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Symposium on *The Gambling Animal*: replies to critics

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REVIEW ARTICLE

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Symposium on *The Gambling Animal*: replies to critics

ABSTRACT

The Gambling Animal synthesises material from multiple disciplines, which presents a challenge for any individual reviewer with a specific focus of disciplinary expertise. The critical analyses that have been produced for the symposium rise, at least as a set, to that challenge. In this article, we respond to our reviewers. Responding to them has led us to reflect more deeply on various aspects of our natural history of human risk management, with comparative attention to elephants. We hope that readers who cannot prioritise reading our book will gather a useful sense of what it aims to accomplish and how, and that other readers will be inspired by the symposium to turn attention to the book itself.

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Introduction

We are grateful to the editors and book review editors of the *Journal of Economic Methodology* for organising the symposium on our book *The Gambling Animal* (henceforth, *TGA*). As several of the critics note, the book synthesises material from multiple disciplines, which presents a challenge for any individual reviewer with a specific focus of disciplinary expertise. The critical analyses that have been produced for the symposium rise, at least as a set, to that challenge. Responding to them has led us to reflect more deeply on various aspects of our natural history of human risk management, with comparative attention to elephants. We hope that readers who cannot prioritise reading our book will gather a useful sense of what it aims to accomplish and how; and that other readers will be inspired by the symposium to turn attention to the book itself.

Response to Grayot

Grayot has read our book with considerable care, as demonstrated in the elegant and accurate synopsis that opens his review. The critical questions he goes on to raise are thus well calibrated to the details of our exposition.

He identifies some apparent 'tensions' between the view of economic agency defended in Ross (2005, 2012) and elsewhere, and the conception of agency that features in *TGA*.

The first of these is that whereas Ross's earlier model stresses coherence and stability, *TGA* 'treats incoherence, instability, and systematic distortion as normal outcomes of risk-sensitive learning in complex environments'. 'Tension' – as opposed to contradiction – is the right word here. Ross's model of economic agency always allowed for error; what is required of the economic agent is a measurable *pattern* of coherence, not consistency that is manifest in each and every individual choice. Furthermore, coherence does not imply either accuracy of representation or practical success. Most economic agents come to grief from time to time, not just because events violate their expectations, but because their expectations are poorly calibrated. Indeed, sometimes economic agents suffer harms that might have been avoided if they were *less* constrained by pressures for coherence. An agent might, for example, forego a good investment because they have constructed themselves as 'not the sort of person' who accepts the necessary degree of risk.

This review has been corrected with minor changes. These changes do not impact the academic content of the review article.

Grayot's second tension concerns the relative conceptual priority of preferences and risk. Is risk to be defined by reference to preferences, or do preferences arise under pressure of risk? We regard this as a false conceptual dichotomy. An economic agent's utility function is not simply a preference ordering, but crucially *incorporates* risk preferences. This is a necessary feature because coherence sufficient for economic agency would be easy to achieve and maintain – and thus not very interesting – if the world did not present risky choice options. Thus utility and risk are co-dependent aspects of agency, and the question of which has conceptual priority reflects a misplaced urge for 'grounding'. Ross (2023) criticises the general applicability of this philosophical urge for logical hygiene to social science in general. The value of theory in science lies in its contribution to stability of measurement, not in service to ontological systematics.

Grayot explicitly mentions grounding in his identification of a third tension. He suggests that the earlier Ross conception of agency is justified by reference to explanatory utility, whereas in *TGA* agency reflects biological reality. Again, there is a false dichotomy here. On Ross's original conception, people construct themselves as economic agents – that is, participate in the social practice of agency attribution, including self-attribution – in order to be able to participate in the kinds of cooperative enterprises that form the basic ecological maintenance strategy of an intensely social animal. Economic agency on Ross's account is absolutely not a mere theoretical instrument; it is, following Dennett (1991a), a real pattern.

Grayot wonders whether, on our account, 'market-level systems' are agents. Markets do not make choices, and that is a necessary condition for economic agency, so markets are not agents. Perhaps by a 'market-level system' Grayot means a corporation (or guild, or labour union, etc.). They indeed are economic agents, because they *do* make choices (by which we mean 'select options under responsiveness to incentives'). In that case, Grayot wonders whether, on our picture, 'market-level systems are agents only in a metaphorical sense, or … the autopoietic grounding of agency must be relaxed or abandoned when scaling up.' Again, we note our dissent from the view that theoretical concepts in science need philosophical grounding. As Grayot says, constructed institutions are indeed not autopoietic[1] in the ways that biological organisms are. But so what? Grayot worries that if institutional agents do not share *all* the properties of organisms relevant to their agency then 'different notions of agency are tacitly in play'. 'Notion' here seems again to appeal to the philosopher's fetish for a degree of conceptual order based in systematic ontology that is not demanded in science. Denying this urge does not entail tolerating a lack of clarity; economic theory specifies the application conditions for economic agency relatively exactly. It indeed says nothing about its general grounding, and we acknowledge no defensible reason that it should.

The same general issue lies behind Grayot's worry that in giving the brain a 'privileged explanatory role' we somehow undermine our emphasis on scaffolded cognition. If 'privileged' simply means 'important', then there is evidently no tension here. The distinctive architecture of the human brain, which is a fundamental enabler of the way in which humans became ecologically dominant, works its ecological power through exploitation of scaffolds. So we suspect that 'privileged' is supposed to again allude to grounding. This interpretation is supported by Grayot's immediately subsequent reference to what is *constitutive* (his emphasis) of cognitive systems that manage risk. Constitutiveness is a close cousin of grounding, another expression of the philosopher's itch for conceptual structures that map neatly onto ontologies. As it happens, Ross and Ladyman (2010) explicitly consider and reject, as unscientific, appeals to constitution in the extended-mind literature.

Economic theory, and scientific theory in cognitive science, are tools for enabling stable measurement of phenomena. They are not ontological systems, though they track real patterns. To economists, this is relatively clear. Philosophers have played a much more important role in the history of cognitive science, so in that field there is considerable ambiguity about the role and nature of theory. Gratuitous philosophical concept regulation is best resisted not by a general campaign, but by keeping watch for philosophers' idiosyncratic preoccupations in specific contexts and choking off their influence as and when they are spotted.

Another remark on the substantive role of 4E cognition in our account is in order. We think that three of the Es – extended, enacted, embedded – apply across the full range of economic agents. Embodiment does not. By this we don't mean that there are any agents that have no material basis. Rather, we mean that economic theory abstracts away from the aspects of cognition that arise from embodiment even in the case of organisms. In principle a person and a corporation could have the same utility function, in which case they are members of an equivalence class so far as economic theory is concerned. A full and rich picture of embodied economic agency is provided by Basso and Herrmann-Pillath (2024). They distinguish at length and in depth between inward and outward embodiment. The extent to which the latter adds anything important to embeddedness is complicated, and beyond the scope of what we can address here. But we are sceptical about the idea that inward embodiment must be represented in economic theory, and that is the only kind of embodiment for which autopoiesis might be held to be a component. A passion for grounded conceptual frameworks can obscure these complications. Semi-autonomous, inwardly disembodied AIs will be full-blown economic agents but not (at all) people, and that is a major part of what is dangerous about them.

Grayot's suggestions for better recognising the role of emotions, and in particular management of damage from emotional trauma, in understanding addiction are well taken. The closing sentence of his commentary puts his general and important point elegantly. People turn others into addicts both by creating environments that exploit the brain's hunger for learning probabilities *and* by undermining their capacity for feeling emotionally safe. That is to say, people are multifariously bad to one another. (Elephants are also no angels, but those who indulge in cruelty do so less insidiously.)

Response to Nagatsu

Like Grayot, Nagatsu emphasises the ambiguous – or perhaps merely complex – role of agency in the story told in *TGA*. But unlike Grayot, Nagatsu does not promote recourse to philosophical grounding in hopes of taming this ambiguity or complexity. After nicely framing the question(s) about it, he perspicaciously locates an answer we endorse. Between focus on 'a goal-oriented protagonist with whom the reader is invited to identify' and recognition that species, as populations, are not agents in any sense that science should recognise, we have 'a gestalt-like effect' where '[i]n the first perspective, agency endures, whereas in the second, it appears, merges, disappears, and transforms into something else'.

This is a gracious and thoughtful reading of our work, which invites reflections that go beyond what we wrote in the book. Let us first be clear that no species has ever been an agent, because species do not make choices, in the sense indicated in our response to Grayot. Note that this does not say that no species *could possibly* be an agent, an important point that Nagatsu opens later and to which we return. Nagatsu refers to 'analysis by evolutionary game theory', which is indeed to the point. EGT is game theory with agency stripped out, though it must be added that EGT is precisely the modelling approach needed to study how agency can naturally arise from processes with no agency (Barrett, 2025). But there *are* agents who make causal differences in history; some of which are individual organisms (so far, perhaps, these have only been human individuals), and many of which are collectives with sufficiently stable governance dynamics. Nagatsu opens the question in the later part of his review of whether humanity as a whole could ever achieve governance sufficient for agency, and he suggests a pathway to such an achievement. We will come back to this.

First, however, let us say something more about our valuation of the protagonist in our narrative. There is no contradiction in denying that a species is an agent, and also valuing the strategies that characterise members of a species, where again 'strategy' is understood in the EGT sense. We say in the book that we love some aspects of the human ecological strategy, specifically as they have generated art, science, sport, and mutual self-awareness dynamics in sets of lovers (see our footnote on p. 313 citing the Cowboy Junkies). But it would be emotionally dissonant to love the human species unconditionally, or perhaps even generally, while acknowledging, as the facts compel, that the surge

of ecological power by that species has represented a catastrophe for most others and probably for itself. This catastrophe must also be a catastrophe for billions of *people* who *are* agents, whose welfare should be respected whether one loves them or not.

We can simplify this issue by focusing on our other protagonist in the book, elephants. As we stress, their evolutionary strategy kit, like that of humans, is unique, complex, and subtle, and elicits in us feelings of admiration and appreciation. This strategy kit is under much greater risk of extinction than the human one (and it matters that the human strategy kit will be its likely cause). Its loss would be worthy of great grief for anyone who can focus cognitive attention on that emotion. And as in the case of catastrophe for the human species, the extinction of elephants implies severe suffering for tens of thousands of identifiable agents – individual elephants and particular family groups with stable agency conditions resting on the distinctive elephant management style. Most of these agents are loved by other agents. (Not all are, and not all behave in ways that sustain such responses, as with people.) Unlike in the case of people, we could almost watch each of them die as individuals because there are now so many fewer of them.

What if our two protagonists cannot co-exist, because each demands too many resources that are also demanded by the other? If that is so, it is a perfect example of a tragedy, and Aristotle identified the appropriate emotions for it. If the tragedy is inescapable, and we could choose a policy that would decide between our protagonists, which one would we favour? We prefer, at the risk of bad faith, not to apply our minds to that question. But note that we do not simply jump to say 'humans'. We make this point to address Nagatsu's questions about our attitude toward our book's main character *as* a protagonist.

We now return to Nagatsu's main theme in the second part of his review. If our model of human economic, ecological, and cultural history as a risk ratchet conditionally predicts civilisational collapse, what basis of hope for avoidance of such an outcome remains? To what extent does such hope depend on the prospect of a species, for the first time in history, exercising collective agency?

Nagatsu's ploy here is to draw on our emphasis on the special facility for imagination that characterises the human ecological strategy (in the EGT sense). As he nicely consolidates our story, imaginative power made possible a pro-growth feedback cycle between technological progress and the massive expansion of constructed moral circles. As in any such loop, the cycle is prone to reversal, and some modellers interpret recent evidence as suggesting that this is happening. For example, Gordon (2016) argues, with focus on the past century's primary locus of innovation, the United States, that despite the distracting advances in technology for processing and disseminating information, the era of rapid growth in material wealth had drawn off all the low-hanging fruit (in transportation, public health and safety, energy conversion efficiency at giant scale, and mass literacy) by the end of the 1950s. As with the pessimism of another dominant voice from economics, Phelps (2013), this gloomy prognosis is based on the operation of endogenous processes.[2] The picture darkens enormously when endogenously *generated* effects on the natural environment are fed back into models as multipliers. Again from the perspective of standard economic logic, the pessimistic analysis that emphasises climate change and biodiversity collapse is elegantly consolidated by Naudé (2023).

Against these trends there are some hopeful strands, such as very rapid expansion in educational access for women, which in turn is levelling the explosion in the global human population (Galor, 2011, 2022), and current downward spiralling of the relative cost of renewable energy sources, particularly driven by a surge of improvement in storage technologies (Liñeiro & Müssgens, 2025). However, as Naudé stresses, the demographic transition that is good news in the long run is bad news in the critical short-to-medium run, because it pushes down economic growth, threatens welfare state viability, and thereby imperils political stability. Nagatsu additionally argues that in *TGA* we underestimate the social cost of carbon emissions. Our agreement with Dasgupta (2021) on appropriate discount rates for shadow pricing of natural assets suggests that Nagatsu is probably right to correct us here, though we have not reworked the arithmetic.

Nagatsu argues that the only – slim – basis for genuine optimism is that human imagination might allow for a *general* reconceptualisation of our species' relationship to other species, such

that a new morality might allow humanity to acquire effective agency in arresting the destruction of natural capital. More specifically, he hopes that the cultural evolution of such a morality could create conditions necessary for the implementation of the 'more-than-human economics' programme (Ortiz-Przychodzka et al., 2023). Essentially, this is the idea that policy should optimise a social welfare function that takes the welfare of non-human lives, in addition to human ones, among its arguments. We think it is radically premature to commit to overcoming the severe measurement challenges entailed by this suggestion, if the more practical course promoted by Dasgupta (2021) of fully accounting for natural capital would identify substantially similar policy targets (see Treich, 2025 for careful discussion).[3] These important technical details aside, there is a strong sense in which Nagatsu's moral impetus is indeed part of our own scientific agenda.

None of the reviews in the present symposium mention our programme of experiments with elephants that are briefly described in *TGA*. We therefore note that we are explicit that a core policy motivation for this project is to establish a communication bandwidth with elephants that could allow for interspecific exchange of information about quantitative risk in foraging choice contexts. This could, in principle, foster a basis for new *mutualisms* (Dunn, 2025) between humans and wild elephants – for example, elephants locating water for human livestock during droughts, in exchange for provision of food transported by humans from unafflicted regions. It will be evident why this is a particularly beguiling idea to an economist. We think it is in the spirit of Nagatsu's desired new morality, in recruiting the agency of non-humans to support both their welfare and that of people.

This nascent policy approach, however, would call on the agency of specific human communities and specific elephant families, so it sidesteps Nagatsu's larger question about the possibility of species-level agency. We do not see that as logically *impossible*, but we are very doubtful that the cultural evolution of new morality could occur fast enough to do the job, if indeed the evolutionary game necessary to generate it as an equilibrium could actually arise. We think it is much easier to imagine more directly political dynamics. For example, imagine geopolitical hegemony of a Chinese regime that based its utility function on perpetuating the supremacy of the Communist Party and was convinced that civilisational collapse was inimical to that objective. Such a state of affairs would be deeply unpleasant for anyone with liberal moral sensibilities, but it could allow hierarchically subjugated species to act as ecological agents. Priors on such prospects must necessarily be highly diffuse, but likelihoods should not be underestimated due to failures of imagination. For example, the scenario just sketched need not require literal world conquest by a unitary state. As we note in *TGA,* a single large country could *already* implement solar geoengineering to create buffer space while it invests in capacity for the implementation of carbon removal mega-projects; and a powerful enough country could simply intimidate others into tolerating this. As we also say in the book, solar geoengineering would raise a vast new risk to ourselves and to the entire biosphere – precisely what the risk-ratchet process to date might lead one to expect.

Is this a variety of optimism or a variety of pessimism? There could only be a clear answer to that question if a utility function could be assigned to the whole human species; but that would require it to already have revealed preferences through exercise of agency. A non-anthropocentric Kantianism would aim to dance around this conundrum. But here we appeal to one of Dennett's most potent intuition pumps, his distinction between skyhooks and cranes in modelling system evolution (Dennett, 1995). Kantians seek skyhooks, solutions based on the alleged motivating power of moral 'rationality' untethered to the reality of economic, social, and political complexity. If we might save civilisation, this will require cranes. That does not at all preclude the possibility that future humans might enjoy a more admirable moral culture than current ones; but if that good fortune comes to pass, it will not be by design.

Response to Vromen

Vromen seems to wish that we had written a book about evolution, particularly cultural evolution, rather than the book we set out to write, about the role of risk management as a force that has

constrained and shaped our evolution. The overarching theme of our book is that many of the explanatory factors concerning the ecological dominance of our species can be productively framed in terms of the economics of risk and risk management. Vromen points out that some of these factors can also be framed in terms of cultural evolution. That is true, important, and acknowledged in our book. We cite other excellent books that have been published on this subject and synopsise them in general terms rather than repeat their details. Vromen complains about this, but we fail to understand why.

As we see this relationship, our economic perspective is simply a level of abstraction higher than that of accounts in terms of gene-culture coevolution. It is common in accounts of complex phenomena, which obviously includes human history among its instances, for models to provide explanatory insight at different, and complementary, scales of analysis (Ladyman & Wiesner, 2020). Vromen seems to think that such models must compete. We think this is an error in the specific case at hand, and quite generally. We agree with him that a more synoptic history of humanity would incorporate other lines of explanation, such as that of Henrich (2016), along with ours. However, that is just to say that interactions between models at different levels of abstraction are a worthy focus of attention. However, the models to be interacted must each be developed in the first place. We hope that someone will be inspired by our work to examine such interactions.

Vromen makes a number of more specific errors of interpretation that contribute to his general one. We do *not* say that our ancestors were more successful than elephants in the Rift Valley during the middle Pleistocene, as Vromen says. Rather, the human population exploded after it developed civilisation, on the basis of long-distance trade. So this was a consequence of the cultural evolutionary dynamics that Vromen accuses us of neglecting. We do not deny, as Vromen suggests we do, that human capacities for purposive innovation have played asignificant role in our history. However, such contributions occur on a more granular temporal scale than the dynamics our book is about. As a (crude) approximation, clever ideas can be viewed as one of several *mechanisms* by which cultural change *implemented* the unfolding of the risk ratchet that is our central theme.

Vromen suggests that there is a tension between providing an economic explanation of human ecological dominance with the evident failures of welfare optimisation. That there is no such tension is a basic insight of applications of game theory, both static and dynamic, to economic modelling. Many equilibria of many games are welfare inefficient or even disastrous. As we argue, that appears to be true of the long-run outcomes of the risk ratchet humans have been working since civilisation began.

With respect to the current predicament, Vromen says that we 'settle on a fairly standard economic answer: international coordination of higher carbon taxes.' This is another misreading. What we say is that reducing the relative price of carbon emissions is a necessary condition for slowing or reversing global warming; that a high global carbon tax would be the simplest way to achieve this; *but* that extensive evidence indicates that such a tax is not politically feasible. (In *TGA* we review the history of the failure of Ontario's carbon tax to illustrate the political problem.) Contrary to Vromen's reading, we are *not* optimists, at all, about the prospects for any general solution to the current ecological crisis. We have discussed this above in our response to Nagatsu's much more accurate understanding of this part of *TGA*.

Vromen fails to understand the core concepts of idiosyncratic risk, correlated risk, and common risk. These three concepts describe how risk is distributed across individuals or assets, which matters for whether markets can handle risk efficiently.

Idiosyncratic risk is specific to an individual, firm, or asset. Crucially for the economics of risk, it is independent across people or assets. Simple examples might be your house burning down or your car being stolen. These events are typically uncorrelated with what is happening to everyone else. The key economic property of idiosyncratic risk is that it is diversifiable and insurable. Because the risks are independent, a large pool of people facing the same idiosyncratic risk can share it among themselves. The law of large numbers does the work here. Private insurance markets can handle idiosyncratic risk well, and portfolio diversification eliminates it in financial markets.

Correlated risks tend to move together across individuals or assets, but not perfectly and not universally. Some people or assets are more exposed than others, and the correlation is partial rather than complete. Simple examples might be industry risk, where a downturn in the demand for autos hurts all automakers to varying degrees, or regional risk in which a drought hurts farmers in one region more than others. Correlated risk is harder to diversify away than idiosyncratic risk, but not impossible to manage. Mechanisms can still diversify across industries, regions, and asset classes. But insurance becomes more expensive because when one policyholder suffers a loss, others are more likely to as well.

Common risk affects everyone simultaneously and in the same direction: it is the extreme case of correlated risk. Simple examples might be a general financial crisis, a pandemic, or a major war. In this case there is no one to diversify with, because everyone is on the same side of the bet at the same time. This is the undiversifiable residual risk. No matter how large your insurance pool or how broad your portfolio, you cannot escape it. It is also why governments rather than private insurance or stock markets typically handle truly aggregate, common risks.

It is central to the economics of risk, and the design of institutions, laws and policy to manage risk, to identify these risks. Often, what looks like idiosyncratic risk can turn out to be correlated or even common, a key point in our book. Mortgage defaults seemed idiosyncratic before 2008 in the United States, but turned out to be highly correlated due to shared exposure to house prices. Much of the financial risk management is about correctly identifying where on this spectrum a given risk actually falls.

In our book, we introduced these concepts and explained them on pages 117–121, largely to be clear about the objective risks posed by COVID-19 and climate change. Indeed, in this part of the book, the only reason for stressing idiosyncratic risk is to be able to explain the difference between common risk and correlated risk. For COVID-19, we then explicitly proposed lumping correlated risk and common risk together, since at a fundamental level, everyone's interest and incentives were aligned in terms of the risks of morbidity and mortality. Everyone wanted to avoid getting sick or dying from the virus, at least to a sensible approximation for our purposes. The same is true for climate risk, when considered as an existential risk.

But here again is the key insight, familiar from the economics of risk: even in a world of common objective risk, there is still considerable heterogeneity with respect to subjective risk. Whether this heterogeneity comes from people being inconsistent Bayesians, updating their beliefs about the objective risks of COVID-19 as the sample data rolled in over time, as we studied in Harrison et al. (2022), or from tribal politics leading to deliberate mis-information from public officials, the effect is the same. It does not follow that these subjectively idiosyncratic risks can then be handled by private markets, since the underlying risk is still objectively common, and the law of large numbers does not come to the rescue. In fact, what we see, during COVID-19 and climate change debates, are strategic issues getting in the way of finding simple collective solutions to the common objective risk, due to each strategic actor seeing a risk as weakly correlated or even idiosyncratic.

The core mistake that Vromen makes is to think that we are saying in the book that some risks are common and can be solved by collective action, and that some other risks are idiosyncratic, and can be solved by private action. Instead, we are saying for two of the most important risks in recent times threatening our species that it is the confusion of common objective risks and the subjective, heterogenous perception of them as idiosyncratic risks that is the challenging risk management problem for the species. A quote attributed to Mencken captures our concern here from the economics of risk and Vromen's error: 'Complex problems have well known, simple, easy to understand, wrong answers.'

Response to Schultz

We're of course pleased by Schultz's expressions of appreciation for our book, and glad that we perceive similar objectives with respect to accounts of human social and cognitive ecology based on

natural history. However, we're not at all moved to reconsideration by his criticisms of our specific account.

Reference to general human 'risk management' by no means amounts only to suggesting that humans and other animals face challenges of uncertainty in coping with their environments, as Schultz alleges. As indicated in our response to Vromen, there are rich and detailed economic models of risk, and applying them at various scales, from populations to individuals, involves quite specific understandings of a range of concepts including variance, skew, kurtosis, subjective belief about probabilities, learning models, and dimensions of response heterogeneity. Of course in a popular book we kept these technical ideas in the background. But our narrative reflects our framing of problems in terms of them.

Schultz complains that our 'abstract' perspective distracts attention from where the real action is, in cognitive mechanisms of decision. We do not agree with this at all. Ours is a Dennettian stance that emphasises statistically real patterns at population scales. We think that this is the most appropriate scale for explaining the general arc of our species' evolution and construction of ecological niches. In this frame, individual decisions are sources of minor perturbations – essentially noise. The Dennettian stance is a non-eliminativist (i.e. anti-Skinnerian) behaviourism, as is mainstream economics based on revealed preference theory (see Ross, 2026; and also Harrison & Ross, 2024 and Alexeev et al., 2026, for details). This is why, in explaining a phenomenon like gambling addiction, we concentrate on 'situational factors', something Schultz complains about. And we are thorough-going sceptics about the empirical relevance of *any* version of analytic decision theory. Our broad methodological approach and the one favoured by Schultz are not complements, as he suggests, in the way that our story is complementary to one emphasising gene-culture co-evolution, as per our response to Vromen above. Rather, our strategy and Schultz's suggested strategy are methodological rivals.

Schultz claims that we pay inadequate 'attention to the fact that humans differ quite strongly *from each other* in many dimensions'. This charge has things entirely upside down. The models we use to study risk responses in populations emphasise heterogeneity of risk preferences and subjective beliefs (Harrison et al., 2007; Harrison et al., 2022; Harrison et al., 2026; Harrison & Rutström, 2009). Our account of the expansion of human moral communities, and our analysis of the intractable nature of massively upscaled risks such as the current ecological catastrophe, are based on *conflict* between groups of people, which emerge from this heterogeneity.

We reject Schultz's claim that emphasis on the importance of markets 'sits poorly' with the fact that 'some cultures rely very little on trade'. Such cultures have been rendered tiny and vestigial, driven to the margins of the global population, because the growth enabled by long-distance trade crowds out ways of life that do not partake in it. Of course, as per Nagatsu's comments and our response to him, we do not regard this with any sort of triumphalist attitude. Growth through trade accounts for our ecological dominance and population and wealth explosions, but it has now landed us in a looming global disaster. The isolated cultures that 'trade very little' are least to blame for this, and among the most immediately threatened by it.

We are amazed when Schutz attributes to us the view that 'there is convergence of interests concerning climate action, and that suitable institutions and technologies will make it the case that these common interests can be harnessed towards the same goals.' The picture we present in *TGA* is the *opposite* of that view! We predict civilisational collapse as the most likely outcome *because* human interests and circumstances are too diffuse for this upscaled risk to be managed at a global scale. As we document in some detail, humanity fared badly in collectively managing the far more tractable challenge of a global pandemic, when it really *was* a shared goal of almost everyone to avoid extreme morbidity and mortality.

This is also why we consider it unhelpful to represent the ecological threat as a simple tragedy of the commons. Adequate modelling of the crisis requires attention to an array of more complex games at varying geographical, cultural, and economic scales (Ortmann & Veit, 2023; Steel, 2026). Most involve elements of both cooperation and competition. Some will evolve towards welfare-protecting equilibria. A great many will not.

Schultz expresses another error when he comments on our reference to the prisoner's dilemma (PD). As Binmore (1994) has emphasised, it is a kind of litmus test of someone's understanding of economic logic whether they consider it coherent to regard the PD as having more than one equilibrium. It does not. Any game that has any equilibrium other than mutual defection is, as a matter of mathematics, not an instance of the PD. Of course if a PD is repeated among players with reliable memories or records, then, by the folk theorem, anything can happen, so framing an empirical situation as a repeated PD is unhelpful for policy purposes. Humans very seldom play one-shot PDs. Therefore, as we say in the book, the PD, including its *n*-person form, the tragedy of the commons, is of little real use for modelling important empirical cases such as the ecological crisis.

These reflections lead us to the conclusion that, despite Schultz's welcome appreciation of our efforts, we do not share similar theoretical perspectives on large-scale human evolution, culture, or economics.

Response to Kaplan

We regret that Kaplan was induced to read a book he evidently didn't much like and found to be too long; evidently, this was a welfare loss.

Kaplan's main source of jaundice in his review seems to be a dislike of the ground rules of popular science writing. The role of such books is to allow people with (at least ideally) established scientific credentials to express consolidated bodies of their own reflective judgments. Editors of such books allow authors to cite sources, but they insist on two things that depart from academic norms. First, they discourage stringent caveating, as Kaplan notes and complains about. Second, they do not allow rehearsals of dialectics with other academics. Editors think that general readers have no patience for such in-house conversations, and we accepted that our editor knew much more about this than we did. Kaplan would likely have preferred the first draft of our book, but we don't share that hypothesised preference. Our expressed gratitude to our editor is sincere. (We had never previously encountered truly professional and relentlessly detailed editing, which leads us to think that doing a pop science book would be salutary for all academics.) However, our failure to manifest 'very British charm' is our own deficit, as mere colonials (Australia and Canada / South Africa).

Kaplan thinks that *TGA* is not only too long and too brash, but also 'decidedly odd'. (The intended contribution of 'decidedly' in this judgment is undecidable to us.) Unfortunately, this feeling is symmetrical; we find it peculiar that he opted to review a book that is largely founded on specialist literature he doesn't appear to know. For example, he seems unacquainted with the large body of work on 'the Bayesian brain' (Clark, 2026; Griffiths et al., 2024; Parr et al., 2022; Rescorla, 2025). This is a theory about (at least) mammals, not merely humans. Kaplan appears to import the assumption, common in parts of philosophy but not in economics, that inferential power depends on explicit representation of capacities. On the hypothesis that fire services were among the first instances of specialisation for trade, his reported 'quick dip' into the literature missed the wide support among paleoeconomists for that hypothesis, due originally to Ofek (2001).

This problem extends to the philosophical literature. In his criticism of our citations on externalism, Kaplan confuses the *semantic* externalism that indeed originated with Putnam and Burge with *cognitive-processing* externalism. Though indeed Clark and Chalmers (1998) wrote a famous paper on this, it goes back slightly further, to Hutchins (1995), McClamrock (1995), and others. One of us has argued explicitly (Ross, 1994, 2015) that it traces originally to Dennett, and this recognition has not been controversial among core experts. Semantic and cognitive-processing externalism are complementary ideas, but separate and independent in implications. Furthermore, the former is a purely philosophical theory and the latter is based on empirical evidence. Ross (2005 and elsewhere) argues that only cognitive-processing externalism is relevant to economics; we do not consider semantic externalism relevant to any science. In an academic book, one would exhume this trail,

and indeed we did in the first draft of *TGA*. Our editor sent it to the cutting-room floor, and as noted above we think she was right to do so.

We will grant that it was once possible to sell popular science books that observed academic norms much more closely. Dennett's *Consciousness Explained* (1991b) and *Darwin's Dangerous Idea* (1995) are excellent examples. As we can confirm from conversations with peers who have more recently published popular science, those days have passed. It seems that Kaplan clearly regrets this. Our own bias is to respect the demand side of the market.

Conclusion

We close by again expressing our thanks for all the attention devoted to our book by the participants in the symposium. As the reader will have seen, there is considerable variation in the extent to which the five critics share common ground with us. Grayot synopsises our general perspective accurately and sympathetically. Our differences with him turn mainly on a certain kind of philosophical attitude towards the demands it is appropriate to make on scientific concepts. But his focus on complexities concerning agency is astute. Nagatsu shares this same alertness in his reading, but his view of the problem is closer to ours. Our critical dialectic with him settles ultimately on how optimistic it is reasonable to be about the consequences of the next turn in the human risk ratchet. In some respects we are less optimistic than he is, and in other respects more optimistic. But all such judgments are necessarily speculative. With the other three critics we have sharper disagreements. Still, reflecting on them was helpful in providing us an opportunity to elucidate issues on which, to judge from their interpretations, we could have been clearer in the book.

Notes

1. Autopoiesis is the name for the general suite of biological processes involving feedback by which organisms act on their environments so as to allow them to transfer energy across their own system boundaries without fatally eroding those boundaries. It is the basic entropy-resistance strategy of cells, and it includes cellular specialisation in multicellular organisms. Of course, the laws of thermodynamics prevail, and autopoiesis doesn't work indefinitely - cells and organisms age and die.
2. Phelps comes closer than Gordon to identifying the 'reversing scissors' dynamic that Nagatsu draws from *TGA*.
3. We are sceptical about SWF-based approaches to welfare identification and estimation even for humans, but exploring this here would require a lengthy digression.

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