Decision-Making under Uncertainty[1]

I. The Current Research Landscape.

A. Real-World Decisions.

1. In 2017, the UK research councils set up two networks (CRUISSE and M2D) to collaborate "with practitioner organisations to analyse real world decisions and identify where multi-disciplinary research can develop new approaches to improve decision making under uncertainty." The EPSRC described its investment as a "response to a demonstrable call from businesses, policy makers, regulators and other institutions who must make risk-based judgements considering multiple possible consequences, that factor in uncertainty."
2. How could the research community respond to this challenge from the users of research? The CRUISSE network approach has been to focus on users' needs and to work with them to articulate how research might help them with the decisions they faced. This included, amongst others, variations on the following themes: Should they expand their company into a new product or technology? How should they prepare for yet unforeseen cyber-security threats? What significant infrastructure should be built? What to do to alleviate the impact of climate change? What level of research, training and innovation spending to choose for maximum benefit of the UK economy? How to change the planning system to increase affordable housing? Which investments to prioritise in the health service? How to mitigate humanitarian crises? When to restrict growth in credit to protect financial stability? What levels of immigration to allow?[2]
3. Challenges like those in [2] typify many decisions that must be made in business, policy and regulation in that they have features of what we call *radical uncertainty*. Decision makers know that it is *impossible* to imagine all choices or to calculate all the significant possible outcomes of the one selected. They know that their decisions have complex and interacting consequences that are hard to foresee. Moreover, because the decisions will have a big effect on the future well-being of their companies, society and the overall economy, they are also "high impact" eventually with high visibility. In less technical language, many of these problems are *Wicked Problem*s (53), and clearly require ideas, findings and expert knowledge from a multiplicity of disciplinary perspectives.
4. We refer to decisions of the kind in (2) as made under *radical* uncertainty, rather than just uncertainty, because terminology has become confused both within and between disciplines[3]. Uncertainty is "*radical*" because decisions like those listed above are often so novel or complex that the relevance of existing knowledge and data *is questionable*. To decide requires imagination and judgment that is cognisant of a dynamic, complex and interconnected world in which, although we cannot be sure what will happen, we can be certain that unexpected Big Surprises happen.
5. Below we examine the landscape of academic research related to real-world decision-making under *radical* uncertainty by drawing a distinction between (a) research on *optimal choice* outcomes and (b) research on *processes* of actual decision making. Whereas the former refers to research that explores what it is *best* to choose, the latter refers to research into the process of *making choices,* hopefully better ones. We will show that although there is much research using an *optimal choice framework* (OCF), there is comparatively little which examines *decision process* in real world contexts under *radical* uncertainty and almost no research into the two combined. The landscape is

[1] Note. [] in the text cross-refers to paragraph numbers. () cross-refers to the citations at the end of the document.
[2] This list of decisions is an extended version of the list published in the EPSRC research call. See: https://epsrc.ukri.org/research/ourportfolio/themes/mathematics/introduction/decision-making-under-uncertainty-networks/
[3] Terms like Knightian risk, Knightian uncertainty, ambiguity, ontological uncertainty, epistemic uncertainty, truth uncertainty, semantic uncertainty, deep uncertainty, etc., can be found, not always with the same meaning, in social sciences. Terms like imprecision, ambiguity, intractability and indeterminacy are found in the natural sciences. We choose the term "*radical* uncertainty" and distinguish it from (probabilistic) risk to avoid confusion and in agreement with the use of both terms by John Kay and Mervyn King (Kay, 2015; King, 2016; Kay and King (forthcoming).

surprising because a feature of facing the challenges in [2] is that there may not be sufficient information to define (much less identify) an "optimal" choice.

B. Research Supply: An Optimum Choice Framework (OCF)

6. Quantitative techniques, going back to the early operations research developments in WW2, have proved very valuable for helping real-world decision-makers to make choices - formal experimentation with different ways of painting aircraft, for example, allowed air force officers to make choices that saved the lives of many of their bomber pilots. Much more ambitious formal modelling of supply chains and optimum pathways followed. Such techniques, merely by creating awareness of the complexity of the chain and working through what assumptions, data and judgments were needed, could improve the decisions made on all scales. Similarly, the collection and analysis of statistics about medical treatments, test results and mortality provide vital information about health choices; the collection and analysis of data about the likely costs and benefits of a local new road can guide planning judgements; weather forecasters can guide farmers on when to harvest; and in general there are many cases where the relevant parameters can be easily identified, calibrated and encompassed within a model which has a high degree of external validity.
7. However, such risk management techniques and cost-benefit analyses[4] derive from an OCF – that is, they explore *optimal choice* by treating decision-making as a gambling problem to which (known) probabilities can be applied. The optimal choice is the one giving the best "chance" to maximise expected utility.
8. A crucial distinction can be made between the use of OCF techniques as *tools to explore* different courses of action under different normative frameworks (29) as suggested in [6], above, and their use as *warrants to create a sense of authority* actually to make choices – perhaps even in an automated fashion. Clearly OCF techniques can only be treated as a reliable way to make choices *if we are confident* all relevant options and outcomes have been included and their underlying assumptions are relevant, well understood and made transparent. To this end, a range of foresight techniques can be added to help decision-makers imagine the scope of the future options available to them, even if the exact future decision trajectory is not known. They include scenario planning, in which the unknown elements of the future can be explored within the constraints provided by the quantitative analysis (50).
9. The academic disciplines that create and refine tools to aid optimal choice include: Applied Mathematics, Engineering, Operational Research, Economics, Epidemiology, Finance, Environmental Science, Management Science, Computer Science, Risk, Medical Informatics, Statistics and Applied Probability, Data Science and Information Science, and particularly welfare economics - for instance, in the form of providing indicators of inequality, poverty, mobility, living standards, etc., to guide policy (57).
10. However, if they rely on OCF techniques in conditions of radical uncertainty, decision-makers *must* always be at risk of suffering massive failure within an atmosphere of deep but unwarranted confidence. Regardless of whether the uncertainties are openly recognised as *radical*, decisions in that condition are necessarily too complex for standard treatments to make them mathematically tractable so that they may lead to potential disaster. Often, there is no relevant data. Simplifying assumptions limit questions and understanding of the system in which the decision is being made, and the likely long-term consequences. Core and relevant parts of the problem may just not be known well enough to make optimisation over what is known reasonable[5]. It follows that if problems are forced into an OCF framework and those using them are not fully aware of their

[4] We define a real-world problem as having the characteristics of risk, if the choice of action to produce an optimum outcome can be "modelled" by analysing data using the tools of applied mathematics. To be useful *there must be agreement among decision-makers* (1) that the data contains all relevant possibilities and (2) that the relevant factors determining outcome that have been included in the model and can reasonably be applied to the real situation.

[5] King and Kay (36) present numerous examples of how things can go wrong.

strengths and weaknesses – as with financial risk models – the risk of disaster is easily underestimated[6]. In fact, it seems likely that programmes aimed at achieving optimality, such as reliance on traditional cost-benefit analysis to make decisions based on welfare implications, potentially build brittleness and lack of resilience into a system.

11. The CRUISSE network conclusion is that great caution must be exercised whenever decisions largely rest on applied mathematical tools that rely on optimisation if, when thinking about a decision any one of three features apply. If it is not possible to (1) reliably and consensually to model probabilities, (2) confidently and consensually to agree on the relevance of data, or (3) securely to achieve agreement among stakeholders that relevant values and variables have been included. In these circumstances, there is a mixture of what can be called Model Uncertainty (doubt about whether there is a useful model to capture the underlying dynamics that create the future on the table), Data Uncertainty (doubt as to whether data available is sufficiently informative and robust) and, if there are reasons to believe the future may evolve in not yet imagined ways due to innovation or reflexive shifts in behaviour, what is best labelled as Ontological Uncertainty or the probability of a big surprise (real doubt as to whether we know what will happen).
12. The decisions listed at [2] characterise the types of choices often necessary at senior policy maker level and within the boardrooms of major corporations. The challenge with them is that they involve inherently unknowable and potentially unimagined features and interactions, whose understanding can be aided by models, but not captured within them. This may be why Jeff Bezos (Amazon) would say, "All of my best decisions in business and in life have been made with heart, intuition, guts... not analysis."
13. In summary, to make real world choices, decision makers need to be able to distinguish which aspects of their decision are amenable to the OCF framework and which are not. If, as with the questions in [2], significant aspects of the decision are *radically* uncertain, awareness of these limitations is crucial.

C. Research Supply: Processes of Decision-Making

14. The earliest modern research into the processes through which business and government reach decisions was conducted by organization and management theorists. In the post-war years that saw the widespread development of the modern firm and modern government, this then nascent field was cross disciplinary using sociology, psychology and economics, to recognise that a major challenge for organizational decision-makers was how to manage and overcome precisely the lack of knowledge about the future evolution of plans that we have called *radical* uncertainty (e.g.14, 16, 20, 22, 40, 58, 59, 63). Scholars from this tradition examined characteristics of the environment, formal and informal structures, the consequences of the separation of ownership and control, company narratives, cognitive processes, behavioural response repertoires, and social expectations (15). There was also a developing interest in how decisions were influenced by group and cognitive-affective processes – for example Janis' work on groupthink (32) and Festinger's work on cognitive dissonance (19), and Katz & Kahn's work on organizational processes (35).
15. Research in this tradition was generally not oriented towards the identification of model relevant quantifiable parameters, but observational, descriptive, interpretive and focussed on structures and processes with historical case studies playing a major role. In the 1970's, however, organization research shifted, influenced by the introduction of Applied Mathematics in operation research and economics - particularly Game theory (69), expected utility theory (69), the Nash Equilibrium (48) and subjective utility theory (57). Particularly under the influence of economics (7, 21), research moved from studying real-world organizations via observation and interviews[7] towards the more

[6] Beckert and Bronk (7) discuss the potentially disastrous consequences of concretely treating models as representations of an uncertain real world.

[7] Self-reports were deemed unreliable in economics (21) and also psychology (48)

abstract task of explaining organizations as a collection of individual utility functions whose behaviour could be theoretically modelled.

16. A very similar development took place in Political Science in which an "interpretative framework" based on understanding political decision-making within a framework of meaning and culture, gave way, first to a behaviourist view that became criticised for its abstractness (17) and then to a modelling "rational" choice framework with roots in economics and utility calculation. Essentially an OCF that claimed to be better at addressing 'real life' problems, but in fact has increasingly come to be seen as divorced from the reality of decision-making, as this current paper argues (Fieschi, network conversation).
17. As is widely known, Friedman (21) successfully argued that economists should use Savage's approach to subjective probability to model decision-making under uncertainty. He ushered in a major analytic shift in the subject[8]. What is less clear in hindsight is how far, by making optimisation *the* benchmark for economic behaviour and terming it "rational", he and others realised they were departing from the ability to explain the "large" world decision-making of economic actors like Bezos (12), to whom Savage himself did not wish to apply his ideas (36).
18. It is not so widely known how far research in the psychology of decision-making follows an OCF path. Pre-war behaviourism gave way to the cognitive revolution and the cognitive revolution gave way to a limited version of itself with the theory of the mind as computer (9). Applied to the study of judgement and decision-making, psychology research compared human decision-making with a normative model of decision making resting on an OCF framework. In the laboratory and in some field experiments, it became clear that human decision-makers, rather than optimising the data made available to them so that they could make judgments "rationally", frequently missed cues[9] or drew on only part of the data – demonstrating the representative bias, affect heuristic, availability bias, the optimism bias, and irrational loss aversion (11, 33, 34, 47). In other words, in line with the OCF, psychology research focused first on prescribing how decision-makers *should act* rationally by maximising information as if they were in "small" world conditions, but then concluded they did not.
19. Under the OCF agenda research becomes blind to real-world uncertainty. For instance, even post-2008, Powell et al (51) are arguing that behavioural strategy research should learn from the success of behavioural economics by focusing on non-rational (non-optimising) behaviour. Referring to the challenge as to how to explain events at Lehman Brothers, Bear Stearns, and BP as well as failed mergers such as AOL/Time-Warner and HP/Compaq, they suggest "the facts" "overwhelming implicate poor executive judgment, or larger macro-cultures of poor judgment" (51 p1370) and argue matters could be improved by making use of research which specifies how the design of "contextual architectures" can be nudged to promote "sound judgment" and better "firm performance" (e.g. 60).
20. "Nudge", which is the main practical outcome of the standard approach in psychology, is a way to obtain optimal choices by structuring options. It works demonstrably well for the design of tax forms and pension choices, where a good outcome is simple to measure and can be consensually agreed. However, the scope to apply it to the decisions in [2] is very limited. The essence of those decisions is that they often involve conflict over goals and assumptions so that decision-makers will necessarily need to be cautious before imposing them by design. As John Kay and Mervyn King have argued, by reducing uncertainty to a well-defined game of chance, OCF research in behavioural

[8] See Langlois and Cosgel (39) for their account of how economics, relying on Bayesian approaches to subjective probability treated Knightian uncertainty as ambiguity about probabilities and collapsed this to risk.

[9] A well-known example used by Kahneman is the Linda problem. Linda is described as 31, bright, outspoken majoring in philosophy, concerned with social justice and anti-nuclear demonstrations, etc. Participants were asked: Which of the following is more likely? 'Linda is a bank teller' or 'Linda is a bank teller and is active in the feminist movement.' The most common answer is that Linda is more likely to be a feminist bank teller than a bank teller, a result treated as an error on the grounds that the probability of two statements being true is less than one being true – a result which changes very few people's minds (35)

economics has abdicated from exploring the big economic questions in favour of harvesting the "low-lying fruit grown from probabilistic reasoning" (36).

21. We have devoted considerable space to set out how decision-making research in several disciplines shifted away from the study of real-world decision process, which necessarily take place under *radical* uncertainty, towards either the creation of OCF or "rational" choice frameworks or to their mirror image, the study of bias and error. The shift, which to a degree has happened silently, and without recognition of the consequences, accompanied the increasing institutionalisation of separate disciplines (and sub-disciplines within disciplines) each with separate definitions of relevant research questions. The unintended effects seem to have been both to enable academic research in sub-disciplines to follow disciplinary silo defined courses, and to make addressing less tractable or cross-disciplinary questions, such as radical uncertainty, more difficult and less academically rewarding to address. A range of factors all underpinned such consequences: disciplinary boundaries and niches, the "elegance" of general solutions, the promise of invariant laws, the role of incentives in directing action to mathematically tractable problems and 'publishable' results, the reduction of uncertainty to risk or to 'ambiguity' in economics and strategy research (unknown probabilities), the reification of evolutionary metaphors in organisation studies (individual adaptation and population level selection), the reification of the computer metaphor for the brain and an associated conception of rationality.
22. The point is that as the study of organizational decision-making became dominated by the study of *individual* decision making under OCF assumptions[10], the study of *radical* uncertainty and real-world decision-making in groups organisations and societies became seriously neglected. However, we do not imply that the OCF has been so dominant there has been no other work in decision-making nor other ways to apply formal approaches to it. Although explicit recognition of *radical uncertainty* (or its terminological variants) is very rare in current literature, there are efforts to look at "real" decision-making such as by military commanders or fire-fighters (e.g. 39) as well as attempts to formalise decision-making frameworks that do not require complete knowledge of outcomes, probabilities, etc.[11]. In some settings, risk analysis is used to provide only one of many lines of evidence to inform a semi-structured or deliberative approach to decision making emphasising *judgment*.
23. In addition, recently, management research is making an "uncertainty turn" (2). There is growing recognition of complexity and the way rapid changes in the global economy have made it necessary to re-evaluate the exclusive applicability of OFC for business (2, 3, 4, 63). It is creativity, entrepreneurship, innovation, and invention that are the hallmark of human decision-making in many businesses[12]. They are regularly required to create "something out of nothing" (5), enter contexts where neither means nor ends are understood (3; 55), or create teams specifically to navigate uncertainty (23)[13].
24. In psychology there may be growing interest in the work of researchers who have questioned the general validity of the OCF and looked directly at real-world decision-making (usually in collaboration with real world decision-makers), to discover the widespread and effective use of short cuts and simple (non-optimising) rules well adapted to the local environment (24, 26, 59). Other researchers have looked at how intuition and tacit rules, based on internalised expertise, provide effective solutions to decision-making in complex situations (38). Experimental interventions have compared a simple triage of patients attending an emergency clinic (three levels of binary choice)

[10] Critical (but marginalised) traditions have remained (as in Economics).
[11] E.g. sequential decision-making frameworks, robust decision making, minimax and low-regret principles, etc.
[12] Interestingly, recent work by Cicera and Maloney at the World Bank follows this turn and highlights the impact on development policy if properly understood See https://openknowledge.worldbank.org/bitstream/handle/10986/28341/9781464811609.pdf
[13] A special issue of the high prestige *Academy of Management Journal* in 2019 (2) aims to reorient the field to the idea "unanticipatable" events in a firm's environment (54), poor understanding of cause-and-effect relationships in a firm's business activities (42) and information-processing limitations (58), all make it impossible for the outcomes of many business decisions to be known with certainty at the time they are made (1, 14).

versus a complex optimisation model (ref). The Bank of England is exploring if framing regulations as heuristics could reduce the complexity and improve effectiveness. Klein and others studied how to train fire fighters or military commanders to make use of available sources of expertise (27, 39).

25. Perhaps significantly, a CRUISSE network participant argued that the OCF stance may be contributing to the destructive battle around expertise that is a feature of the aftermath of the Global Financial Crisis and the Brexit and Trump campaigns with many worrying implications. Politicians and the public express scepticism of expert-based models or just claims to expertise. Experts, using the term "irrational" to characterise non-optimising behaviour, implicitly criticise politicians and the public for non-acceptance.
26. In summary, although the formation of the CRUISSE network and the work of people associated with it are part of a trend reversal away from supplying research based on OCF back towards studying how people can make better decisions under *radical* uncertainty, the outcome of the academic study of decision-making is currently too limited to provide support for better decision-making in real world and therefore *radically* uncertain situations.

D. The Demand for Research

27. The CRUISSE network received far more requests for participation from practitioner organisations in business, government and the third sector than it was able to meet within resources[14]. Significant numbers were present at our two conferences. Any organization faced with problems of the type in [2] could be clients of a new research agenda. Contact with CRUISSE and the issues raised in this document appear to have opened the landscape.
28. In 2012, a report for the US National Research Council of the National Academies (68) put forward a "two communities" metaphor to describe a longstanding model of scientific research. Briefly, scientists do research on what interests them, and policy makers and businesses are expected to use it – perhaps with some help. The "two communities' metaphor" is a silo metaphor which describes the current state of decision-making research. In 2012, the USNRCNA argued this approach is "not a fruitful way to frame the study of knowledge use in policy". As they summarised the problem, although emphasising impact had altered the production of knowledge, there was "no evidence" that it had altered its use. The current landscape suggests a need to lean against the tendency to allow research to be framed "more by the research community in a particular silo than by the demands of understanding actual decision-making in real world situations" (52).
29. In addition, therefore, to the poor links between researchers working in different disciplinary silos and their language difficulties, there is a research gap created by emphasising OCF to the exclusion of studying *processes* of decision-making under real-world or *radical* uncertainty. A further set of silos, which perhaps maintains the current situation, is a gap between *academic research* and *decision-maker*s.

E. Research Availability and Capacity.

30. The Web of Science lists 23,000 articles published in the last five years with the words "uncertainty" and "decision" both present.
31. 2,860 of these articles had one UK based author. Among those articles, 230 focused in some way on policy. In those 17% of researchers were UK funded by EPSRC (7.5%), MRC (3.5%), ESRC (3%), NERC (2.3%) and BBSRC (0.7%). Other funders were NIH (6%), the Wellcome (2.7%), NNSFC (China) (2%), EU (2%) and bodies like Leverhulme, Chief Scientists Office, NSF, Department of Health, Royal Society and Cancer Research UK (0.5% or less each).
32. Examining the abstracts of the 230 articles, most explored optimal choice. Around 20 articles addressed broader topics such as: hostage negotiation, "back" and "front" stage policy making in local government, ways of "listening" to the public to increase vaccination take-up, public

[14] We estimate there were several times the number of requests we could handle, even with only limited publicity (See Final Report)

perceptions of geoengineering and their implications, scenario use to influence stakeholders in climate adaptation, expert elicitation, local government official use of information, use of RCT's in policy choice, knowledge co-production with stakeholders, no-fault decision-making, (mis)understanding of scientific uncertainty, assumptions in evaluating model quality, narrative interpretations of climate risk, implementation of natural resource decisions, how resilience is understood by decision-makers, and improving scenario methods for infrastructure planning. We estimate, that while most articles focused on optimal choice, less than 1% directly addressed whether or how the choices could be implemented in decision-making processes.

33. We also examined abstracts from the 50 highest cited articles with "uncertainty", "decision" and policy". Most of the papers presented technical or evaluative findings relevant to optimal choice. Two papers were about the neuroscience of decision-making, one addressed when and how expert views should be elicited, one addressed involving the public in health care choices and one looked at how to improve academic inputs into science policy for biodiversity. No paper explored the process of decision-making or how to improve it.
34. 111 articles with the terms "Knightian Uncertainty" or "*Radical* Uncertainty" were found - slightly less than half of one per cent of the number discovered by the simple combination of uncertainty and decision. Of the 111, 22 mentioned policy and 15 had a UK author. Inspecting the abstracts of the entire set of 111 revealed that the articles were mostly reviews or highly theoretical.
35. Considered from a real-world decision-maker point of view, the academic research literature offers academically framed information about optimal choices in different areas but little else. Given the challenge of translating research into practice, there is a significant possibility of the misapplications of modelling solutions to what are in fact uncertainty problems, with potentially serious negative consequences[15].
36. The CRUISSE and M2D networks and other events and discussions organized by the research councils are recognition that the research capacity available in the UK to meet calls for cross-disciplinary study of decision-making – including collaborations between social and psychological scientists and those using an OCF framework or research workers and practitioners – needs development. They are examples of efforts to start to address the gap and a sign that due to the interests, expertise and community size, there may be the ability and appetite in the UK to make it the best place to undertake such cross-discipline research.[16]
37. Nonetheless, the relative absence of published work on process and by cross-disciplinary teams is evident. One reason is in part due to incentive problems created by institutional frameworks for disciplinary advancement and interaction. A related gap is the absence of pathways to promote cross-disciplinary doctoral students and postdoctoral researchers trained to work together in a co-creation knowledge environment with decision-makers. Such trends inhibit the kind of problem-solving research that might allow for the establishment of the expertise and motivation to aid decision-makers and to publish research findings that fit the two together. We also noted that at least some important decision-makers have very limited awareness of the issues raised in this paper and at the same time are not aware of the current strengths of what the academic community can offer (including in the OCF area), just as academics seem largely unaware of what decision makers might like help with.
38. Centres of excellence for OCF approaches are listed in the M2D report. We also note Bristol's Cabot Institute (natural hazards) and Durham's Institute of Hazard, Risk and Resilience. There is also CARR at LSE (mostly public administration) and KCL's Centre for Risk Management (focused on human geography). Exemplary in the field of uncertainty communication and decision choice are the Winton Centre for Risk and Evidence Communication (Cambridge) and in Berlin the Harding

[15] See Cartwright and Hardie (11). Leverage ratios in banking may be the prime example.
[16] A view expressed to us by Dr Ronnie Bowman of DSTL, with which we concur.

Centre for Risk Literacy. Similar centres exist in the US. However, perhaps because of the dominance of the OCF framework identified we find no well-funded centre focused on developing cross-disciplinary, practitioner sensitive real-world decision-making research *under radical uncertainty*[17]. The gap is a huge opportunity.

F. The Existing Research Landscape: Summary and Conclusion.

39. Although decisions under uncertainty are at the heart of what happens in government, business and the third sector, the existing research landscape reveals a **significant gap in understanding** how these kinds of decisions are taken and how they can go wrong. The landscape also suggests there is **a significant gap in research to help** decision-makers cope effectively and to determine "what works" in this context.
40. To develop the potential benefit that could accrue to the UK from research funding in core disciplines means targeting *new* forms of research into decision-making under uncertainty, founded on *new models* of cross-disciplinary academic-decision maker co-operation or co-creation.
41. Better understanding of making decisions when faced with *radical* uncertainty has the potential to support today's leaders to address the major challenges facing society. If successful, cross-disciplinary research filling these gaps would produce world-leading research and consultancy generating significant UK advantage – in profitability, governance, effective resource allocation and resilience to uncertainty[18].

II A Future Research Agenda

G. Principles and Types of Research.

42. Co-Creation. A significant limitation of current research knowledge is that it is the outcome of questions posed in academic research silos. What is not being achieved to date is a research agenda with scope for co-created research challenges, formed through a process of academics identifying with decision-makers the decision-making problems the latter experience. If such co-creation could occur it would be likely (a) to enable research workers to design questions and ways to investigate them that address real-world problems and (b) to incentivize research workers to innovate, to work with relevant people from other disciplines and to use all their imagination and skill to devise new ways of investigation. In this way successful research impact would be automatic.

43. **Descriptive and Documentary Research**. Because research has mainly been based within the OCF and so excluded both decision-making process and the *radical* uncertainty real-world decision-makers must manage, we lack basic knowledge of what is going on. To draw credible conclusions about better decision-making we need to gain descriptive knowledge of real-world decision-making framed by different disciplinary perspectives. How do decision-makers cope with the challenges of *radical* uncertainty? To what use do they put the OCF research that has been completed? What do they think they lack? Looking at decisions of the kind listed above, what has happened before? Where did ideas come from? Who put them forward? What evidence and expert opinion was established? Who were the stakeholders? What was the motivation? How did leadership work? How were experts selected? Who was and was not in the decision network? Who seems to have been influential? What were the conflicts? What ideas were dropped at what stage and on what authority? What options were and were not considered? What are the regrets today? How are/were decisions monitored? What has been learned?
44. **(Mostly) Field Experiments.** Descriptive work can produce hypotheses and build a case for their initial validity, but field experiments are likely to be the best way to test different ideas about effective decision-making and particularly to examine how it might be improved. Some hypotheses are suitable for lab conditions, others might be introduced into an organization in action research. In

[17] CECAN at the University of Surrey does do social science based real world policy evaluation across the food-energy-water-environment nexus.
[18] In a paper given at the Treasury in October 2018, Alvarez and Barney (4) from the US argued that the key to "durable competitive advantage" is for firms to develop "Knightian rent", that is to take advantage of their superior understanding of the opportunities provided by uncertainty.

this way ideas such as how decision makers facing with real-world uncertainty respond to different narratives, expert sources or different ways of analysing and presenting information in situations can be developed through iterations. For example, a large firm might want to explore ways of co-ordinating employees to understand and be able to implement a new business model or a regulator might want to build in new ways to get feedback on the effect of regulations and whether this was then used in the regulatory organization to adjust their approach.

H. **New Research Opportunities**

45. **The Gaps.** A co-created research agenda should await the problems it has to address. Nonetheless, we have identified significant research gaps in general understanding of decision processes and, particularly, in our specific understanding of how decision-makers cope with *radical* uncertainty. Below, we suggest ten prime research questions. They comprise a cross disciplinary agenda for scientists *working together* with each other and with decision-makers *across the range* of academic disciplines in the UKRI stable. Most obviously, the agenda could create high impact advances in social anthropology, sociology, science and technology studies (STS), psychology, economics and management studies but it provides new opportunities for effective contributions from maths and statistics as well as for new insights and advances in medicine, engineering and computer science. Decision-making is at the heart of innovation and impact.

i. **Narratives.** ***How do narratives influence real-world decision-making under radical uncertainty?*** Narratives have been identified as an important component of human reasoning well adapted to coping with uncertainty (13, 36, 66). They appear to have powerful sense-making and identifying functions in situations in which decision-makers have a lot of information but are (*radically*) uncertain as to what information is going to be useful to judge the outcome of their actions. Narratives have also been identified as suitable for imagining and simulating different futures and, via connections between cognitive and affective brain functions, are particularly suitable for creating the "conviction" to act when outcomes are uncertain. Nonetheless, the present state of our detailed knowledge is derisory. For instance, we know narratives have powerful communicative and influencing properties, but we have very little precise understanding. What exactly are narratives? Are they of different types? How do they work in real life decision-making and how do some succeed, and others fail? What makes narratives convincing and how do they gain or lose credibility?

ii. **Emotion.** ***Can we better specify the role of emotion in supporting or hindering better decision-making?*** A revolution in the science of emotion has emerged in recent decades, with the potential to create a paradigm shift in decision theories (42). Emotions constitute potent, pervasive, predictable, sometimes harmful and sometimes beneficial drivers of decision making. More specifically, uncertainty has long been recognised to be avoided because an aversive state. Although it is becoming much more widely apparent that emotions play a powerful role in good and bad decision-making both in academic circles and through observation of events – in politics, finance, debates around climate change and trade, etc.- it is still something of a taboo topic away from those disciplines that directly discuss it. A very large research opportunity to connect the new research and real-world decision-making exists, therefore, starting with descriptive work and extending into experiments and practical approaches.

iii. **Quantification and Communication in Real-World Practice.** ***How can mathematical expertise contribute to decision-making under radical uncertainty*****?** *Can we create a code of practice to help avoid the unthinking application of the OCF and better bring the benefits of quantification and mathematical framing into real-world decision making?* The network uncovered many problems described by decision-makers that applied mathematicians may be able to reframe, investigate and communicate about in ways which are both challenging for the research workers and realistic and useful for decision-makers. There is potential to extend knowledge of how to draw conclusions from data and communicate them successfully to decision makers and to work closely

with practitioners to co-create relevant mathematical framing and analysis. A CRUISSE pilot project explored the latter. Research into which formats work best to communicate to patients or health service officials and doctors to improve their understanding of the advantages and disadvantages of procedures (see 20, 25,) are examples of the latter. Wider questions are what is a better health decision? (46, 65, 67).

iv. **Expertise.** ***Can we specify and create better understanding of the role of expertise in decision-making and enhance its effective use?*** It became very clear in CRUISSE network activity that different disciplines have different ways of understanding expertise and its relevance in decision-making. One reason is the implicit dominance of the OCF. Under OCF expertise is limited to the ability correctly to calculate probabilities and to follow pre-defined courses of action dependent on these probabilities. Under *radical* uncertainty, probabilistic rule-following and, indeed, algorithmic processes in general, are brittle. Rather than following rules, successful performance from individuals, teams and organisations under *radical* uncertainty depends on innovative adaptation (70). When there is uncertainty and complexity, decision-makers must resolve and prioritise conflicting advice from multi/interdisciplinary expert thinking ranging over a variety of specialist backgrounds – medical, legal, science, engineering, mathematical, psychological, economic, AI, etc. Which experts should be at the table, how are conflicts between them resolved, does a blame culture inhibit decisions and how do experts make their advice useful? Given the increased technological component of modern business and government inter-silo communication and the selection and use of expertise has become more and more salient.

v. **Learning and Feedback from Mistakes.** *How can decision-makers learn from their mistakes?* In the OCF framework mistakes play little role. In economics, for example, there is no literature. A consequence is no learning. Mistakes are ad-hoc. Yet, under radical uncertainty they must be expected - everything cannot be anticipated. Obtaining feedback and tracking mistakes become tools for potential improvement, helping to build intuition and improve decision making *over time.* Whereas OCF tends to focus on getting it right first time, effectively removing time from the analysis, studies of expertise need to explore the ability to cope with and learn from mistakes over time. For instance, the process of Planned Adaptive Regulation (or Governance) (30, 31, 44) builds on the idea that no decision is final – effective organizations create planning systems for getting feedback about consequences and making adaptations. From such perspectives a large and potentially very rewarding research agenda emerges: studying expert performance, studying mistakes, looking at decision-making over time, etc. As a research goal, can useful guidelines for decision-making and implementation be established?

vi. **Adaptive Heuristics and Simple Rules.** Under *radical* uncertainty, optimisation is impossible. But a substantial body of work (18, 24, 26, 28) suggests that humans adapt rather well. Success is often the outcome of using simple rules or adaptive context-specific heuristics (along the lines of a hammer not a screwdriver for a nail). However, past stress on optimal choice frameworks has left us not very well informed about understanding when decision-makers arrive at adaptive rules, how they assess them as they work and when they stop doing so? Starting from observational studies, there is huge scope for studying how to simplify procedures and to test when this works and does not.

vii. **Conviction Narratives.** If optimal decisions are not available to support policy or business choices, then they require more than calculation. To adopt and implement a strategy requires decision-makers to have conviction and to project their grounds for conviction - a mixed affective and cognitive state of mind. Questions then surround how conviction is reached. Is it via the careful exploration of alternatives in a way that overcomes anxiety and paralysis in an "integrated state" way (66) or perhaps via hubris, silencing diverse views allowing overconfidence and charismatic leadership to "turn a blind eye". From this perspective, research questions include trying to understand the role of emotion in narratives and how it builds support for action. Potential

topics for study would be organizational and product innovation or how to implement adaptation to climate change.

viii. **Diversity and Debate.** R*adical* uncertainty means it is very likely decisions can and should be contested and need debate. If so, are all relevant views considered? How can you know? How can decision-makers structure processes to use multiple viewpoints and to avoid capture by short-term or limited viewpoints and close off options too quickly? The research model of a single lone optimiser will often be misleading. Humans work in teams, decisions will often be contested by vested interests and the basis for human reasoning is social. In this situation do different structural arrangements work better? Recent research (45), perhaps counter-intuitively, suggests that "Myside" bias (believing one's own opinion without sufficient doubt) is not only endemic but adaptive – implying, for a well-organized team, that individuals should speak up and facilitate a process of group argumentation to sort out a consensus. How then do social processes of argumentation, contest and resolution work in real-world decision-making and how can they be improved?

ix. **Leadership, Group Processes and Collaborative Network interactions.** Leadership under radical uncertainty poses a test. What are the roles of the leader and those around her in helping and/or challenging? How does groupthink, a sense of over certainty and lack of diverging opinions in a situation which manifestly is uncertain, get recognised and prevented? It has been recognised for 50 years, based on Janis' (32) analysis of the Kennedy White House decisions to invade Cuba. It rests on the pioneering experiments on group dynamics led by the decorated Army officer and psychoanalyst, Wilfred Bion (8), who emphasised that the primary driver of the phenomenon was feeling. One stream of subsequent research implicitly follows the optimal choice and nudge approach (62) but another, particularly based round work at NASA, has looked at collaboration, trust and innovation – particularly around teamwork (54). There is a great deal more to do – particularly once the focus is on coping under *radical* uncertainty.

x. **Role-Play and Experience-Based Anticipation**. What will the future be and what will it feel like? Decision-makers need to find ways plausibly to imagine the future and develop confidence about what they think is going to happen. Scenario and foresight techniques (for example those pioneered by Shell and in Cambridge) are an important feature of trying to make better decisions and imply explicit recognition that the future is yet to be made. Can better theoretical understanding of foresight exercises be developed and then turned into a code of practice?

I. Conclusion. A Research Gap and an Opportunity

46. Surveying the landscape of research into real-world decision-making under uncertainty, leads us to the conclusion that there is a significant gap in knowledge of how to make real world decisions effectively.
47. Specifically, academic preoccupation with an OCF has side-lined some very important questions about *decision processes* in the real world. How can we better understand and better organize decision-making under the *radical* uncertainty that is a necessary feature of much real-world decision-making?
48. We believe the way really to move forward requires a major effort to create the conditions (a) for practitioners to have the opportunity to co-create research questions with academics and (b) for those with different research skill sets to work together on them. Certainly, those using the OCF and other formal skill sets could work much more productively with those who explore the process of decision-making and with decision makers themselves.
49. The challenge is to develop tools to help decision makers ensure that decisions like those in [2] are as fully thought through, justified and reasoned as they can be. The objective will require co-ordinated research effort in all the different communities, noting that even in the areas of non-radical

uncertainty we are far from the point at which the mathematics developed is mature enough to address the problems.

50. Our proposal is that UKRI fund a major effort to create an Institutional framework in which the effort of the previous paragraph [58] can be set. It needs to bring academics and real-world decision-makers together to co-create and implement a cross-disciplinary research agenda particularly, but not exclusively, around the questions identified above. For this purpose, but not necessarily for all others, the agenda, in fact, needs to be fluid and determined by the problems that practitioners actually have.
51. Such an effort and research agenda have potential both to make significant research breakthroughs in and across a wide range of disciplines and to create international and profitable impact across business, government and the third sector. We note that in the US, within government agencies at least, there is a marked preference for decision-makers to prefer precise, definitive analysis outputs. The reasoning seems to be that if uncertainty is explicitly acknowledged, then decisions are open to legal attack. Network discussions suggest the situation may be more open in the UK, with signs of a current appetite for the open discussion and analysis of uncertainties in the UK (in industry and government), perhaps in part due to the different legal/ political cultures. Given the real world is radically uncertain, this could provide significant comparative advantage.
52. We have also had discussions in CRUISSE about the problem, particularly for young career researchers, as to how to commit to cross-disciplinary co-created research when career structures incentivise sustained contributions to a single discipline agenda with specific requirements about what it takes to publish and the specific journals that count. The problem relates to the institutionalisation of disciplinary niches mentioned in [21]. A future research agenda, therefore, should include plans as to how to build a critical mass of researchers and high-status publication opportunities that explicitly recognise the need to support career structures that incentivise co-creation and cross-disciplinary working. UKRI might establish an Institute structure able to drive a co-created research agenda and to collaborate with UKRI, industry and government towards this goal.

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