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# In preparation for future and extreme situations: Orientations for affirmed conjectural research on social and ecological systems

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**Abstract**

Over the last three decades, under pressure of concern about global change, studies about possible futures of social and ecological systems have rapidly developed. Some of them have reached an impressive level of ambition and impact on policy debates. However, only a limited number of research fields have yet embarked on such studies, whereas many more should become involved. Development of such research is also limited by the fact that it is still considered almost exclusively as a matter of collective assessment, at the interface between academia and policy making, rather as the fundamental scientific endeavour it is really. To push further, a number of stumbling blocks have still to be overcome until the conjectural nature of any research about future events and processes is fully accepted and scientists widen their repertoire of approaches for the study of possible futures. Such a study is necessary to prepare both for long-term transformations of the environment and for extreme events: beyond some significant differences, both domains share the fundamental traits that make the study of futures such a peculiar and challenging endeavour. In this effort, the resources provided by the futures studies field could be more thoroughly mobilized. ***To cite this article: L. Mermet, C. R. Geoscience 340 (2008).***


**Résumé**

**Se préparer à des situations futures et des événements extrêmes : investir dans les recherches conjecturales sur les socio-écosystèmes.** Au cours des trois dernières décennies, les recherches sur les futurs possibles des systèmes sociaux et écologiques se sont développées rapidement, notamment sous la pression de la demande sociale liée aux changements globaux. Certains des travaux ont atteint un niveau remarquable d'ambition et d'impact sur les débats de politique publique. Cependant, seul un nombre limité de domaines de recherche ont réellement investi dans de telles études, alors que d'autres domaines, tout aussi nécessaires pour traiter des problèmes posés, restent à l'écart. Le développement de ce type de recherches est également entravé par le fait qu'elles sont encore trop souvent considérées comme relevant exclusivement de l'expertise collective, à l'interface entre science et politique, plutôt que comme un travail de recherche scientifique important par lui-même. Un certain nombre d'obstacles devront encore être surmontés pour que les recherches conjecturales sur des événements et des processus futurs soient pleinement acceptées, et que les scientifiques élargissent le répertoire des approches qu'ils mobilisent pour l'étude des futurs possibles. De telles études sont nécessaires à la fois pour se préparer aux transformations à long terme de l'environnement et aux événements extrêmes. Malgré des différences significatives, ces deux domaines partagent nombre des traits fondamentaux qui font de l'étude de

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situations futures possibles un champ de recherche si particulier et si délicat. Pour le développement de ces travaux, on devrait mobiliser plus et mieux qu'aujourd'hui les ressources théoriques et méthodologiques offertes par les travaux du domaine de la prospective. ***Pour citer cet article : L. Mermet, C. R. Geoscience 340 (2008).***


*Keywords:* Futures studies; Ecosystems; Scenarios; Modelling; Integrated assessment

*Mots clés :* Prospective ; Écosystèmes ; Scénarios ; Modélisation ; Évaluation intégrée

## 1. Introduction

Over the last two decades, ecology has come to underline the importance of extreme events for the structuring and functioning of ecosystems. In doing so, it has joined other disciplines – such as climate research, hydrology, history, etc. – in a shared effort to understand extreme events of the past and the present as well as their consequences. This growing awareness and understanding of extreme events, and of the climatic, ecological, technological, geopolitical transformation of socio-ecosystems over time, is closely connected with the current and urgent social and academic debate about upcoming global change. In the light of what we understand of past extreme events, preparing for future ones appears as a moral and political obligation. How should, and how can research contribute to the effort? Studying past and present extreme events is certainly an appropriate, necessary, response. But is it sufficient? We will plead here that understanding past and present events, and from them, drawing pragmatic lessons for the future, is not enough. It is necessary also to study and debate in depth, in an academic context, possible future events. Some recent large-scale and high-impact achievements – such as the Intergovernmental Panel on Climate Change (IPCC, see in particular IPCC [16]) or Millennium Ecosystem Assessment (MEA, see in particular [6]) reports – indicate that this challenge is starting to be picked up at a large scale. Analysing them, however, also reveals some of the main challenges that still limit academic investment in the study of social and ecological futures.

This is hardly surprising, since moving from the study of past and present ecological and social situations to the study of future ones, that do not yet exist, raises major difficulties. Such difficulties are compounded, in the case of extreme events, by the added complications of extremely uncertain time scales and of processes that extend beyond the boundaries of the well-studied, the repetitive, the reasonably forecastable. These difficulties should not be used to feed reticence against research on future situations and systems. On the contrary, they need to be actively identified, researched, discussed; inventing responses, not excuses, should be on the agenda. In this effort, we believe that the often overlooked field of futures studies[1] can be called upon more actively than is already the case, to provide useful resources to academic communities for whom the future is still a very unfamiliar dimension.

Based on a recently completed research project and book [25] and on a new starting one (Coreau and Mermet, in prep.) [9], the paper will first propose a brief overview of the state of the art – which relies mostly on the 'integrated assessment' model of studying future situations – and of its limits. Since these limits mostly stem from the direct focus of such work on policy-making application, the paper will then re-examine the challenges – both academic and political – involved in developing more fundamental studies of possible future social and ecological situations and systems. To take up such challenges, it will be necessary to insist that we need a new perspective on the scope and purpose of such studies. Ecological futures research should encourage investigation of possible future social and ecological systems that are both more profound academically and less directly dependent on the demand for expertise and assessment from policy-making communities. This perspective sheds a new light on the way ahead in the study of future ecological situations. In this paper, we will not propose a specific new approach. We will rather propose a general orientation that may be useful to stimulate a diversity of new approaches the current situation calls for. We will show that the current formula for integrated assessment relies on an opening up and widening from the narrowest uses of predictive modelling, a process that delivers some remarkable results today, but that will have to be pursued further. In particular, we will stress the need to develop specific conceptual bases for the conjectural uses of models, and to rebalance the attention given respectively to models and to narrative scenarios. We will also suggest that a

[1] And also of the foresight approach, which is currently dominant within futures studies.

more intensive use of the concepts and intellectual tools provided by futures studies could be very useful in this respect – but that such an effort will also be a major challenge for futures studies themselves. As a conclusion, we will show that preparation for extreme events and preparation for long-term evolutions of ecology and society, beyond some significant differences, have much in common.

## 2. Integrated assessment: the state-of-the-art formula and its limits

Research on ecological systems is based on straightforward operations: describing, measuring, experimenting, modelling, validating models against new measurements and experiments. When the object of study is a situation in the future, these operations are jeopardized. Only descriptions resting on imagination and modelling simulation are possible, but without the ability to validate the models empirically. It is hard to conceive a more challenging situation for scientists. However, the need to study ecological futures is so pressing that the challenge has been repeatedly met over the last four decades. From the early efforts of the Club of Rome's 'Limits to growth' exercise in the 1970s [20] to Clark and Munn's programmatic book in the 1980s [8], from the image models since the 1990s [19] to the IPCC and MEA reports in the years 2000 (see references above), major efforts, with high impact, have been made, each one taking up in its own terms the needs and the inputs of scientific and public debate in its time. Over these four decades, the evolution of modelling concepts and technology, as well as of the knowledge and data on Earth systems, show very clearly the design, ambition and complexity of the models and the foresight exercises they are nested in. What is less visible is the constant crossover that has been going on, from the very start, between the field of modelling, and the field of futures studies (FS). The latter has produced, over the last four decades, a whole literature on issues, methods and projects relating to the study of social, technological, political, ecological futures[2]. The best-known outcome of this collective effort is the range of scenario methodologies of which some have found their way into the study of future ecologies where they play a central role (such as in the IPCC reports or the MEA). This crossover has rested on participation, in all the major exercises mentioned above and in many others, of a limited number of experts and independent thinkers from the FS field. This participation has been instrumental in the results of such exercises. One obvious example is the way the scenarios developed by the Global Scenario Group (GSG) [12], a panel run by long-time experts in futures studies, have directly and profoundly influenced the IPCC and MEA scenarios. Over the decades, the issues and the methods of this participation has partly been made explicit. Examples are the proposals for policy exercises in the late 1980s [4,33], for policy dialogues in the 1990s [2], and more recently, the 'story-and-simulation' approach that partly clarifies the current state of the art [1]. However, much of the crossover has remained informal, resting on the participation of a few atypical experts of futures studies in each large project for modelling future ecological and sustainable development issues.

The major current international exercises (IPCC, MEA) reflect a consensus, partly explicit, partly implicit, on the overall aims, design and quality criteria of such endeavours. This 'formula' is best captured by the phrase 'Integrated Assessment' (IA), often specified as 'Integrated Assessment Modelling' – a concept that has been gradually elaborated over the last 15 years [17,31]. It can be roughly summarized in the following way. The one legitimate aim of studying future situations would be to provide decision-makers and the public with information, knowledge and guidance that they require when decisions about the future cannot (or should not) be postponed, despite insufficient knowledge. The phrase 'integrated assessment' reflects the quality criteria for such exercises: they should allow assessment of the situation under study, based on the best available scientific knowledge, from diverse fields relevant to the situation (climatology, ecology, economy, sociology, etc.), and on in-depth discussion both between scientists themselves and between scientists, decision-makers and the public concerned by the situation and the decisions to be taken in preparation. This definition of purpose and scope is followed by a train of methodological tools and priorities. Modelling is considered the centre of the effort to synthesize scientific understanding and knowledge. Scenarios are to be used as a complement to introduce both aspects that are difficult to model, and differences in overall socio-political perceptions and framing of the situation and its issues. Participation is to be ensured both by 'hybrid' panels of scientists and decision makers, and by participatory exercises with the public. This (recently but firmly) established pattern has also its counterparts at lower spatial scales, for instance with local integrated participatory projects based on modelling – for instance, on multi-agent simulation models.

[2] For an overview of the field, see reports of the millenium project [13]. About its relevance for the study of ecological futures, see [24].

Such exercises as the IPCC reports and the pressure to emulate them, for instance in the MEA, bear witness to the success of the integrated assessment formula. But they also make its limits quite tangible.

They show a clear imbalance between the three pillars of IA. Modelling – along with synthesis of the literature – clearly consumes the major part of research efforts. Scenarios, although they are well in evidence when communicating results, do not by far benefit from a comparable level of attention, elaboration, and critical discussion[3]. Communicating with policy-makers is a crucial part of the design, but certainly consumes much less time and resources than model building. Public participation may be an important aside, but only quite indirectly related to the content and conclusion of the work. This imbalance results in an instrumental bias towards what can reasonably be modelled, when in fact non-modellable issues and processes may be just as decisive or more. This bias affects the balance between different areas of life or earth sciences. It also affects the 'human dimension' in a major way: whereas demography or economy can be modelled to a significant extent in this context, aspects like geostrategy, culture, education, etc., are currently beyond modelling, whereas they are clearly decisive.

Current exercises also reflect the prevailing ambiguity of both scientists and policy makers towards the serious study of future situations. On the side of scientists, only a very limited number of disciplines – and within them, quite specialized fields – have invested heavily in studying future trends and conditions: demography, economics of energy, climate change. Some are following more or less rapidly, such as agriculture and water resources management. But for most disciplines, studying futures is either outside of the agenda, chronically marginal (as in geography), or just starting (ecology is in this situation). For most scientists, future situations lie out of reach of what they see as serious (that is, empirically validated) research, and is to be confined to some interface discussion with policy makers. The latter, despite their insistence on receiving relevant results on future situations, are also ambiguous: it is not clear at all that they would be ready to consider (except as foils to more conservative scenarios) future (ecological, social or political) conditions that would be at acute variance with currently accepted views and political balances. In exercises where their participation and control are quite strong, this clearly limits the scope of investigations.

Integrated assessment can live and prosper with these ambiguities. Yet one can go only so far by discussing the lessons to be drawn from the study of the past and present, and one rapidly reaches the point where the depth with which possible futures are researched and critically discussed becomes the limiting factor of our discussions about future ecologies and the policies by which we might prepare (for) them. Focusing on modelling the reasonably modellable, building rapidly a set of scenarios, packing all the rest in collective drafting of impressive state-of-the-art reviews, organizing participatory processes both for decision makers and for the public have proved an ambitious and extremely useful project where it has been done. However, it does not provide us with a blueprint for pushing the investigation of future ecological situations and systems beyond current inhibitions and ambiguities.

## 3. A blueprint for ecological futures research

### *3.1. Which orientations would such a blueprint provide?*

A first indication may be given by looking at the way research on ecology and the environment is dealing with the past. Over the last three decades, a spectacular movement of re-historicisation has occurred; whereby we have moved from rather static concepts of ecology and society to a highly dynamic, evolutive view of the transformation of ecosystems and societies. Most environmental sciences disciplines have been involved in this movement in a major way: ecology, pedology, hydrology, climatology, etc., and they have been met in the effort by history, archaeology, and other 'human dimension' disciplines. As a result, we now have a richly textured view, an experience, and many examples, of what kinds of understanding we are looking for when trying to analyse and evaluate the transformation of ecological and social systems over time (over decades, centuries, millennia). This view, experience and examples provide us with a compelling indication of what we ought to aim at when studying possible future transformations and conditions of ecological and social systems. We should seek to describe these in terms similar to those we use when describing past conditions and transformations. This idea lay behind the phrase '*retrospective et prospective*' (hindsight and foresight) that was put forward by the research program 'Environnement, vie et société'

[3] Scenarios are thought to be easily built, in ''a few days'', through interdisciplinary workshops [3]. Their methods are usually not clearly described in scientific papers [5].

(terminated, 2005) of the CNRS (the French national centre for scientific research) to promote work on long-term transformations of socio-ecosystems.

In practice though, such continuity between studying past and future has proven very difficult to operate. This comes as no big surprise: since the onset of futures studies in the 1960s, it has been clear that the stakes and means of studying future situations differed in some fundamental ways from those of studying the past. In his seminal 1964 book on *The Art of Conjecture*, Bertrand de Jouvenel [10] insisted that the past is the domain of the knowable but unactionable, and the future, of the actionable but not knowable. Realizing this has led authors in the field to promote a pragmatic approach of futures studies, rooting the field, both in theory and practice in the needs of decision makers to organise their thinking about future issues. This is exactly the option that lies at the roots of the integrated assessment formula that dominates the scene today. However, based on our critical review of the field [25,26], we conclude that limiting the study of futures to such pragmatic perspectives is an insufficient option, which imposes unnecessary limits and hinders medium-term perspectives for the development of research on future ecologies and societies. What is then the alternative?

To identify and to hold other orientations, one has to distinguish clearly between the kind of understanding we are aiming at, the (epistemic) status of that understanding, and the theoretical and methodological means by which it is constructed and formalized. The blueprint we propose rests on the following options:

(1) the aim should be to be able to describe and understand possible future situations with the same kind of multidimensional detail, rigorous analysis of transformations, and reflexive distance with which we describe the past;
(2) the status of such analysis cannot be that of knowing what is going to happen and should not be focusing mostly on forecasting what is most likely to happen[4]; it should accept its fundamentally conjectural nature (what we are affirming here is that scenarios and models about the future are, in fact, elaborate conjectures) and set the quality of conjecture elaboration and discussion as its epistemic touchstone;
(3) theories and methods should be adjusted and designed to this aim and these epistemic underpinnings; they should be drawn from (and debated and evaluated in) both the disciplines studying socio-ecosystems and the futures studies field.

Beyond current exercises of collective expertise – and some of them are impressive achievements indeed – what has to be developed is a conjectural ecology, hydrology, environmental policy science, etc. They should be justified primarily not so much by their immediate pragmatic use to decision makers or for public debate as by their ability to hold their ground in relevant academic arenas of debate – that ability being both the best guarantee of medium- and long-term social usefulness, and the best incentive for disciplines to seriously invest in futures ecological (or hydrological, etc.) research.

## 4. Overcoming too narrow epistemological rules, habits and beliefs

If this blueprint gives us a better idea of what we ought to be aiming at, it does not indicate how to get there. This question cannot be answered by a set of recipes. No 'tool-box' (as those often promoted by directly applied concepts of foresight) is large enough to accommodate the existing and potential instruments of knowledge about possible future transformations of social or ecological systems: it is potentially the whole repertoire of the involved disciplines that may be used. So the stake is one of the mobilizing resources from an unlimited repertoire, and of inventing new ones. Seeing some of the difficulties ecology meets as it embarks on the study of futures [9], this is no easy task. It involves on the one side serious stumbling blocks and on the other side the need for more actively mobilizing some existing resources. The inventory of such blocks and resources is only starting. Presenting already available elements or specific research proposals would exceed the scope of this paper: here we will only propose general orientations based on our previous and current work, but which we think can be of use to a wide variety of research projects in the field.

A first approach in that direction was that of Poux et al. [29]. Within a project on possible methodologies for studying wetlands futures at a micro-regional scale, these authors interviewed a number of scientists from various disciplines and asked them what the opportu-

[4] Developping this crucial theme is beyond the scope of this paper. Let us just quote the title of a seminal book of the French school of Futures studies in the 1970s: *Crisis of forecasting, rise of foresight*, M. Godet, *Crise de la prévision, essor de la prospective – exemples et méthodes*, PUF, Paris, 1977 [14]. By adapting it to the orientations defended here, we might propose the phrase: 'limits of prediction, rise of conjectural research'.

nities and difficulties were, in their respective disciplines, when attempting to study future states and transformations of the systems they studied. Two frequent profiles came out that are of special interest here [27]. The first can be labelled 'nomothetic'[5]: scientists who analyse correlations between spatialized dynamic series, trying to identify correlations and mechanisms, 'laws' on which they could base forecasts of future dynamics and states of affairs. The strength of their approach relies in the richness of data, the rigour of analysis, the power of computer modelling when simulating future dynamics and conditions. When working on the future, however, such approaches have many limitations, inter alia: the implicit hypotheses that structures of the system that control its dynamics will remain stable over time, that no new factor will come into play and alter the dynamics, that the categories used will retain their meaning over time, etc. Nomothetic approaches also are quite limited when it comes to integrating a multiplicity of factors that combine in infinitely varied ways and are difficult to quantify, measure or model, such as representations, culture, geopolitics, etc. The other profile can be labelled 'idiothetic': it is the approach of disciplines that focus on the in-depth description and understanding of particular situations and insist on the specifics of time and place. History, anthropology, geography often adopt this stance. Its strength lies precisely where the drawbacks of nomothetic approaches lay. They are highly sensitive to intricate and surprising interplay of multiple factors, to the intervention of unique factors, to qualitative aspects and perceptions. There is hardly a discipline that would be better suited than history, for instance, to appreciate the transformation over time of complex social-ecological systems and the uniqueness of extreme events that are a part of it. However, on their possible way to studying futures, idiothetic approaches are hindered by one major difficulty. In one way or another, these disciplines are codified so that elements from the present and past, concretely and systematically collected by the researcher, are the only acceptable base for serious research. On the whole, historians will not work where there is not an archive, witnesses, or archaeological evidence. And those are radically missing about the future.

From this simple introduction, one can measure the magnitude of theoretical and methodological difficulties and innovations that are at stake. The further development of studies of future social ecological situations will take time and mobilise large academic communities. However, before innovations in theory and method are seriously undertaken, a more fundamental barrier will have to be overcome, which lies in the epistemological beliefs of many – maybe of most – scientists involved in environmental research: the belief that scientific activity of value can only be about establishing positive facts by an empiric-deductive approach. Although this belief is contradicted – or at least strongly qualified – every day by the investment of scientists in endeavours like the modelling of global change, it is still very actively prevalent and hinders or limits the investment of whole communities in studying futures. It takes much reflexion and debate to recall that science is based both on conjecture and observation/measurement/verification. Serious research on futures should not be sought so much in proven predictions and forecasts about future situations, as in new developments in the conjectural dimension of theory, methodology and academic debate in the disciplines studying social and ecological systems.

## 5. A gradual – and still incomplete – opening up from the restricted use of predictive modelling

Much of what we have been witnessing with the developments in the study of futures over the last 25 years is a gradual opening up and complexification starting from a restricted concept of predictive modelling. It is this process that should be actively and reflexively pushed further. A series of diagrams will be used here to illustrate the gradual widening from the narrowest concept of predictive modelling to the current model of integrated assessment, and from there to more open and ambitious patterns of ecological futures research.

In the most classical concept of predictive models (Fig. 1), available data, combined with current understanding of mechanisms and observed correlations, allows us to propose a model. Model runs provide predictions about facts that must result from data and current understanding. These predictions can be checked against new data – the confrontation either strengthens current understanding, or points to limits, triggering new investigations. For example, papers in ecology, predicting the presence of species based on habitat and climate data follow this logic. Here, prediction does not refer to predicting future facts, but facts in domains about which one lacks data but could acquire them.

[5] The terms 'nomothetic' and 'idiothetic' are classics in the social science literature when discussing epistemological issues. When using them, we refer more particularly to Ricoeur [30].

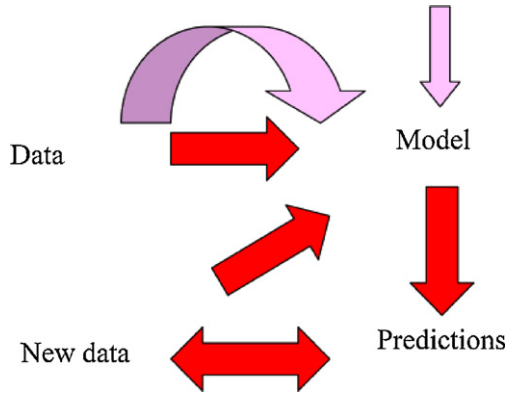


Fig. 1. The narrower concept of predictive modelling.
*La conception restreinte de la modélisation prédictive.*

One can, however, use the same kinds of models to predict – or forecast – future dynamics and states of affairs, about which one also lacks data. The problem then arises that such predictions cannot be checked against new data: no factual data can be generated about future events. As a result, authors of forecasts have to find substitutes for the strengthening of conjectures by new data, both to help build the models and to underpin model 'validation' (Fig. 2).

On the model-building side, models are fed with hypothetical data about future circumstances, likes scenarios providing input data. On the side of model results interpretation, validation against new data is replaced by interpretive discussion of results: what would be the consequences? Would some qualitative elements mitigate the interpretation of model results? What do model results contribute, when confronted with other anticipations on the same issue? What do model results suggest in terms of policy?

Over the last 25 years, as more and more ambitious and resource-consuming modelling exercises about ecological futures were launched, this complementation of modelling by scenarios and discussion procedure has gradually been reinforced, both in practice and doctrine (Fig. 3).

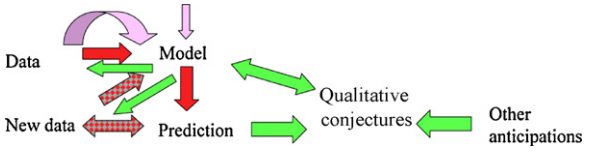


Fig. 2. Venturing in domains where the collection of new data is not possible, predictive modelling comes to be buttressed by other sources of conjectural discussion
*Lorsqu'elle s'aventure dans des domaines où l'acquisition de nouvelles données n'est pas possible, la modélisation prédictive doit être étayée par d'autres sources de discussion conjecturale.*

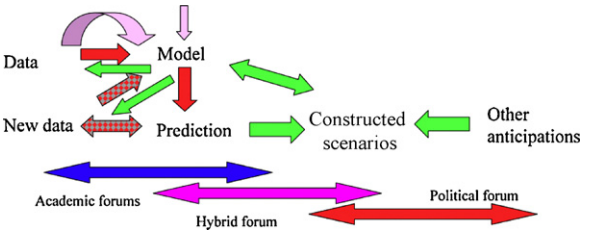


Fig. 3. The buttressing of models by other sources of conjectures becomes more explicit and organized.
*L'étayage des modèles par d'autres sources de conjectures devient plus explicite et organisé.*

On the procedural side, the main efforts have borne on improving interactions between modellers and decision makers (and the public). In the 1980s, Functowicz and Ravetz [11] summarized beautifully the issue by stating that 'hard decisions' (about the future) had to be taken on 'soft data' (results of models based on incomplete knowledge, impossible to validate). Modelling exercises marked by uncertainty and arbitrary choice of many hypotheses, leading to open interpretation in complex debates, could have, when used in policy making, major and direct consequences for the public. Functowicz and Ravetz conclude that decision makers have to be made part of the discussions of model design and interpretation. Many other writings (see for instance [18]) have followed similar lines, so that 'hybrid forums' have now become the guiding ideal for model-centred exercises with potential decision-making consequences. The success of policy-dialogues in the early 1990s, the generalization of similar designs at much larger scales – such as in the IPCC or the MEA reports – have made this the state of the art.

On the methodological side, efforts have gone towards using more explicitly constructed conjectures to provide both input to models and context for results interpretation. Following the early calls of Michael Thompson on the irreducible diversity in the framing of issues prior to modelling [32], or of Clark [7] on the necessity of innovative qualitative methodology to accommodate surprise and breaking points, scenario exercises have been used more and more to complement modelling (see the Global Scenario Group publications mentioned above). As quoted above, Alcamo summarises the state of the art by the phrase 'storyline and simulation' – that is, modelling complemented by scenarios.

This brings us up to precisely the state of the art we described in the first part of the paper: integrated assessment, based on collaboration between scientists and decision makers, and using mostly modelling, complemented by some scenario building and public

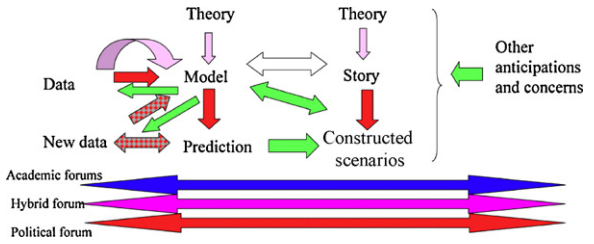


Fig. 4. Next on the agenda: deepening narrative conjectures and their discussion, expanding academic initiative in studying and debating futures.

*Les enjeux pour la suite : approfondir l'élaboration et la discussion des conjectures narratives, amplifier l'initiative académique dans l'étude et la discussion des futurs.*

participation. But by showing how this way of studying futures relies on a gradual opening up from the strictest use of predictive modelling, it allows us to better understand its limits: they stem from the fact that this opening up has been taken up as yet by only a small number of research fields and that, even in those fields that have pioneered it, it goes only half of the way to the point indicated by our blueprint. Which orientations may guide us beyond (Fig. 4)?

On the side of methodology, investing more seriously into scenario building and evaluation should allow us to remedy the imbalance in favour of modelling. In his book *Filters against folly*, Garrett Hardin [15] shows the complementary role of 'numeracy' (of which modelling is an epitome) and of 'literacy', that is, of apprehending situations through sophisticated use of words and stories. For such balance, the way to go from the current state of the art in studying future ecologies is to be just as demanding (in sophistication of construction, in rigour and theoretical grounding, in resources invested in academic critical discussion) about scenarios and stories as one is about models. One should break the habit of thinking that scenarios require just 'a few days' of meetings, followed by some drafting, being just an accessory to modelling. It is striking to see that the academic disciplines that master word and stories are only marginally involved in the study of future ecologies. There is a large margin of progress in that direction [21]. This rebalancing could both mitigate the instrumental bias within life and earth sciences in favour of the modellable (by encouraging in-depth conjectural discussion of the not-or-not-yet-modellable), and the current imbalance in disfavour of social and political dimensions of ecological futures.

Narcy's [28] research on the social management of flood hazard in the Oise catchment provides a good example and analysis of why such a rebalancing is particularly relevant in the study of extreme events and their consequences. He shows that two completely distinct perspectives apply to major floods. The first treats processes, events and their consequences as being 'under control' (in particular from a cognitive point of view). Even if they are extreme in amplitude and have very negative consequences, they unfold according to known, probabilistically forecastable and managerially mitigable processes. Planning in this perspective can rely largely on the modelling of floods, based on hydrological data and taking into account existing infrastructures. The second perspective posits extreme event as being 'out of control' (also from a cognitive viewpoint). They involve unforeseen ruptures and highly contingent sequences of events with catastrophic consequences. A historical study of floods has shown, for example, that over the centuries, not one major flood has occurred in the Oise catchment without heavy consequences of the unexpected rupture of some of the infrastructure. This 'out-of-control' dimension of extreme events cannot be usefully modelled: here the challenge is not to compute consequences of a known situation, but to envisage, in an elaborate and rational way, hard-to-imagine combinations of surprising events, and their consequences. Planning in this perspective has to rely on considering inter alia worst-case possibilities, worst historical occurrences, and on taking a precautionary approach. Modelling the 'under control' and envisaging the 'out of control' through in-depth scenario treatment are complementary challenges in the treatment of future extreme events.

On the procedural side, the major rebalancing to be effected is in the emergence of autonomous initiative from academic arenas in the construction and discussion of conjectures on future societies and ecologies. This freedom of initiative is left de facto to communities of life and earth sciences; we advocate, however, that most of them should much more actively take advantage of it. The issue is more problematic for social and political sciences, which are less sheltered by the technicalities of computer simulation; here, the principle that every conjectural work has to be undertaken in mixed assemblies sponsored by policy makers results in serious limitations regarding the origins and content of conjectures on social and ecological futures[6]. To give just one example, it would be essential to write and discuss in-depth geopolitical scenarios linked to future

[6] Let us recall that on top of such limitations, social and political sciences also have to stumble with epistemological stumbling blocks of their own.

ecological issues; it is dubious whether the United Nations- and the World Bank-sponsored forums provide the best framework for such research that tends to challenge directly the consensus-based balances of such intergovernmental institutions. The importance of hybrid forums should not lead to neutralization of the creative and critical capacities of academic forums, not any more than it would be acceptable that the role of scientists and experts would lead to hold in leash the autonomous dynamics of political forums. In other words, the importance of hybrid forums where decision makers and academics work together does not make it less essential for academic communities, both in natural and human sciences to take into their own hands the initiative of ambitious studies of possible futures.

However, the question remains of how the specific stumbling blocks that currently restrain or slow down such developments in most disciplines are to be overcome. Eventually, of course, innovative new works will prove themselves and establish new bases. However, the road can be prepared in two ways. The first is through active reflexion and debate, within each discipline – and even, within each relevant field of research – on the specific theoretical and methodological tools it can bring to the study of futures and on the particular rules, habits or shared views that hinder it from doing so. The second is through more active mobilisation of ideas, methods and research results from futures studies – a field which has been struggling for four decades with the issue of studying futures.

Such resources can be classified into three categories. The first one, and the one futures studies specialists insist most on, is the repertoire of methodologies that are constantly put forward, diversified, and recombined. A review of the literature in ecology, for instance, shows that the transfer of such methods can have high impact (witness, the MEA), but that it is currently limited to a small range of futures studies authors and approaches. The second group of resources is what may be called a repertoire of framework conjectures. Progressing towards more a sophisticated study of future ecologies inevitably means often focusing on very specific questions. This, however, is possible only if specialized conjectures can be linked with other, often more general, sophisticated conjectures. Producing, publishing, critiquing, confronting such conjectures is a responsibility and an asset of futures studies. The third resource that the latter can provide is a host of concepts, of theoretical publications and debates by researchers who have grappled collectively for decades with precisely the same difficulties about studying futures that researchers embarking on the study of future ecologies are now confronted with. This, in our view, may be the most important contribution futures studies may provide today. Whereas there is no room here to expand more on the subject[7], one more point ought to be stressed. One will not find in futures studies the whole range of theory that will be necessary if our blueprint for futures ecological research is to be realized. This is because, in a rather similar way to the current 'integrated assessment' formula – but to a considerably lesser degree –, there has long been an overly pragmatic, directly decision-making oriented, bend in futures studies. For them too, such study of ecological futures that have to be undertaken today and tomorrow is a serious challenge!

## 6. Discussion: preparing for extreme events

As the ideas and orientations put forward here derive from work on the study of the long-term transformation of social and ecological systems, we ought to discuss, as a conclusion, how they bear on the preparation of extreme events affecting them.

A first remark is that if we are ready to prepare for extreme events, these are necessarily future events. Not only in the obvious sense that they have not occurred yet, but also that, the more extreme the possible event, the more likely a long interval of time will elapse before it occurs. If we prepare for a millennial flood, we can hardly limit ourselves to the perspective of it happening in the next five years. The remark may seem trivial, but it is not. We see very often approaches where natural events or transformations expected to occur 50 or 100 years from now are assumed to occur in a world that remains essentially unchanged (or only incrementally changed) in most aspects. This is highly unrealistic: preparing for extreme events has to be a part of preparing for a future that will encompass both extreme events and profound transformations of ecosystems and societies.

A second remark is that extreme events share with long-term transformations several of the essential attributes that create the special difficulty of studying futures. The main one is the non-repetitive nature of such events, which severely limits the use of narrowly predictive modelling and calls for the opening of wider conjectural investigation and debate such as we have advocated in this paper. Another such attribute is the high social and political charge carried by extreme

[7] See also [26].

events. Just as in the study of long-term transformations, this charge results in a disconcerting and ambiguous mixture of eager curiosity and unreadiness to face many perfectly plausible processes and outcomes – a condition that has far-reaching consequences on the way these are studied. Another such shared attribute is surprise. Preparing for extreme events is inevitably, at least in part, preparing for the unexpected. This paradoxical proposition is shared by studies of long-term social and ecological transformations. In his seminal writings of the 1980s, Clark [7] rightly insisted that studying future societies and ecologies necessarily implied taking up this challenge and including discontinuities and surprises in our stories and models – extreme events being the most obvious of them.

Although there are so profound similarities, differences between long-term transformation of socio-ecosystems and extreme events should not be overlooked. The duration of extreme events is very short when compared to the magnitude of their effects. This has important consequences for studying such events and preparing for them. It reduces complexity to a certain extent: some overwhelming driving force triggers major transformations throughout the system, so that maybe less retroactions, adaptations, evolutions and combined transformations have to be considered. This also bears on the social aspect of such events: in extreme circumstances, organizational issues tend to become simplified, for example by one authority taking over for planned emergency reaction, or by social solidarity partly replacing the complex scheming of each part of society towards the others, which is such an important moving force of long-term transformation[8]. This goes with another difference: values are much more easily shared regarding catastrophes (which many extreme events are) than when considering the many possible directions in which societies and their environments may be made to transform themselves. This creates a profound difference between the field of risk, where the assumption that we jointly want to avoid risk – for instance to human lives – provides a common ground, and the field of environment, where – despite some noisy claims to the contrary – there is no real consensus on the content or the level of environmental quality that is really to be reached through policies.

[8] This difference is, in our view, the main reason for the relative failure of the Policy Exercise concept put forward and tested in the late 1980s and early 1990s, which proposed to transfer methodologies from the field of extreme geopolitical events (military gaming) to the field of societal management of long-term tranformations [23].

Despite such differences, however, shared attributes are so important that it is probably not excessive to claim that the study of long-term futures and of future extreme events are two aspects of the same challenge, the terms of which, we believe, have to be extended and partly renewed.

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