12 Alternative Worlds

Reasonable Worlds? Plausible Worlds?

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12.1 Introduction

Scientists and historians are practically adept at reasoning about alternative possible worlds relevant for their field. The question here is: how do they judge that an alternative world is possible and how do they limit their investigation of 'the possible' so that it does not fall over into 'the impossible'? Given that the gap between possible and impossible worlds is not well defined, and there are no clear lines of demarcation, scientists rely not just on specific, subject-matter, knowledge and more general field constraints, but on developing systematic ways of exploring alternative accounts about how the world works in order to police the boundaries between possible- and impossible-world accounts.

There are two elements in the analysis here: first, to examine how scientists explore 'alternative worlds' – that is, their alternative accounts of the world; then to gain insight into their ways of judging the validity of these alternative worlds in terms that take into account both their theoretical considerations and empirical ones.

The term: 'alternative worlds' is chosen both to deepen and widen the focus on how scientists frame their thinking about possible worlds, and the practices they use to explore and validate them. Sjölin Wirling and Grüne-Yanoff have suggested (this volume) three themes in the literature characterising the essentials of such thinking about alternative worlds and judgements upon them[2]. The first is that the imagination is central to the mode of thinking, the second is that 'background knowledge' plays a part in judging the validity of outcomes, and thirdly, that similarity may lie at the root of such judgements. The argument, and examples, here find that both imagination and background knowledge are surely involved, but questions, and analyses, how they fit together in scientific practices. Imagination comes when scientists thinking out not only alternative theoretical accounts for their phenomena of interest but also alternative descriptions of their actual worlds. Background information is important on both sides – in developing theoretical and empirical accounts. Similarity issues are also critically relevant in the account and examples here, but by modes of direct comparisons (rather than

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via analogical reasoning as Sjölin Wirling and Grüne-Yanoff suggest) in judging the validity of the imagined world accounts not just in terms of possible or impossible worlds but in enabling finer-grained judgements.

One of the usual sites for systemic exploration into alternative worlds is by working with models. Another medium for exploration is, as we shall see, in the use of counterfactuals; and sometimes they work together. In both modes, scientific imagination is needed to think through how the world works in conjunction with background information. Both modes of work provide reasoning tools, and they do so in part by limiting both the imagination and the terrain of investigation to make such reasoning practicable. That combination may produce sensible accounts, but it may also produce non-sensical or impossible world accounts. This raises important questions about how such reasoning and outcomes can be judged – what are the quality criteria that scientists use to judge the reasonableness and plausibility of their model or counterfactual reasoning and outcomes? As we shall see, these characteristics are matters of scientific community practices, rather than laid down as philosophical rules or precepts. The community cases used here come from economics, but their modes of investigation are generic across many fields, and so the analysis is more widely relevant.

The term 'alternative worlds', as we will see, covers a range of things in scientific work, from the world according to scientists' theories and models of it, to the actual world (including its historical development and empirical descriptions), and in both frames, judgements are based not just on outcomes but in terms of pathways to outcomes. Scientists using other modes of investigation of possible worlds may well also use other forms of alternative worlds and use them in different ways. For example, Koskinen (2017) is concerned with 'how-possibly' alternatives to the existing world, alternatives which might be made differently, or fulfil the same function but be constituted differently. Nordmann's account of 'prospective models and modelling' (this volume) is concerned with a more open notion of alternative worlds that speak to the design and build approaches of some of the arts and sciences. These two approaches have resonance with the work of chemists in figuring out the pathways and recipes to synthesise new things in their world.

12.2 Sketching Out Alternative Worlds in Models

Let me start with models to see how they are used to sketch out alternative worlds (and leave counterfactuals till later). Scientists' models come in all sorts, from the model organisms of biology to the mathematical, statistical or diagrammatic models of economics and physics. In these latter formats, models can be considered 'artefacts', a label due to Knuuttila (2011) meaning that they are made-up, constructed objects that help their scientists to think about their worlds. In the 'models as mediators' view (Morrison and Morgan 1999), this generic kind of model is typically constructed from a mixture of all sorts of elements, some theoretical notions, some empirical elements, each

with more or less validity as descriptions of, or claims about, the world. And in this 'models as mediators' account, these constructed/artefactual objects are used to mediate between the theoretical and the empirical knowledge of a science, a role they exercise by virtue of being made up of elements of both and being partially of them both (but not being in any direct sandwich-line between them, nor being shorthand versions of either). Because of this make-up, models cannot simply be judged as purely deductive machines or purely inductive ones. Rather, this mediating function is fulfilled by scientists using their models to explore the implications of their theory in relation to the world it is about, even where that world is only loosely and partially described in their models. It is not just their construction, but their usage that is of interest here. It is in usage that scientists manipulate their models as tools of reasoning to outline their alternative world accounts. It is in usage that those alternative worlds created by their models provide the materials for scientists to judge the validity and usefulness of their models.

The 'models as mediators' account did not offer a general recipe into how that mediating role worked, nor how it should, or could, be characterised in creating alternative world accounts. Various papers in that Morgan and Morrison (1999) collection explored particular examples of such mediation. Marcel Boumans (1999) perceptively observed that bringing in empirical characteristics into a largely theory-driven model created a certain 'built in justification'. Another paper by Ursula Klein (1999) discussed the way paper-tool models enabled chemists to move back and forward between particulars and general levels. R.I.G. Hughes (1999) discussed the Ising model's role across various empirical domains in physics based on his earlier 'DDI' account of the usage of models in science: a three-step process: 'denote' (construct the model), 'demonstrate' (by manipulating the model) and 'interpret' (those outcomes). This was extended in Morgan 2012, to insert 'questions' as the second step (needed to prompt the specific demonstration process and so focus the inferences to follow) and 'narrative' – a broader sense than 'interpret' (intended to link the questions to both the demonstrations and the outcomes, and so mediate between theories and empirics). Mari and Giordani (2014), in a slightly different but helpful generalising move, suggested that the 'models as mediators account' offered scientists two 'tools': "a model is used both as a theoretical tool for interpreting our concepts and an operational tool for studying the corresponding portion of the world" (p 83). Nordmann (this volume, p4) notices the importance of 'what if' questions, as suggested in Morgan 2012, to return the focus onto the model usage as one tool, characterising the mediating activity of using models as "…a free play of 'what ifs' [that] configures a specific relation of model, theory and reality… as one seamlessly moves between the actual and possible [worlds]".

The argument here continues with this characterisation, namely on how the mediating quality of model usage serves both ends at the same time, namely that scientists use models both to explore their theories and their relevance to the world jointly. The point of such model building and using is

to find models that are useful in helping to characterise the world and to answer their questions about the world. They judge the quality of their models in terms of the alternative accounts of the world that the model offers: are those worlds plausible and reasonable or impossible or non-sensical? These qualities are relevant to both the theory and the empirical domains: models need to make sense in both domains. And they are not separate domains, but related ones. Asking questions with the model focusses on how the world might work in theory terms, and so on the explanatory power of model work in opening up alternative world possibilities. But equally, if the model explorations have no contact with empirical domains, they might be quite suspect. Models in usage need to produce alternative world accounts that are theoretically reasonable and empirically plausible[3].

The question addressed here then asks: How does a scientist in a field distinguish between the alternative worlds generated by such model usage? This paper explores two different dimensions of judgement of these alternative worlds found through modelling. The first involves reasoning with a model (formal or informal) to see how the elements of the model knit together along a possible path to an outcome: this looks for and judges the *reasonable*, or perhaps a better label, the *well-reasoned* path and outcome. The second comes from judging the congruence of both the pathways implied by model reasoning and their outcomes with what is already known about those aspects of the world, asking if this is a *plausible world* account, that is: could it plausibly happen, or have happened, like this?

These two characteristics, well-reasoned and plausible, are rather loose criteria. And implying that lines can be easily drawn between the plausible and implausible, and between the reasonable and unreasonable, are equally problematic, because of the difficulty of characterising both the 'reasonable' and the 'plausible'. The distinction I want to create is:

Reasonable – focusses more on the theoretical aspects of the model and its usefulness in providing a well-reasoned account of the possible world-in-the-model in terms that are in line with both existing ideas and well-attested knowledge about how the world works; or in developing theories beyond these boundaries but still ones that can be accepted by the community of the time. Judging this reasonableness involves not only judging the outcome of model reasoning but, more importantly, judging the paths of model reasoning on the way. Think of it this way: models are not black boxes, but reasoning devices in which the scientist can see into and learn from the model manipulations on the way. However convincing are the inputs and outcomes (the assumptions and predictions), if the path of reasoning in the model world produces completely non-sensical points in terms of its subject implications, then that model world might well be judged an impossible world. And even if the inputs and outcomes of the model seem possible, it might be that explorations with the model show some pretty surprising paths.

Plausible – focusses more on using subject matter evidence to judge the model materials. The aim is not only to avoid the impossible world pathways and outcomes that come from models that might be very little constrained by evidence ('common knowledge', or formally obtained by scientific methods); but also to strengthen judgements of possibility from the theoretical side into judgements of plausibility using evidence from the actual world. This is where we find overlap with counterfactual questions and modes of reasoning.

Scientists judge the validity of their models in terms of the alternative world accounts they create: are they reasonable and plausible, or are they impossible, unbelievable, and non-sensical? How does this work in practice? The arguments here are based on the analysis of cases and practices in economics, cases that are often regarded as paradigm cases in their own scientific field, and some have excited the interests of commentaries in philosophy of science.[4] These cases are used to parse out the kinds of criteria that scientists use (or maybe misuse) in judging the usefulness of models in their field. By approaching these cases as model-based explorations of alternative worlds, the aim is to see how economic scientists who use artefactual models judge the plausibility and reasonableness of how these worlds work in the models they create. I stress again, it is the use of models that is key to these judgements, not the model judged as a constructed, but largely passive, object. The arguments begin with a discussion of reasonable or well-reasoned alternative worlds, then of plausible such worlds, and finish with a discussion of practices where reasonable and plausible worlds are found together. This prompts some further reflections on the role of the actual world in defining and making judgements in relation to alternative world accounts, for the mode of investigations may well go along with other notions of alternative worlds.

12.3 Reasoning about the Possibilities of the Model World: Well-Reasoned and Reasonable Alternative Worlds

Let me start with a compressed example, one which has a strangely mixed fictional/factual framing. The Edgworth Box[5] – an innovative diagrammatic model developed in the late 19th century that became paradigmatic to economists - uses the example of Robinson Crusoe as the basis for reasoning about the situation and behaviour of two individuals, isolated on an island, in exchanging labour for goods. An implausible, fictional model-based account? Perhaps. The book of that title was regarded as one of the first novels and so a fictional account, but its prose dressed it up as a news account: a kind of realistic storytelling. In fact, the story was probably based on a real account of a ship's captain abandoned on an island following his crew's mutiny, and who did not make it back to Scotland for many years. Did that fictional/factual aspect of the original situation matter to the evolution of the diagrammatic and mathematical model of that situation, and its reasoning usage,

amongst economists? Surely not. For them, its innovation was to capture the important heart of a knotty problem in economics, namely how two people haggle or reason to get to a point of exchange. The realism of assumptions or accuracy of outcomes (predictions) was much less important to that community than the insight from the reasoning process the diagrammatic model enabled and demonstrated. This was reasoning about how one got from the starting assumptions to the possible outcomes; the path to the best outcome perceived from both sides was not self-evident or direct and depended on the details of the representational model, which enabled economists to see the reasoned pathway to that best outcome. That pathway, and outcome, were then taken to be relevant for the general exchange problem, yet the outcome was only agreed upon because of the conviction given by the economic reasoning middle in using the model. To label this as either a possible or impossible world makes little sense. It was a highly imaginative pathway and solution, translated through an artefactual model into a well-reasoned world in economic terms. And because it was well reasoned, it seemed reasonable to the scientific community of the day.

Of course, it is possible to argue about lots of elements of the practical reasoning processes found in the sciences. And it is difficult to characterise the qualities of accounts and outcomes that make them seem reasonable or possible, though it may be much easier to see them as impossible. In these terms, all such judgements of what counts as reasonable are those tested against and accepted by the community of scientists in the field at the time. There is no outside judgement of what is reasonable except in subject contexts. This is not an attempt to revitalise the notion of 'normal science'. On the contrary. In the Edgworth Box example above, the assumptions, elements, accounts and outcomes were all reasonable for that community – it was the way the assumptions about behaviour and starting points were put together into a new diagrammatic model, and the reasoning with that model, that were seen as novel and unusual and led to new ways to think about the problem. Contemporaries took it as a starting point and developed it because it was considered an original contribution that generated a new direction in terms of both representational device and reasoning mode within its field (evidenced by the fact that historians have documented its continuing innovation over the following decades; see Humphrey 1996).

Another key example of model reasoning in economics, one that starts with an impossible world outcome and is developed in such a way that can suggest a reasonable world outcome, is found in Malthus's account of the population problem of his day.[6] His starting point was to attack the writers of his time (the end of the 18th century) who assumed that mankind and its society were set irreversibly on a path towards a socio-economic utopia. He complained bitterly, and with brilliant rhetoric, against both their rose-tinted visions and their lack of evidence of such an evolutionary path that would convince him, or anyone of sense, about this process and its outcome. Malthus thought they were imagining an impossible future and challenged them

in their own rhetorical terms to show any evidence of the path by which, for example, "a man becomes an ostrich" (an example he chose to match their outlandish claims about social evolution). Malthus eschewed their rhetoric in his own riposte, beginning with sober arguments that enabled him to separate out a possible from an impossible world in the context of the viable future of a population. This distinction was a key element in his argument against the impossibility of their promised future utopia.

Malthus was working before modelling became the standard way of doing economics, yet he had a set of assumptions which he combined to think through the future path of mankind in quite a formal way. He started with two postulates that surely did not sound unreasonable: "That food is necessary to the existence of man" and "That the passion between the sexes…" will continue "nearly in its present state" (Malthus 1798/1976, pp. 19–20). Then, to generate his 'model' reasoning about the future, he argued that population growth would grow geometrically and food supply arithmetically. These modelling assumptions might seem as if they were picked out of thin air, but they rested upon two generic pieces of evidence of his day. One was that the food supply had recently grown rapidly in Britain because of the contemporaneous 'agricultural revolution'; his assumption formed an upper bound, for such further growth possibilities seemed more limited. The population growth evidence was based on the experience of immigrant communities (such as the Amish in the USA) whose population growth was not restricted by limited land and so food supply – another upper bound. These input assumptions were poorly attested by modern standards, but for his period passed as fairly good pieces of evidence of the most optimistic possibilities for both growth rates. His reasoning with these resources (that is, his postulates and assumptions about growth rates), all of which seemed reasonable in themselves yet, when combined, quickly opened up an impossible world outcome when tried out on the British situation. Thus, the population of his time (around 1800) was thought to be 7 million; and his 'model account' of arithmetic versus geometric growth told him that:

- in the first 25 years, population would grow to 14 million, and food output support 14 million;
- in the next 25 years, population would grow to 28 million, but food growth supports only 21 million;
- and, after 100 years, population would grow to 112 million, but food only supports 35 million. (Figures abstracted from Malthus text, 1798/1976, pp. 22–23.)

Thus, while the model world seemed based on a set of reasonable assumptions, its usage in tracing out the future of the population quickly revealed an impossible world.

What was impossible about the future world that Malthus outlined? Note that there was no logical or deductive failure in the reasoning: the arithmetical

calculations told Malthus how many people could be fed and how many would die from starvation (or not exist) in the future world if his assumptions held. But while it is possible to imagine these numbers in the arithmetical model, not so in the actual world that his model was used to explore. Since this implied mass death of the arithmetic world was surely an impossibility in the actual economic world, Malthus asked himself what would happen instead. Using these same starting assumptions of behaviour and evidence but adding in some additional economic arguments about how people behave when there is pressure on food supplies (delaying marriage, etc.), he created an alternative, and now possible, world account. This was a narratively reasoned account, weaving together some other elements of economic consideration into a story of repeating periods of relative well-being and then hungry poverty as the population growth cycled above and below the food supply growth.

Here was an alternative account of how the world worked, and in the process provided a way to save the original assumptions (both theoretical and empirical) and to understand why neither the envisaged utopia nor mass starvation was a likely outcome given the constraints of the actual world. As he pointed out, there was no observable evidence to match his additional reasoning categories; they were the conceptual (or theoretical) categories of economics, and so this example fits well with Mari and Giordani's account of model's mediating work. Yet it was a well-reasoned account, and might be considered entirely reasonable for its period (and reads so since the same conceptual categories are used by economists today). Narrative plays an important role here; it is where the elements of the account are joined up and made sense of together; it is not a description, but rather a causal account, in which the reasoning is made evident. This facility of narrative to make sense of a set of elements in a model and embed reasoning with the model is ubiquitous not just in economics (as we shall see), but in other sciences, particularly the natural sciences, where time is an essential element in explanations[7].

We can find other similar examples in economics where the model's mathematics implies a particular outcome or set of outcomes that are judged impossible in the actual world by the economist. For example, solving an algebraic model of how different generations overlap and manage their economic responsibilities to each other (called the 'overlapping generations model') revealed a negative root, which implied that people lived negative years of life! This is like the Malthus example, where a rational mathematical model implied an impossibility in the economic world envisaged in the model, and in this case, that root was rejected in favour of working with the positive root solution of the model.

Another more complex example leads us into the next section of the argument. This example is less obviously an impossible world example and comes with an algebraic model of the aggregate economy (the so-called 'macroeconomy') by Samuelson in 1939.[8] His little 3-equation model was simulated by hand (this was the first simulation of a model, before computers) by plugging in alternative parameter values and running the model for nine periods.

The question explored was: 'What would happen if the government spent an extra unit in this little model economy?' The economic context was the 1930s, and the model was built to represent Keynes's breakthrough in theorising and policy work. The values of the parameters on this model of his theory were chosen to explore the theory in the model (not because they had been established by any prior statistical work). Different parameter values produced different narrative paths and outcomes for the little economy: cycles, a small increase then stability, or a huge take-off. This final simulation implying a rocket-trajectory would be regarded as an impossible world to any economists of that day, and indeed since. So again, this was a mathematical model possibility, but was just *implausible* for anyone with background knowledge of how far and fast economies might grow, whereas Malthus' model world was more genuinely an *impossible* actual world: so many more mouths than food must mean mass starvation, but it would never have gotten to that outcome as far as economists were concerned, and thus Malthus reasoned out an alternative account.

The point here is not that Samuelson's algebraic model portrayed an impossible world, nor that it was an idealised toy model, nor that the reasoning in the model was at fault. Rather, the issue was that certain posited parameter values in the equations created extremely implausible paths in his explorations with the model; other values created plausible paths. Just because of this finding of variety, Samuelson speculated as to the further possible world results that his model might generate: he solved the algebraic model so as to chart the full range of behaviours, and concluded that almost anything could happen in his model world. His model might, of course, be saved by bringing in more that was known about the world into the model world, just as Malthus had done. For Samuelson, this would have meant by using a parameter value on the equations that were more plausible and so create narrative reasoning (pathways and outcomes) with the model that could be judged plausible. Exploring such plausibility comes next.

12.4 Exploring the Limits of the Actual World: Plausible Alternative Worlds

One of the main foci for judging plausibility is paying attention to the context of the actual world and using data about the actual world in formulating the alternative model world. One place where this is dealt with in quite well-understood ways, according to some well-known recipes, is in the statistical work of economics, labelled econometrics.

In this field, there are standard modes by which the statistical qualities of the model are assessed (goodness of fit, etc.). The recipes for gaining statistical satisfaction have been developed over the past century, and rely in economics on some heavy-duty statistical and probability theorising along with tried and well-tested ways of working with statistical data. That is, they are not one-off, casual methods, but fully informed and tested ways of

judging empirical adequacy. And just like the examples in the above section using arithmetic or algebra – what makes sense in technical reasoning may not make sense in economic terms, either as an account of the pathway or the outcome. Statistical adequacy does not necessarily align with plausibility in the economic sense. Thus, an element initially considered an important cause by the economist could pass the statistical test but be at such a low parameter value to imply it was economically insignificant. In addition, there are the various *ceteris paribus* tests of economic relevance: to judge variables as insignificant because of their marginal relevance, to judge others as irrelevant enough to be omitted, and to locate those variables that remain stable in value in the period whose relevance cannot be judged. Judging the economic qualities of the econometric model requires attention to signals like the size and sign of coefficients in causal relationship models, the patterns in time-series data, the time dynamics of relationships, the internal cross-relationships between different equations of the model, etc. There are also standard recipes for the process of model building: for example, start the modelling simple and make it more expansive to cover omitted variables, or v.v., start with a big encompassing model and slimming it down to get rid of irrelevant factors; testing it out on different groups of agents, or time periods, to assess its range and scope. The framing here is to find plausible models, models that seem to fit the data from the actual world well enough, or satisfactorily enough, to be accepted as working objects for the economist[9].

Another, but less common mode of exploring plausible worlds is by the construction of counterfactual worlds[10]. The most well-known counterfactual for economists is that by Robert Fogel (1964), investigating the importance of the railways in the development of the American economy in the 19th century. This was a tour de force of American economic history, asking the question: What would have happened to American economic growth and development if there had been no railways? It was widely assumed in the 1960s (when he asked this question) that railways had been essential to the growth and the development of the economy – both geographically, and in terms of the growth and distribution of the sectors of the economy. His counterfactual world was designed to 'test' this widely held belief, not by trying to prove it true or false, but to see how far a plausible counterfactual analysis would challenge those beliefs both in the alternative path of history and in its final outcome.

Assuming no railroads could be an extremely far-reaching claim, depending on how far back and sideways this question implied: for example, the counterfactual of assuming no invention of steam technology is not very helpful to Fogel's question for it rewrites so much of the past as to lose contact with the problem he explored. Fogel narrowed his counterfactual question to judge only the effect between the beginning and end of the main growth of the railway system in the USA up till 1890, and by figuring out the alternative costs of transporting the main four agricultural crops of the period by water and by road. But having narrowed down the scope in these directions,

he immediately had to open it up to ask what would have happened in this alternative world: where would people have lived (the alternative settlement pattern, given this period was when 'the West' was opened up); what economic crops would they have grown; and how would they have gotten them to market? With no railways, water and roadways would have been used, but more radically, less of the geographical scope of the USA would have been 'settled' and opened up to intensive economic engagement of the kind that happened during that century. The counterfactual question required the economist to imagine and fill in an alternative world account of what would have happened in those 60 years, so Fogel drew out maps of his imagined new waterway transport routes: designing canals and river-widening for an imagined alternative economy that enabled access to the maximum possible settlement and agriculture.

In other words, even to answer his limited counterfactual question about alternative transport costs, he had to imagine an entirely different history of alternative growth, settlement and distribution patterns within the USA. There was no formal model at work until his final calculations, but rather the application of basic economic assumptions and standard economic calculations about individuals' efficient reactions to circumstances to optimise settlement and output and so the overall growth of the economy – all in very different ways in the counterfactual world from that actual world. That is, his counterfactual world was assumed to work according to the same economic reasoning as the world with railways did, just its development and its outcomes would be very different.

The recipe for using counterfactuals to create 'plausible worlds' is best laid out by Geoffrey Hawthorn (1991), who does so primarily for social scientists and historians, but perhaps it could equally well apply in other sciences. His 3-rule recipe tells the scientist/historian: to choose the starting point of the counterfactual on the basis of facts (not theories) that do not require a rewrite of the past; to discern and make use of the agency of relevant materials in the world that will drive the counterfactual direction from that starting point; and to minimise the use of theories or models unless they fit the question and circumstances very closely. Fogel seems to have followed just such ground rules. In contrast, perhaps as a light-hearted critique of Fogel, McAfee (1983) used a mathematically model-driven starting point to ask what would have happened by 2000 if Columbus had not discovered America but fell off the edge of the flat earth? A non-sensical question (given the knowledge set of 1983!) breaks all of Hawthorn's injunctions for plausible world counterfactuals. In Hawthorn's recipe, it is the strict limitations or constraints on which to base the counterfactual drivers to fill in the counterfactual path that depend on actual world knowledge (not on any theoretical assumptions) to provide plausibility for the alternative world sketched out. In both McAfee and Fogel, the alternative world used for comparison is clear, it is the actual world in its historical development, but with very different recipes, constraints, and so outcomes.

The reaction to Fogel's work, in the context of this discussion about the ability to distinguish between possible and impossible worlds was quite revealing. Economists of that moment (the 1960s) found the final social savings figure that Fogel delivered (the effect of not having railways) was the equivalent of only 1 year's growth, an implausible number to them, because it was so tiny compared to their widely shared belief that the railways had been essential to 19th century American economic growth and development. Most did not so much decry the counterfactual itself, or the methods Fogel used, but rather the extent of the limitations he introduced – tracing out effects for only four crops, no effects on manufacturers, on passenger transport/business travel, on retail distribution, on big finance – that is no exploration of all the backward and forward linkages that these other economists had been busy writing about for many years. For those economists, Fogel's alternative world picture was only a small part of the story, perhaps it set a lower-bound to calculations of the effect of the railways, but it missed out too much for their comfort. (There were also arguments about the precise nature of the calculations, but these are not especially relevant for this argument on possible vs impossible worlds.)

This alternative counterfactual world he had created was not considered 'impossible' in any technical sense for it was made up of plausible 'factual' alternatives that his contemporary economists recognised as such. Although, taken together, they created a quite startling alternative world that was considered implausible because radically incomplete. But neither did it seem entirely 'plausible' as an alternative imagined world. Here, the actual world and its factual characteristics play a stronger role than in the calculus of Malthus's alternative world. In Fogel's case, the factual characteristics both constrain and shape the alternative world, and it is the measured aggregate outcome that was regarded as implausible, not the counterfactual world analysis as it goes along. It was not nonsensical – in the sense of negative-lived lives – nor impossible in the sense of unimaginable in the actual world as in the implications from the calculations of Malthus's simple 'model'. Rather, it was simply the incomplete answer to the counterfactual question: plausible as an account of some of the pathways, implausible (because incomplete) in its final full outcome.

An alternative counterfactual practice of creating alternative worlds provides a contrast. These are ones that explicitly set out to explore alternative factual worlds in as broad a way as possible to reflect both more deeply and more broadly on the actual world. This is the agenda for a group of papers that provide a strong contrast to Fogel's work, that appeared under the title *Unmaking the West* (Tetlock et al. 2008). The basic question is to rethink the rise of the West, politically and economically, by exploring if that account can be undermined in some critical ways or at some critical points by tracing out ways in which it might have happened differently. How could it have been that the West did not become the dominant economic and political power over the last 300 years or so, but that China and Asia did? There were

good reasons for this framing, as recent literature on Chinese economic history, and as the current position of China and India in the world's industrial economies reflect.

This is not about exploring how else the West might have been made, but how the West might never have been made. So, the book opens with an imagined group of Chinese scholars, working in an imagined world in which the Chinese had been the dominant culture and power over the past centuries, being asked to imagine some small key or 'hinge' events where their own (fictional) ascendency might have been stymied. The contributing authors to this volume (grounded in politics, economics, and history) were asked to trace the economic, social or political historical outcome that might follow from such a very small, but significant, hinge event in Western history that might have led to that imagined Asian ascendency. For example, what if England had remained Catholic in the 16th century? What if Britain had no coal, no colonies, and no modern science? Would any of these hinge events have created a path in which there was no industrial revolution in Britain? Tracing alternative worlds from these 'turning points', leads each writer to 'create' a set of changes in the world, and then trace out second and third-round counterfactual changes. Maybe these further changes are reversionary – taking the economy or society or polity back onto its original path, or maybe they are expansionary – taking the alternative world ever further away from the actual world.

The group of writers was asked to abide by "three exacting quality-control questions" (p. 9), but in comparison with Hawthorn's recipe, they are not so 'exacting'. Their first 'minimal rewrite rule' requires the counterfactual to start with a very small change in the historical record; this hinge event is rather like Hawthorn's first rule. But thereafter, the constraints are not so evident; rather, the basis for creating these expansionary alternative worlds is to follow the causal implications of such hinge events being different – what leads on to what and how. Rather than to constrain or limit the counterfactual, the recipe here gives full rein to the use of reasoning in tracing through implications far and wide. These are not just exercises in description, for there would be no alternative world spun out without framing the considerable differences that occur in the narrative accounts, i.e., in loosely causal or associational accounts of where and how those differences would create that alternative path. In other words, narrative reasoning about the path was needed to justify the alternative world outcome as in Fogel's case, but now it is an account much more loosely constrained by history, whereas by contrast, McAfee's highly imagined counterfactual history is led by his mathematical model world.

We are back in a sense to the 'anything can happen', or rather 'anything can have happened'. However, the constraints here are not model-based constraints or even strong factual constraints, but alternative fact construction using social science and historical knowledge and methods. How should we label these alternative worlds found in *Unmaking the West*? Given the first

careful choice of hinge events as starting points for their counterfactuals, we might label them possible worlds, but their multiple further rounds of counterfactual possibilities, spinning out further paths that take them ever further from actual events, might suggest we should label them implausible (not impossible, because they are well traced out alternatives). By contrast, the tight factual constraints in Fogel's counterfactual world enabled him to judge, even measure, the difference from that single first counterfactual choice, and so judge the path taken by his alternative factual world as plausible, even while his contemporaries found the counterfactual world he created incomplete, and so the outcome implausible.

It has become apparent from these counterfactual cases how important the contrast or comparison element is in judging the validity of alternative world accounts, both in reasoning and in plausibility terms[11]. In some of these cases, the comparison is provided by history as the alternative to the actual world, yet the modes of doing so vary from tracing out one very 'factual' alternative world (Fogel's work) to numerous imagined alternatives (in imagining the economic development of the West compared to Asia) to opening up a completely different past alternative that has fragile connections and so comparisons with actual history (McAfee's case). In all these cases, history is the actual world, and the counterfactual alternatives seemed to be judged mainly on plausibility grounds, but the reasoning is not absent – those counterfactuals have to be driven by some economic agency that needs to be filled in and some reasoned pathways that may not be so evidenced. That economic reasoning side is more obvious in Malthus modelling case: we are led to imagine (with difficulty) a very different alternative future history, driven by the model assumptions and reasoning to an initial implausible outcome that is then saved by further economic reasoning. But history is not the only contrasting world that is useful in making these judgements. The little theoretical mathematical model worlds that generate variations in pathways and outcomes (the examples of model simulations, negative lives, etc.) tell us that much less specific background knowledge of the world is equally valuable in providing contrasts and comparisons that inform judgements about the alternative world built in such models.

In these economic cases, historical actuals have played an important role, but the actual worlds of other sciences may not be historical ones. Many sciences are involved in making their own alternative actual worlds: synthetic biology, chemical synthesis, drug 'discovery', etc. Here the key task may be to recreate or synthesise something that already exists in the world, or it might be to create something new that has certain qualities, i.e., to turn a possible object into an actual world one. Here, the task is to design a possible object and then to 'engineer' that something into an actual world object. Perhaps the possible object is the same as one that already exists, but is not yet something that can be made; perhaps it is a slightly different alternative actual, or perhaps the problem it is to find an alternative pathway or recipe to making an already recognised actual. This is a minor part of economists'

work – but one such example is the case of auctions, where the use of theories and close study of how different kinds of auctions work were used in order to design auctions to attain certain aims. These fit the 'how possibly' creative work of synthetic biology discussed by Koskinen (2017), who also points out the importance of information that comes from learning how things can not be done (p. 504). Here, the contrasting/comparative actuals are not historical contrasts, yet if we take the range of approaches in chemical or biological synthesis as indicative, it seems that the range of both possible and actual alternative worlds could be pretty unconstrained. This could well extend the analysis of the modes of creating and judging possible alternative worlds as used by scientists.

12.5 Triangulation: Where Well-Reasoned and Plausible Worlds Come Together

The first recipe discussed above was to use the reasoning facility that comes with a model to see if it created reasonable and well-reasoned worlds, alternative worlds that were possible in those senses, but that reasonableness may also have included some fit to the actual world in order to have the judgement of being a plausible world. The second recipe was to use the factual aspects that come with 'applying' models to see if the evidence-based reasoning with models looked plausible and produced plausible outcomes. Of course, a model usually embeds some theoretical claims or concepts, so that reasoning with them is not 'theory free'. In other words, we can see two elements of fit being used here in both approaches. Yet the theory-side is not, as Mari and Giordani argue (above), only an exploration of the concepts in the theory side but might rather be an exploration of the reasoning the theory entails or allows, and judgements of reasonableness that are not ones of logic but of fit with some knowledge of the actual world as in Malthus. It is important that both the pathways found in using the models, and the model outcomes (predictions, final points, etc.) need to be considered in these judgements of reasonableness and plausibility; and I repeat, these are judgements of the community of scientists involved based on their wider field knowledge. This third recipe is perhaps not so much a different recipe as just more explicit in its use of both resources – a triangulation process between the model, its reasoning resources, and the evidence base relevant to the model – to judge the quality of the model according to the alternative worlds its usage suggests.

'Analytical narratives' offer one such version for generating alternative worlds in the social sciences, ones in which there is a back-and-forth pattern of analysis between model reasoning and qualitative evidence, using the models to analyse the factual and using the factual to constrain the particular choices within versions of a model and between different models. The model here effectively stands in as a shorthand to explore alternative accounts of 'reality' against the 'narrative', which is offered by qualitative evidence of

what actually happened. The introduction and usage of this mode of working in political science has been primarily engaged in using game-theory models to explore the roles of institutions in political economy. They largely address questions about why political institutions emerge and work differently in different contexts, different times and places, and in solving different problems. This is not a way of testing any general theory, or making big wide claims, but a means, again, of exploring how the world works by triangulating between evidence of the actual worlds and reasoning about alternative theoretical models as possible designs of institutions and their impacts[12].

Another version of this 'analytic narratives' recipe (with a slightly different name) is found in economic history, which takes a sequential view of the role of models and narrative evidence (Bates et al., 1998). That is, a question is raised about why and how some economic institutions formed, disappeared, or perhaps collapsed. For example, Why did the possibility of buying out from military conscription disappear? Why did absolute monarchies disappear? What accounts for the formation and subsequent collapse of an international coffee cartel? The latter account (Bates, 1998) begins with a narrative evidence account of that cartel formation and then applies a game theory model to explain that formation. Bates then proceeds through an alternating sequence of narratives, statistical evidence, and different models to capture the formation, history, and then collapse of the cartel. Why is the sequence needed? Because one model is not sufficient to explain the whole historical evidence sequence, rather, different models provide analysis for different aspects at different periods. This sequence of model application to evidence, followed by a new model applied to the remaining evidence, etc., continues until the scholar is happy that the narrative of evidence is covered by the sequence of models, and so the whole 'explanation' of events is achieved.

These model-narrative-evidence recipes are best understood as exploring the actual world by systematic use of alternative worlds embedded in models – either in statistically evidenced models or in theory-based models – that can be applied in formal and informal ways to explore alternative explanations for the specific phenomena at issue.

Another set of examples shows this triangulation at work in economics research, which aims to deal with different sets of model paths and outcomes within the same domain that ought to align, but initially fail to do so. Here models are used to explore the misalignment of model-evidence directly by counterfactual methods, leading both to developments in the model to adapt to the problem set, and enquiries into the data to choose (or develop) data that is more closely relevant to the problem question in hand. The salient point here, and the difference with the counterfactuals discussed earlier (such as Fogel's case), is that here, both sides of the comparison are model worlds (that is, the alternative to one model account is another model account of the same or a parallel world), not the development of a fictional alternative world to a factual one that existed.

Ramey's work (2019a,b; Orchard et al. 2023) offers a variety of examples using counterfactuals in this way that she calls 'plausibility tests'. The context for her work (often with collaborators) was provided by US government interventions over the past two decades designed to support the economy in times of crisis. This was done by paying out either tax rebates (or direct grants) to households to support them, or massive government spending programmes; both kinds of actions were taken in order to avoid a major economic downturn. The problem she investigates is where different models produce inconsistent results for the same event, or produce extremely unlikely results when comparing model outcomes from similar actions at different times. Her basic approach is to see these comparative model sites as implying or raising counterfactual questions and then to pursue the calculations to investigate the lack of fit between closely comparable model results and so 'test' the plausibility of one model's result with another model's results. These investigations created a positive programme of how to search for reconciliation that Ramey has labelled a "Macro Counterfactual Plausibility Analysis".

One of these cases examined is the effect of the 2008 tax rebate in the US,[13] in which a key number that characterises the individual household effect (i.e., at the 'micro' level) was found to disagree very considerably with the parallel number that is taken to characterise the aggregate economy effect (i.e., at the 'macro' level). It is not entirely clear that they should be exactly aligned, but the wide differences in statistical work produced considerable surprise amongst economists. The team developed counterfactual questions to explore the two alternative model-world accounts at work here, i.e., as portrayed in the micro and macro models. So neither was taken as the fixed-base case (i.e., equivalent of the actual world history as in Fogel's counterfactual that explored an alternative fictional world) – rather interpretation and numbers in both accounts were potentially revisable since both were model accounts.

A simple analysis of the macro data for this 2008 event suggested that despite this sudden increase in income for households, there was no sudden equivalent spike in aggregate spending. This was not initially seen as problematic, the inference being that households saved rather than spent their sudden windfall. But research soon showed that if one studied the micro-household data, it seemed that, indeed, households did respond with a spike increase in their spending, and the majority of the spike went on buying automobiles rather than normal household goods. On their own, the household findings also made sense to economists: households use the rebate to buy a durable good, not more everyday consumption. But in putting these two findings together (each of which made separate sense for economists focussing on micro and those concentrated on macro problems and their data), they were not just surprising, but suspect: why don't they agree? Were these alternative model-world pictures consistent, or impossible? Suspicious of the results, these economists calculated a simple counterfactual on

the micro-model, ignoring the macro: 'imagine no tax rebates' (here using a fictional counterfactual), and found that without the tax rebates, there would have been a very large fall of 87% spending on automobiles at the micro-level according to the model and data. The equivalent macro counterfactual calculations assuming no rebates suggest that the household expenditure on autos would have dropped from $17bn in March 2008 to $3bn in June, whereas the actual lowest level throughout the period was $12bn in April 2009. As Ramey's group expressed it: "This counterfactual [outcome] strains credulity" (Orchard et al. 2023, p. 2). Clearly, that set of results does not make sense when taken together. A further counterfactual exercise on the same topic generated an outcome labelled "preposterous"(Ramey 2019a, 125)!

To make better sense of this 2008 episode, the counterfactual question then changed to ask: 'what if the micro number were used in recalibrating a standard macro model?' What kind of counterfactual path would this counterfactual macro model reveal by plotting the difference in spending paths between the two models – the standard one and the recalibrated one? The results suggested that the counterfactual micro-model path and outcomes are implausible because various historical events that, evaluated post hoc (10+ years later), would have made the micro estimates implausibly high, and so brought down the difference between the counterfactual micro-model path and outcomes compared to the actual macro-model path and outcomes. At the same time, looking at the data with 'improved econometric techniques' meant some adjustments that lowered the micro parameter better to fit the macro measurement.

In this kind of counterfactual work using models on both sides of the comparisons, both the data and the model are potentially adaptable. This search for plausibility and reason in modelling and results comes from a search for alignment in both the theoretical and empirical domains. Both data and models need to be 'the right tools for the job', that is the right tools for answering some particular question put to a model about a specific problem or situation that has arisen in the actual world. The use of counterfactual questions, and exploring their answering paths and outcomes, provide information on what is not plausible and so prod triangulation efforts to align the right data and right model for the specific question to be answered, which may itself only be revealed during the alignment process as the scientists learn more and more about the situation. Such alignment offers another criterion for the validity of the model reasoning and the plausible explanation of the world using model alternatives. In general, these triangulations involved searching out data more relevant to the problem, and the use of models compatible with the counterfactual questions and the specifics of the data. Plausibility then comes not only in the agreement of the numbers or paths with fictional or real historical events treated as comparable counterfactuals, but confidence in both data and model being more carefully aligned to the question. Here we see very practical ways in which models are

used as a thinking or mediating tool in exploring how the world works by triangulating between alternative theoretical worlds and alternative actual world materials.

12.6 Conclusion

This account began with the problem of how to distinguish possible world accounts in sciences from impossible ones in such a way to validate the former as useful accounts. Using a more neutral terminology of 'alternative' worlds, it quickly became clear from the economic examples that there are some worlds outlined using modelling or counterfactual approaches that are possible but implausible or incomplete in some way, and some that are technically possible but non-sensical or unbelievable in subject matters for various reasons. Developing the terminology of alternative worlds, and their validation, prompted judgements based on the reasoned implications and the plausibility of those alternative worlds with respect to field knowledge. Alternative worlds could be validated as plausible worlds and well-reasoned worlds with respect to theoretical and empirical knowledge in the field, and preferably both.

It is perhaps worth noting that economists following philosophy of economics injunctions, have traditionally worried about the assumptions that go into a model or the accuracy of predictions from a model, and validated them on those grounds. This is beginning to change, as we can see from the triangulation work of the final section (for example, in the work of Ramey and crew), but new criteria appropriate for modelling and counterfactual work have not been worked through. As suggested here, the appropriate criteria for models and counterfactuals are much broader than those earlier recipes: the focus is not just on the beginning or end, but on the value of the account all the way through the middle, i.e., through the reasoning middle. This does not mean assumptions and predictions do not matter, but they are not the only things that matter, rather that exploring alternative worlds with models and counterfactuals may be a much more informative process about both theories and our world than only one or the other. The counter-factualist spinning alternatives to the actual worlds might well change their earlier interpretation of the events of the actual world, just as the modellers might well revise their theories from such alternative-world explorations. Maybe the railroads did not really matter that much to US economic development; maybe highly idealised models or well-trained econometric models don't tell you much about processes or outcomes. But exploring the possibilities of alternative worlds with models and counterfactuals may well reveal much about both the actual world, and about the kind of theory that offers a reasonable and plausible account of that world.

How should this exploratory process be characterised? What kind of ingredients are involved? Sjölin Wirling and Grüne-Yanoff's survey of the literature (see earlier and in this volume) suggested imagination and background

knowledge were involved. The argument made here is that spinning alternative worlds requires *both* imagination *and* background knowledge *and that they must work together to be effective*. The background knowledge evident in our cases encompasses lots of different kinds of things, from very specific subject matter to quite general matters, from laws and theories to formal and informal empirical knowledge, and comparable case knowledge. Imagination is also clearly a part of the recipe of generating alternative world accounts, perhaps more so with counterfactuals where there is potentially greater freedom to spin alternative worlds than in working with models where the assumptions and structure of relations fix certain boundaries.

Spinning alternative worlds in models and counterfactuals with lots of imagination but no background knowledge is a recipe for creating impossible and implausible outcomes: plausible and possible worlds may come from luck, but not from design. From the model side: internal coherence in a model is clearly a plus, but not enough on its own to generate plausible and possible worlds rather than falling over into impossible ones. From the background knowledge side, factual or theoretical knowledge from the scientist's background, either in modelling or in counterfactuals, is not enough to generate the additional knowledge that reasoning through alternative worlds in modelling or with counterfactuals can create.

What we find in the cases discussed is not only the combination of imagination and background knowledge, but also the importance of limitations, either framed by the dimensions and design of the model itself, or the particular decisions in counterfactuals. Without limits on the imagination, anything can happen. Background knowledge, theoretical and factual, does not only provide those constraints. Rather, it is what enables scientists to use models as exploration tools, tools to explore the possibilities of different values of parameters, of different sets of elements, of different design decisions, etc. Using models in this way enables scientists to test out the implications of their imagined world. But it is the limitations given by the model or the counterfactual question or the background knowledge that make the feedback from alternative imagined worlds useful in figuring out the validity of the model (what range of phenomena are covered, etc.; what happens when you change something in the model, etc.). Learning that something that is impossible in the model world is often as useful as learning that something is possible, just as in learning about the im/possibilities in the actual world. Imagination is needed not just to construct models and counterfactuals, but to work through their implications, so explorations with models and counterfactuals are indirect tests of our imaginations. Is that world really credible, really possible, really plausible? How do we judge – by using the imagination in various ways to test the construction, but also to test the model's usefulness in relation to how the world works? We use our models to explore our imagination of alternative worlds that might be possible, and we use our imagination to test out the boundaries and possibilities of our model or counterfactual world.

Notes

1 Thanks go the participants at the 'Modelling the Possible' workshop hosted by Vienna in 2021, especially to Tarja Knuuttila and Rami Koskinen, Till Grüne-Yanoff, Andrea Loettgers, Alfred Nordmann, Mauricio Suarez, Paul Teller, and others. Thanks also go to two anonymous referees whose thoughtful comments helped me to revise the paper.
2 See references therein for the most salient philosophy of science literature on possible worlds; further references are also found in two accounts that have parallels with mine here but are about designing and engineering alternative worlds in science: namely Nordmann (this volume) and Koskinen (2017).
3 The problem is framed here as an account of the practises of using models, and how scientists learn from them, not about a particular kind of model, nor about conceptualising and/or defining the philosophical qualities of possible world models, or subclasses of such possible-world models (as, for example, in Grüne-Yanoff and Verreault-Julien, 2021).
4 Where these are cases I have worked on before, full accounts, and references to relevant philosophical literature, can be found in the references given.
5 Discussed fully in Morgan (2012, Chapter 3).
6 For a full account, see Morgan (2021).
7 See Morgan (2022), the introductory chapter of Morgan, Hajek, and Berry (2022) on 'narrative science'. Several other chapters in that book are particularly concerned with the narrative reasoning that goes on with models: John Beatty on reasoning backwards in evolutionary theory; Paula Olmos on the narratives of 'just-so reasoning'; John Huss on competing model-based explanations of extinctions; and Teru Miyake on earthquake narratives. For a 'handbook' account of the relationships of narratives to models, see Morgan (2024) and for economics examples, see Morgan and Stapleford (2023) and particularly Biddle (2023).
8 This case, and some comparable ones, are discussed in Morgan (2012, Chapter 6).
9 There is a vast literature about all this inside econometrics; for two papers which provide a small entry into some of the issues from a viewpoint compatible with this paper, see Morgan (1988) and Boumans and Morgan (2001).
10 Of course there is a huge literature on counterfactuals in philosophy of science; I restrict coverage here to their usage in fairly specific traditions in social science. The case of Fogel's counterfactual is analysed in detail in Morgan (2014).
11 I thank one of the referees for pushing me to think more clearly about the 'alternative' worlds and their role in comparisons that drive the judgements of both counterfactual and model worlds.
12 See for example papers by Quack and Herfeld (2023) and Skarbeck and Skarbeck (2023), and references therein.
13 Another site for these plausibility tests was judging the economic outcomes of the American Recovery and Reinvestment Act (ARRA) of 2010, see Ramey (2019b).

References

Bates, Robert. 1998. "The International Coffee Organisation: An International Institution." In Analytic Narratives, edited by Robert Bates et al., 194–230. Princeton: Princeton University Press.

Bates, Robert et al. 1998. *Analytic Narratives*. Princeton: Princeton University Press.

Beatty, John. 2022. "Narrative Solutions to a Common Evolutionary Problem." In *Narrative Science*, edited by Mary S. Morgan, Kim Hajek, and Dominic Berry, 405–23. Cambridge: Cambridge University Press.

Biddle, Jeff. 2023. "Narratives and Empirical Strategies in Zvi Griliches' Early Research." *Narrative in Economics: Historical Experiences*, edited by Mary S. Morgan and Thomas Stapleford, Special Issue of *History of Political Economy*, 55(3): 447–70.

Boumans, M.J. 1999. "Built-In Justification." In *Models as Mediators*, edited by Mary S. Morgan, and Margaret Morrison, 66–96. Cambridge: Cambridge University Press.

Boumans, Marcel, and Mary S Morgan. 2001. "*Ceteris Paribus* Conditions: Materiality and the Application of Economic Theories."*Journal of Economic Methodology* 8(1): 11–26.

Fogel, R.W. 1964. *Railroads and American Economic Growth: Essays in Econometric History*. Baltimore: Johns Hopkins Press.

Grüne-Yanoff, Till, and Philippe Verreault-Julien. 2021. "How-Possibly Explanations in Economics: Anything Goes?."*Journal of Economic Methodology* 28(1): 114–23.

Hawthorn, Geoffrey. 1991. *Plausible Worlds*. Cambridge: Cambridge University Press.

Hughes, R.I.G. 1999. "The Ising Model, Computer Simulation, and Universal Physics." In *Models as Mediators*, edited by Mary S. Morgan, and Margaret Morrison, 97–145. Cambridge: Cambridge University Press.

Humphrey, Thomas. 1996. "The Early History of the Box Diagram." *Federal Reserve Bank of Richmond Economic Quarterly* 82(1): 37–75.

Huss, John E. 2022. "Mass Extinctions and Narratives of Recurrence." In *Narrative Science*, edited by Mary S. Morgan, Kim Hajek, and Dominic Berry, 61–81. Cambridge: Cambridge University Press.

Klein, Ursula. 1999. "Techniques of Modelling and Paper-Tools in Classical Chemistry." In *Models as Mediators*, edited by Mary S. Morgan, and Margaret Morrison, 146–67. Cambridge: Cambridge University Press.

Knuuttila, Tarja. 2011. "Modelling and Representing: An Artefactual Approach to Model-Based Representation." *Studies in History and Philosophy of Science* 42: 262–71.

Koskinen, Rami. 2017. "Synthetic Biology and the Search for Alternative Genetic Systems: Taking How-Possibly Models Seriously." *European Journal of Philosophy of Science* 7: 493–506.

Malthus, Thomas Robert. 1798. *An Essay on the Principle of Population*, edited by Philip Appleman. Critical Edition. New York: Norton, 1976.

Mari, Luca, and Alessandro Giordani. 2014. "Modelling Measurement: Error and Uncertainty." In *Error and Uncertainty in Scientific Practice*, edited by Marcel Boumans, Giora Hon, and Arthur C. Petersen, 79–96. London: Pickering & Chatto.

McAfee, R. Preston. 1983. "American Economic Growth and the Voyage of Columbus." *American Economic Review* 73: 735–40.

Miyake, Teru. 2022. "Reasoning from Narratives and Models: Reconstructing the Tohoku Earthquake." In *Narrative Science: Reasoning, Representing and Knowing Since 1800*, edited by Mary S. Morgan, Kim M. Hajek, and Dominic J. Berry, 104–121. Cambridge: Cambridge University Press.

Morgan, Mary S. 1988. "Finding a Satisfactory Empirical Model." In *The Popperian Legacy in Economics*, edited by Neil. de Marchi, 199–211. Cambridge: Cambridge University Press. (Reprinted in *The Philosophy and Methodology of Economics* Ed: B. Caldwell (Edward Elgar, 1993).)

Morgan, Mary S. 2012. *The World in the Model: How Economists Work and Think*. Cambridge: Cambridge University Press.

Morgan, Mary S. 2014. "What If? Models, Fact and Fiction in Economics." *Journal of the British Academy* 2: 231–68.

Morgan, Mary S. 2021. "Narrative Inference with and without Statistics: Making Sense of Economic Cycles with Malthus and Kondratiev." *History of Political Economy* 53(SI): 113–38.

Morgan, Mary S. 2022. "Narrative: A General Purpose Technology for Science." In *Narrative Science: Reasoning, Representing and Knowing Since 1800*, edited by. Morgan, Mary. S, Kim M. Hajek and, Dominic J., Berry, 3–30. Cambridge: Cambridge University Press.

Morgan, Mary S., and Margaret Morrison, eds. 1999. *Models as Mediators*. Cambridge University Press., *Ideas in Context* Series.

Morgan, Mary S., Kim M. Hajek, and Dominic J. Berry, eds. 2022. *Narrative Science: Reasoning, Representing and Knowing Since 1800*. Cambridge: Cambridge University Press. Open Access.

Morgan, Mary S, and Tom Stapleford, eds. 2023. *Narrative in Economics: Historical Experiences* Special Issue of *History of Political Economy*, *55*(3).

Morgan, Mary S, and Thomas A Stapleford. 2023. "Narrative in Economics: A New Turn on the Past." *Narrative in Economics: Historical Experiences*, edited by Mary S. Morgan and Tom Stapleford, 395–422. Special issue of *History of Political Economy*, *55*(3).

Morrison, Margaret, and Mary S Morgan. 1999. "Models as Mediating Instruments." In *Models as Mediators*, edited by Mary S. Morgan, and Margaret Morrison, 10–37. Cambridge: Cambridge University Press.

Nordmann, Alfred. 2023. "Prospective Modeling" (this volume).

Olmos, Paula 2022. "Just-So What?". *Narrative Science: Reasoning, Representing and Knowing Since 1800*, edited by Mary S. Morgan, Kim M. Hajek, and Dominic J. Berry, 424–44. Cambridge: Cambridge University Press.

Orchard, Jacob, Valerie A Ramey, and Johannes F Wieland. 2023. "Micro-MPCs and Macro-Counterfactuals: The Case of the 2008 Rebates" Technical Report, Working Paper, June 27, 2023.

Quack, Alexandra, and Catherine Herfeld. 2023. "The Role of Narratives in Transferring Rational Choice Models."*History of Political Economy* *55*(3): 549–76.

Ramey, Valerie. 2019a. "Fiscal Policy: Tax and Spending Multipliers in the United States." In *Evolution or Revolution: Rethinking Macroeconomic Policy After the Great Recession*, edited by Olivier Blanchard, and Lawrence H. Summers, MIT Press and the Petersen Institute for International Economics, 225–254. Cambridge, MA: MIT Press.

Ramey, Valerie. 2019b. "Ten Years After the Financial Crisis: What Have We Learned from the Renaissance in Fiscal Research." *Journal of Economic Perspectives* 33(2): 89–114.

Samuelson, P.A. 1939. "Interactions between the Multiplier Analysis and the Principle of Acceleration."*Review of Economics and Statistics* 21: 75–8.

Sjölin Wirling, Ylwa, and Til Grüne-Yanoff. 2024. "Through the Prism of Modal Epistemology: Perspective on Modal Modelling" (this volume)

Skarbeck, David, and Emily Skarbeck. 2023. "Analytic Narratives in Political Economy." *History of Political Economy* *55*(4): 609–38.

Tetlock, Philip E., R. N. Lebow, and G. Parker, eds. 2008. *Unmaking the West*. Ann Arbor: University of Michigan Press.