Title

Towards robust interdisciplinary modeling of global human-environmental dynamics

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Abstract

Real-world 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 interdisciplinary coupled modeling of human-environment relationships, with a focus on the classical model virtues precision, generality, and realism. I draw examples mainly from integrated assessment models – popular coupled modeling frameworks that are increasingly coupled further with ecological models to address biodiversity questions in the context of broader global-change and sustainability challenges. 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 robust 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

The Global IPBES Assessment (IPBES 2019) has highlighted not only the magnitude of global biodiversity changes, but also biodiversity's interactions with many environmental and societal processes within a highly complex, global human-environment system (or social-ecological system; (Liu et al. 2007)). Within this system, land-use change (LUC) not only constitutes the dominant proximate driver of (terrestrial) biodiversity change (Jaureguiberry et al. 2022), but also a major nexus through which multiple socioeconomic processes indirectly interact with biota and their abiotic environments (IPCC 2019; IPBES 2019). As a limiting resource, land, and how we use it, systemically links biodiversity-related concerns such as avoiding extinctions, restoring ecosystems, or managing invasive pests to manifold other concerns, from food, energy, and water security to climate change, human rights, and economic development (Verburg et al. 2015). Given these systemic interactions and the oftentimes ensuing trade-offs between different sustainable-development goals (SDGs), classical ecological models that link species or assemblages to proximate environmental pressures are no longer sufficient to inform biodiversity policies or management strategies.

Over the past decade, more interdisciplinary models that link biodiversity to 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 be distinguished. 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 biodiversity 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 a particularly effective strategy for rapidly advancing interdisciplinary modeling projects.

However, approaching human-environment dynamics via interdisciplinary model-coupling also entails risks. The purpose of this essay is to raise awareness of several of these risks, and of associated structural problems in current coupled-modeling cultures. As such, this paper is targeted at researchers, science funders, and science-policy brokers who engage with coupled models (or their results) without being intimately familiar with such models, as well as at modelers who themselves do interdisciplinary model-coupling. Although I focus on global-scale coupled models, most of the identified issues should similarly apply to many other interdisciplinary model-coupling efforts. Specifically, I will draw examples mainly from integrated assessment models (IAMs), which has the advantage that 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). 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. Readers interested in technical and epistemic challenges in coupled modeling of human-environment systems may resort to, e.g., Voinov and Shugart (2013), Verburg et al. (2016), Macleod and Nagatsu (2018), Elsawah et al. (2020), Iwanaga et al. (2021), and Farahbakhsh, Bauch, and Anand (2022). Finally, I will give a few suggestions for different stakeholders to help overcome these hurdles, hoping that these may inspire more intensive efforts to develop solutions.

Part I: What are IAMs?

Integrated assessment models (IAMs, Fig. 1) 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. At the cores of most IAMs lie different agroeconomic models of land use. Most IAMs, agroeconomic land-use models, and other types of coupled human-environment 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. 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. 1, 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. 1, pink parts). This makes sense, given that 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. 1). 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), 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 sensible, *given* a particular scenario.

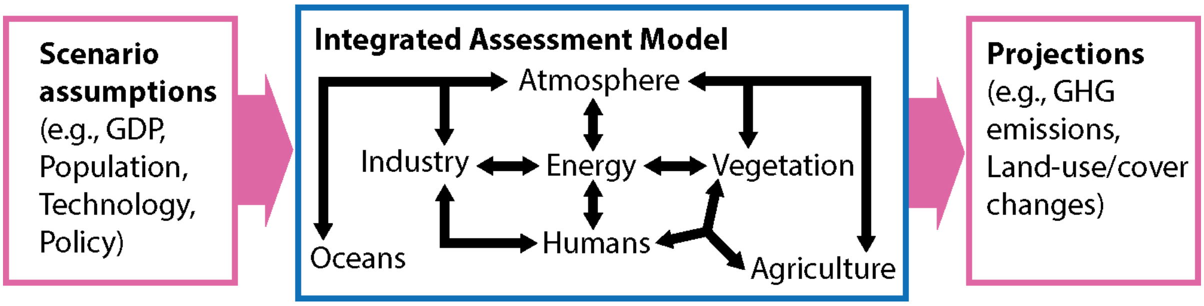

Figure 1. Simplified schematic overview of an Integrated Assessment Model (IAM; adapted from Harfoot et al. (2014)), used in this paper to illustrate different problems in interdisciplinary model-coupling. The scenario inputs to IAMs, and thus by extension, also the future projections that IAMs produce as outputs (both in pink) do not necessarily aim to be accurate. 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, whereas the core agroeconomic models within some IAMs are partial-equilibrium models that 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 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 grid 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). For example, projections of changes in basic land-cover classes made by commonly used global land-use models differ not only in terms of the specific regions affected, but even in direction and orders of magnitudes of the aggregate areal changes at continental to global scales (Alexander et al. 2017). This uncertainty translates into low confidence in future scenario analyses and related systemic insights in the social-ecological dynamics driving biodiversity change, and implies weak scientific support for model-derived management recommendations and proposals for transformative policies for biodiversity conservation and recovery.

<u>Part II: The scientific virtues of interdisciplinary coupled models</u>

Before discussing the 'quality' of interdisciplinary models from a scientific perspective, I shall stress that interdisciplinary modeling can serve many valid purposes beyond those that are strictly scientific, such as supporting policies (Verburg et al. 2016) or ensuring stakeholder participation and buy-in (Moallemi et al. 2021). From a more narrowly scientific perspective, however, interdisciplinary models are no different to disciplinary models in that to be useful given their specific aim(s), they ought to score high in one or several model virtues. Levins (1966) classically identified three main model virtues. Each of these Levinsian virtues vitally depends either on solid data, solid theory, or both. Firstly, ***model precision*** refers to the ability to predict the phenomenon of interest accurately at high detail. Building precise models requires reliable data for model training or parametrization, calibration, and validation. Secondly, ***model realism*** refers to the accurate representation of the variables, structures, and interactions that in the real world determine the system's behavior with respect to the phenomena of interest. Achieving high realism requires reliable *a-priori* information to accurately identify those key system components, in addition to reliable data on those. Except for very small or simple systems where it might be feasible to directly measure all components before modeling , the quality of this prior information will depend on having solid theory (although other models or expert opinion might also help). Finally, ***model generality*** describes the ability to give accurate predictions or explanations in many different situations. Developing generalizable model representations of any complex system, without giving up on the model's precision to an extent that its predictions or explanations become useless, will require very extensive and representative data. Similarly, affirming a model's generality requires extensive and representative data for rigorous testing over multiple spatial and temporal scales and under many different contexts.

The Levinsian virtues (precision, realism, and generality) of a given coupled model are constrained by the virtues of its sub-models, and thus ultimately, by the quality of each sub-models' input data and of its underpinning theoretical assumptions. The old proverb 'garbage in, garbage out' applies to coupled models as much as it does to their individual sub-models. In other words, if the development of a given sub-model's underlying theory had been backed by extensive empirical testing and if that theory's predictions had indeed proven mostly accurate under many different conditions, then we would expect these virtues of the underpinning theory (Keas 2018; Kuhn 1977) to translate into high Levinsian virtues of the respective sub-model (as long as the model accurately represents the theory). And if the same was true for all sub-models, we might expect that also the coupled models be highly virtuous. Conversely, if a given sub-model was trained with non-representative input data or be designed based on largely inaccurate theory, we would expect this to results in biased predictions of both this sub-model and of the larger, coupled model.

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

Most regularly encountered sub-models of interdisciplinary coupled models make one or several theoretical assumptions. Built-in theoretical assumptions not only characterize models in disciplines such as economics that have long epistemic traditions of theoretical modeling, but are also common in models in 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 in and of themselves, provided that those assumptions are indeed valid. However, the theoretical underpinnings of different sub-models of coupled human-environment models may differ tremendously in their empirical evidence base, validity, and generality, reflecting both different disciplinary scrutiny standards and differences in the data and tools that were available for testing the theories at the time of their development (among other factors). Many foundational theories are not subject to *continuous* scrutiny and/or have never been subject to *systematic* scrutiny, such that we simply do not know how accurate their predictions really are. Some theoretical assumptions are so deeply engraved into disciplinary modeling traditions, that they are routinely re-applied despite substantial evidence that they are mostly invalid (e.g., Urbina and Ruiz-Villaverde 2019). In certain cases, making those assumptions may have even made sense for the sub-models' specific original purposes. However, reapplying these models outside of their original design specifications will often push them outside their range of validity (Rykiel 1996). In interdisciplinary model-coupling, many such flawed assumptions may accumulate. For example, a recent exploration of scenarios for global biodiversity recovery based on coupled IAM-biodiversity modeling (Leclère et al. 2020) accumulates problematic assumptions regarding, among other things, rational, strategic 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 underpinning the used climate models), and species being at equilibria with their environments (Dormann 2007; implicit in species-distribution models and other biodiversity-environment models).

While few disciplines have ever produced law-like theories that are so general that their predictions hold across all possible scales and contexts, we do not usually know just how (non-)generalizable different theories really are. Few studies have mapped existing theories into the specific contexts where they presumably apply based on the authors' expert-based system understanding and case-study experience (e.g., Meyfroidt et al. 2018), and even fewer have systematically tested theoretical predictions across different spatiotemporal scales and contexts (e.g., Pacheco & Meyer 2022). 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 principal be valid, is usually either not considered at all during the coupling process or is determined by the modelers' intuition, rather than by any formal criteria or assessments.

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

As with the virtues of underlying theories, the validity of human-environment models will rise and fall with the quality and coverage of the data used for training, calibration, and validation. Generally, globally available data for most environmental and socioeconomic variables are heterogeneous and highly uncertain. Modelers typically rely on disparate biophysical data (mostly from remote sensing) and socioeconomic data (mostly from censuses and statistical surveys) to enable globally contiguous model applications. However, both data types have already passed a stage of abstraction from the *primary* evidence regarding the respective variables' states at specific points in space and time (e.g., from field observations or household surveys), and the primary data often have very limited and unrepresentative coverage (Meyer et al. 2015, 2016; Pengra et al. 2020; Wollburg et al. 2021). With few if any exceptions, all global 'gridded data' that might capture variables of interest contiguously at adequate spatial resolutions are not actually data, but modelled predictions, which nearly always comes with high uncertainties.

To illustrate these points, consider 'gridded data' on current patterns and historical trajectories of basic land-cover/use classes. Such data directly or indirectly inform IAMs as well as many other coupled human-environment models in various way. For instance, remote-sensing-based maps of croplands and other land-cover/use classes are commonly used to set initial conditions for future simulations. However, especially in many tropical regions subject to rapid land-use changes (and thus presumably, rapid biodiversity changes), available datasets vastly disagree on even basic questions such as whether or not there is any cropland in a given pixel (Pérez-Hoyos et al. 2017). Uncertainties tend to be even higher for gridded information on livestock grazing (Fetzel et al. 2017) or forest management (Erb et al. 2017). Similarly, massive uncertainties characterize the historical land-use simulations used as inputs for forward-looking climate models and certain IAMs. Particularly for earlier decades, the land-use models cannot rely on much data for calibration or validation, such that the historical trajectories simulated by different models (e.g., Hyde (Klein Goldewijk et al. 2010), KK10 (Kaplan et al. 2010)) are largely driven by alternative assumptions about land-use change processes. This process uncertainty, in turn, translates into substantial disagreements among different land-use reconstructions in terms of when significant land use started and how much land has recovered since local land-use peaks (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 interdisciplinary models, we can resort to comparisons of agro-economic models of land-use (the 'cores' of many IAMs). Alexander et al. (2017) 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., partial equilibrium vs. cellular automata-based). 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 inputs data and the models' structural assumptions. As projections go farther towards the end of the 21st century, the definition of initial conditions still remains responsible for the bulk of uncertainty in pasture areas, whereas for cropland and forest areas, model type and model cell number, i.e., the spatial resolution at which processes are represented, become the most or next-most important sources of uncertainty.

These results 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 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. 1), 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 the model's underpinning data and theory, and given the apparent limitations in both (see preceding sections), we should expect that low model precision *sensu* Levins (1966) should be the norm. In other words, current interdisciplinary models should mostly fail to accurately predict the properties of interest to conservation and sustainability at the detail needed by policy and management. In fact, the few systematic efforts that validated multiple comparable coupled models using rigorous validation protocols largely confirm this expectation (see McCalla and Revoredo (2001) and Hertel et al. (2016) for ex-post validations of the land-use change projections made earlier by various agro-economic models).

Unfortunately, however, we cannot know this with certainty, because we do not have a model-validation culture. A recent survey of 10,739 modeling studies related to resource management 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 coupled interdisciplinary models and many of their sub-models were never formally validated against independent data (Gomes et al. 2021; Sivagurunathan et al. 2022). For example, uncertainties in scenario-specific projections of Integrated Assessment Models (IAMs) are commonly assessed via intercomparisons across different IAMs (Wilson et al. 2021). Yet, there have been few formal validations of the models' abilities to simulate sensible trajectories *given* a particular scenario via historical simulations (although that was more prominent in early IAM evaluation practice), and those few were very limited in spatial and temporal scope (Wilson et al. 2021), considering that IAMs are regularly applied for global, long-term projections.

How general could our interdisciplinary models be?

For obvious reasons then, we also do not know much about the generality of coupled interdisciplinary models. When building such models for broad-scale applications such as global scenario-modeling, however, we implicitly assume very high generality, as the models will make predictions into many different specific socio-environmental contexts, which at least on average will need to be sufficiently accurate and precise for the broader modelling results to be useful. But how general should we expect our coupled models of complex human-natural systems to be?

To explore this question, let us entertain a hypothetical scenario in which we wish to predict the dynamics of some random variables of interest that interact within a larger, complex system. For the sake of the argument, let us further assume that we have already managed to correctly identify the most important variables and causal interactions that are really the primary determinants of our focal variable's dynamics, and that our model structures moreover adequately represent those interactions, and also that our training data accurately and representatively capture each variable. In complex systems, even important patterns and processes are rarely universal but usually more or less constrained by some contextual factors (Lawton 1999; Meyfroidt et al. 2018). Thus, even if we assume that all our 'most important' relationships are *relatively* general, there will still be variation 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 relevant pair of a causal 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, with specific local strengths of any effects depending on the local exposures and sensitivities (**Fig. 2A**). Such simple bivariate causal relationships will be highly general if these conditions are given in many locations i.

Things are much more complicated, however, for causal relationships that are composed of multiple interactions among random variables, either because they are indirect (e.g., $X_i \rightarrow Y_i \rightarrow Z_i$) or because they only work if multiple causal variables interact (e.g., $[X_i \leftrightarrow Y_i] \rightarrow Z_i$). For such more complex relationships to be general, it is not only necessary that for each of the constituent interactions (e.g., $X_i \rightarrow Y_i$ and $Y_i \rightarrow Z_i$, or $X_i \leftrightarrow Y_i$ and $[*] \rightarrow Z_i$), the above-described exposure and sensitivity conditions are given at many locations, but additionally, that the *specific* locations i where they are given are mostly the same across all interactions (**Fig. 2B**). 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 causal relationships will typically be less general and weaker than the more direct interactions of which they are composed (**Fig. 2**).

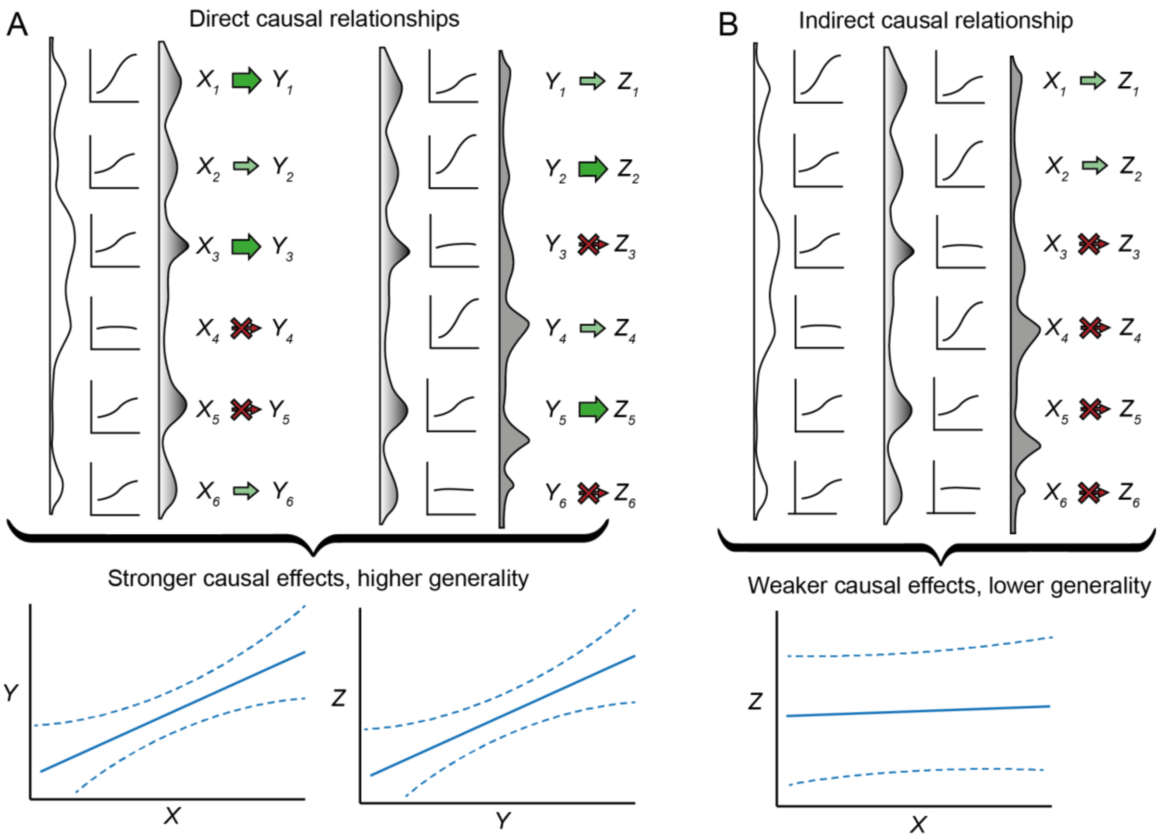

Figure 2. Conceptual graphic showing how natural variation in exposures and sensitivities of outcome variables to causal variables constraints the generality and strength of indirect causal relationships.

Complex human-environment systems are characterized by many indirect and multivariate (combinatory) causal relationships (Cilliers et al. 2013; Meyfroidt 2015), which is the very reason why we try to couple multiple models to study these systems in the first place. Within the global human-environment system, many patterns and relationships thus contribute to driving our focal variables' dynamics (e.g., the change in some biodiversity metric). Many of these patterns and relationships might individually be highly general, and indeed expressed very well in models that can make decent predictions under most circumstances (e.g., consider Engel's law of income-dependent food expenditures (Houthakker 1957; Seale & Regmi 2006) or the species-area relationship (Lomolino 2000); see further examples in Currie (2019)). However, for the above reasons, we should expect that even major social-ecological processes (e.g. the indirect effect of rising per-capita incomes on biodiversity) will be substantially weaker and less general than their constituent processes within the ecological, economic, or other sub-systems of the global human-natural system.

Paradoxically, even if we were to stitch together multiple models that are all individually fairly general, the 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 or not this is true for all coupled models is ultimately an empirical question, but I argue that this is a sensible null expectation. The burden of proof whether models can make accurate predictions certainly lies on those who develop and use coupled models for applications that depend on the models' predictive capacities. To demonstrate that our coupled models work, we will need to go beyond ascertaining that each sub-model is valid (Belete et al. 2017), to design validation studies that allow us to compare the predictions across our coupled models' complete mathematical statements against empirical data.

Are we pushing for greater realism?

Improving realism is a main motivation for developing interdisciplinary models. Given the multivariate complexity of real-world human-environment systems, this is often taken as equivalent to representing more system components, leading to ever more complex models (e.g., Motesharrei et al. 2016). The implicit assumption, here, is that representing more of the relevant factors will lead to more realistic model structures (Schlueter et al. 2012), which will show more realistic model behaviors, especially under new conditions, and thus, allow us to make more accurate predictions. Unfortunately, studies that actually tested whether predictions of more complex models are indeed more consistent with the real world found that models actually tend to perform worse as they become more complex (McCalla & Revoredo 2001).

Many predictive modelers might indeed expect this due to the bias-variance trade-off (Hastie et al. 2017), the conceptual problem that as more important parameters are incorporated into a model, systemic bias is reduced but at the same time, variance due to random measurement error increases, leading to lowest levels of overall model inaccuracy at some intermediate level of complexity (Fig. 3A). O'Neill (1973) further noted a role of non-random measurement gaps and errors and similarly argued for intermediate levels of model complexity, noting that representing more processes may increase total model inaccuracy unless those processes are essential, well understood, and reliably estimated (Fig. 3A). In the real world, severe data biases characterize even globally comprehensive databases and already hamper unbiased model representations of comparatively well-documented processes that we routinely include in models (see earlier sections). Thus, already as we exceed fairly small levels of model complexity, more and more of our added processes may be (ill)parametrized given fewer and fewer and less and less representative data. Adding processes that are entirely mis-measured should increase, rather than decrease systemic bias. We thus face a trade-off between increasing model realism by adding relevant system components, and quickly losing realism again by completely misrepresenting many of them (Fig. 3B). This trade-off is consistent with the earlier-made observation that *data* are the most critical limiting factor for reducing model uncertainties.

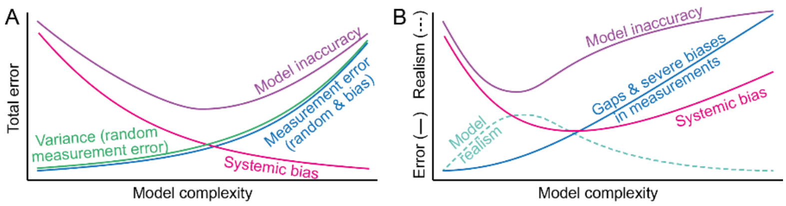

Figure 3. Conceptual relationships between model complexity and realism. A) Adding important *and* well-measured system components to overly simplistic models will reduce systemic bias and thus increase realism of the models' representations of the real-world systems. Yet, modelers have variously noted that total model error may be lowest at intermediate levels of model complexity due to the bias-variance trade-off (Hastie et al. 2017) and to an increasing influence of non-random measurement errors (O'Neill's (1973); graphical representation adapted from Turner and Gardner's (2015)). B) In real-world global-scale modelling of human-environment systems, however, we should arguably expect that adding more processes will only decrease systemic bias in selected cases, as severe gaps and biases in most global data bases will cause substantial parametrization errors and indeed new systemic bias, such that highest model realism may be achieved at much lower complexity levels than typically presumed.

How are we prioritizing our efforts to add complexity?

This trade-off implies that, in order to improve model realism, we ought to be very strategic in choosing the specific layers of complexity that we add to our models. Which bears the question: how do we currently prioritize our decisions to make models more complex?

Again, activities to couple agroeconomic land-use models with biodiversity models may serve as an illustrative example. How realistically we model future patterns of agricultural land use expansion and intensification is particularly critical for the utility of the coupled models for guiding biodiversity policy, given that *i)* land-use change is the biggest cause of biodiversity loss (IPBES 2019), *ii)* the two alternative agricultural growth paths imply different impacts on biodiversity (i.e., expansion mainly via habitat conversion vs. intensification via habitat alteration/pollution; Zabel et al. 2019), and *iii)* biodiversity is distributed heterogeneously across current and potential future agricultural production areas (Delzeit et al. 2016; Kehoe et al. 2015). Agroeconomic models have long been criticized for systematically excluding environmental factors (Tietenberg & Lewis 2018), and recent attempts to incorporate both climate change and changes in the provision of certain ecosystem services such as pollination indeed showed that those factors can have tremendous impacts on modelled agricultural outcomes (Johnson et al. 2021). However, in order to demonstrate that also *biodiversity* loss can cause losses in agroeconomic productivity or production stability via feedbacks into ecosystem services, arguments are prominently made to also include biodiversity into economic models (Dasgupta 2021; TEEB 2018). Yet, available evidence does not support that specifically the *diversity* of natural biota is *key* for maintaining productivity (i.e., effect sizes are inconsistent and mostly small, relative to other 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 quantitatively determined a suite of *economic* priority factors that really do need better model representation if we are to more realistically project agricultural patterns (e.g., labor inputs, capital inputs, total factor productivity growth; Hertel et al. 2016). Yet, those economic factors are largely absent from discussions of how to improve models for applications in biodiversity policy (Akçakaya et al. 2016).

This example illustrates that our interdisciplinary model 'enhancements' may often be motivated more by our wish to better represent our *systems of interest* than by our insights on how we could more realistically model real-world, *ontological systems*. From a perspective of better informing policy by reducing uncertainty in projections, we might not need to include our favorite processes into coupled models, but we may have to do so if our goal is to actively shape global policy agendas. The latter is not a problem *per se*. However, we should be very clear whether we strive to inform or to shape when we promote additions of our pet processes to already-complex models, especially if evidence suggests that other processes are likely *more* important (ontologically speaking) for the modelled systems' dynamics, and acknowledge that those efforts then may have little to do with improving model realism.

Part III: Cultural and systemic hurdles to advancing Levinsian virtues

Challenges in interdisciplinary modeling of human-environment systems are most commonly discussed with a focus on either technical hurdles in modeling complex systems (e.g., Schlueter et al. 2012; Verburg et al. 2016; Voinov and Shugart 2013), or on intellectual or epistemic hurdles to effective interdisciplinary exchange (Macleod & Nagatsu 2018). Here, I want to contemplate on cultural and systemic hurdles to advancing the Levinsian virtues of interdisciplinary coupled models. Motivations of interdisciplinary modelers' for developing and applying such models are rarely restricted to scientific rationales for adequately representing their systems of interest or promoting Levinsian virtues. Depending on personal aptitudes and circumstances, most interdisciplinary modelers of human-environment interactions additionally strive to *a)* help improve the world by collaboratively addressing pressing problems and/or *b)* succeed in the academic reward system. Notably, these cultural-normative and economic motivations for interdisciplinary modeling are largely orthogonal to promoting Levinsian virtues. In the following paragraphs, I argue that they can even directly counteract Levinsian virtues by inhibiting critical reflections on the limitations of current models, causing excessive path-dependencies in dominant modeling frameworks, inhibiting model diversification, and disincentivizing deep changes.

Delusive democracies, meaningless means, and consensus cultures

A first hurdle to promoting Levinsian virtues lies in the pluralistic, consensus-seeking culture of most interdisciplinary modeling efforts, which tends to avoid deeper epistemic conflicts. Whereas this culture helps in overcoming disciplinary boundaries and allowing coupled models to emerge in the first place

(Macleod & Nagatsu 2018), it hinders more confrontational but ultimately necessary activities such as determining which of the different team members' models are actually fit for a given modeling purpose (Hamilton et al. 2022). Again, IAM-based scenario modeling may serve as an illustrative example.

In preparation of major science-policy assessment reports (IPBES 2019; IPCC 2019), the modeling teams behind different IAMs commonly join forces to explore some set of specific policy-relevant questions under different future scenarios. For each question and each scenario, the contributing IAMs make multiple simulations and the combined projections across all IAMs are then commonly presented as multiple-model ensemble envelopes to capture systemic uncertainties associated with the choice of any one model. Since different assumptions and process representations of the IAMs' core models cause greater uncertainty in their projections than the scenarios explored (Alexander et al. 2017), it is not surprising that the overlap between the scenarios' ensembles is often greater than their differences (although plotted lines of envelop means may give a false illusion of clear, interpretable differences). 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, different policy questions are clearly related to very specific combinations of sub-systems and process interactions within the global human-environment system, so the adequacy of a model's answer to any particular question naturally depends on how well it can represent that question's specific combination. As IAMs make very different assumptions and represent specific processes at different levels of detail, all IAMs cannot possibly be equally helpful for addressing any one question. Therefore, an egalitarian *model* democracy (*i.e.*, one model, one vote) can in fact hurt science-policy processes, by allowing irrelevant or redundant models to inflate noise and potentially introduce severe bias (Beisbart & Saam 2019; Knutti 2010).

Open discussion about the fitness-for-purpose of different modeling frameworks and selection and/or weighting on sensible criteria are both critical steps in interdisciplinary modeling, especially if the modeling results are to be used to inform policies. For example, depending on the specific purpose of a scenario modeling exercises, a sensible re-weighting of model ensembles might be based on their tested forecasting ability (predictive accuracy) for specific variables of interest, their realism of representing key processes of interest, and/or their mutual non-independence (downweighing redundant models; Beisbart and Saam 2019).

The reputation economy of interdisciplinary modeling

It would be foolish to expect that such discussions about the fitness-for-purpose of different models will always go smoothly. Not because of any epistemic or normative disagreements between modelers, but because of vested economic interests.

The academic reward system can be described as a 'prestige economy' *(Merton 1957; Bourdieu 1988; Blackmore and Kandiko 2011; Münch 2014; Fecher et al. 2017)*. For most modelers, the most regular reputational income source is publications that either present comparatively small methodological innovations over, or novel applications of, some earlier modeling framework. To persist and thrive in the prestige economy, modelers (just like other scientists) need to manage to either accumulate high reputational capital (citations, papers, grant money, etc.) and/or be able to generate high reputational returns per invested *own* resources (time, acquired funds), both of which select for rational-strategic behavior (cf. *'Homo Academicus'*, Bourdieu 1988). Note that I am aware of the irony that, after I earlier lamented on poorly supported model assumptions, I now adopt a particularly notorious one myself (*'Homo Economicus'*, Urbina and Ruiz-Villaverde 2019). Compared to disciplinary modeling, interdisciplinary modeling tends to require higher time investments and thus reduces one's regular reputational income (publishing productivity; Leahey, Beckman, and Stanko 2017), but increases the potential for occasionally very high income (highly-cited, field-advancing papers; Chen, Arsenault, and Larivière 2015).

An application-ready, global human-environment model may be most comparable to a large, operational diamond mine. Both can produce highly valued products that go through independent, third-party quality-assessments before being offered to customers. Both exist within oligopolistic markets characterized by a dominance of few players (e.g., mining companies, IAM teams) and high entrance barriers. Both need to raise venture capital from investors for their initial development, who consider them high-risk investments and judge risks vs. potential rewards using indicators of the applicants' past performance (among others). Finally, both usually become operational only after long periods of repeated, large investments. Just as developing (buying) mines only makes economic sense if those can later be operated highly profitably over extended periods, it makes economic sense for modeling consortia to frequently reuse their modeling frameworks, once those work, to generate profits. So much on their similarities. Key reasons for why the diamond economy is arguably much more effective than the interdisciplinary modeling industry, however, lie in their differences in expected time horizons for success, in how investments are guided by past success measures, how product quality is controlled, and how prices are determined.

Firstly, the investors, developers, and operators involved in diamond mining generally acknowledge that this is a complex endeavor that requires patience by all parties involved before any rewards can be expected. By contrast, the greater conceptual breadth and structural complexity of interdisciplinary modeling (relative to disciplinary modeling), is not usually compensated by bigger, longer-lasting grants nor by lower pressure on modelers to publish novel results. Given fixed resources, the models' larger breadths trade off with their achievable depths in terms of careful model design and quality assurance. Expressed somewhat cynically, we address questions spanning across four or five sub-systems of the global human-environment system, by hastily stitching together whatever tools we can readily attain from ecology, physical geography, land-use science, and economics, to provide answers that are *feasible* within two to three years, publish those answers for reputational income, and quickly move on to the next questions. Or, if we find a pre-existing coupled model, we may make some minor adjustments to make it usable for our purpose and even address multiple different questions over the course of a PhD or grant project.

A second difference is that the goals and success metrics of diamond companies and investors are closely aligned. Investors are primarily concerned with *their* monetary returns on *their* invested capital (ROIC), which strongly depends on *the company's* ROIC (the company leadership's primary concern), and investors use direct measures of both to compare past and expected future performance among multiple companies. This contrasts the poor alignment of scientists' and research funders' goals and success metrics. Funders want researchers to generate high returns on (*the funders'*) investments in terms of positively impactful information transactions (e.g., guiding sensible policies, advancing knowledge), whereas researchers seek reputational income. Instead of measuring return-*on*-investments, funders measure returns *and* investments (i.e., prior grant successes), thereby risking sunk-cost fallacies. They also measure returns in *total* rather than *positively impactful* transactions (e.g., not distinguishing positive from critical citations; Xu, Ding, and Lin 2022), comparable to investors adding as opposed to subtracting a company's total liabilities from its total assets to determine its net worth. Finally, funders barely assess a model's past and potential future adequacy *relative* to alternatives. Instead, they implicitly assume that competition for grants and customers (other researchers or policy-makers) stimulates innovation and ensures that those prevail who most effectively produce and disseminate the highest-quality information. A flaw in this assumption, at least in the case of interdisciplinary models, is that here, model competitiveness is largely disconnected from model adequacy due to both the customers' limited capacity to evaluate different products according to their preferences (see below) and to oligopolistic market structures. In fact, science funders' preference to invest in previously 'proven' (i.e., well-cited and well-funded) modelers and models further reinforces the dominant market positions of a few modeling frameworks and incentivizes the modelers' further investment in their models' *competitiveness* rather than *adequacy*.

A third key difference lies in the quality-control system. Diamond samples are objectively evaluated through the paid services of specialized, independent laboratories. Peer reviewers, by contrast, often lack the specialist expertise to detect flaws in complex models or their applications, and the models are often intransparent or their documentation scattered and hardly comparable. The complexity of certain model classes such as IAMs also means that the independence of third-party evaluators cannot always be assured, as the only people qualified to critically comment on any model specifics have personal incentives not to criticize these models on any *fundamental* grounds, as their reputational incomes, too, depend on the same general model classes. Moreover, as the reputational rewards of reviewing are miniscule compared to publishing papers, reviewers may unconsciously take the evolutionary shortcut of trusting the pre-selections by earlier reviewers (Jiménez & Mesoudi 2019). In fact, many scientific authors have experienced that – unless a new tool is a central, innovative element of a paper – re-applying previously published tools will make it easier to pass reviewers than using newly developed tools that they need to describe 'on the side', even when the latter are more adequate. Authors can fairly easily avoid uncomfortable but critical question of whether the applied tools are fit-for-purpose, by stressing how they are 'the best currently available'. Similarly, authors may proactively acknowledge *inevitable* or *unquantifiable* uncertainties (e.g., inherently unpredictable dynamics in complex dynamical systems; Kellert 1993) to distract from other uncertainties that were *avoidable* or have been *ignored* (e.g., use of inadequate training data, failure to take time-consuming but feasible validation steps). Although such tricks technically constitute poor scientific practice, the reputational risks are largely outweighed by the rewards from additional papers and citations.

A final, fundamental difference lies in how customers price quality vs. timeliness of the products. Unlike diamonds, modeling results tend to rapidly depreciate in value. Policy and corporate domains are used to rapid information turnover. Time windows for modelers to inform major science-policy processes such as the IPBES work program or the CBD's and UNFCCC's SBSTTA meetings are tight. Missing deadlines means that our modeling results will not be considered. Our customers in academia are similarly impatient. Given pressure to publish and attract funds, most of them would rather cite available modeling results now to support arguments in their papers or grant proposals, than postpone their submissions until they find better references. Once our models are good enough to get past reviewers, any additional investments in their quality thus entail potentially large opportunity costs. Costs in the form that our results are too late for policy processes, are judged as less impactful by editors, are published later and in lower-ranking journals, generate fewer citations.

Urgency and importance

Of course, it is not *solely* for economic reasons that we prioritize new applications or minor enhancements of our models over their validation or any fundamental restructuring. Other reasons are normative and psychological. Most if not all environmental-change-related scientific fields have deep normative underpinnings and cultivate a strong sense of urgency for using science to address pressing environmental problems. Essentially, it is 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). Certainly, many issues are genuinely urgent. Highly valued components of biodiversity are eroding in many parts of the world or are not recovering as fast as we might like (IPBES 2019), while certain undesired environmental changes are intensifying exponentially or showing signs of becoming less reversible (IPCC Working Group II 2014). Plus, many important policy and societal-transformation processes are moving fast and will not wait patiently until we resolved outstanding modeling issues, let alone until we developed completely new models. However, a constant or exaggerated sense of urgency commonly leads to false pragmatism and poor decisions (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 logical trap, as it confounds importance with urgency. It is psychologically understandable that we prioritize based on the latter (Soman et al. 2005). Yet, realistically, it will still be five minutes before twelve in 2030, in 2040, and in 2050. The specific pressing questions will differ, but the fact that they call for urgent answers will not.

Arguably, what is even more important than answering urgent questions today is whether or not we give meaningful answers to all kinds of urgent questions during the next several decades. This does not imply that we should stop using current coupled models to inform pressing policy processes. We should, however, find a better balance between such tactical efforts and more strategic, longer-term efforts to generally improve our support. I propose that one quarter of the research funds currently spent on two- or three-year projects that apply or marginally improve *best-available* coupled models would be better applied if invested in a handful of long-term programs that seek to develop fundamentally new frameworks that are actually *fit-for-purpose*.

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

Interdisciplinary modelers are challenged to support the grand conservation and sustainable-development challenges of coming decades with *adequately* precise, realistic, and general models. Parts 2-4 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**).

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 (Barlas & Carpenter 1990; Beisbart & Saam 2019; Feinstein & Cannon 2003; Fildes & Kourentzes 2011; Friedman 1953; Mankin et al. 1977; Oreskes et al. 1994). To be fair, it may often not be possible to meet validation 'gold standards', such as testing projections made into the future (or other *a-priori* unknowable spaces) against validation data generated only long after the modeling and thereby assuring that they could not 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. Ever-expanding spatiotemporal scopes of historical data time-series make it increasingly feasible to use quite rigorous alternatives. For example, any IAM modeling team could arguably do ex-post forecasting (Dietrich et al. 2014; Fildes & Kourentzes 2011), i.e., train their models on earlier historical observations and apply them to the forcing variables' (covariates') later historical observations to forecast the focal variables' dynamics into that later period, and then compare those forecasts against the (previously unexplored) observable dynamics. Implementing such tests using out-of-sample and block-cross-validation designs (Bergmeir & Benítez 2012; Roberts et al. 2017; Tashman 2000), moreover, allows evaluating how well models can predict into novel socio-environmental conditions – a crucial test before putting any confidence in future projection. Validation standards are emerging in various disciplines (e.g., (Araújo et al. 2019; Eddy et al. 2012; Grimm et al. 2014; Harmel et al. 2014; Jakeman et al. 2006; Planque et al. 2022; Tropsha 2010), but unfortunately, 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, 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 modeling experts 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, while learned societies and journal editors could promote these 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. 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.

While these and other measures may help promote greater depth in interdisciplinary modeling by incentivizing more careful model design and quality-assurance, they will only benefit science and society if it is still feasible, then, to address broad, interdisciplinary questions in the first place. 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 and Scientometrics communities:** • 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. • 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 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 early adopters of high modeling standards as chairs of expert groups leading modeling chapters in science-policy assessments.

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Conflict of interest

None.

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