The Earth in the Model:

The nomothetic, idiographic, and plural epistemic aims of planetary modelling.

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Penultimate version.

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The nineteenth-century distinction between the nomothetic and the idiographic approach to scientific inquiry can provide valuable insight into the epistemic challenges faced in contemporary earth modelling. However, as it stands, the nomothetic-idiographic dichotomy does not fully encompass the range of modelling commitments and trade-offs that geoscientists need to navigate in their practice. Adopting a historical epistemology perspective, I propose to further spell out this dichotomy as a set of modelling decisions concerning historicity, model complexity, scale, and closure. Then, I suggest that, to address the challenges posed by these decisions, a pluralist stance towards the cognitive aims of earth modelling should be endorsed, especially beyond predictive aims.

Contemporary geoscientists, working in fields ranging from paleontology to climate science, including areas like geomorphology, oceanography, hydrology, and paleoclimatology, are commonly invested in the study of highly complex earth systems. It is well-recognized that alongside empirical evidence such as core samples and fossil records, a widespread investigative tool that geoscientists count on to carry out their research are models (Bokulich and Oreskes 2017). Models can be understood, in the broadest sense, as artifacts constructed by scientists with the aim of gaining new understanding of natural phenomena to which they have limited access. The variety of models there is in science (concrete, analogical, numerical, conceptual, simulations) and the plurality of epistemic functions they may perform in research (representational, explanatory, exploratory, predictive) have motivated philosophers and historians of science to join efforts over the last few decades to investigate the impact that model reasoning has had on modern and contemporary societies (Knuuttila et al. 2024; Magnani and Bertolotti 2017; de Chaderevian and Hopwood 2004).

A dilemma often addressed by philosophers and historians of science is that, on the one hand, the use of models seems both unavoidable and enormously effective in scientific research. On the other hand, if observed with an analytical eye, models are notoriously characterized by their idealizing, abstracting, simplifying, and often distorting nature with respect to the geophysical, ecological, or social phenomena they stand for. The challenge is to explain how exactly this dilemma is only apparent, insofar as models, as a matter of fact, have proved to illuminate the very same objects they misrepresent.[1]

This article is concerned with the dilemma of misrepresentation in contemporary geoscience. Over the last two decades, there has been a growing interest in critically examining the varied practices of constructing earth models, ranging from those that represent specific target systems, such as ocean circulation, landscape formation, the internal structure of the earth, or evolutionary processes in the deep past, to those that concern *the* Earth System, that is, models that undertake the ambitious project of representing the numerous interactions between the atmosphere, oceans, vegetation, and land on a global scale in a unifying manner (known as Earth System Models, henceforth ESMs, to be distinguished from earth models or geoscientific models more generally). This array of earth modelling practices raises especially pressing questions regarding misrepresentation, given the sources of uncertainty that shape these practices, the high levels of complexity demanded by numerical simulations in new interdisciplinary fields such as Earth System Science (ESS), where ESMs are produced, and the urgent issue of when communities are justified in implementing policies based on model results (Gramelsberger, Lenhard & Parker 2020; Winsberg 2018; Bokulich and Oreskes 2017).

The workshop that motivated this special issue, *Historical Epistemologies of Planetary Modelling* (Berlin, June 2023), was dedicated to reflecting on the historicity of the knowledge processes that led to the adoption of the 'Earth System' as an (incredibly large) unit of analysis in recent decades. The introductory thoughts to the workshop began, perhaps not surprisingly, in the nineteenth century. They specifically evoked the distinction between the *idiographic* and *nomothetic* approaches to scientific inquiry, originally proposed by the philosopher Wilhelm Windelband (1894). Calling attention to this distinction aimed to capture a central decision that earth modellers still face in their practices today: prioritizing one mode of investigation to the

[1] Even if there is general agreement that the dilemma of misrepresentation is only apparent, scholars have had strong disagreements about how to address it precisely. There tend to be two types of answer: those that address the dilemma by distinguishing between the true parts and the misrepresenting parts (e.g., idealizations, abstractions) of a model (Wimsatt 1987; Pincock 2011), and those that vindicate the epistemic value of the misrepresenting parts (Potochnik 2017; Elgin 2017; Rice 2021).

detriment of another. The nomothetic approach to scientific research, Windelband argued, seeks to understand "the general in the form of the law of nature" and treats the particular only as an exemplar of a generalizable phenomenon. Meanwhile, the idiographic approach studies "the particular in the form of the historically defined structure", seeking to provide a comprehensive reading of processes (1894, 175). If attention is given to the general and subsumable under natural laws, so the argument goes, scientists would misrepresent (disregard, distort) the historical nature of the phenomenon investigated; if attention is given to unique occurrences in the historical flow of events, models would fail to describe noteworthy generalities and causes. A trade-off or relation of attenuation exists between the two approaches (Potochnik 2015; Matthewson & Weisberg 2009).

The specific context where Windelband proposed the nomothetic-idiographic distinction was the debate taking place in the late nineteenth century about the separation between the *Naturwissenschaften* (or natural sciences) and the *Geisteswissenschaften* (or sciences of the spirit, identified with the humanities later), which would persist until the twentieth century in the form of the "two cultures" debate (Snow 1959; Lowenthal 2019, 6-8). The philosopher Wilhelm Dilthey had proposed this division based on the perceived differences in content between the two domains (nature vs. the spirit) as well as on the type of experience that granted access to that content (outer vs. inner experience) (Dilthey 1883; Kinzel 2021). Windelband rejected Dilthey's characterization of the humanities, though, as he judged the inner or introspective form of experience a dubious form of investigation. Instead, Windelband maintained that both the natural and the human sciences were "empirical sciences", resting upon a "scientifically refined and critically disciplined form of experience", where "perceptions must be scrupulously educated" (Windelband 1894, 177; Kinzel 2021). Yet, one could still identify formal differences between *modes* of investigation, the nomothetic and the idiographic, but these could be equally adopted by researchers studying one and the same phenomenon in geology, physiology, or linguistics (1894, 177).

More generally, Windelband's proposal should be understood in relation to the growing specialization of knowledge in the nineteenth century, and the professionalization of fields such as psychology and history, which prompted academic demands to recognize the methodological autonomy of these fields, in opposition to the view that there is only one scientific method, that of physics or natural science (Kinzel 2021). Methodological monism, or the aspiration to identify and commend *one* scientific method, however, did not vanish after these disputes. In the early twentieth century it gained renewed force, through the project of the logical positivism personified by the Vienna Circle, and remained persistent for generations of post-positivist thought.[2]

Foregrounding the nomothetic-idiographic distinction at the workshop aligned with the drive of historical epistemologists to revive portions of the scientific tradition to address contemporary challenges in a new light, such as those emerging in the Anthropocene debates. I particularly viewed it as a suggestive strategy to reflect on and update the dilemma of misrepresentation in current earth modelling practices. However, the subsequent discussions at the workshop revealed that the nomothetic–idiographic distinction falls short of encapsulating the various decision processes that geoscientists need to go navigate in their modelling practice. Collapsing them into one decision concerning two types of inquiry fails to capture the multiple trade-offs entailed in the construction of models of complex earth phenomena. I propose to spell out the nomothetic-idiographic pair as a wider set of modelling decisions concerning **historicity**, **model-complexity, scale,** and **closure**, and discuss them separately below. Then, I refer back to Windelband's original proposal to make a suggestion about how to deal with the challenges posed by these decision processes. Namely, the plurality of epistemic aims of earth modelling should not only be openly recognized, against methodological monism, but actively encouraged, especially beyond predictive aims.

Historicity

The nomothetic-idiographic dichotomy captures, first and most evidently, an apparent tension between prioritizing historicity or lawlikeness in the investigation of a natural phenomenon. Windelband argued that while in research that prioritizes lawlikeness, thought "moves from the confirmation of particulars to the comprehension of general relationships", and the datum is valuable insofar as it is a token of a type, research that prioritizes historicity is "devoted to the faithful delineation of particulars", and data (at least those data that qualify as 'historical facts') have an intrinsic scientific value (1894, 178). What seems to be at stake

[2] See Carnap et al. (1929) for a classic positivist view endorsing methodological monism (based on the inductive method) and the unification of the sciences; and Popper (1963) as the clearest example of the persistence of methodological monism in post-positivist thought, although endorsing the hypothetical-deductive method. See also Lowenthal (2019) for a rich historical reconstruction of the origins and contemporary reinterpretations of the humanities–natural sciences divide, and its connection to the monism-pluralism debate regarding the scientific method.

here is a different temporal conception of the phenomenon under investigation, either as timeless or existing in the specific flow of historical time.[3]

In the geosciences, these two conceptions are distinctively manifest. In nineteenth-century geology for instance, we find different considerations to the role of historicity in the study of land formation. Grove K. Gilbert proposed to go from the observation of geographic features to the creation of a dynamic model based on physical principles, by axiomatizing erosive processes into a set of laws governing land sculpture (Pyne 1980, 87-89). Meanwhile, his contemporaries Clarence Dutton and William Davis used the narrative form to construct an evolutionary structural history of land and river formation in sequential stages. If in the former case a geological system was conceived as a dynamic unit of geologic processes in equilibrium, operating in a steady state, in the latter, it was conceived as a historic system, forming chains in evolutionary progression (ibid., 89).

A comparable dispute on historicity is found in mid-twentieth-century geography. Richard Hartshorne viewed geography as a historically descriptive discipline, dedicated to the assembly of unique geographical facts, while Fred K. Schaefer, influenced by the logical positivists, believed that geography should seek general laws expressible mathematically, in order to explain the mechanisms causing geographic differences (Barnes and van Meeteren 2022; see Hartshorne 1959; Schaefer 1953). Historicity motivated "regional geography" for Hartshorne, and put the emphasis on the uniqueness of specific regions. Schaefer's "systematic geography", on the other hand, postulated the homogeneity of the spatial distribution of geographic phenomena, permitting to advance morphological lawlike statements (ibid.). Schaefer's approach would be endorsed more widely in the second half of the twentieth century. In fluviology, for instance, the production of universalizable mathematical explanations of river form was pursued by hydrologists such as Luna Leopold in the 1950s and 1960s. Even when Leopold failed to produce the generalizable results he expected, he did not abandon his universalizing ambition, but salvaged it by replacing deterministic models by stochastic ones (Benson 2020).

[3] I thank the editors of this special issue for their input on this and several other points throughout the article. I also thank a reviewer for prompting me to clarify that the opposite to "lawlikeness" is not "particularity" for Windelband, but "historicity". The particular (*Besondere*) is involved in both types of inquiry in the form of individual data. What to make out of particulars is what distinguishes the nomothetic from the idiographic. The key contrast is between studying "change" and the "invariable form of change" (1894, 178-79).

In the last quarter of the twentieth century, transitions from predominantly historicist to nomological perspectives occurred as well. Paleontology shifted from "being an 'ideographic' field concerned mostly with digging up, describing, and cataloguing individual fossils" to "developing large-scale quantitative analyses of patterns in the history of life" (Sepkoski 2012, 2). In the words of Stephen Jay Gould, who led the 'paleobiological revolution' in the 1980s, some researchers began to ask –without undervaluing the idiographic component or surrendering to "physics envy"– whether stochastic models could be applied to study general problems in the history of diversity or patterns of extinction (Gould 1980, 113-14; see also Turner 2014). In geomorphology, the emphasis moved from the study of sequences of events that give rise to particular landscapes to incorporating thresholds of time and space to study cause and effect in the development of landforms (Schumm 1979; Bokulich 2021, 174-5).

These methodological debates are critically reproduced by contemporary philosophers of science. Carol Cleland (2002, 480) has argued that paleontology and archaeology are indeed historical fields, concerned with "event-tokens instead of regularities among event-types". Paying attention to the complexities of the causal chains leading to a certain event, she says, "bur[ies] the regularities in a welter of contingencies" (ibid.; see also Currie 2019, 27). Ben Jeffares (2008, 474) disagrees, and responds that geologists don't "just account for a volcano by appeals to a particular history", but "to the general process of volcanic activity". Meghan Page (2020) adds that, vice versa, historical investigation often plays an "ineliminable role in advancing our knowledge of causal structure and stable regularities". I would like to suggest, in line with Adrian Currie (2019, 28-29) and Derek Turner (2014, 502-03), that it is possible to break with this impasse by admitting that, in actual practices of earth modelling, there is an iterative relationship between the two approaches more than a clear-cut distinction between them. There are at least three reasons for that.

One, in a shallow sense, history matters for all practices of model building across the geosciences. Evidence has a past, and where the data came from and how it was produced is crucial to make it usable in practices such as model calibration and evaluation (Currie 2019, 24; on data journeys and the difference between data and models, see Leonelli 2019; Boyd 2018).

Two, laws have been traditionally defined as non-accidental, universal regularities. However, as scholars in the geosciences insist, keeping the notion of law in this domain would require accommodating generalizations that are in some degree contingent (Kleinhans, Buskes and de Regt 2005, 294). Plate tectonics, for instance, postulates an underlying process responsible for the forces that cause the plates to move, and provides a model of crust formation that allows to produce generalizations about phenomena such as earthquake generation. But it does not contain any law in the sense of a universally applicable causal statement (ibid.; Oreskes 1999; Hallam 1983, chap. 6). There is a way, though, in which laws, understood in the traditional sense, intervene in earth modelling. Simple physical laws, such as the conservation of momentum and conservation of mass, which apply to fluid flow, can be implemented (as simplified Navier-Stokes equations) with the help of computer models. They can then be used to check if a hypothesis, or more precisely, the narratives used in geoscientific explanations, conflict with those physical laws (Kleinhans, Buskes and de Regt 2005, 309). At any rate, the issue here is that these would not be genuine geoscientific laws, but attempts to reduce parts of their explanations to simplified physical laws. The hard explanatory work would fall on the historical narrative, responsible for identifying the relevant sequence of events that precede a certain occurrence and explain what could otherwise have been the case (ibid.).

Three, the distinction between historicity and lawlikeness blurs in actual earth modelling practice because studying sequences of events requires understanding patterns in a non-trivial way (Currie 2019, 62). Trying to understand the deep past demands paleobiologists, for instance, to learn about the relations between geography, biodiversity, and adaptation, as well as the conditions required for the emergence of stable dynamics: "we don't simply tell a history, but uncover the conditions of existence" (ibid., 63). In geology, explanations of events are narrative in structure, but this narrative often concerns lawlike recurrence (Huss 2022; Hopkins 2022). Interestingly, Windelband anticipated this intricate connection when he admitted that "general propositions are necessary at every stage of inquiry in the idiographic sciences", and that causal explanation of any historical occurrences presupposes general ideas (1894, 182-3).

The alleged contrast between modelling an earth system as timeless or as embedded in the specific flow of historical time can be partly contained if we acknowledge the iteration between the two approaches in practice (Currie 2019, 28-29). The interest in lawlike statements often sets research questions, helps establish what is significant about individual events, guides how traces are interpreted, and provides grounds for linking different cases. Meanwhile, the interest in historicity provides concrete examples for ideas that would be otherwise mere abstractions, affords empirical support and tests for theories, inspires hypotheses, and generates surprises potentially worthy of further investigation (ibid.).

Model complexity

The nomothetic-idiographic dichotomy invites us to consider other research decisions involved in the practice of earth modelling that do not exactly match the historicity/lawlikeness pair. Windelband claimed that in nomothetic science there is "a bias in favor of abstraction" to help reveal the immutability governing real events (1894, 178-79). Meanwhile, in idiographic science, there is a "bias in favor of perceptuality", clarity, or vividness [*Anschaulichkeit*] to capture the "structural form" of things (1894, 178). Prioritizing either structural form or immutability in real events seems to mainly concern the level of model complexity that earth scientists consider ideal. Reducing the complexity of a model, by limiting the number of parameters and rules comprised in it, can be especially helpful to clarify and picture vividly the basic structure of a certain phenomenon, even if no relevant extrapolations can be made from the model to real events. Meanwhile, increasing the complexity of a model, and so the number of processes and parameters included in it, could help reveal notable regularities, enhancing the model capacity to extrapolate to multiple actual systems.[4] In some way, of course, all models simplify and reduce the complexity of the system studied, in order to make it manageable and have some control over the operations involved in it (what would be the point of modelling otherwise?). But different trends in the extent to which model complexity is considered ideal or, to the contrary, an epistemic disadvantage, are manifest in the geosciences.

Climate modelling has clearly tended towards the increase of model complexity, running in parallel to the, so far relentless, increase of computational power, augmenting generation after generation since digital computers arrived in the field in the mid-twentieth century (Edwards 2010; Shukla et al. 2010; Gramelsberger and Fleichter 2011; Heymann, Gramelsberger & Mahony 2017b). Earth System Models (ESMs) belong to the most recent generation of complex climate models, meant to replace a previous generation of coupled ocean-atmosphere GCMs (general circulation models). ESMs simulate not only atmospheric and oceanic motions, but also their interactions with land surface and sea ice, atmospheric and

[4] Some philosophers of science might find it odd to associate "abstraction" in Windelband (1894, 178) with an increase of model complexity, since a common claim in current modelling literature is that abstract models are poor-in-detail (see for instance Jones 2005). However, the sense of abstraction that Windelband introduces is linked with the capacity to make realistic predictions across many systems, which requires the accumulation of numerous data (taken as tokens of a kind). The idea of an abstract or toy model that philosophers of science usually have in mind aligns more closely with Windelband's notion of inquiry that favours the vivid identification of the elementary "structural form" of a phenomenon. In any case, the correspondence is not straightforward either, which reinforces the point of this article that there are multiple trade-offs between the decisions earth modellers need to make, rather than a simple contrast between two modes of inquiry.

ocean biochemistry processes, vegetation, snow, and even agriculture, all at the highest resolution possible (Randall et al. 2019; Parker 2018, §4; Katzav and Parker 2015, 279-280). For each of the components of the model, different processes are considered. For atmospheric motions, there is consideration to its dynamics, with fluid dynamical and thermodynamic equations simulating the transport of heat, mass and moisture; and to its physics, concerning processes such as cloud formation and precipitation, occurring at sub-grid scales (the grid size in climate models is about a hundred kilometres) (Parker 2018, §4; Guillemot 2017, 122-26). These complex climate models involve more than a million lines of computer code, require vast material resources,[5] have high energy demands,[6] and rely on the collaboration of numerous scientific teams.[7] Many initiatives in the ESS community are already working on the improvement of the spatial resolution of ESMs, in order to include less coarse simulations of components like cloud systems, ocean eddies, or precipitation in tropical regions (Katzav and Parker 2015, 477; Guillemot 2017, 122-24). The efforts to build complex models are justified in terms of their ability to mimic earth systems realistically, that is, to reveal regularities that match empirical observations made in the last century, and make projections about real climates.[8] The ongoing projects of building digital twins of the Earth are the best example of the aspiration to produce ever more high-quality simulations, able to function at a global kilometre-scale using real-time and historical data.[9]

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[5] In 2020, the UK government invested £1.2bn in a supercomputer, expected to be the most advanced of its kind, designed to improve extreme weather and climate forecasting (Met Office 2020, <https://www.metoffice.gov.uk/about-us/press-office/news/corporate/2020/supercomputer-funding-2020>, retrieved in February 2024), while the U.S. National Oceanic and Atmospheric Administration (NOAA) announced that it was tripling the investment in their Weather and Climate Operational Supercomputing System (NOAA 2020, <https://www.noaa.gov/media-release/us-to-triple-operational-weather-and-climate-supercomputing-capacity>, retrieved in February 2024).

[6] Running high-resolution climate simulations faces significant challenges regarding computational efficiency, as computing resources are very costly and energy intensive. Some recent studies examine the possibilities to minimize the carbon footprint of computing activities in climate modelling laboratories. See Fuhrer et al. (2018) and Loft (2020).

[7] Examples of community-based networks for climate modelling include the European Partnership for Research Infrastructures in Earth System Modelling (PRISM), the Community Earth System Model (CESM) of the National Center for Atmospheric Research (NCAR), and the Earth System Modeling Framework (ESMF) in the U.S. The aim of these collaborations is not only to conduct Earth system research, but also to standardize methods and develop software environments (Gramelsberger and Fleichter 2011, 49-50).

[8] The IPCC in particular justifies the use of ESMs and GCMs in terms of the extensive physical knowledge that these models implement and their ability to simulate a wide variety of aspects of observed climates (Katzav and Parker 2015, 477).

[9] See for instance the project to build the European Digital Twin of the Ocean (https://research-and-innovation.ec.europa.eu/funding/funding-opportunities/funding-programmes-and-open-calls/horizon-europe/eu-missions-horizon-europe/restore-our-ocean-and-waters/european-digital-twin-ocean-european-dto_en#what-is-the-european-digital-twin-of-the-ocean; retrieved in February 2024), and the Destination Earth project, where the

Despite the recognized virtues and current status of highly complex earth models, their use motivates what Naomi Oreskes calls the model-complexity paradox: "a complex model may be more realistic, yet, ironically, as we add more factors to a model, the certainty of its predictions may decrease even as our intuitive faith in the model increases" (2003, 13). The reason is that, for each process added to the model, scientists need to ask how well the process is represented (how well do the governing equations map onto the natural process?) (ibid., 17-18). So, with each new process, the uncertainty about the accuracy of the model increases, and the possibilities of error in individual parameters accumulate. In other words, "the truer the model, the more difficult it is to show that it is true" (ibid., 20; see also Gramelsberger and Fleichter 2011, 62-69). Hélène Guillemot spells out the paradox in comparable terms. She observes how geoscientists have overemphasized the importance of integrating new components into ESMs, in a practice of progressive complexification (2017, 122). This practice has made it difficult to attribute the characteristics of a simulation to specific components of the model, given the multitude of processes and interactions at many scales operating in it (ibid., 124).

Compare the latest generation of ESMs to the use of *reduced complexity models*, or *synthesist* models, in fields like geomorphology (Murray and Paola 1994; Bokulich 2013, 117). The difficulties to represent the many processes operating in landscape formation, such as of braided rivers, where channels are dynamically shifting, has encouraged some research teams to approach their study with the help of simple, deterministic numerical models (coupled lattice models), which represent the morphology of rivers as simple grids of cells, including only a few parameters and rules (Bokulich 2013, 119-120). Although these reduced complexity models "neglect most of the physics known to govern fluvial hydraulics", namely, they do not solve Navier-Stokes equations, they have successfully shown statistical properties of braided rivers (Nicholas 2010, 1; quoted in ibid.). Results obtained by Murray and Paola (1994, 54) suggest that, in fact, there are only two essential factors for braiding: bedload sediment transport and laterally unconstrained free-surface flow (in Bokulich 2013, 118). These models afford clarity onto the question of why certain landscape patterns emerge, and help visualize with vividness the basic structure of a still opaque phenomenon. There are, however, no

Weather-Induced Extremes Digital Twin and the Climate Change Adaptation Digital Twin are being built (https://destination-earth.eu/destination-earth/destines-components/digital-twins-digital-twin-engine/; retrieved in February 2024).

expectations that specific extrapolations to real fluvial systems can be made with these reduced complexity models.

The decision regarding what level of model-complexity to adopt in earth modelling is not a straightforward one. Far from being a mere practical matter, only constrained by the computational power and amount of empirical data available, selecting the adequate degree of complexity entails an epistemic trade-off between the capacity of a model to make realistic extrapolations and its ability to bring clarity on the basic mechanisms operating in a system. Moreover, there should be no presumption that a middle point in the spectrum of complexity would compensate for the limitations of the two ends: the representational risks associated to that specific level of complexity will remain present.

Scale

A third type of modelling decision that the nomothetic-idiographic distinction conceals concerns the scale or grain at which a geophysical or ecological phenomenon is represented. Windelband recognized that some entities treated nomothetically, and so assumed invariable within a certain timespan, might need to be treated as unique or transitory events if a larger, more comprehensive perspective were adopted (1894, 175-7). Scale, temporal and spatial, can reshape the conceptualization of a phenomenon.

Thinking about scale in earth modelling brings us to the history of the practice itself, since the tendency since the mid-twentieth century, especially in fields like meteorology, has been to aim for ever larger scales, from regional weather models to hemispheric models in the 1950s, and to global models in the 1970s (Edwards 2010, 14). By then, the planet as a whole became a knowable entity that could be grasped as a dynamic system, while local forces started to be understood as elements of a planetary order (ibid., 1-2). Accordingly, data had to become global, and new infrastructures, able to link macro, meso, and micro scales of time, space, and social organization, were established (ibid., 14; Gramelsberger and Fleichter 2011, 44-62). At the end of the twentieth century, however, geoscientists became very aware that earth systems exhibit a multiscale behaviour (Bokulich 2021, 168). Critical voices have questioned since then the idea that with greater computational power one could just zoom in and out, and the object under investigation would be seen at different grains of detail without need to rethink it (Tsing 2013). In contrast, emphasizing the *nonscalability* of earth systems invites to reflect on the objects that geoscientists deny or erase when changing the grain of representation (ibid.; see also Coen 2016; Camprubí 2018).

Decisions regarding what scale to adopt go hand in hand with the estimation of the uncertainties associated with specific scales and the resources available to curb the effects of those uncertainties. Physical scale models offer a clear illustration of this process. For instance, in hydraulic research, the first half of the twentieth century saw the flourishing of river and coastline scale models (also called physical models) to study both general processes like sedimentation or the effects of tidal changes, and specific intervention plans such as for flood control or water supply management. Famous examples of hydraulic scale models are the Mississippi Basin Model and the San Francisco Bay Model, built by the U.S. Army Corps respectively in the 1940s and 1960s for prevention plans purposes (Weisberg 2013; Sterrett 2017; Sánchez-Dorado 2019, Ch. 4). The principles of physical similarity, formulated by Edgar Buckingham in 1914 and rearticulated for the geological sciences by M. King Hubbert in the 1930s, constitute the methodological basis for the construction of scale models, while dimensional analysis is the mathematical tool employed to implement it. Yet, in practice, numerous challenges related to scale effects affect the process of building physical models, because the forces that dominate a system at the scale of the physical model are not the same as the forces that dominate at the scale of the target system (Bokulich 2021, 183). So not all features of a real system can be scaled by the same ratio if the aim is to achieve relevant physical similarity in a hydrodynamical model (Sterrett 2017). Successful scale models require to incorporate –paradoxically– multiple geometric and dynamic scale distortions. Then, the consequences of those scale distortions have to be minimized using strategies like 'avoidance' (replacement of fluids or sedimentary materials in a model to avoid the distortions generated by, for instance, kinematic viscosity), "compensation" (inclusion of additional distortions in favour of an improved model-prototype similarity) and "correction" (posterior adjustment of knowingly distorted results with available data from the prototype) (Heller 2011; Sánchez-Dorado 2019, 135).

One of the advantages of modelling with digital computers is that simulations avoid the scale effects inherent to the practice of physical modelling. But other forms of scale uncertainties remain, since modellers still need to ask if the target under study is influenced by processes emerging at scales above and below the one adopted in the simulation (Bokulich 2021, 168; Kleinhans, Buskes and de Regt 2005, 297-301). The problem of parameterization in climate modelling is a paradigmatic case of this challenge. Interactions that occur below the scale of the chosen model grid cannot be calculated using the continuous equations that have been discretized at the grid size, so they must be replaced by simpler mathematical descriptions (Guillemot 2017, 122-124; Winsberg 2018, 47-48). Cloud formation, for instance, affects climate greatly but, since it takes place two orders of magnitude below GCM resolution, it remains an important source of uncertainty in climate models.[10] If during the 1990s and 2000s core physical parameterization was rather neglected, there has been a growing awareness of the need to improve it (Edwards 2010, 340; Guillemot 2017, 122). With increased computational power, the horizontal spacing of the grid points for the representation of the atmosphere has been reduced considerably, and the next grand challenge is to refine it to the kilometre scale, as this will help address some open questions about the impact of anthropogenic changes on the planet (Fuhrer et al. 2018, 1666; Hannah et al. 2020; Parker 2018). At any rate, the implementation of new parameterizations would require multiple adjustments, as models are typically tuned to compensate for the flaws of existing parameterizations. Altering this balance with new interventions can have undesirable effects on the whole (Guillemot 2017, 128).

A different type of challenge concerning the scale of earth models is temporal scaling, necessary for practices such as the use of paleoclimate analogues (Watkins 2023; Rosol 2015). When using a paleoclimate episode to model present climate change, it is assumed that the past and present earths are dynamically similar. But this assumption is not enough: a scaling procedure that translates paleoclimate rates into commensurable rates of contemporary climate is required, so modellers need to set a lower and upper bound on the durations that are acceptable to use in the comparison (Watkins 2023, 40).

In short, the fact that many phenomena of interest for earth modellers operate at a wide range of temporal and geographic scales has motivated the development of ingenious strategies to "tame the tyranny of scale" (Bokulich 2021, 167). These are strategies adopted to deal with scale uncertainties in particular contexts, but no general solution is expected to tell scientists what the ideal compromise between the foreseeable uncertainties generated by a certain scale, the taming strategies available, and the consequences of occluding phenomena at scales above and below is.

[10] In a 2010 white paper of the World Climate Research Program, this scale challenge in clearly presented: "Model development is hindered by a lack of understanding of how a poor representation of cloud scale processes and cloud scale dynamics contribute to model biases in the large scale circulation features and influence future projections" (Quoted in Guillemot 2017, 124).

Closure

Decisions on the historicity, complexity, and scale of a model might seem, at the end of the day, to be all about the level of detail at which an earth phenomenon is studied. In some sense, this might be right, but with an important caveat. All these modelling decisions –and possibly others not captured by these categories– overlap with the problem of closure, that is, the risk of leaving out certain details (features, processes, relations between variables) that are central to the functioning of a system (Lane 2003, 288; Bokulich and Oreskes 2017, 903). Geophysical phenomena, as well as the models representing them, function holistically (Rice 2021). So if there is a causal dependency or a feedback relationship between a feature included in a model and another feature left out, such dependency or relationship are lost: scientists are inevitably idealizing the overall functioning of the system. Closure uncertainties are concerning because they go hand in hand with the risk of introducing undesirable falsehoods in the model.

In some cases, leaving out entire processes, or assigning them a low value, has led to important errors. In earth modelling, an example is found in early research on ozone depletion. Until the discovery of the Antarctic ozone hole in the mid-1980s, scientists had not anticipated to find it. This is partly because they had not deemed relevant to include a concrete parameter in their models until then: the multiphase chemical reactions involving anthropogenic chlorine in ozone (O'Reilly et al. 2011, 731; Bokulich and Oreskes 2017, 903). The history of science is full of cases where previously unsuspected correlations are later found important, and where features are left out for practical reasons of time or resources without certainty about their relevance (Oreskes 2003, 18).

Idealizations, in any case, should not necessarily lead to errors. Falsehoods included in models can be "felicitous" too (Elgin 2017). They can play a crucial role in helping to uncover counterfactual dependencies between parameters, in ways that the realistic treatment of variables would not allow (Rice 2021, 124). The crucial point is that selecting and omitting features, in accordance with the demands of a certain model format, scale, etc., is far from a passive and (necessarily) harmless task: it gets in the way, for better or for worse, of accurate representation (ibid., 4 and 120).

The plurality of aims in earth modelling.

In the face of an anthropogenic planetary crisis, with the knowledge advanced by earth models playing a protagonist role in decision-making processes, what should we make of the previous identification of compromises, regarding historicity, model-complexity, scale, and closure, that scientists need to reach in modelling practices? To find an adequate balance between the potential epistemic benefits and the uncertainties posed by each of those modelling decisions, it is crucial that geoscientists attend to the *specific aims* of the modelling practice. As trivial as it sounds, the consequences of admitting that earth modelling is an aim-directed practice cannot be emphasized enough. I come back to Windelband's proposal now, as it can shed light on the consequences of articulating explicitly the aims of modelling in the geosciences.

After proposing the nomothetic-idiographic dichotomy, Windelband asked: "which is more valuable, knowledge of laws or knowledge of events? […] From the outset, it is clear that this question can only be resolved on the basis of reflections concerning the ultimate aims of scientific research" (1894, 180). To this, he added that one could reflect on the aims of science from two standpoints, the "utility" of knowledge, and the "immanent", cognitive value of the objects of knowledge (ibid., 180-181). Contemporary philosophers of science frame the debate of the aims of scientific modelling in comparable terms. One can identify "utility" aims of earth modelling, in the form of a variety of social, economic, ethical, and practical aims (utilization of earth resources, conservation, increased industrialization, hazard prevention, etc.). These aims have been historically pursued in combination with various epistemic or cognitive aims (understanding, explanation, prediction, exploration, etc.) (Potochnik 2015; Douglas 2013). Crucially, neither geoscientists nor the general public are in a position to judge the accuracy of a model, or to respond to the question of "which mode of investigation is more valuable?", without considering the particular utility and cognitive aims that the model in question serves.

To merely talk about the accuracy of a model is equivocal, insofar as the good performance of a model regarding certain aims does not guarantee its good performance regarding others. Models are only "adequate-for-purpose" (Parker 2020: 460). Moreover, success with respect to one aim often occurs at the expenses of others. In climate science, seismology, or volcanology, the aim of accurate prediction would demand to build models that deliver the most likely outcomes, while the aim of guiding hazard prevention would produce models that display the riskiest possible outcomes (Potochnik 2015, 76). Perhaps the clearest case of trade-off or relation of attention between different modelling aims is that the gain in explanatory power or understanding often requires sacrificing predictive accuracy, and vice versa (ibid.; Matthewson & Weisberg 2009). On occasion, though, some models have been severely criticized for their inadequacy, especially in the public debate, without actually been assessed by the standards set for the aims they attempted to serve. Being judged for not making accurate quantitative predictions, when the goal of a model is to explain what the core mechanisms of a system might be, constitutes a case of misguided evaluation (Bokulich 2013, 115-116).

Borrowing from these insights in the philosophy of modelling, and building off of the distinctions drawn in the previous section, my analysis offers a concrete recommendation: earth modellers, especially those whose outcomes might emerge in policymaking processes, should state the aims that guided the construction of their models as explicitly as possible, as well as the standards used to evaluate success with respect to those aims (e.g., empirical corroboration, robustness analysis, etc.) (see Longino 2004, and Douglas 2013 for similar suggestions).

This takes us to a second observation. Windelband described the two modes of investigation not just to showcase the variety of approaches to the understanding of a scientific object, but to support a normative stance: it is beneficial to keep different modes of investigation in order to maximize the utility and cognitive aims of science (1894, 181-183). Specifically regarding utility, Windelband argued that the two modes of investigation were equally justifiable, since the nomothetic "has the practical value of making possible prediction and human intervention in the course of events", and the idiographic helps to "produce a vital effect" on historical structures, by uncovering the traces of singular or violent events of the past, and assisting humanity to carry the burden of history in case it becomes too heavy in the future (ibid., 180). Now, regarding the cognitive aims of science, Windelband introduces a crucial consideration: we have historically inherited a bias or preference for the nomothetic, namely, for the generic, abstract, and capable of producing predictions, to the point of assuming that the idiographic could be eventually subsumed under lawlike statements. In contrast, Windelband suggests, we need to remember that what we value as humans is deeply grounded on the singular and unique. Hence, the two modes of investigation, and their respective cognitive aims, should remain independent and juxtaposed (ibid., 181-183).

The invitation here is to adopt a normative pluralist stance. Translated into the current modelling debate, it entails an active commitment to encourage the construction of a plurality of scientific models around the same object of investigation, built at different scales, levels of complexity, historical viewpoints, and exploiting different idealizations, in order to maximize a diversity of cognitive aims in the study of earth systems. The most prominent advocate of a normative pluralism in the recent debate is Chang (2012, chap. 5). He proposes to foster alternative scientific systems of practice, modelling styles, and experimental strategies, “alongside the orthodox and the fashionable”, even to the point of redeeming historical practices that, despite having been considered superseded, can still advance pursuitworthy questions (2012, 284). Of course, Chang says, it is likely that “scientists are already being as pluralistic as their professional constraints allow” (ibid., 285), since there are varied pragmatic reasons of time and funding that limit the possibility of multiplying research approaches. Still, the task of the active pluralist, especially coming from the history and philosophy of science, is to challenge dominant monist biases that favour certain aims and modelling approaches beyond those pragmatic constraints.

Today there is still an open preference or bias for the nomothetic in earth modelling, or more precisely, for the pursuit of predictive aims, associated with the construction of complex, generalizing numerical models, which, paradoxically, are the most costly and resource-intensive ones (see footnotes 5; Heymann, Gramelsberger & Mahony 2017b). Evidence of this bias is found, first and foremost, in the immense institutional investment worldwide in this kind of model approach, to the detriment of others, such as reduced complexity modelling or physical modelling, which might favour understanding and exploratory goals (see footnote 4; Gramelsberger and Fleichter 2011, 45-53; on exploratory models, see Gelfert 2016, Chap. 4). More subtly, it is also found in observations by Canham et al. (2001) and by Oreskes (2003, 14), who notices that modellers themselves are “well aware of the value of models in integrating data and generating predictions, but are less well informed about the heuristic value of models in guiding observation and experiment”, that is, they are not fully aware of the possibilities of using models for the exploration of empirical questions. In the absence of a well-formed theory, or when the phenomenon under investigation is poorly defined, models can play exploratory roles, helping to delineate the target system and isolate it from background noise (Gelfert 2019, 93). And even when a target is well defined, scientists can use models to explore counterfactual possibilities, thereby expanding the scope of what is possible and impossible about a phenomenon (Ibid., 15; see also Fisher et al. 2021). The dominating predictive paradigm often obscures the important exploratory functions that models can serve.

One can step back and identify the origins of this predictive paradigm in earth modelling. Around the 1960s, the concept of model started to mean what it mainly means today, namely a numerical model, leaving behind the idea of a model as a physical artifact or analogue (Oreskes 2007, 93). The semantic change in the term "model" occurred at the same time than the expansion of simulation work with personal computers and the projection of a new expectation onto earth models, largely set by the patrons –Cold War sponsors– that funded them: to predict! (ibid., 114). The frequency of usage of the term "prediction" in printed sources steeply increased after 1945, as the preoccupation with the future of politicians, strategists, social planners, and entrepreneurs intensified (Heymann, Gramelsberger & Mahony 2017a, 3). New "cultures of prediction" emerged in the twentieth century as reflection of the anxieties and ambitions of post-war societies, leading to new hierarchies of knowledge and the professionalization of predictive expertise (Heymann, Gramelsberger & Mahony 2017b, 18).[11]

Prediction for social purpose promised significant rewards in the form of policy-relevant knowledge (Heymann, Gramelsberger & Mahony 2017b, 21). However, some scientists are recently questioning if the immense efforts of global climate science in the preparation of projections, for instance for the IPCC, reflect a genuine improvement in the understanding of the atmosphere and other earth systems, since a shared impression is that this seems to have been stagnating for years (ibid.; Heymann and Hundebøl 2017, 116). As we have seen, models that are primarily predictive may not be the most suitable for understanding patterns of behaviour of an earth system or for exploring empirical hypotheses. Isaac Held (2005) agrees and further notes that with the continued increase in computational power, the importance of understanding itself is sometimes questioned. This is even though a holistic understanding of climate dynamics is often crucial for effectively comparing complex climate simulations –a fundamental practice in the international climate change research landscape.

So, a second recommendation advanced in this paper is to echo Chang's pluralist stance, in a manner already hinted at by Windelband (1894), and encourage historians and philosophers of science to critically examine how monist attitudes regarding predictive aims, and associated modelling approaches, are constraining the resources available to understand earth systems. Bringing attention to valuable earth models, current and past, that are not subsumed under the inherited predictive paradigm, in contrast, can reveal promising paths of inquiry that have not been explored enough yet. Conceivable examples are to pay attention to the potential usefulness of building more data-driven and semi-empirical models in climate science (Katzav and Parker 2015, 475-6); of using more "heuristic models" of the atmosphere,

[11] Gramelsberger (2017) locates the roots of twentieth-century "cultures of prediction" in meteorological research of the late nineteenth-century Europe.

meant to improve theoretical understanding in a qualitative form (Heymann and Hundebøl 2017, 101; Dahan-Delmedico 2001); of tolerating hot-searches and creative model hypotheses in paleontology (Currie 2018); of introducing simple dynamic ecological models to explore the mechanisms of change in ecosystems (Pace 2003); of constructing experimental models in geomorphology that challenge the principles of physical similarity and explore self-similar patterns of landscape formation (Paola et al. 2009); and even of revisiting historical practices like hydraulic and atmospheric analogue modelling, in order to ask whether they fostered creative skills (haptic, visual, intuitive) that should be rehabilitated in some way in contemporary, predominantly digital and numerical, modelling practices (Freeman 1930; Fultz 1951).

The result of this proposal is not the replacement of complex simulations with reduced-complexity models, but rather the promotion of an open-ended plurality of models in systematic communication with one another (Katzav and Parker 2015). This would include some high-end simulations, which have valuable pragmatic functions in the present but are likely to become obsolete in the future (Held 2005, 1613); some "elegant models", or models of lasting value, that are only as elaborate as needed "to capture the essence of a particular source of complexity" (ibid.); and models of intermediate complexity that fill gaps and facilitating communication between the other types (ibid., 1614). The main suggestion here is that investing more effort in elegant models can significantly expand the range of cognitive aims achieved in the study of earth systems. Beyond the limited "idealized theoretical work" that Held (ibid.) attributes to elegant models, historians and philosophers of science can illuminate important aims such as empirical exploration, narrative explanation, and mechanistic understanding that elegant models can fulfil.

The familiarization with specific modelling approaches took some scientific communities in the twentieth century from a situation where they saw "the world in the model" to another where they saw "the model in the world" (Morgan 2012, 405-406). They started by looking at parts of the world through the lens of their newly built models, using them to interpret phenomena, but after some time working and arguing with them, they ended up seeing those models operating in the world, and hardly beyond them. Here lies the worldmaking potential of successful representations, including models. The discussion in this paper suggests that the tendency to favour predictive, complex, numerical earth models at the expenses of other types could prompt that same shift in the community of geosciences soon. However, perhaps we ought to remain at the stage of seeing the "Earth in the model" for a bit longer, interpreting earth systems in the light of a variety of artifacts that uncover different, sometimes conflicting, aspects of them. Adopting a pluralist attitude could prevent that one of those artifacts gets to be so preponderant and familiar that it becomes a totalising authority that determines all we see in the world.

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