Towards more reliable interdisciplinary modeling of global human-environment dynamics driving biodiversity change

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Abstract

Environmental problems are typically vast, urgent, and complex. Confronted with such problems, we are often tempted to act fast by pulling together little bits and pieces from different fields and simply adding these to pre-existing models and frameworks. Seldom, though, do we pause long enough to look whether and for how long those larger structures we build can support reliable answers to our questions. In this Perspective, I critically discuss the current state of broad-scale, interdisciplinary coupled modeling of human-environment relationships, with a focus on the classical model virtues precision, generality, and realism. I focus on models used to address land-use-driven biodiversity change in the context of broader global-change and sustainability challenges, for which I draw examples mainly from agroeconomic land-use models and integrated assessment models – popular coupled modeling frameworks that are increasingly coupled further with ecological models. Specifically, I discuss *i)* how limitations in our models' training data and underpinning theories translate into excessively uncertain predictions, *ii)* how coupling even highly general sub-models can lead to hardly generalizable representations of indirect human-environment relationships, and *iii)* how representing ever more processes decreases rather than increases realism due to greater average measurement bias, a problem further exacerbated as we add processes based on their relevance for our own systems of interest, rather than for the real-world systems' dynamics. I also explore barriers to advancing scientific modeling virtues amid other, non-scientific motivations for interdisciplinary modeling (e.g., cultural, economic, normative). Finally, I offer suggestions to modelers and other actors in science, science administration, and science policy to help promote a transition to more reliable interdisciplinary coupled models that can remain powerful for addressing major sustainability challenges far beyond the next iteration of science-policy assessments.

Introducing interdisciplinary model-coupling

Recent global assessments (IPBES 2019, 2024; IPCC 2023) highlight not only the magnitude of global environmental changes, but also the interactions between environmental and societal processes within a highly complex, global human-environment system (or social-ecological system; (Liu et al. 2007)). Within this system, resource-use decisions such as land-use changes act as key interfaces through which multiple socioeconomic drivers propagate to shape biotic and abiotic conditions and, in turn, human well-being and risk (Verburg et al. 2015). As these interactions often expose trade-offs and co-benefits between sustainability goals, models linking outcomes such as biodiversity change only to their proximate pressures are frequently insufficient to inform policy or management. Over the past decade, more interdisciplinary models that link environmental outcomes to both direct and indirect drivers have thus become increasingly popular (e.g., Kim et al. 2018; Leclère et al. 2020).

Two conceptually distinct approaches to interdisciplinary modeling can here be identified (although various combinations of these exist). The first approach starts from the elementary model components (e.g., variables, agents, interactions) and essentially assembles any more complex model structures from scratch while treating the components contributed by any discipline in the same way, using domain-neutral modeling frameworks such as agent-based modeling, system dynamics modeling, dynamic Bayesian networks, artificial neural networks, or multiple regression (Macleod & Nagatsu 2018). Whereas such domain-neutral, 'natively interdisciplinary' modeling can allow simulating highly complex and entangled social-ecological processes (Schlueter et al. 2012), it plays a relatively small role in current large-scale, social-ecological modeling of environmental change. In this essay, I focus on the second, currently dominant approach, which can be described as interdisciplinary 'model-coupling' (Kelly et al. 2013; Macleod & Nagatsu 2018; Voinov & Shugart 2013). In model-coupling, different research fields contribute 'legacy models', i.e., pre-existing modeling frameworks originally developed for more disciplinary purposes (Voinov & Shugart 2013). These models become modules of larger, interdisciplinary models, in which the output (e.g., explained, predicted, or outcome variable) of one sub-model serves as input (e.g., explanatory, predictor, or causal variable) for one or several other sub-models.

The current dominance of model-coupling over natively interdisciplinary modeling has been attributed to advantages of the former in terms of reducing common hurdles in interdisciplinary modeling (MacLeod & Nagatsu 2018). These hurdles may range from 'mere' technical issues (e.g., mismatching spatial/temporal scales at which disciplines traditionally record/model their phenomena of interest; Verburg et al. 2016) or differences in basic conceptualizations (e.g., of 'natural' vs. 'anthropogenic'; DesRoches, Inkpen, and Green 2019), to fundamental epistemic differences (e.g., how useful collaborating modelers perceive theory- vs. data-driven modeling; Armsworth et al. 2009) or even deeply conflicting normative orientations (e.g., anthropocentric vs. biocentric values; Campbell 2005). Although model-coupling, too, certainly faces interdisciplinary challenges, it tends to largely avoid any deep ontological, epistemic, or normative conflicts, because the disciplinary assumptions and traditions behind each sub-model can remain largely preserved and its contributors can remain responsible not only for evaluating its theoretical and empirical validity and reliability, but even for defining the standards to be met (Macleod & Nagatsu 2018). As a result of these practical advantages, model-coupling can be an effective strategy for advancing interdisciplinary modeling projects relatively quickly and is thus often chosen under constraints on time and human and financial resources.

However, approaching human-environment dynamics via interdisciplinary model-coupling also entails risks (Fig. 1). This essay is meant to raise awareness of several risks of Broad-Scale, Interdisciplinary, Coupled Human Environment (hereafter 'BSICHE') modeling, and of associated structural problems in current BSICHE modeling cultures. I target, firstly, (self-)critical modelers, to provoke debate on underappreciated risks and limitations in contemporary BSICHE models and on how non-scientific motivations can hinder scientific model virtues. Secondly, I seek to inform researchers, science funders, and science-policy brokers who engage as non-experts with BSICHE models or their results about these risks and highlight potential actions towards stronger scientific model virtues.

I use 'we' to refer to the diverse community of scientists (including myself) who develop or apply BSICHE models and are thus both responsible for and exposed to the identified risks. Because these risks are widespread, I avoid singling out individual papers and, where I do, focus on work I am co-responsible for (my apologies to colleagues I thereby also put on the spot). I will draw examples mainly from agroeconomic land-use models and integrated assessment models (IAMs), especially those that are coupled further with ecological models of (land-use-driven) biodiversity change. I made this choice because *a)* land use is a particularly salient interface between human decision-making and ecological outcomes, *b)* I understand these models, *c)* they feature prominently in science-policy processes, and *d)* there is a history of intercomparison of multiple models designed for the same general purpose, and, at least in the case of major sub-models, of thorough comparative validation efforts (McCalla and Revoredo 2001; Hertel, Baldos, and Mensbrugghe 2016). At the same time, the central arguments of this paper are all either based on empirical observations that are consistently made across diverse scientific fields and modeling applications, or logically reasoned based on generic principles, or both. As such, they may extend to different types of BSICHE models.

This essay is structured in four parts, starting with a brief overview of IAMs, and continuing with an extended critical discussion of, firstly, the scientific virtues of global coupled models of human-environment dynamics, and secondly, several cultural and systemic hurdles that hinder progress in improving interdisciplinary modeling. Finally, I will give suggestions for different stakeholders to help overcome these hurdles, hoping that these may inspire more intensive efforts to develop solutions.

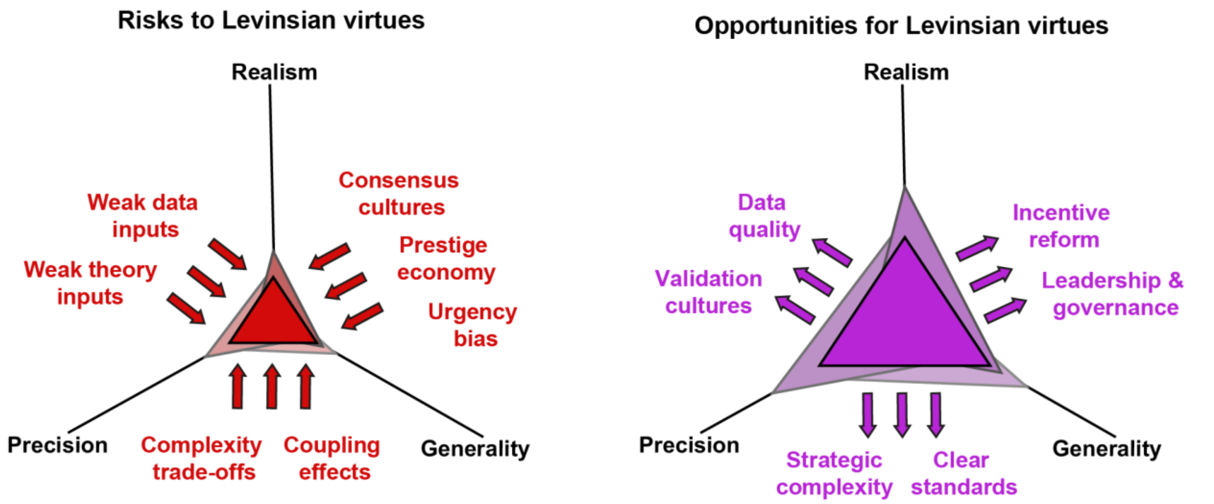

Figure 1. Conceptual graphic summarizing how different problems (red terms) and potential solutions (violet terms) constrain (inward arrows) or expand (outward arrows) the Levinsian modeling virtues (precision, realism, generality) achievable by broad-scale, interdisciplinary, coupled human-environment models. The areas of red/violet triangles illustrate the overall quality achieved by different hypothetical models, and the proximity of their edges to the axis headers illustrates how high each model scores with respect to the three virtues. The different problems are discussed in detail in Parts II and III, whereas potential solutions are presented in Part IV and Table 1.

Part I: What are IAMs, and how are they used to model biodiversity-relevant human–environment dynamics?

Integrated assessment models (IAMs, Fig. 2) are the bedrock of scenario-based modeling and assessments within major global science-policy interfaces such as IPBES and IPCC. IAMs are particularly complex coupled models that aim to represent substantial portions of the global human-environment system by integrating biophysical information on climate, hydrology, and ecology, with socioeconomic information on agricultural production and consumption, trade, technology, energy, and other domains; in some cases, they also include macroeconomic components to estimate long-term economic trends (e.g., GDP or investment flows). Many IAMs include explicit representations of land use and land-use change, often implemented through agroeconomic sub-models, or are coupled to such models in downstream analyses. In the following, I focus on land-use–relevant IAMs and agroeconomic land-use models.

In the context of biodiversity-relevant applications, it is important to note that most IAMs, agroeconomic land-use models, and other types of BSICHE models do not originally represent biodiversity. In recent years, however, such models are increasingly coupled further with ecological models, to allow projecting and assessing biodiversity change under climate, socioeconomic, or policy scenarios and in the context of broader social-ecological interactions (Harfoot et al. 2014). For example, such coupled models have recently been used to explore policy needs for reconciling biodiversity conservation and restoration with food security (Leclère et al. 2020), or to evaluate direct biophysical and indirect socioeconomic effects of climate change on biodiversity (Kapitza et al. 2021).

Whereas the IAMs' base models are *ontological* system models, meaning that they aim to provide broadly accurate (if simplified) representations of how the real world works (Fig. 2, blue part), the scenario assumptions that form inputs into the IAMs, and thus, also the IAMs' projections, may in part be *teleological*, e.g., representing how more desirable worlds might work or how the future ought to be (Fig. 2, pink parts). This makes sense, given that many IAMs are primarily designed to enable long-term explorations of plausible futures under alternative scenarios, rather than to project specific futures that are 'likely' to occur (Fig. 2). Accordingly, uncertainties in IAMs' projections that reflect differences in their scenario inputs are not *per se* undesirable. However, even when exploring highly speculative or normative scenarios (e.g., target-seeking scenarios that explore what policies would be needed to achieve a given desirable future), the ability of the IAM base models to make accurate forecasts is nevertheless crucial, as this ability determines in how far the IAMs' projections are plausible, *given* a particular scenario (even though the scenarios themselves are subject to substantial uncertainties, Rounsevell et al. 2021).

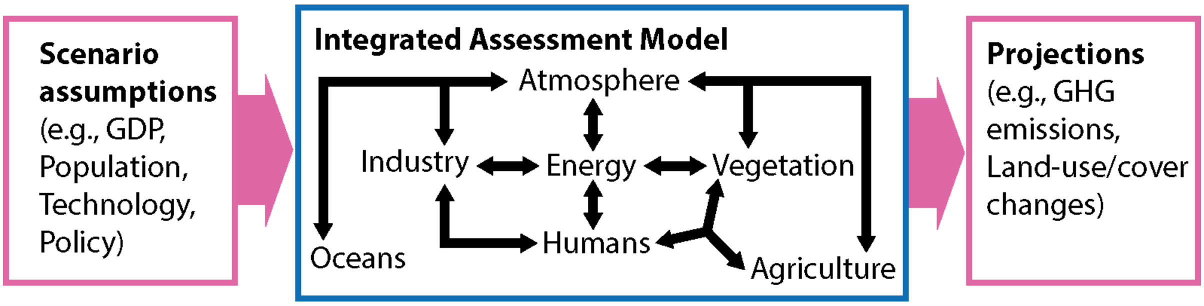

Figure 2. Simplified schematic overview of an Integrated Assessment Model (IAM; adapted from Harfoot et al. (2014)), used in this paper to illustrate different problems in interdisciplinary model-coupling. The scenario inputs to IAMs, and thus by extension, also the future projections that IAMs produce as outputs (both in pink), may include teleological components and/or represent what might be possible and, as such, do not necessarily aim to provide accurate future predictions. The core IAM (in blue), however, aims to provide an accurate (if highly simplified) representation of how the real world works or would work, *given* certain hypothetical assumptions about future conditions. Note that any of the IAMs' base models' components may, in principle, be further coupled to specialized ecological models to allow exploring the scenarios' implications for biodiversity.

The ontological base models of existing IAMs differ substantially in their structural and process assumptions, in their levels of detail and complexity, and in the specific interconnections considered (Schmitz et al. 2014; von Lampe et al. 2014). For example, the core agroeconomic models within some IAMs are partial-equilibrium models, focusing on specific economic sectors, and incorporate spatially explicit land use as part of the model solution (e.g., as in REMIND-MAgPIE (Popp et al. 2011) or MESSAGE-GLOBIOM (Fricko et al. 2017)). Other IAMs incorporate general-equilibrium models that consider interactions across the entire economy but determine land use only at the level of broad agro-ecological zones (e.g., as in AIM (Fujimori et al. 2012) or FARM (Sands et al. 2013)), which may be further downscaled to pixel-level via yet other models. IAMs also differ in their (static or dynamic) representation of different land-use classes and in fundamental assumptions regarding which lands are potentially available for expansions of different classes (Schmitz et al. 2014).

These structural differences between existing IAMs cause great variation among the future projections that different models make for a given scenario (Alexander et al. 2017; Hertel et al. 2016). In many cases, projected changes in basic land-cover classes differ not only in their spatial patterns, but even in direction and orders of magnitudes at continental to global scales. Such divergence directly limits confidence in model-based scenario analyses and weakens the reliability of systemic insights and policy-relevant conclusions drawn from coupled modeling exercises.

Part II: The scientific virtues of interdisciplinary coupled models

Before discussing the 'quality' of BSICHE models from a scientific perspective, I stress that modeling serves many valid purposes beyond strictly scientific ones, such as supporting policies (Verburg et al. 2016) or ensuring stakeholder participation and buy-in (Moallemi et al. 2021). From a narrowly scientific perspective, however, BSICHE models are no different from disciplinary models in that to be useful given their specific aims, they should score high in one or several model virtues.

Levins (1966) classically identified three main virtues (Fig. 1). Each of these depends on solid data, solid theory, or both. Firstly, ***model precision*** refers to the ability to predict the phenomenon of interest accurately at high detail. Achieving precision requires reliable data for model training or parametrization, calibration, and validation. Secondly, ***model realism*** concerns the accurate representation of the variables, structures, and interactions that determine system behavior. High realism requires reliable *a-priori* information to identify key system components, in addition to reliable data on those. Except in very small or simple systems where one might feasibly measure all components before modeling, such prior knowledge depends largely on solid theory (although other models or expert opinion might also contribute). Finally, ***model generality*** describes the ability to predict or explain accurately in many different situations. Developing generalizable representations of complex systems – without sacrificing precision to the point of uselessness – requires extensive and representative data. Similarly, demonstrating generality requires such data for rigorous testing across multiple spatial and temporal scales and contexts.

Importantly, models cannot simultaneously maximize precision, generality, and realism (Levins 1966). Modelers must thus balance these virtues in light of their objectives.

The Levinsian virtues of a coupled model are constrained by those of its sub-models and thus by the quality of their input data and theoretical foundations (Fig. 1). The proverb 'garbage in, garbage out' applies equally to coupled models and their components. If a sub-model is grounded in extensively tested theory whose predictions have proven accurate across diverse conditions, we would expect those strenghts (Keas 2018; Kuhn 1977) to translate into high Levinsian virtues of the sub-model (provided it faithfully represents the theory). If the same holds for all sub-models, the coupled models should also be highly virtuous. Conversely, if a sub-model relies on non-representative data or weak theoretical foundations, biased predictions are likely to propagate through both the sub-model and the larger, coupled model.

How solid are the theoretical assumptions behind our models' structures?

BSICHE models inherit multiple theoretical assumptions from their sub-models, which may be weakly tested, outdated, invalid, or misapplied, thereby undermining Levinsian virtues (Fig. 1). Sub-models with strong theoretical assumptions not only come from disciplines with long epistemic traditions of theoretical modeling, such as economics, but also from ecology (e.g., niche theory, island biogeography theory), geography (e.g., Tobler's law of proximity-similarity relationships, Christaller's theory of central places), and other fields. Strong theoretical assumptions are not a problem *per se*, provided that they are valid. However, the theoretical underpinnings accumulating in BSICHE models differ tremendously in their empirical evidence base and validity, reflecting both disciplinary scrutiny standards and the data and tools available for testing when the theories were developed. Some theoretical assumptions are so deeply embedded in disciplinary modeling traditions that they are routinely re-applied despite evidence that they are mostly invalid (e.g., 'Homo Economicus'; Urbina and Ruiz-Villaverde 2019). While such assumptions may have been defensible in the sub-models' original contexts, reapplying these models outside of their original design specifications will often exceed their range of validity (Rykiel 1996). In interdisciplinary model-coupling, many flawed assumptions may compound. For example, global biodiversity-recovery scenarios based on coupled IAM-biodiversity modeling (Leclère et al. 2020) accumulate problematic assumptions on rational agents and efficient international markets (Memon et al. 2022; Urbina & Ruiz-Villaverde 2019; in IAMs' agroeconomic sub-models), historical land-use intensification processes (Ellis et al. 2013; in the land-use reconstructions used in climate sub-models), and species being at equilibrium with their environments (Dormann 2007; in species-distribution models and other biodiversity-environment models).

Most theories underpinning BSICHE models, moreover, have dubious generality. Whereas a somewhat limited generality is to be expected (Lawton 1999; Meyfroidt et al. 2018) and is not a problem *per se*, we do not usually know just how (non-)generalizable different theories are. Few studies have mapped existing theories into the specific contexts where they presumably apply based on the authors' system understanding and case-study experience (e.g., Meyfroidt et al. 2018), and few have systematically tested the predictions of theories across different spatiotemporal scales and contexts (e.g., Pacheco & Meyer 2022). Even some foundational theories have never been tested across relevant scales, leaving the extents of their validity unclear (e.g., 'carrying capacity'; Seidl & Tisdell 1999). As a result, at which specific scales and contexts a given theory should hold, and thus, where re-applying a respective sub-model would in principle be valid, is often not explicitly assessed during the coupling process and is instead determined implicitly through expert judgement, rather than through formalized criteria or systematic assessments. Addressing this problem would require systematic efforts to delineate the contexts and scales over which key theoretical assumptions are valid, and to make such validity assessments explicit during model coupling – an issue returned to in Part IV.

How reliable are the data we use for training & validation?

As with underlying theories, the validity of BSICHE models depends critically on the quality and coverage of the data used for training, calibration, and validation (Fig. 1). Globally available data for most relevant variables are heterogeneous and highly uncertain. Modelers rely on disparate biophysical data (often from remote sensing) and socioeconomic data (from censuses and surveys) to enable model applications over broad scales. However, both have already undergone abstraction from *primary* evidence (e.g., field observations or household surveys), which itself is often sparse and unrepresentative (Meyer et al. 2015, 2016; Pengra et al. 2020; Wollburg et al. 2021). With few exceptions, global 'gridded data' that capture variables contiguously at adequate spatial resolutions are not actually data, but model-based predictions. Despite improvements from new data streams and data-integration strategies (e.g., Liu et al. 2021; Parente et al. 2025), these predictions typically carry substantial, often unreported, regional uncertainties.

To illustrate, consider 'gridded data' on basic land-cover/use classes. Such data directly or indirectly inform IAMs and other BSICHE models (Kasampalis et al. 2018; Lawrence et al. 2016; Nelson et al. 2009; Parker et al. 2003). For instance, remote-sensing-based maps are commonly used to initialize simulations. Yet, in many tropical regions undergoing rapid land-use (and thus, biodiversity) changes, datasets disagree even on whether cropland exists in a given pixel (Pérez-Hoyos et al. 2017). Uncertainty is even higher for grazing (Fetzel et al. 2017) and forest management (Erb et al. 2017). Similarly, massive uncertainties characterize historical land-use reconstructions used in climate models and certain IAMs. Particularly for earlier decades, limited calibration data mean that trajectories simulated by different models (e.g., Hyde (Klein Goldewijk et al. 2010), KK10 (Kaplan et al. 2010)) are largely driven by differing assumptions about land-use-change processes. These process uncertainties translate into substantial disagreements about the timing of land-use onset and the extent of post-peak recovery (Ellis et al. 2013).

How important, really, are reliable data and solid theory?

To explore just how important *reliable* data and *solid* theory are for the predictive capacity of BSICHE models, we can resort to comparisons of agro-economic models of land-use (the 'cores' of many IAMs). Alexander et al. (2017) and Prestele et al. (2016) partitioned the total variation in future cropland, pasture, and forest area predictions across 18 land-use models (each with multiple scenarios) into different components. They evaluated, firstly, how much uncertainty across these models' projections is attributable to differences in spatial patterns of initial land-use conditions, which reflect uncertainties in the land-use data used for model initialization. Secondly, they attributed variation to differences in generic model types, which reflect major differences in the implemented processes and theoretical assumptions (e.g., whether land-use changes affect specific cells because that would be economically most efficient, or because land-use spreads in a spatially constrained and path-dependent way). Thirdly, they attributed variation to differences in model cell number, i.e., in the spatial resolution at which processes are represented, which reflect differences in the implicit theoretical assumptions about which scales matter, but also, the constraints set by the spatial resolutions of available input data (as well as computational constraints). Finally, they attributed variation to differences in the scenario assumptions that define the possible alternative futures that the models are meant to explore.

The results of this exercise showed that the uncertainties in input data on initial land-use conditions are by far the single largest cause of uncertainty in modelled pasture and forest projections until the middle of the 21st century, with uncertainty in cropland projections being mostly shared between differences in input data and the models' structural assumptions. As projections go farther towards the end of the 21st century, the definition of initial conditions remains responsible for the bulk of uncertainty in pasture areas, whereas for cropland and forest areas, model type and the spatial resolution at which processes are represented become the most or next-most important sources of uncertainty.

These results by Alexander et al. (2017) and Prestele et al. (2016) have several implications. Firstly, they imply that developing reliable global reference data on key variables to accurately depict the initial conditions will be the single most effective activity to reduce uncertainties in current models, and thus, from the standpoint of improving model support for policy, should be prioritized over any improvements in the models themselves. The next-most important priorities are arguably *i)* to provide input data at adequate (e.g., fine/multiple) spatial resolutions, and *ii)* to get the models' structural assumptions right by improving/affirming their theoretical or empirical foundations. The second major implication concerns current models' fitness-for-purpose for supporting policy by means of scenario exploration. Even for long-term land-use projections until the end of the 21st century, the combined uncertainty attributable to limitations in the models' data and theoretical foundations is far larger than the total variation attributable to differences in scenario assumptions. This means that one cannot 'justify' high uncertainties in scenario projections by the fact that certain future behaviors of complex human-environment systems are simply unknowable (e.g., how future climate policies might interact with future levels of international cooperation), nor can uncertainties be blamed on diverging scenario assumptions. The real problem is limitations in the ontological cores of our models (Fig. 2), i.e., in the data and structural assumptions behind the IAMs and their sub-models. Worryingly, this dominance of data and model limitations over scenario assumptions ultimately implies that current practices of employing multiple coupled models to explore alternative scenarios are largely unfit for this very purpose.

How precise are our interdisciplinary models, then?

Given that the predictive capacity of global interdisciplinary models seems to depend primarily on their underlying data and theory, and given the limitations in both (see preceding sections), we should expect low model precision *sensu* Levins (1966) to be the norm. In other words, current BSICHE models will often fail to accurately predict the properties of interest to conservation and sustainability at the detail required for policy and management. The few systematic efforts that validated multiple comparable coupled models using rigorous validation protocols largely support this expectation (see McCalla and Revoredo (2001); Hertel et al. (2016), based on ex-post validation of earlier agro-economic land-use-change projections).

However, we cannot know this with certainty because we lack a model-validation culture. A survey of 10,739 resource-management modeling studies found that since 1970, consistently <1% addressed model validation (Eker et al. 2018), suggesting that we are ultimately more concerned with developing, coupling, and applying models to new questions than with whether or not our models can actually yield reliable answers. Most BSICHE models and many of their sub-models were never validated against independent data (Gomes et al. 2021; Sivagurunathan et al. 2022). For example, uncertainties in IAM-based projections are commonly assessed via model intercomparisons and other 'soft' evaluation forms (Wilson et al. 2021), rather than through formal validation. Only few studies have tested whether models can simulate sensible trajectories *given* a particular scenario via historical simulations (although that was more prominent in early IAM evaluation practice), and those were limited in spatial and temporal scope (Wilson et al. 2021), despite IAMs being routinely used for global, long-term projections.

How general could our interdisciplinary models be?

Given the general shortage of model validations, we also know little about the generality of BSYCHE models. Yet, when such models are developed for broad-scale applications (e.g., global scenario-modeling), they implicitly assume high generality, as they generate projections across diverse socio-environmental contexts that must, on average, be sufficiently accurate and precise for assessments to be meaningful.

In the following, I invoke *causality* of indirect relationships – not as a normative benchmark of 'good' models or theory – but as a diagnostic concept to reason about generality, transferability, and contextual validity of indirect relationships represented in coupled models. BSICHE models typically represent multi-step, indirect chains of cause and effect, rather than just simple one-to-one relationships. But how general can such indirect causal relations realistically be?

To explore this, consider a hypothetical system in which we wish to predict the dynamics of interacting variables. Assume that we correctly identify the key variables and causal interactions, represent them accurately in the model, and have accurate, representative training data. Even under these favorable conditions, important processes in complex systems are rarely universal but usually depend on contextual factors (Lawton 1999; Meyfroidt et al. 2018). Thus, even *relatively* general relationships will vary in their existence and strength across space, time, scales, or relevant social and environmental dimensions. Specifically, at any location i in this multidimensional space, a causal effect $X_i \rightarrow Y_i$ between a causal driver variable X_i and an outcome variable Y_i can only exist if Y_i is exposed to a sufficient intensity of X_i to spark a response in Y_i, given Y_i's local sensitivity to X_i. The local strengths of effects depend on local exposures and sensitivities (**Fig. 3A**). Such simple bivariate relationships will be highly general if these conditions are given across many contexts.

However, more complex causal relationships – whether indirect (e.g., $X_i \rightarrow Y_i \rightarrow Z_i$) or involving interacting drivers (e.g., $[X_i \leftrightarrow Y_i] \rightarrow Z_i$) – require more than each interaction satisfying the above-described exposure and sensitivity conditions. For such more complex relationships to be general, these conditions must not only hold for each constituent interaction (e.g., $X_i \rightarrow Y_i$ and $Y_i \rightarrow Z_i$, or $X_i \leftrightarrow Y_i$ and $[*] \rightarrow Z_i$), at many locations, but the *specific* locations i where they hold must be mostly the same across all interactions (**Fig. 3B**). The more variable interactions are involved in a given causal relationship, the less likely it statistically becomes that there are many locations where, across all those interactions, all contextual factors that influence their respective local exposure and sensitivity conditions are perfectly aligned. For this statistical reason alone, indirect and multivariate relationships will typically be less general and weaker than the more direct interactions of which they are composed (**Fig. 3**).

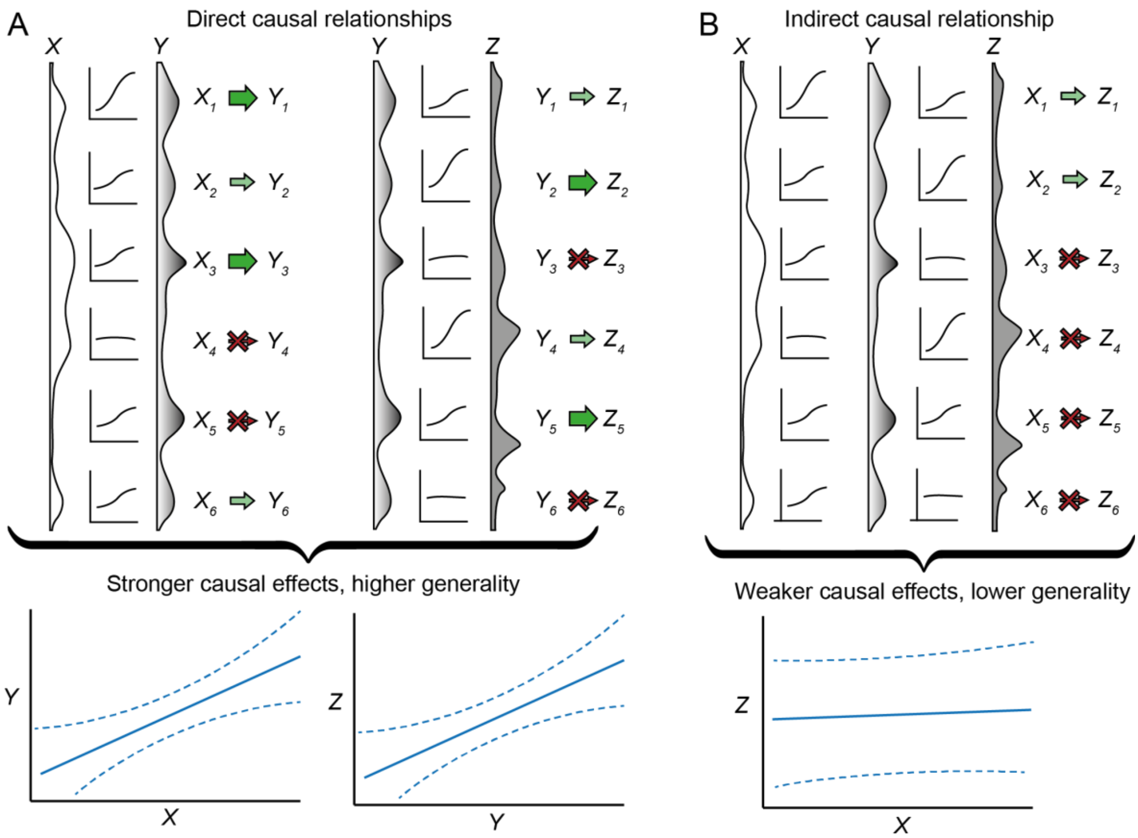

Figure 3. Conceptual graphic showing how natural variation in exposures and sensitivities of outcome to driver variables constrains the generality and strength of indirect causal relations. The distributions along the vertical axis depict natural variability of variables X, Y, and Z across different locations (across space, time, scales), with a high/low amplitude of the left-hand curve indicating a high/low local exposure of the outcome variable (right) to this driver variable. Local exposure and local sensitivity (depicted by steepness of dose-response curves) jointly determine whether there is locally a causal effect (green arrow) and how strong that effect is (width of green arrow). Existence/strength of local effects across locations determine the strength and generality of the aggregate effect.

Human-environment systems are characterized by many indirect and multivariate relationships (Cilliers et al. 2013; Meyfroidt 2015), which is why we couple multiple models to study these systems in the first place. Many of the constituent relationships might individually be highly general and well captured in models (e.g., Engel's law of income-dependent food expenditures (Houthakker 1957; Seale & Regmi 2006); species-area relationship (Lomolino 2000)). However, for the above reasons, even major social-ecological processes – such as the net, model-implied effect of rising per-capita incomes on biodiversity via indirect pathways (e.g., changes in consumption, production, land use, and associated pressures) – should be weaker and less general than their constituent processes.

Paradoxically, even if the individual sub-models are all fairly general, their resulting coupled models may still be invalid or have little predictive capacity in a majority of socio-environmental contexts in which they are applied. Whether this holds universally is ultimately an empirical question, but it represents a reasonable null expectation. The burden of proof whether models can predict accurately certainly lies with those applying coupled models in contexts where predictive capacity matters (Collins et al. 2024; Sargent 2010). Demonstrating model validity requires going beyond validating sub-models (Belete et al. 2017) and instead comparing the predictions of our full, coupled models against empirical data (Atamturktur et al. 2016; Stevens & Atamturktur 2017).

Are we pushing for greater realism?

Improving realism is a central motivation for many modelers when developing interdisciplinary models. Given the multivariate complexity of human-environment systems, this is often equated to representing more system components, leading to increasingly complex models (Fisher-Vanden & Weyant 2020; Pan et al. 2025; Thampi et al. 2024). The implicit assumption is that representing more relevant factors yields more realistic model structures (Schlueter et al. 2012), more realistic model behaviors – especially under new conditions – and thus more accurate or empirically adequate predictions of system behavior. Unfortunately, studies testing this assumption found that model performance often declines as complexity increases (McCalla & Revoredo 2001; Perretti et al. 2013; Warren & Seifert 2011).

This pattern is consistent with the bias-variance trade-off (Hastie et al. 2017): incorporating additional processes or parameters to better reflect real-world mechanisms (e.g., detailed land-use decision-making or climate-production feedbacks) may reduce systemic bias, but increases variance due to random measurement error, implying lowest overall model inaccuracy – and highest realism – at intermediate complexity (**Fig. 4A**). O'Neill (1973) further emphasized the role of non-random measurement gaps and errors, arguing similarly that representing more processes can increase total inaccuracy unless they are essential, well understood, and reliably estimated (**Fig. 4A**).

A particular problem for *broad-scale* modeling is that representative, broad-scale data coverage is very rare. Instead, severe data biases characterize even globally comprehensive databases and already hamper unbiased model representations of comparatively well-documented processes (see earlier sections). Thus, already as we exceed fairly low model-complexity levels, a growing share of added processes may be ill-parametrized, given sparse and unrepresentative data. Adding strongly mis-measured processes increases, rather than decreases systemic bias. We thus face a trade-off between increasing realism by adding relevant system components and losing realism through their misrepresentation (**Fig. 4B**). This reinforces the earlier conclusion that *data* limitations are the primary constraint on reducing model uncertainty.

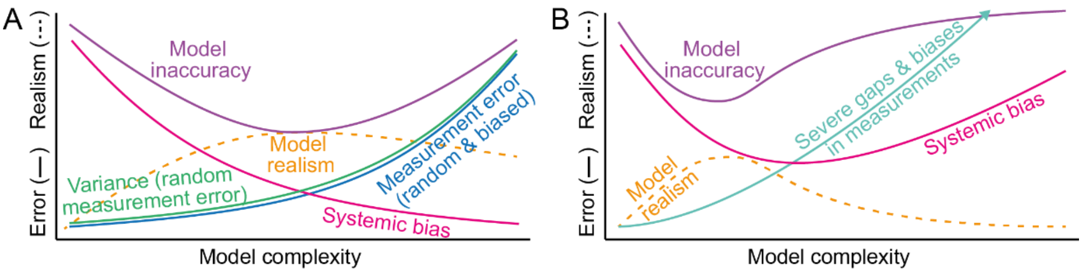

Figure 4. Conceptual relationships between model complexity, error types, and realism. A) Adding important, well-measured system components to overly simple models reduces systemic bias and thus increases realism. Yet, total model error may be lowest at intermediate levels of complexity due to the bias-variance trade-off (green line; Hastie et al. 2017) and increasing effects of non-random measurement errors (blue line; O'Neill's (1973); graphical representation adapted from Turner and Gardner (2015)). Thus, achievable realism declines at higher complexity levels despite decreasing systemic bias, because overall inaccuracies increase. B) In broad-scale human-environment modeling, adding processes may reduce systemic bias only in selected cases. Given severe gaps and biases in global databases, additional processes often introduce substantial errors and new systemic bias, implying that maximum realism may occur at much lower complexity levels than typically assumed.

How are we prioritizing our efforts to add complexity?

This trade-off implies that improving model realism requires strategic choices about which layers of complexity to add. This raises the question: how are such decisions currently prioritized?

Again, activities to couple agroeconomic land-use models with biodiversity models provide an illustrative example. Realistic representation of future agricultural expansion and intensification is critical for using such models to inform biodiversity policy. Agroeconomic models have long been criticized for excluding environmental factors (Tietenberg & Lewis 2018), and recent efforts to incorporate climate change and ecosystem services such as pollination indeed show that those factors substantially affect modelled agricultural outcomes (Johnson et al. 2021). However, to demonstrate that also *biodiversity* loss can cause losses in agroeconomic productivity or production stability via ecosystem-services feedbacks, arguments are prominently made to include biodiversity itself in economic models (Dasgupta 2021; TEEB 2018). Yet, available evidence suggests that *high diversity* of natural biota is not *key* for maintaining productivity, with effect sizes often small or context-dependent relative to factors such as climate or soil (Craven et al. 2021; Dormann, Schneider, and Gorges 2019; Pillai and Gouhier 2019; van der Plas 2019). At the same time, extensive evaluations of agroeconomic models have identified *economic* factors – such as labor inputs, capital inputs, or total factor productivity growth – as critical for improving projections (Hertel et al. 2016). Yet, those factors are largely absent from discussions on improving models for biodiversity policy (Akçakaya et al. 2016).

This example suggests that model 'enhancements' are often motivated more by our wish to better represent our *systems of interest* than by evidence on how we could more realistically model real-world, *ontological systems*. From a perspective of better informing policy by reducing uncertainty in projections, this is problematic, as improving realism requires prioritizing the empirically most influential processes. We may still need to include our favorite processes if our goal is to actively shape policy agendas. The latter is not inherently problematic. However, we should transparently distinguish between informing and shaping policy when advocating additions to already-complex models – especially when evidence suggests that other processes are likely *more* important (ontologically speaking) for system dynamics – and acknowledge that such efforts may not improve model realism.

Part III: Cultural and systemic hurdles to advancing Levinsian virtues

Challenges in interdisciplinary modeling are most commonly framed as technical or epistemic (Elsawah et al. 2020; Farahbakhsh et al. 2022; Iwanaga et al. 2021; Macleod & Nagatsu 2018; Schlueter et al. 2012; Verburg et al. 2016; Voinov & Shugart 2013). Here, I focus on cultural and systemic hurdles that shape modeling practice irrespective of what is technically possible or epistemically defensible. BSICHE modelers are rarely motivated solely by scientific rationales such as improving Levinsian virtues. Many also aim to help solve pressing problems and to succeed within the academic reward system. These cultural-normative and economic motivations are largely orthogonal to Levinsian virtues and – as I argue below – can even counteract them, by discouraging critical reflection on limitations, entrenching path-dependencies in dominant frameworks, inhibiting diversification, and disincentivizing deep changes.

Delusive democracies, meaningless means, and consensus cultures

Pluralistic, consensus-seeking cultures help to overcome disciplinary boundaries and permit coupled models to emerge in the first place (Macleod & Nagatsu 2018). Yet, the same cultures can hinder necessary confrontation about which models are actually fit for a given purpose (Hamilton et al. 2022). Again, IAM-based scenario modeling may serve as an illustrative example.

In preparation for major science–policy assessments (IPBES 2019; IPCC 2019), teams behind different IAMs often join forces to explore specific policy-relevant questions under alternative scenarios. For each question and scenario, multiple IAMs run multiple simulations. Projections are then combined as multi-model ensemble envelopes to represent uncertainty. Because structural and process assumptions differ so strongly among IAMs, these differences often exceed those among the scenarios themselves (Alexander et al. 2017). As a result, ensemble envelopes can blur or even invert signal and noise. Lines showing ensemble means but not variances may create a false impression of robust, interpretable differences when overlap across models dominates variation. Effectively, this 'model democracy' (Knutti 2010) thus limits the combined ability of IAMs to do the very thing they are supposed to, i.e., inform policy about implications of *different scenarios*.

This begs the question whether all models should generally get 'a seat at the table' when the goal is to provide policy-makers with *best-available* scientific evidence. Democracy and openness are vital principles for assuring representation of different perspectives and worldviews and the buy-in of policy-makers and civic societies into the assessment processes. However, not all models can be equally informative for any given question. Different questions hinge on specific combinations of processes and scales within the global human–environment system, and existing IAMs represent these combinations with very different structures and levels of detail. Therefore, an egalitarian *model* democracy (one model, one vote; Knutti 2010) can undermine science-policy processes by allowing irrelevant or redundant models to inflate noise and introduce bias (Beisbart & Saam 2019; Knutti 2010). Two steps are critical. First, discuss fitness for purpose openly: which frameworks represent the relevant processes realistically at the relevant scales? Which have demonstrated predictive skill for the variables of interest? Second, select and/or weigh models on principled criteria. Depending on the exercise, sensible weights could be based on demonstrated forecasting ability for specific variables, realism in representing key processes, and mutual non-independence (down-weighting near-duplicates; Beisbart and Saam 2019). Naturally, judgments about models' relevance, redundancy, and other criteria should be embedded in social processes that involve trust, negotiation, and shared practices (Beisbart & Saam 2019). Ideally, such judgments should be made collectively, based on community standards. Without such steps, 'consensus' can be the enemy of adequacy, and the very practice meant to capture uncertainty can end up obscuring where models meaningfully agree or disagree.

The reputation economy of interdisciplinary modeling

Academic life operates as a 'prestige economy' (Merton 1957; Bourdieu 1988; Blackmore and Kandiko 2011; Münch 2014; Fecher et al. 2017). For modelers, regular reputational income mostly derives from publications that offer incremental methodological innovations or apply existing frameworks to new questions. BSICHE modeling typically demands high up-front investments of time, coordination, and infrastructure, reducing short-term productivity (Leahey, Beckman, and Stanko 2017) even if occasionally yielding high-impact payoffs (Chen, Arsenault, and Larivière 2015). Under these conditions, rational-strategic behavior maximizes throughput: once a complex coupled framework is operational, teams preferentially reuse and tweak it instead of investing scarce resources in deeper redesigns that slow publication and jeopardize the next grant cycle.

Funding practices can amplify these dynamics. Funders aim for positive returns in terms of better evidence for policy and robust knowledge. Yet, greater breadth and complexity of BSICHE modeling are compensated neither by longer-lasting grants nor lower pressure to publish and thus trade off with achievable depth in model design and quality assurance. Additionally, selection and evaluation are based on proxies such as past grants, paper counts, and citation numbers – metrics that conflate returns with investments and do not distinguish positive from critical citations (Xu, Ding, and Lin 2022). Preferential attachment can then reinforce dominant frameworks regardless of adequacy, producing oligopolistic markets for models. Because reviewers and panelists have limited capacity to evaluate complex frameworks across multiple disciplines, and because many of the few qualified experts may have overlapping incentives, assuring rigorous assessment of model adequacy is difficult. Documentation is scattered, transparency incomplete, and incentives for careful review are weak relative to incentives for publishing another paper. This creates conditions under which rhetorical moves become effective: authors can present tools as 'best available', point to prior use, or emphasize inherently unknowable uncertainties (e.g., chaotic dynamics; Kellert 1993) to divert attention from avoidable ones (e.g., inadequate training data, skipped validation steps). Reviewers – also under time pressure and thin rewards – may accept these moves (Jiménez & Mesoudi 2019).

Product characteristics and customer behavior further reinforce the problem. Modeling results depreciate quickly. Policy windows (e.g., IPBES work programs; CBD and UNFCCC SBSTTA meetings) are time-bound; missing a deadline can render results irrelevant to the intended process. Academic users face similar time constraints. Given opportunity costs, once a framework is 'good enough' to pass reviewers, additional investments in data quality, validation, or structural redesign may translate into later publication, fewer citations, and reduced competitiveness for subsequent funding. In practice, timeliness is priced higher than adequacy – even when adequacy is essential for Levinsian virtues.

These mechanisms – incentives to maximize reuse, mismeasurement of returns, oligopolistic markets, weak quality control, and pricing of timeliness over adequacy – jointly create problematic economic incentives. They push modelers to prioritize activities that are rewarded quickly (new applications, minor enhancements) over those that improve Levinsian virtues (building data foundations, rigorous validation, deeper structural changes). Substantial improvements in model precision, realism, and generality thus likely depend on changes in how model adequacy is assessed and rewarded.

Urgency and importance

Normative commitments and a pervasive sense of urgency are central to environmental research cultures. It is nearly always 'five minutes before twelve'. We *must* provide answers now – the last big chance to use science to avert even bigger catastrophes (Lamontagne et al. 2019), to put society on track towards sustainability (Halonen et al. 2021), to bend the curve of biodiversity loss (Leclère et al. 2020). Many issues are genuinely urgent: valued components of biodiversity are eroding in many parts of the world or recovering only slowly (IPBES 2019), and some environmental changes accelerate or become less reversible (IPCC Working Group II 2014). Policy and societal transformation processes also move quickly and will not wait for perfect models. Whereas this urgency is real, a constant or exaggerated sense of urgency can foster false pragmatism (Andrews & Farris 1972; Cyders & Smith 2008; Keinan et al. 1987; Rastegary & Landy 1993). We may tell ourselves that what is *most important* is to ensure that major policy decisions are based on some science at all, so, unfortunately, we must use our admittedly inadequate models now, to ensure that things 'at least move in the right direction'. However, this is a psychological trap, as it confounds importance with urgency (Soman et al. 2005). The result is a systematic bias toward short-term applications and incremental tweaks at the expense of quality assurance and deeper innovation.

Realistically, there will still be urgent questions in 2040 and 2050. Arguably, even more important than providing some answers today is providing meaningful answers across many such windows. Tactical deployment of current coupled models in policy contexts remains necessary, but it should be balanced with strategic investments that improve Levinsian virtues over the long run. I propose that a modest reallocation of resources from numerous two-to-three-year projects that apply or marginally extend existing frameworks toward a smaller number of sustained, long-term programs explicitly mandated to support data foundations, systematic validation, and critical structural redesign – including regular external scrutiny and conceptual turnover – would likely yield larger reductions in decision-relevant uncertainty.

Part IV: Creating an enabling environment for fit-for-future interdisciplinary modeling

BSICHE modelers are challenged to confront the grand conservation and sustainability challenges of coming decades with *adequately* precise, realistic, and general models. Parts II-III of this essay imply that, at least from a Levinsian perspective, interdisciplinary modeling is in a deep crisis. Escaping it requires changes in current interdisciplinary modeling cultures, to which each modeler is challenged to contribute. With this final section, I offer some suggestions on how modelers, but also other actors in science, science administration, and science-policy, could help promote such changes, hoping that these may inspire creative further thinking about solutions (see proposals in **Table 1**).

In particular, we need a cultural shift towards making adequate model validation an integral part of any modeling exercise, *particularly* if models are to be used to inform policy, as has already been requested by modeling scholars for decades (Friedman 1953; Mankin et al. 1977; Barlas & Carpenter 1990; Oreskes et al. 1994; Feinstein & Cannon 2003; Fildes & Kourentzes 2011; Beisbart & Saam 2019). To be fair, it may often not be possible to *fully* validate particularly complex BSICHE models such as IAMs, nor to meet validation 'gold standards', such as testing projections made into the future (or other *a-priori* unknowable spaces) against validation data generated after the modeling (thereby assuring that they cannot possibly influence model and validation design; but see, e.g., McCalla and Revoredo (2001) and Hertel et al. (2016) for positive examples). Yet, this does not mean that modelers should get off the hook without validating their models to the extent possible.

For example, individual sub-modules of complex models may be separately validated via ex-post forecasting (Dietrich et al. 2014; Fildes & Kourentzes 2011), i.e., training of models on earlier historical observations and applying them to the forcing variables' (covariates') later historical observations to forecast the focal variables' dynamics into that later period, and then comparing those forecasts against the (previously unexplored) observable dynamics. Until recently, options for such tests were limited as the full historical data time-series were needed for calibration (e.g., to obtain stable demand curves). However, longer and more reliable time-series for land-cover/use, climate, GDP, population, and other key variables from different independent sources – e.g., from sub-national statistics (e.g., Lee et al. 2024) and local reference samples (e.g., Stanimirova et al. 2023), as well as from remote sensing (e.g., Zhang et al. 2023) – make competition for data between calibration and validation less of a limiting factor. For those sub-modules that can be run separately for distinct regions without complete interdependence of their global dynamics, implementing validation tests using out-of-sample and careful block-cross-validation designs avoiding data leakage (Bergmeir & Benítez 2012; Roberts et al. 2017; Stock et al. 2023; Tashman 2000), moreover, allows evaluating how well models can predict into novel socio-environmental conditions – a crucial test before putting any confidence in future projections. Even for global equilibrium-based economic modules, historical dynamic simulation paths towards long-run equilibria can be compared with historical observations (e.g., Chaturvedi et al. 2013; Fujimori et al. 2016), and their behavioral realism can be tested via comparative static analysis (Kriegler et al. 2015), where model responses to exogenous variables such as policies are compared with empirical data and/or theoretical expectations. Importantly, what constitutes meaningful validation necessarily depends on a model's intended use, implying that validation efforts should prioritize those model components and behaviors that matter most for a given application.

Validation standards are emerging in various disciplines (e.g., Jakeman et al. 2006; Tropsha 2010; Eddy et al. 2012; Grimm et al. 2014; Harmel et al. 2014; Araújo et al. 2019; Planque et al. 2022). For example, the International Land Model Benchmarking project (ILAMB; Collier et al. 2018) operationalizes model validation against observational data using standardized, repeatable workflows. Unfortunately, however, such standards remain poorly developed for interdisciplinary modeling of human-environment systems, and proposed standards and workflows are rarely applied (Belete et al. 2017; Bennett et al. 2013; Holzworth et al. 2011; Verburg et al. 2016). This needs to change, as poorly designed tests could do more harm than good by unduly raising our confidence in models. For example, back-casting into earlier years is a too-easy-to-pass test for models intended for future projections (constituting a case of data leakage), as, due to path-dependencies in social-ecological systems, today's training data contain much more information about yesterday than about tomorrow (Simmonds et al. 2013). Experienced modelers should lead the way in mainstreaming best practices by offering training and guidance and leading by example.

Given the systemic hurdles to bringing about such changes, other actors beyond modelers should contribute their parts to creating an enabling environment, including funding organizations, informatics communities, journal editors, and key actors at science-policy interfaces (**Table 1**). For example, one way of supporting the transition would be via sustained financing of globally accessible, cloud-based platforms that offer massive integrated computing and data-storage capacities for model development and testing (Chen et al. 2020). Such infrastructure would allow larger communities to collaboratively develop pipelines for routine tasks, such as validating models and testing underlying theories and assumptions against historical data, thereby reducing costs to the individual modeler (McIntire et al. 2022). Devoted 'housekeeping' grants could help assure that the interdisciplinary modeling frameworks we invest in today will remain powerful for addressing tomorrow's challenges, for instance, by financing the upgrading of functional modules within larger coupled models for better alternatives that have become available, or pipeline automations to periodically update model tests against latest validation data. Funding agencies could also offer targeted support for developing standards for interdisciplinary modeling, validation, and associated metadata. For example, initiatives such as the Community Surface Dynamics Modeling System (CSDMS; Tucker et al. 2022) demonstrate how explicit metadata standards can make modeling assumptions more transparent and comparable. Learned societies and journal editors, in turn, could promote these standards by awarding best practices. Such 'carrots' could be effectively complemented with regulatory 'sticks' by journals and funding agencies, e.g., via successively stricter requirements to adopt new standards. Expanding coalitions such as the Open Modeling Foundation (Barton et al. 2022) could be another key leverage point for mainstreaming higher standards. Similarly, relevant bodies in science-policy interfaces could help by including requirements to meet modeling standards in calls for assessments, and by selecting early adopters as chairs of appointed modeling expert groups.

Many of the proposed actions involve short-term costs for individual actors while producing benefits that are largely collective and long-term. As a result, isolated or early adoption by individual researchers, projects, or institutions is unlikely to be effective and may be disincentivized under current academic reward structures. This collective-action problem will likely contribute to the persistence of current practices even when there is more widespread awareness of their limitations. This is a structural problem, not moral failure. While leadership will be needed from different actors, durable change will require coordination across levels.

The measures I propose here may help promote greater depth in interdisciplinary modeling by incentivizing more careful model design and quality-assurance. Yet, they are not meant as a roadmap but to serve as initial suggestions to spark and fuel broader discussion. A process to develop practical, effective, and equitable solutions and concrete pathways for systemwide changes should involve a broader community of experts and stakeholders, for instance, under chairmanship of one or several learned societies and the Open Modeling Foundation.

Importantly, any solutions to advancing Levinsian virtues in BSICHE modeling will only benefit science and society if it is still feasible, then, to address broad, interdisciplinary questions in the first place. For as long as there are no broader systemic changes, individual modelers taking the suggested steps may be disadvantaged in the academic reward system. Ultimately, modelers can only escape the trade-off between breadth and depth if they can adjust either the speed or the costs of their projects (or both). This will depend as much on the willingness of funding agencies to grant longer funding horizons and more resources for quality-assurance, as on the recognition of promotion and grand-selection committees that doing broad *and* deep science will usually slow personal publishing rates.

Table 1: Recommendations to different actors to improve interdisciplinary model-coupling

Recommendations to modelers and tool developers	Recommendations to actors at leverage points in academia
Early-career modelers (ECRs) • Invest in solid data and theory foundations and proper validation of your models before applying them to any interesting questions. Focus on model quality, not quantity of applications. E.g., as a rule of thumb, spend 2/5 of available time on compiling data, and 1/5 each on model development, application, and validation. • Invest in well-documented FAIR/open code, data, and metadata of your models to enable their reuse as modules in coupled models. • When choosing modeling mentors, look for signs of an emphasis on model quality (e.g., critical reviews, history of model validation). **Individual established modelers, PIs, advisors, and mentors** • Foster a validation culture. Teach ECRs, and lead by example. Widen your own horizon regarding model quality-assurance and reproducibility. Learn from your mentees. • Be humble about your models and know their limits. Focus on further improving their quality, not on maximizing their re-use. Do not promote applications outside their established range of validity. Teach scientific, rather than economic, modeling virtues. • When advising ECRs, do not overemphasize the importance of 'the question' so far that they neglect the adequacy of models and data for answering it. • Before coupling models, formally integrate underlying theoretical statements and test the resulting 'composite' theories for their predictive accuracy/causal validity (depending on targeted uses). Validate the full coupled model, or at least as much of it as possible. • Team up with skilled software developers/computer scientists to develop your modeling frameworks as openly accessible, modular pipelines, to make it easier for you/others to replace/complement individual modules as better alternatives become available. **Inter- & multi-disciplinary modeling communities & consortia** • Aim higher than consensus; challenge and help one another grow. • Develop/promote standards and best practices for validating different classes of (coupled) interdisciplinary models. • Develop/promote metadata standards for documenting different classes of (disciplinary) models to facilitate their sound coupling. • Build catalogs of assumptions behind your fields' models/theories. • Work with your fields' empiricists and theoreticians to (re)test and synthesize evidence supporting/contradicting all those assumptions to establish their domains of validity. • Start building the model classes we will need to inform policies after 2030 *now*. Dare to toss away many features of your current models. **Informatics community:** • To help promote rigor in interdisciplinary model-coupling, develop metadata catalogs of (pre-tested) scientific virtues of existing models and their theoretical assumptions that enable systematic comparisons and fitness-for-purpose assessments by modelers without formal training in all the respective disciplines. • To reduce tradeoffs between rigorous model validation and time for model applications, develop online platforms for automated regular re-testing/validation of models and theoretical assumptions against latest empirical data via pipelines. Think big and be creative. **Scientometrics community:** • To enable adequate evaluations of modeling frameworks by grant reviewers, develop technologies and metrics to quantify negative and positive information exchange (e.g., beyond citation numbers).	**Journal editors** • Remember that 'garbage-in-garbage-out' not only applies to inputs into models, but also to models *as* inputs into scientific studies. • Require that any models proposed or used for specific applications be validated in their capacity to support those. Ask reviewers to evaluate whether models and their input data are *fit-for-purpose*. Ignore authors' responses to reviewer criticism arguing that models/data are 'best available' or have previously been 'successfully'/'widely' used – consider only arguments that support their fitness-for-purpose based on tested model capacities/data qualities. • Publish research that shows rigor in model design and validation. Promote ECRs following best practices (e.g., via special paper slots where validation rigor is weighted more than novelty or impact). • Think beyond novelty in choosing what you accept. Only advancing your (inter)discipline's front (i.e., innovation in modeling, theory, or applications) without securing its hinterland (data, re-testing of old theories) leaves it vulnerable. Publish synthesis and data papers. **Funders of research projects and infrastructures** • Allocate sufficient budgets not only to modeling efforts that address pressing questions and promise rapid results, but also to long-term efforts to fundamentally improve modeling frameworks. Recognize that 'garbage-in-garbage-out' not only applies to inputs into models but also to models as inputs into science-policy processes. • Prioritize funds to address the main drivers of uncertainty in policy-relevant models. Acknowledge that input data are currently more limiting than scope or detail of represented processes. • Grant longer funding horizons and resources for quality-assurance to the interdisciplinary modeling projects selected for funding. • Offer 'housekeeping' grants to support updating of functional modules within larger modeling frameworks for better alternatives. • Invest in open, cloud-based infrastructures and software/pipelines facilitating regular re-testing of models and theoretical assumptions. • Fund development of model-quality and quality-metadata standards. • Require (and fund) rigorous model validation. Ask grant reviewers to look for provided evidence demonstrating past model-validation behavior, rather than prospective promises that you cannot enforce. • Help break preferential-attachment dynamics that favor inadequate models with many accumulated citations by *i)* investing in science metrics of model quality and impact (positive/negative), *ii)* requiring modelers to confront published criticisms of their models, and *iii)* offering guidance on quickly evaluating if criticism is addressed well and if models are among the most adequate ones for a given use case. **Promotion committees and grant review panels** • Acknowledge that pursuing broad *and* deep science generally slows personal publishing rates. • Abandon high citation *numbers* as proxies for models' merits, as those might equally be driven by, e.g., critical citations, frequent model misuses, oligopolistic market structures, first-mover advantages, or large mutual-citation networks of model developers/users. **Leverage points in science-policy (e.g., IPBES/IPCC bureaus)** • Require models cited in policy-reports to be validated and fully transparent (complete source code, input data, and validation data must be FAIR and openly accessible). • Appoint scientists who actively promote transparency, validation, and open modeling practices as chairs or coordinating leads of modeling-heavy chapters in science–policy assessments. • Join/collaborate with the Open Modeling Foundation to help promote common standards and best practices for modeling.

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Conflict of interest

None.

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