense of a perspectival version of realism.

[20] See also Hacking (1983), Chang (2022).

quired for warranting claims of realism. I suggest that real phenomena are those things in the world that are sufficiently stable to afford the coordination of the results of both causal detection *and* structural representation.

7.1. Interaction and Affordance

My view is inspired by the work of the 20th century ecological psychologist J. J. Gibson who coined the term “affordance” to explain human and animal behavior (Gibson, 1979). Gibson suggested we need a term to identify properties or entities that would convey the joint contribution of both the actor and the environment in which they act. Gibson recognizes the similarity of his proposal to the idea of an ecological niche which identifies the features of the external world that are salient to particular species’ capacities to interact with it. Gibson coined the term “affordance” to accommodate that interactive relationship in the psychology of perception. For Gibson, affordances are invariant features of the external environment, that are perceived, classified, etc. dependent on an animal’s capacities to interact with it.

> The *affordances* of the environment are what it *offers* the animal, what it *provides* or *furnishes*, either for good or ill. The verb to *afford* is found in the dictionary, but the noun affordance is not. I have made it up. I mean by it something that refers to both the environment and the animal in a way that no existing term does. It implies the complementarity of the animal and the environment. (Gibson, 1977)

Affordances for Gibson are real, objective properties of the environment-plus-organism that make specific behaviors possible. For example, Gibson (1979, 133) indicated that “to be graspable, an object must have opposite surfaces separated by a distance less than the span of the hand.” The affordance, however, is not “out there” for the organism to engage, like a dispositional or causal capacity, rather it is constructed by the engagement with the organism.

> An important fact about the affordances of the environment is that they are in a sense objective, real, and physical, unlike values and meanings, which are often supposed to be subjective, phenomenal and mental. But, actually, an affordance is neither an objective property nor a subjective property; or it is both if you like. An affordance cuts across the dichotomy of subjective-objective and helps us to understand its inadequacy. (Gibson, 1979, p. 129)

I propose that we consider what is justifiably real in science to share this type of interaction relation. The joint contributions of causally grounded experimental data and theoretically structured representational models together specify what is real and what is not. I suggest we take Gibson’s characterization of affordances in animal behavior, if we substitute “scientist” for “animal” and “what is real” for “environment” to get a framework better able to accommodate the warranted assertibility of claims of realism Affordances are properties/entities/structures taken with reference to the conceptual framework.

> The affordances of real phenomena are what they *offer* the scientist, what they *provide* or *furnish to experiment and representation*. I (Mitchell) mean by it something that refers to both the causal properties in nature and the representational framework of the scientist. It implies the complementarity of both in establishing what is real. (Mitchell, 2023)

This interactionist strategy rejects both top-down and bottom-up fundamentalism, while embracing the contributions of each. What we project back as being constitutive of nature is contingent on a complementary relationship between what we actually detect and how we represent it. When that interaction is successful, judged by its consequences for human action, we, as limited beings, are warranted in claiming something is real. Realism on this view delivers a contingent, pragmatist ontology of stable, detectable and representable, entities and relations, i.e. affordances built from both experiment and theory.

Let's return to how affordance realism transfigures Bogen and Woodward's account of data and phenomena. An assumption of any experimental detection procedure is that there is a phenomenon, independent of the detecting device, that is being causally engaged. Real phenomena afford stable, repeatable causal interactions. They might be detected by means of a variety of different procedures, each producing idiosyncratic data as emphasized in Bogen and Woodward's distinction. When phenomenal stability is conjoined with reliable detection, then convergent data are taken to support claims of realism. Causal reliability is a judgment about the causal process generating data in an experiment. As discussed above, when multiple different types of experiment generate convergent data from which "the same" phenomenon is inferred, then realism is justified. When there are substantially different, if not strictly independent, ways of causally detecting the source of the signals in the experiments, and they all yield the same measurement of a feature or features of that source, then it must be because the source is a real phenomenon independent of each individual experiment with its own idiosyncratic assumptions or models of the experiment. Experimental convergence is taken as the strongest "bottom-up" evidence for realism. I contend that judgments about the reliability of data require both a theoretical characterization of the type of phenomenon being investigated, as well as a philosophical theory of causality.

Woodward's (2005) influential account of causation characterizes causal stability as invariance under intervention. On his view, invariance need not be exceptionless (see also Mitchell, 1997, 2000). To count as causal, "A generalization can be invariant within a certain domain even though it has exceptions outside that domain. Moreover, unlike strict lawfulness, invariance comes in gradations or degrees." Woodward, 2005, p. 199). Woodward's interventionist account of causation is not intended to be merely methodological, but is a philosophical, or conceptual account of causation: "… for Y to change under an appropriate intervention on X just is what it is for X to cause Y" (2005, fn1 p 204-5). One alternative account of causation that Woodward rejects, is a regularity account that requires strict laws. On the strict law view, for X to cause Y is for the generalization describing the causal relationship to be universal, exceptionless, true and naturally necessary.

Reliability in experimentation is a judgment about how well the causal signal from the phenomenon is reflected in the measurements or data of the detecting device. On Woodward's account of causation, causal relations and the generalizations that describe them can differ in degrees of invariance and still count as causes. Whether data counts as reliable evidence of a real phenomenon or not depends on the kind and degree of stability or invariance that is attributed to the causal structure by a theory about the phenomenon. Contrary to the claim that the theory of the phenomenon is or should be independent of the experimental data, for data to be deemed reliable, that theory has to be invoked.

The roles of the theory of the phenomena and causal theory in determining reliability is somewhat invisible in the easy cases of the most stable phenomena, like the melting point of lead, discussed in Bogen & Woodward (1988). In what follows I will illustrate the roles in a case of emergent biological phenomena.

What is the relationship between the reliability of an experiment and the degree of invariance of the causal relation? The melting point of lead has the properties that fit the Bogen-Woodward conception of phenomena, i.e. stability over a wide range of experimentally enacted causal conditions. On Woodward's interventionist account (2005) of what causality is, that means that the functional relation among the variables explicitly represented in the theoretical model predicting the melting point is *invariant under intervention*. For Woodward's account the functional relation is also independent of other causal factors operating, i.e. it satisfies the condition of *modularity* or independent disruptability. And the functional relation is *insensitive* to a host of conditions of varying, non-represented or exogenous background conditions. The melting of lead is predicted to occur when the forces associated with the thermal motion of the free electrons exceed the electromagnetic forces holding the electrons and nuclei in a solid lattice structure or, as Bogen and Woodward put it, "the melting of lead occurs whenever samples of lead are present at the appropriate temperature and pressure, and results from a characteristic change in the crystalline structure of this metal." (1988, p. 319). There are *ab initio* theoretical predictions for melting points of metals and there are experimental measuring procedures. While experimental measures deliver a range of data points, their average is of 327.5 degrees Celsius, though no single measurement may precisely match that value. When does the relatively close agreement between the theoretically predicted value and data obtained from multiple different measuring techniques warrant belief in the reality of the phenomenon?

For the inference to realism from convergence to be justified, the range of variation of the measured values within and between experiments has to be sufficiently narrow for the practical facts of the experiment to be interpreted as satisfying the theoretical fact of the prediction. But what if a new type of measuring technique is developed that in multiple replications indicates a wildly different value? Based on the past stability of measurements we might infer that the new protocol is not valid. Or, if there is theoretical justification for the new technique, it might explain why the value is far from the older techniques by identifying some systematic bias shared by all the older techniques. We might accept the new results and revise the account of the phenomenon based on it.

On the strict lawful regularity account of causation, science cannot tolerate inconsistent results. This account of causality requires a strict reading of Bogen and Woodward's 1988 claim that the phenomenon will occur "*whenever* samples of lead are present at the appropriate temperature and pressure." Of course, one could question whether the "appropriate" conditions are met. But presuming a shared judgment of what is appropriate, the strict laws view of causation would demand there be no exceptions. However, Woodward's interventionist account permits causation with degrees of invariance under intervention, rather than all or nothing strictness. If the conditions of the new experiment push the phenomenon outside the range of its causal invariance, both the new and old measurements can be deemed accurate, and hence both experimental processes would be causally reliable. As I will argue below, to decide if an experimental result inconsistent with the theoretical prediction is reliable (and hence a refutation for the strict law view of causality) or reliable but merely reflecting the boundaries of the range of invariance (not necessarily a refutation on the invariance view of causality) or not reliable at all, requires a theory about the type or features of the phenomenon being investigated.

The significance of theory in determining reliability of experiments and inference to the reality of the phenomenon becomes clearer when we consider experimentation on dynamically complex phenomena. Consider cases where a system-level property is caused by behaviors of the components of that system. Lead melts at a temperature (its melting point) when the thermal motions of the free electrons in the atoms composing the lead breaks the electromagnetic forces holding the electrons and nuclei in a solid lattice. There may be exceptions, i.e. lead fails to melt at its "melting point", even when the functional relationship between the variables for thermal motion and electromagnetic forces satisfy the theory, but this case is pretty close to a strict generalization.

Complex systems phenomena, like the genotype-phenotype relationship where a gene is taken to be the cause of the trait (e.g. "gene for" language in the gene for Huntingdon's chorea, or the "gene for" melanism in peppered moths, etc.), are less stably realized, more fraught by exceptions, than melting points of metals. Depending on the account of causation, and the theory of the phenomenon, the reliability of experimental detection of the phenomenon will be judged differently. This is vividly displayed in the history of gene knock-out experiments (see Mitchell, 2009) where intervening to remove (or silence) particular genes in an organism is used to identify their phenotypic function or effect. Comparing the phenotypes of organisms with and without a particular gene, follows the logic of a controlled experiment. Intervene on only one gene, leaving the rest of the genome intact, and any changes to the phenotype should expose the causal effect of that gene. In roughly 30% of experiments with viable knockout mutants, there is there is little or no phenotypic difference. For some, including Mario Capecchi who invented the knock-out technique, the inference from these "failed" experiments was that the experiment was not done correctly and, therefore, the results are not reliable (Travis, 1992). For others, the best inference was that the target gene having little phenotypic effect in a relatively high percentage of cases, should not be identified as the cause of the phenotype. A third alternative was proposed (Greenspan, 2001) that is based on the theory that there is a robustness in the network of genes that interact in the causal production of a phenotype. On Greenspan's account, when one gene is silenced, there is a reorganization of the network of genes such that, in many of knockout mutants, the normal phenotype is still produced, but by a different causal pathway. This is not a case of redundancy, where there are back up copies of genes that step in when one token is removed, but a case of "degeneracy," (Edelman & Gally, 2001) where new causal networks of interaction arise in the absence of the knocked-out node. "The relationships that have been described as pathways are no doubt real, but they need not be invariant. Their relationships are embedded in broader and more plastic networks that can be reconfigured depending on the immediate circumstances" (Greenspan, 2001, p 386).

If data from different experiments converge, then the inference is that the experiments are reliable and the phenomenon is real. But if data from different experiments diverge, then, as in the case of knockout experiments, what is inferred exposes how judgments about reliability and reality are shaped by theories of the phenomena and philosophical accounts of causation.

What justifies a claim of realism in science is a function of the coordination of experimental data (causal reliability and replicability), theoretical expectation (types of phenomena with differing degrees or types of stability) and a theory of causation (strict laws, invariance, etc.). What is real is an affordance built from the joint product of what is external to us and what we can represent.

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In Part 2 I have argued the philosophical interpretations of scientific laws and real entities and causes, deployed to explain what we experience and predict what will experience from practical interventions in the world, need to reflect both the representational capacities of human investigators and the causal capacities of the targets of investigation. I have suggested that there is an ongoing interactive process by which scientific knowledge is stabilized by the coordination of philosophical frameworks (of causation, of laws), scientific theories (of emergent phenomena and robust dynamics) and the results of experimental interventions (perspectival and partial signal detection and representation). Successful science is not judged by how close it approaches 1:1 correspondence of theories to the undescribed world. The partiality and perspectival character of representations of theory and data leave open multiple possibilities for crafting coherent, and pragmatically successful forms of scientific knowledge. What science is warranted in counting as a law, or a causal generalization, or a real entity is a built on a defeasible, coordinated, set of judgments structured by the conceptual and factual resources that are themselves changing and evolving in response to the practices those judgments endorse.[21]

Conclusion

I am extremely grateful to The Spanish Society of Logic, Methodology and Philosophy of Science for inviting me to present the 2021 Raimundus Lullius Lectures. Revisiting my early work on complexity in biology and setting out the early stages of my new project on pragmatist metaphysics has highlighted for me the continuity of pragmatism in my approach. I have been motivated by mismatches between philosophy and scientific practice on the existence of emergent properties and the obligation of reduction, on absolutist governing laws and the evolutionary dynamics of phenomena and the perspectival character of representation. I find myself in agreement with the American pragmatists in considering knowledge to be the outcome of controlled enquiry, designed to solve problems, or resolve indeterminate situations, and with empiricists generally that experimental practice is the source of warrant, while warranting is a practice that also depends on the knower, the philosopher, or the scientist who is both an inquirer into nature, while at the same time being part of nature. In my own travels through the landscape of integrative pluralism I have been buoyed by learning how pluralism and diversity are sources for advancing our understanding of nature, of science and of philosophy. Pluralism becomes productive when it is brought to bear, or integrated to solve shared problems, rather than isolated and siloed into warring camps. In that spirit, I look forward to the challenges, clarifications, objections and extensions of the views presented here as we continue in our joint inquiry of understanding scientific practice.

[21] Chang (2022) and Massimi (2022) travel over some of the same ground in developing their accounts of realism.

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