Paraconsistency, Pluralistic Models and Reasoning in Climate Science

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ABSTRACT

Scientific inquiry is typically focused on particular questions about particular objects and properties. This leads to a multiplicity of models which, even when they draw on a single, consistent body of concepts and principles, often employ different methods and assumptions to model different systems. Pluralists have remarked on how scientists draw on different assumptions to model different systems, different aspects of systems and systems under different conditions and defended the value of distinct, incompatible models within science at any given time. (Cartwright, 1999; Chang, 2012) Paraconsistentists have proposed logical strategies to avoid trivialization when inconsistencies arise by a variety of means.(Batens, 2001; Brown, 1990; Brown, 2002) Here we examine how *chunk and permeate*, a simple approach to paraconsistent reasoning which avoids heterodox logic by confining commitments to separate contexts in which reasoning with them is taken to be reliable while allowing 'permeation' of some conclusions into other contexts, can help to systematize pluralistic reasoning across the boundaries of plural contexts, using regional climate models as an example.(Benham et al., 2014; Brown & Priest 2004, 2015) The result is a kind of unity for science—but a unity achieved by the constrained exchange of specified information between different contexts, rather than the closure of all commitments under some paraconsistent consequence relation.

1. Introduction

Most scientific inquiry is focused on questions about particular objects, properties, events and processes. As progress is made in separate fields,

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answers to these questions sometimes come into conflict, for example in the 19th century dispute between historical geology and Kelvin′s geophysics.(Burchfield, 1975) When this happens, the practical response of scientists has often been to carry on inquiry independently in each field, while awaiting a resolution. The interim result, in such cases, is a state of science in which the union of commitments that scientists working in different disciplines rely on in their work is inconsistent.(Brown, 2015, p. 417)

Pluralism allows for the independence of these different scientific projects, setting aside, at least temporarily, the pursuit of a unified, consistent world view—though perhaps one might be achieved someday in the future. This seems to me to be a healthy, modest approach to thinking about science. But it raises puzzles about reasoning in science. At one extreme, the plural accounts are entirely independent; the resulting account of the world is simply the list of such accounts, and the particulars of one do not constrain any others. But sometimes, even though two fields may have little conceptual contact, they wind up in disagreement over some particular question, such as the age of the earth, that the results of both bear on. Even a plural world will not, in general, be a world for which each kind of story we tell about it is entirely independent of the other stories we tell.

Consider, for example, the separate investigations of late 19th century geologists' and geophysicists' attempts to constrain the age of the earth, the first by appeal to processes such as erosion and sedimentation combining their rates with measures of the cumulative product of the processes, while the second appealed to thermodynamics to model the planet as a solid ball of largely homogeneous material cooling from its melting point.(Burchfield, 1975) The evidence and processes each group appealed to were entirely disjoint, but they seemed to impose conflicting constraints on the earth's age. In other cases, successful accounts of some phenomena have drawn on conflicting principles, as in Planck's treatment of black-body radiation and Bohr's account of the hydrogen spectrum. (Brown & Priest, 2015)

In either of these kinds of case, a systematic account of the 'scientific world view' requires some form of paraconsistency. In the first kind of case this may not amount to much more than a refusal to reason from the *conjunction* of all the sentences relied on by our best, current science. But in the second kind of case, fruitful results emerge from reasoning that draws on incompatible commitments. Treating such cases requires a richer kind of paraconsistent reasoning.

Logical inconsistency threatens to render our commitments trivial. But the availability of paraconsistent logics together with systematic ways of avoiding trivialization even with a non-paraconsistent 'background' logic such as *chunk and permeate* (Benham et al., 2004; Brown & Priest, 2004, 2012) allow for non-trivial reasoning from inconsistent premises. So long as scientists don't specify which background logic they are assuming, *logical ambiguity* and the availability of various methods for non-trivial inconsistency-management allow for reasonable, non-trivial interpretations of such reasoning practices.

Sometimes scientists are not in a position to give a logically rigorous, consistent version of their arguments. When no-one else is either, as in the early calculus, in Planck's account of black-body radiation or Bohr's model of the hydrogen atom, it's hard to interpret or identify the logic 'behind' their arguments. Consistent, rigorous methods emerged later, after the rough-hewn early efforts were found to produce reliable results. In this and other cases, successful though inconsistent early efforts have contributed to the emergence of more rigorous, general treatments.

Beyond pointing the way to later, consistent accounts, such early efforts are significant achievements in their own right. But given their inconsistency, the reliability and even the content of the early accounts seems mysterious. Further, the emergence of later accounts consistently capturing their successes[3] may reduce the felt need to explain their initial success independently. But those accounts are not implicit in the early accounts from the start; they only emerge later, as new accounts building on and extending the successes of the early efforts are developed.

The early, inconsistent methods are often limited to specific applications, but their successes can still be broad and significant. Planck's treatment of black-body radiation, Bohr's model of the hydrogen atom and the old quantum theory that emerged from them constituted a successful program of research, with a wide range of important applications. Further, the radical nature of the theory that eventually supplanted old quantum theory suggests that it might never have been developed without the successes of old quantum theory, which helped to develop empirical and theoretical constraints, pointing the way to the bizarre theory we now call quantum mechanics (Khun, 1972; Mehra & Rechenberg, 1982; Pais, 1991).

When things turn out well, consistent rigorous methods eventually emerge, refining and uniting a patchwork of conflicting principles and reasoning that

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[3] And sometimes in the light of more than one subsequent systematic accounts—for example, the successes of the early calculus can be 'captured,' in this sense, within either standard or non-standard analysis.

successfully addressed certain difficult problems. The discovery of such rigorous methods may even strengthen confidence in the reliability of the earlier, less rigorous methods, providing a more coherent framework in which we can account for their established reliability in particular contexts of application. But convincing evidence of their reliability can emerge before a resolution of their inconsistency is available. Thus, while reasoning in science can be rigorous, systematic and consistent, this state of things is sometimes preceded by preliminary work that falls far short of those standards. And that initial work often both motivates and guides the later rigorous efforts.

Further, I have come to believe that the availability of later methods can obscure our understanding of the early work. To choose a well-known example, the inconsistent commitments of old quantum theory were not rendered consistent by the emergence of consistent approaches. Understanding those commitments and how scientists worked with them requires some account of how they could have reasoned in constrained, reliable and non-trivial ways despite relying on inconsistent premises. Notably, this has typically been done without adopting a heterodox, paraconsistent logic (Norton, 2002, p. 191), while arriving successfully at the same, or satisfactorily similar results as later, consistent treatments. Furthermore, the premises employed in the earlier work cannot be rendered consistent just by dismissing them as accounts that weren't literally believed, as Vickers has suggested (Vickers, 2013, p. 241): the most difficult challenge of understanding inconsistency in science is not about what was believed by the scientists in question, but about how reasoning with inconsistent premises can be done without either trivialization or explicit appeal to heterodox logics.

In both the early calculus and old quantum theory, scientists identified ways of reasoning that produced significant, reliable conclusions despite drawing on incompatible commitments. This reliability, though puzzling in light of the inconsistencies involved together with their clear incompatibility with standard, widely accepted views, turned out to be explicable in the light of subsequent consistent accounts which were able to match their successful applications. But it would be ahistorical to 'read' the later, consistent explanations of these successes into their inconsistent predecessors.

Further, the scientists involved need not have been explicitly or even implicitly paraconsistent in their methods or beliefs, despite working with inconsistent commitments while avoiding trivialization. Whatever their attitudes towards and methods of containing those inconsistencies, dismissing the inconsistencies as philosophically insignificant because they were eventually resolved misses the point. We may feel free to dismiss the inconsistency of such partial accounts once we have a consistent account in whose terms we can try to express the 'true' beliefs of such scientists. But the combination of inconsistency with non-triviality and successful reasoning still needs to be explained, especially so in the absence of openly avowed paraconsistency.

Whether or not past figures held what they knew to be inconsistent beliefs is a matter of personal history; some may have done so but others merely worked with accounts they did not believe to be altogether true. But both those who believe and those who merely reason with inconsistent commitments need to avoid trivialization when they reason. So how trivialization was avoided despite reasoning with inconsistent assumptions is an important question, one that the later emergence of consistent ways to achieve the same or sufficiently similar, reliable results doesn't answer.

2. Paraconsistency and Pluralism: a minimal approach

Reasoning in which results derived from one model feed into another model is extremely common. It is often convenient to build models in a *modular* way rather build a single, unified model of a complex system. For example, consider Airy's discussion of a pendulum clock movement in (Airy, 1826). Airy proved isochronicity for a pendulum driven by a force symmetrically applied near the bottom of its swing, providing a theoretical basis for the already recognized precision and accuracy of clock movements using the *deadbeat escapement*, which isolates the pendulum from forces exerted by the escapement except for small frictional forces produced as the 'dead' face of a pallet slides on the teeth of the wheel and an impulse imparted symmetrically at the bottom of the pendulum's swing as the wheel engages the 'impulse' face of pallet.(Airy, 1826, p. 122-3) His account combined separate models of the pendulum and the escapement, linked by adding to his model of the pendulum the forces exerted on it by the escapement, and to his model of the escapement, the timing of its stepwise motion imposed by the swinging of the pendulum.

There was no barrier to building a single model of the entire clock, but it was not *necessary* to do so, as Airy's treatment of the problem shows: separate models of the behaviour of a pendulum perturbed by external forces in various ways and of the forces applied to the pendulum by the escapement sufficed. In complex cases, processes described by different theories can be invoked when modeling different components of a system. When we can capture the links between components by adding further inputs (derived from models of the linked components) to *separate* models of each component, we don't need a single, integrated model of the system.

Modeling complex systems this way can circumvent concerns about inconsistency, because only *specific* results of reasoning within one part of the model contribute to reasoning in other parts. Such reasoning can use straightforward, apparently classical logic or mathematics. But this does not show that the resulting arguments are consistent all the way through. Even if the set of all the sentences used at one point or another in an argument is inconsistent, we can avoid commitment to arbitrary conclusions, so long as the results derived in one and subsequently taken up in another are consistent with both.

In such models, specific results derived in one context are used as input for others. Premises applied in these partly-isolated contexts of reasoning include initial premises built into each context together with specific results derived in and then transferred from other contexts. Sometimes the claims and principles appearing in each context have established standing as reliable models for certain systems; in other cases, they are speculative, even explicitly contradicting principles that are causing trouble while the model retains those principles in other contexts where they are needed to obtain known reliable results.

The underlying approach is familiar: assembling larger, more complex models out of independently specified components. Some sub-models are speculative efforts to 'fill a gap,' providing an account of some phenomena, related to other phenomena already well understood in terms of models established as reliable, but which already-available models and theoretical principles have not been able to account for. Such new, speculative sub-models generally draw on concepts and inference patterns that are already available, but they will often modify or drop some to avoid difficulties encountered by more straightforward applications of the accepted principles. For example, Bohr's model of the hydrogen atom fits this description.

The general form of such inconsistency-tolerating reasoning proceeds by drawing conclusions from one sub-model and feeding them into other sub-models as premises. Trivialization is avoided so long as the premises derived from earlier arguments and allowed into the later argument are *logically compatible* with the principles applied in that later reasoning.

3. Pluralism and reasonable inconsistency

There is a wide gap between the logical notion of content and the actual practice of reasoning. As the localized patterns of reasoning we've been discussing suggest, most actual reasoning does not involve commitment to the closure of sets of premises under a consequence relation. Simply determining the contents of such a closure, $Cl(\Gamma,\vdash)$, is difficult for any reasonably rich set of premises Γ and consequence relation $\vdash$. Giving a complete list is generally out of the question, and characterizing the closure in a way that systematically settles (by some applicable test) whether or not $\Gamma \vdash A$ for an arbitrary sentence A, is beyond us for most interesting cases. Nevertheless, logicians typically assume that such a logical closure expresses the *content* of a commitment to Γ, i.e. what someone who accepts the premises in Γ is (logically) *committed to*.

This notion of commitment illuminates the epistemic risks of acceptance. Since we don't know everything that follows from the sentences we accept, in accepting some premises we risk encountering a reductio argument against the position we've adopted.

Pluralism acknowledges a lack of theoretical or conceptual unity in our sciences—but the form of pluralism involved here is particularly strong. These examples go beyond irreducibly different theories and concepts to show that important scientific models can be built out of explicitly inconsistent assumptions. Airy's model invoked the same basic principles in each sub-model while treating the models separately, linked only by exchanging specific results derived within one and used as input for other(s). Other models are pluralistic in the sense that they invoke different principles and/or different approximation methods at different point. But some models in science rely on inconsistent premises to build interacting sub-models which, together, successfully describe important features of our world.

Such cases involve arguments that *cross boundaries*, drawing on results from two or more contexts in which incompatible commitments are used. The early calculus, Bohr′s theory of the hydrogen atom, and Dirac's delta function all present arguments drawing on inconsistent premises. Beyond these examples, familiar philosophical paradoxes and puzzles, including the liar, Zeno's paradoxes and the paradoxes of set theory, arise from deep and persistent tensions over basic concepts including truth, extension and divisibility, time and change, and the challenge of consistently describing arbitrary collections of items. Reasoning in such situations without having a systematic way to limit the damage inconsistency can risks outright trivialization (or at best, simply leaves us without an understanding of how that catastrophe was avoided).

The question we are pursuing here is not whether this can be done, but how (and how well) it has been done. Scientists don't spend much time explaining how such inferences work—they draw on various principles and apply them to derive conclusions by following a line of reasoning, a rough trail across as yet unknown terrain. But logicians are free to explore how the conclusions they draw could be arrived at while avoiding trivialization and other hazards.

In (Brown & Priest, 2004) Graham Priest and I proposed a 'Chunk and Permeate' (*C&P*) model of Newton's calculus. The aim of C&P is to model reasoning in a context where inconsistent premises are drawn on at different points along the way to some conclusions. In the case of the calculus, the premise that $\delta t \neq 0$ is used to arrive at an equation that doesn't have δt in the denominator; the resulting equation 'permeates' from this context to another where we set $\delta t = 0$. The challenge of generalizing this approach to other versions of the early calculus remains open (see Sweeney, 2013). *C&P* has also been applied to build logical models of Bohr's hydrogen atom (Brown & Priest, 2015), Dirac's 'delta function' (Benham et al, 2014), and reasoning with fractions (Bergstra & Bethke, 2014). The C&P approach avoids appeal to a distinct (and inevitably controversial) paraconsistent logic to capture the actual patterns of reasoning employed. Instead, trivialization is avoided by limiting the combination of premises used at different steps. The arguments modeled may rely on inconsistent assumptions, but those assumptions are insulated from each other, preventing trivialization.

C&P is a *weakly aggregative* approach to reasoning with inconsistent premises; it is related to *forcing*, a formal consequence relation which is guaranteed to preserve a measure of (in)consistency called *level* rather than (as classical logic and more other systems do) preserving the consistency of consistent extensions of our premises(Brown, 2016). But *C&P* strengthens forcing in ways that help in modeling inconsistency-management when reasoning with inconsistent premises.

The kinds of pluralist models *C&P* captures substantially weaken the force of reductios based on the full set of sentences appearing in such models: non-trivial reasoning with inconsistent premises is possible even without invoking a heterodox paraconsistent logic, so long as we restrict how premises are combined and what sentences are available (and which are disallowed) in different contexts of reasoning.

A *C&P structure* is a triple, <P, ρ, i_0>, where P is a consistent *covering* of a set of sentences Γ, ρ is a *permeation relation* specifying which sentences are allowed to permeate from each element of P to other elements of P, and i_0 is the *designated chunk* where conclusions are drawn.

The *C&P* consequences of Γ relative to a C&P structure $\mathfrak{p} = <P, \rho, i_0>$ arise from a sequence of closure and permeation steps (Brown & Priest, 2004):

Where γ_i is the i^{th} cell of P, γ_i^n is defined recursively:

$\gamma_i^0 = CL(\gamma_i)$

$\gamma_i^{n+1} = CL(\gamma_i^n \cup \bigcup_{j \in i} (\gamma_j^n \cap \rho(j,i))$

The *C&P* 'consequences' of Γ are the contents of the designated ("output") chunk, i_0, closed under this recursion; thus,

$\Gamma \Vdash_{\mathfrak{p}} \alpha$ iff $\exists n, \alpha \in \gamma_{i_0}^n$

The successive closure and permeation operations ensure that the premises in each chunk can contribute to the consequences of the *C&P* structure.

The result is not a relation between ensembles of sentences and the sentences that follow from them: *C&P* structures take us a long way from the standard concept of a consequence relation. In particular, a *pragmatic* element enters here: formal criteria don't tell us which covering of Γ, which permeation relation or which designated chunk is the 'right one'.

Applications of *C&P* arrive at coverings and permeation relations by examining the inferences we are trying to capture: a well-designed *C&P* structure will support inferences actually made in the course of using a scientific model, such as the old calculus or Bohr's model of the hydrogen atom and block inferences that were not accepted. As a *strategy* for building inconsistency-tolerating inferential machinery, *C&P* attempts to keep our logical models close to the phenomena (i.e. to how people actually reason) while providing a formal reconstruction of how conclusions were reached that avoids arbitrariness and triviality.

4. A different kind of case

To this point, applications of C&P have focused on cases where inconsistency arises out of theoretical accounts of some phenomena that invoke conflicting theoretical principles. A different sort of case arises in the interaction between coupled global and regional climate models. The resolution of global climate models (GCMs) is limited by the available computing power. This has significant implications for identifying risks posed by climate change and how to mitigate them, for example when planning future investments in infrastructure, since the resolution required to anticipate maximum water flows and other important weather-related risk factors requires topographic and other details that can't be modelled at the grid scales characteristic of GCMs.

Climatologists have explored methods for downscaling the results of GCMs to explore potential local impacts of climate change in more detail.(Laprise, 2008) Downscaling methods are either statistical or dynamic; in statistical downscaling, observed statistical patterns linking broader-scale conditions to smaller-scale weather patterns are used to predict changes in the smaller-scale weather patterns given climate conditions projected by the GCM. While this method is straightforward and easy to implement, it assumes that the observed statistical relations remain reliable ('stationary') as the climate changes. In dynamic downscaling, a regional model with smaller grid and shorter time-steps is 'nested' in a GCM, or alternatively, a GCM with variable resolution (VRGCM) is used, with the region of interest being assigned a higher-resolution grid.(Laprise, 2008, 3643)

The variable scale approach elegantly captures the desired phenomena within a single, unified model. However, it is hard to implement – technical challenges arise due to the non-uniform, anisotropic computational grid (related to similar challenges which arise at the poles in a latitude/longitude grid) and changes in parameterization methods applied to model sub-scale processes as the size of grid cells and time steps changes.(Laprise, 2008, 3643)

Here we focus on the nesting approach, as presented in (Laprise, 2008, 3643ff). Rather than delve into the detailed mathematics of such models or their successful application in evaluating climate risks, we focus here on a logical account of how this driving of the RCM by the GCM is implemented in the particular RCM discussed in (Laprise, 2008) and subsequently applied in papers and reports by the Ouranos Consortium (https://www.ouranos.ca/en/publications/). Finer details vary depend on the methods used, but in general the driving process is implemented by replacing or overwriting calculated RCM results for a region along the outer edge of the RCM's grid (the 'sponge zone'), by drawing on the values of temperature, pressure and other variables at nearby points in the GCM. One standard approach uses distance-weighted averages of nearby point values from the GCM to assign values to outermost grid points of the RCM along with smoothing methods for a few outer grid rows in the RCM that reduce the `shock' of imposing new values at the RCM's outside edge (Laprise, 2008, p. 3647).

Thus in the course of a model run, values for air pressure, temperature and other quantities calculated by the RCM are repeatedly overwritten, at regular intervals, with incompatible values, in the cells along the edge of the nested RCM and a few grid-steps in from the edge of the RCM 'nest'. Our C&P model aims to shift attention away from an *algorithmic* view of the process generating the sequence of states of the RCM for each time-step, towards a *logical* picture, in which we infer descriptions of RCM states from the combination of descriptions of prior state(s) of the RCM and information about the states of nearby points as described by the GCM. The point of the exercise is to clarify the *reasoning* involved in such models. Since, in general, the outputs of the global and regional models directly conflict in the 'sponge zone' extending from the outer cells of the RCM to the (else there would be no reason to be interested in how 'adjusting' the RCM to accommodate changes imposed on it based on the GCM affects the subsequent course of the RCM), it's clear that some kind of inconsistency management is involved.

The basic logical challenge is that at each time-step of the RCM there is a straightforward contradiction between the values generated by the RCM for grid points whose values are adjusted to reflect input from the 'driving' GCM, and the new values imposed by the algorithm applied to the cells of the 'sponge zone'. The C&P structure proposed here aims to capture a cycle of repeated calculations which produce only consistent results in each cell of the C&P structure and allow us to generate (in the designated or 'output' chunk) a continuing sequence of time-steps at each of which consistent values for the temperature, pressure, humidity and other meteorological quantities are assigned to the points of the RCM grid.

Our C&P model includes three chunks, γ_{GCM}, γ_{RCM}, and γ_{OUT}, the designated chunk. The logic in each chunk is assumed to be classical. γ_{GCM} includes the GCM output, time-step by time-step. The γ_{RCM} chunk includes RCM output (values for quantities such as temperature, humidity, air pressure, precipitation) for the RCM grid points at each time step. We also assume that the time-steps for the GCM are integral multiples of the time-steps for the RCM, i.e. there are n RCM time steps for each GCM time step.

Each n-1 tuple of RCM states permeates directly to γ_{OUT}, but the nth state must be treated differently. To use the GCM to drive the RCM we require that at each GCM time step, the RCM state be 'adjusted' to reflect the results of the GCM for grid points near the outer edge of the RCM. Modified states of the boundary cells of the RCM are calculated based on values from neighbouring grid points for the corresponding time step of the GCM, so that at each GCM time step, values for grid points in the RCM boundary region are obtained by combining data for the RCM grid points with GCM values assigned to boundary and cells calculated by the GCM, using averaging and smoothing algorithms.

For each sequence of n RCM steps, the contents of the resulting, adjusted RCM state draw on but *contradict* the nth RCM state. We capture this adjustment by feeding the results of the RCM's nth, 2nth, etc. time steps into the γ_{GCM} and applying the adjustment algorithm there. The results of this calculation permeate back into γ_{RCM} with a *new label* (n', $2n'$, etc.) to avoid producing a contradiction with the values already calculated for state n of the RCM. Using each of these newly added states n', $2n''$, $3n'''$ ... as new starting points (i.e. initial conditions), we apply the RCM algorithm again, calculating n new RCM states and repeating the cycle, adjusting the nth new state using input from the GCM and then running the RCM starting with that state to produce the next n states, adjusting with input from the GCM and sending the adjusted state back to the RCM cell as a new starting point, and so on.

Along the way each n-tuple of results for each of the resulting states also permeates into γ_{OUT}, where they are (properly) labeled 1,...,n, n+1... and so on. It's here, in γ_{OUT}, that the model output is treated as representing a continuous temporal sequence of states for the RCM system, *as driven by* the GCM.

The main lesson of this example is that when we combine models at different scales to capture interactions of phenomena on those scales, inconsistencies can arise due to the production of conflicting results for various physical quantities. When a larger-scale model is used to drive a local model, this kind of C&P strategy provides a formal account of how the model works and how it produces consistent, non-trivial output despite the inconsistencies that arise along the way.

5. Conclusion

This case presents a fairly complex application of C&P, aimed at making two broader points. First, I am proposing a hypothesis about a relation between pluralistic reasoning in science and C&P: I think it is illuminating to think of pluralist perspectives as *pre-adapted* for the C&P approach to inconsistency management, and to think of C&P as an *exaptation* arising from the pluralist nature of the scientific enterprise. The point of invoking C&P explicitly here is that it makes the patterns of reasoning typical of such cases more explicit. Second, I believe inconsistencies are involved in scientific reasoning much more frequently than the short list of familiar examples dealt with in the literature would suggest. Reasoning across the boundaries of a multiplicity of perspectives on the natural world is a natural result of attempting to model interactions between related natural systems. Our best accounts of these systems have been developed in their own, separate ways, in response to multiple, varying constraints. When science develops in this way, it is not at all surprising that the different perspectives and models applied are sometimes in conflict with each other.

The inconsistencies involved in this example are, quite rightly, of very little concern to the scientists involved. There is no conflict over fundamental principles, and the well-understood limitations of both the global and regional models provide straightforward reasons for trying to draw on both in order to identify possible consequences, at the regional scale, of climate changes modeled on the global scale.

More broadly, The *C&P* approach to modelling reasoning with inconsistent commitments systematically exploits pre-existing, common-sense patterns of reasoning that allow us to draw non-trivial conclusions from inconsistent premises without any need to appeal to a heterodox, paraconsistent background logic. As I see it, this kind of reasoning often takes place without even being thought of as a means of inconsistency management, especially when the inconsistencies involved arise from shifts of scale and other differences in *locally reliable modelling practices*, rather than deep theoretical tensions.

Science as we know it not only tolerates a plurality of models, it sometimes appeals to models that are inconsistent with each other, drawing on them to arrive at important conclusions. The tolerance of inconsistency proposed here is pragmatic rather than metaphysical, as it has been for some paraconsistent logicians (for example see Priest, 2007): Wimsatt (2007) argues for a shift in philosophical thinking about science, focusing on *locally reliable heuristics* rather than ‘principled’ models aimed at truth. Following Wimsatt, I believe that the *preservation of reliability* is the key value at stake when scientists model complex systems which we have no adequate, unified and consistent model for. Examples include climate, living organisms and evolutionary change–in such complex cases, just as when we lack a consistent ‘background’ theory that is adequate to the phenomena, a *true* account is out of reach, at least pro tem. In the second case we may reasonably hope for a resolution of the conflict. But inconsistency seems inescapable when the phenomena are simply too complex for a single, consistent 'all-purpose' model to do the work. We can often produce patchwork models that enable reliable reasoning about systems we're interested in and provide helpful guidance in further inquiry, but a single, unified model that could serve as a candidate for the 'truth' about our world remains beyond our grasp.

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