Survival Science: Crisis Disciplines and the Shock of the Environment in the 1970s[1]

Michael Egan*

Abstract. The 1970s mark a critical departure point in the history of science. The rise of the environmental crisis prompted not just new avenues of scientific inquiry but also the integration of scientific expertise into complex interactions with politics and society. This paper investigates the history of the new 'crisis disciplines' that emerged in response to explicit fears that the world was on the verge of ecological collapse. Crisis disciplines – a term coined by the conservation biologist Michael Soulé – engage in the urgent and reactionary pursuit of solutions to pressing environmental problems and the evidence scientists bring to bear on their work. Crisis disciplines involve acting 'before knowing all the facts', and therefore constitute 'a mixture of science and art, and their pursuit requires intuition as well as information'. Combined, diverse crisis disciplines constitute a new kind of 'survival science', which emerged in the 1970s.

Keywords. 1970s, crisis disciplines, environmental crisis, history of science, survival science

The 1970s mark a global shift toward a bleaker future. Eric Hobsbawm (1994) refers to a 'landslide' into uncertainty and ambiguity after the golden age of prosperity that followed World War II. He points to a growing sense of instability and crisis after 1973. Similarly, Daniel T. Rodgers (2011) describes the final quarter of the 20th century as an 'age of fracture'. But more than a transition from one historical era to another, the 1970s constitute a decade of crisis. Even if much of the attention and rhetoric was prone to hyperbole, Niall Ferguson (2010, p. 1) concedes that 'the seventies are indeed still popularly remembered … as a time of crisis': economic, political, social, and cultural. In short, the 1970s mark a decade in which imminent—immanent—catastrophe lurked behind shock and crisis. Optimism was replaced with pessimism; progress with decline.

Such broader historical changes had an unmistakable impact on science, both as a model of inquiry and as a tool for progress. Over the course of the 1970s, the relationship between science and policy, between science and its publics, and between science and the questions it sought to answer underwent substantial transformation. Much of this was influenced by the decade that preceded the 1970s. In 1960, *TIME* magazine hailed American scientists as their 'Men of the Year'. They were, according to the editors, the 'true 20th-century

*Department of History, McMaster University, Hamilton, 1280 Main Street West, Ontario L8S 4L9, Canada.
E-mail: egan@mcmaster.ca

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adventurers, the real intellectuals of the day'. They were, moreover, 'statesmen and savants, builders and even priests'. Their work was the engine of progress. They had touched the 'life of every human on the planet'. However, a short decade later, in 1970, scientists were vilified in the popular press. Science was disparaged as an integral part of 'the war/space machine', according to *The Nation*, deemed guilty by a hostile public of having contributed to 'war, pollution, and every manner of evil' (Moore, 2009, p. 1). Across the Atlantic, a similar mantra was evident in the UK. The conservationist Max Nicholson (1970, p. 7), for example, juxtaposed the 'pride of having reached the moon' and 'the humiliation of having gone so far to making a slum of our own native planet'.

The most significant fallout from this transformation came from a countercultural movement from within the scientific community that sought to rehabilitate the popular perception of science through engaging with pressing social problems. It was bounded by a diverse and inchoate series of actions that directed scientific inquiry towards the most pressing social problems of the period. And it produced a new approach to politically engaged science that I mean to call 'survival science'. Survival science constitutes an umbrella concept for a series of 'crisis disciplines', a term introduced by the conservationist Michael Soulé, to describe the changing nature of scientific praxis in the face of ecological disaster (Soulé, 1985). It is further captured in the challenge to contemporary science posed by social needs. As Alvin Weinberg (1972) observed in the 1970s, scientific knowledge was recruited to provide information for which its expertise could not answer. Finally, in its praxis, survival science is inspired by a new method of communicating science and interacting with institutions and publics that Silvio Funtowicz and Jerome Ravetz (1992, p. 254) have termed 'post-normal science', where 'facts are uncertain, values in dispute, stakes high, and decisions urgent'. This final concept stresses the social function of knowledge and the inextricable links between science and society, where social need undermines the more insular processes of a more traditional scientific practice.

This paper is interested in the widespread rise to prominence of this survival science, and explores the manner in which new forms of scientific inquiry—driven by environmental crisis and social need—transformed the manner in which science and society interacted. More significantly, I situate these crisis disciplines as part of a new branch of scientific endeavour that came of age in the 1970s. Multiple crisis disciplines—tasked with solving a number of different but equally urgent elements of the new environmental crisis—transformed scientific research at universities and independent laboratories and the nature of science within government agencies in much of the developed world. From conservation biology to branches of toxicology, cancer biology and the science of the total environment, we might also add much more direct responses to such environmental problems such as multidisciplinary investigations into mercury pollution, acid rain and chlorine-based contamination. Taken together and viewed in hindsight, these scientific inquiries made up a new science of survival, which persists. At the risk of reaching almost too far, I argue that crisis disciplines and the social politics with which they interacted constitute the most profound change in scientific epistemology since World War II. This is, in essence, a speculative venture. It seeks to give shape to an intangible process that worked at the fringes of the 'mainstream' history of science. Nevertheless, the abstraction does provide useful resolution for viewing the shock and crisis that typified so much of 1970s society and culture, especially with respect to its relationship with science.

Survival science is drawn from two distinct historical sources. The first was part of the evolution of science and scientists that occurred after World War II, not least scientists' perceived imprisonment within the military-industrial complex. The second stemmed from what Joachim Radkau (2014) has dubbed 'the great chain reaction' or the ecological revolution that occurred in and around 1970. By the late 1960s, the environment had become an intractable public issue. Much of the impetus behind the welfare state politics of the 1970s was derived from policy agendas designed to preserve and protect a fragile environmental integrity. Equally significant, however, is the simple fact that scientists were at the vanguard of the environmental movement that graduated from the 1960s and into the 1970s. Whereas the typical leadership of countercultural movements consisted of artists and poets, the new environmentalism—one that tried to merge the protection of nature with growing concerns over human health—and its establishment scientists marked a rather different kind of demographic (Worster, 1994). It also indicated a shift within the scientific community.

At the end of 1985, in a short essay in the journal *BioScience*, conservation biologist Michael Soulé used the term 'crisis discipline' to describe his area of specialization. In 'What is Conservation Biology?', Soulé argued that conservation biology was to other biological sciences as 'surgery to physiology and war to political science' (Soulé, 1985, p. 727). The analogy stressed the imperative of action in conservation biology—or practice over theory—but also the nature of the problems scientists confronted. In his conclusion, he observed:

> The current frenzy of environmental degradation is unprecedented, with deforestation, desertification, and destruction of wetlands and coral reefs occurring at rates rivalling the major catastrophes in the fossil record and threatening to eliminate most tropical forests and millions of species in our lifetimes. The response must also be unprecedented. It is fortunate, therefore that conservation biology, and parallel approaches in the social sciences, provides academics and other professionals with constructive outlets for their concern. (Soulé, 1985, p. 733)

According to Soulé, crisis disciplines were synthetic, multidisciplinary sciences in which the boundaries between 'basic' and 'applied' research were blurred or non-existent. Crisis disciplines also implied an epistemological shift away from traditional scientific practice. 'In crisis disciplines', Soulé (1985, p. 733) wrote, 'one must act before knowing all the facts; crisis disciplines are thus a mixture of science and art, and their pursuit requires intuition as well as information'. Such a statement might undermine traditional scientific authority, but to Soulé this was an unavoidable reality. The nature of crisis rarely permitted sufficient time to complete exhaustive research, never mind definitively answer scientific or policy questions posed of it. Scientists engaged in crisis disciplines 'may have to make decisions or recommendations about design and management before he or she is completely comfortable with the theoretical and empirical bases of the analysis' (1985, p. 727).

By way of a rough, working definition for crisis disciplines as they evolved through the 1970s, let me offer some general requisite criteria. Inasmuch as crisis disciplines were a mixture of science and art, requiring intuition as well as information, they were also reactionary, invariably responding to a newly-discovered but extant problem. Crisis disciplines were also mission-oriented, which is to say that they were primarily problem-solving ventures, designed to quickly make sense of a discrete problem. Soulé stressed the multidisciplinary qualities of crisis disciplines, I prefer the biologist Barry Commoner's assertion that such endeavours were *adisciplinary*. In establishing his Center for the Biology of Natural Systems in 1966, Commoner argued that traditional academic disciplines were not independently equipped to tackle environmental problems. Adisciplinarity required a breaking down of traditional disciplinary jargon and vocabulary, creating a more vernacular method of communicating amongst collaborators, but also with the public and with regulators. Finally, crisis disciplines were politically engaged. Their findings, however incomplete, were designed to help shape remedial policies in the face of some environmental emergency. Elsewhere, Soulé artfully used the metaphor of a 'shuttle bus going back and forth, with a cargo of ideas, guidelines, and empirical results in one direction, and a cargo of issues, problems, criticism, constraints, and changed conditions in the other' (1986, p. 3). Ravetz offers a nice comparison along this line. Whereas scientists in a less crisis-driven period 'chose their problems and investigated them under the guidance of the criteria of value and adequacy established by a communal consensus of their peers and mentors …, that haven is no more' (1996, p. xi). These criteria might crudely fit all manner of sciences and scientific endeavours, not least such grand schemes as the Manhattan Project. That should come as little surprise. While approaches to survival science were fractured—or, at the very least, uncoordinated—ventures, they emerged as a direct response to the kinds of heavily funded Big Science that drove what Dwight Eisenhower called the 'military-industrial complex', and served as the source of the crises that crisis disciplines navigated. Survival science was foremost an exercise in small science or a science for the people.

Soulé's essay on conservation biology as a crisis discipline was written in the mid-1980s, but conservation biology gained prominence as a distinct field in the previous decade. One of the seminal texts, *Conservation Biology* (Soulé and Wilcox, 1980), was published in 1980; it was a collection of essays of practitioners from numerous related areas. Soulé's (1985) essay was more a work of synthesis, acknowledging a maturity in conservation biology's praxis, but also providing it with a mission. In so doing, he was introducing conservation biology into an older development in scientific discourse. In 1972, the nuclear physicist Alvin Weinberg lamented that responses to social problems 'hang on answers to questions which can be asked of science and yet *which cannot be answered by science*' (1972, p. 209). For example, seeking an answer to what constituted an acceptable exposure to low-level nuclear radiation was impossible in terms of receiving a specific, individual accounting. There was no magic number after which exposure should be taken more seriously. The best scientists could do was extrapolate averages at which they felt confident that minimal hazard might occur. To some degree, this was educated guesswork. But evaluating risk (a topic to which I shall return) was steeped as much in qualitative moral values and fears as it was in quantifiable scientific empiricism. To Weinberg, this was a 'trans-scientific' question, because its answer transcended—or demanded more than just—science.

Weinberg was consciously responding to the transformation that had occurred in American science in the previous decade (in 1971, Weinberg famously referred to nuclear energy as a 'Faustian bargain'). As a scientific counterpoint to the 1960 Men of the Year *TIME* cover, the magazine's 2 February 1970 cover showed the biologist Barry Commoner, 'the Paul Revere of Ecology', and championed the 'emerging science of survival' against the backdrop of the plundered planet wrought by science and technology. For Weinberg, some of this disenchantment with science stemmed from asking trans-scientific questions, which 'science' could not answer. While crisis disciplines represented recognition that the threat of environmental catastrophe required new approaches, they were also situated in time. By the beginning of the 1970s, distrust in traditional science, which occurred in technical language and was conducted behind closed doors, demanded a new approach to science communication, which became a central tenet of politically engaged crisis disciplines. Historian Jon Agar notes that this was at least in part generational. The baby-boom generation's distrust of its parents' systems and institutions are widely noted (Agar, 2008, pp. 584–586). They were ingrained in the 1960s and coalesced in the science of the 1970s, although it should be noted that many of the survival scientists were part of an older generation (Egan, 2007).

There are two ways to tell this story. The first involves charting the rise of new scientific organizations, subdisciplines and avenues of inquiry, and situating them within their proper historical context. The second consists of examining the overall impact of these changes. I hope that subsequent work will ultimately merge these narratives and flesh out the complexities inherent in this work. For the limited space available to me here, I want to concentrate on the former, which is the safer task: that the long 1970s witnessed the arrival of crisis disciplines and the survival science that united them. I do not mean to pretend specific connections between these fields exist. Nor do I intend to appropriate or fabricate origin stories for these disparate avenues of scientific inquiry. Instead, I want to pull back in order to highlight parallels of practice and chronological commonalities during the period we are putting under the microscope. Which may make for a slightly unsatisfactory analysis, but I hope it contributes more directly to offering an important lens for our larger project of interrogating the 1970s as an era of change in the history of science.

First, some context. The traditional history of environmentalism goes something like this: before World War II, the antecedents of modern environmentalism—ecology, conservation and preservation movements—sought to protect nature from the onslaught of civilization. After World War II, the environmental movement engaged in the more dramatic task of trying to protect civilization from itself. As the biologist Barry Commoner remarked in the 1965 document that sought incipient funding for the Center for the Biology of Natural Systems, 'The scale and intensity of the biological and technological activities of man which affect the environment has now begun to approach the scale of the environment itself' (CBNS, 1965, p. 4). Whereas the environment had typically been regarded as an infinite sink for the hazardous products of human activity, the intensity of technological activity after World War II put into question the total environment's capacity as a reservoir.

Nuclear weapons, the massive expansion of chlorinated hydrocarbons, the widespread adoption of synthetic biocides and fertilizers combined with discoveries that the Earth's biological systems did not function as previously imagined. New, synthetic materials did not break down. Many accumulated, detrimentally, within biological organisms: of most immediate concern, in us. Commoner's four laws of ecology—that everything was connected to everything else; that everything must go somewhere; that nature knew best; and that there was no such thing as a free lunch—introduced in 1971 highlighted these facts.

Commoner's laws of ecology were the product of research into a much broader examination of the physical environment than had previously been undertaken. His adisciplinary approach resisted the boundaries established by singular disciplines such as biology, ecology or plant physiology (in which he had originally been trained). Much of this work had been conducted at the Center for the Biology of Natural Systems, which he had founded at Washington University in 1966. In September 1965, Commoner submitted a proposal to the U.S. Public Health Service for funding for the creation of a scientific research centre that would tackle the growing number of environmental threats to human health. Commoner was the principal investigator of a team of St. Louis-based collaborators, who included members of the departments of botany, zoology, physics and chemistry, as well as the Medical School at Washington University, the St. Louis Zoo and the Missouri Botanical Garden. As Commoner wrote in the grant proposal:

> At the present time, the interactions between man and his environment are undergoing quantitative and qualitative changes of such a magnitude as to create wholly new problems. The present problems of environmental health have rapidly begun to outrun our understanding of the complex processes that mediate the interaction between organisms and the environment. There is, therefore, an urgent need to reorganize our scientific approach to environmental health problems, so that we can find new ways to bring the growing power of modern science to bear on them. (CBNS, 1965, p. 3)

The application is a remarkable document. It was effective in articulating the state of the environmental crisis and how the new centre could serve as intermediary between knowledge production, policy-makers and the public.[2] The Center became a clearinghouse for all manner of environmental investigations, local and national. In the late 1960s researchers did groundbreaking work on synthetic fertilizer use and run-off on farms surrounding Decatur, IL. Their findings raised strong questions about the unquestioning faith in new technologies. At the same time, the Center was one of the early sources of information on mercury pollution in the USA. Photochemical smog in urban areas also received research attention. In each instance, the Center for the Biology of Natural Systems' efforts involved raising public awareness of environmental problems, translating technical information into an accessible, vernacular language for the public and working collaboratively across a number of diverse scientific disciplines (Egan, 2007).

But if the Center for the Biology of Natural Systems (which moved to Queens College, NY, in 1980, and changed its name in 2014 to the Barry Commoner Center for Health and the Environment) provided an institutional home for survival science, many practices were already engaged in aspects of this work. To call toxicology a new discipline is to run up against all manner of anachronistic trouble, but its post-World War II attempts to identify reference doses for new pollutants in air, soil, water and food warrants our attention, not least because toxicology highlights the uncertainty prevalent throughout so much of the work conducted in crisis disciplines.

Measuring the safety factor of chemicals is a feature of post-World War II environmental praxis. Starting in the USA, efforts to identify safe levels for new additives in foods in the mid-1950s prompted interest in articulating safe levels of acute and chronic exposure to harmful chemicals. The first recommendations came from two scientists at the US Food and Drug Administration. In 1954, Arnold Lehman and O. Garth Fitzhugh posited that animal toxicity tests could be extrapolated qualitatively to predict responses in humans, but that quantitative predictions were more problematic (Lehman and Fitzhugh, 1954). To articulate safe levels of a given toxin, they proposed that the reference dose be evaluated by the following formula:

$$\text{Reference Dose (RfD)} = \text{No Observable Adverse Effect Level}/\text{Uncertainty Factor}$$

Lehman and Fitzhugh set their uncertainty factor at a 100-fold margin. That is to say that exposure levels to harmful chemicals should be set a hundred times greater than the point at which no adverse effects had been observed in the laboratory. The justification for the 100-fold safety factor was traditionally interpreted as the product of two separate values, expressing default values to a magnitude of 10. The protocol worked on the assumption, first, that human beings were 10 times more sensitive than the test animal, and, second, that the variability of sensitivity within the human population could be managed within a 10-fold frame. The fundamental premise of the reference dose, as Lehman and Fitzhugh conceived it, was that it was designed to address the untidiness of extrapolating animal data and applying them to human populations outside the lab. In effect, the initial 100-fold reference point was arbitrary, without any real quantitative basis for or against it. It is a principle that has stood up to more recent scientific scrutiny, and variants of it remain in practice 60 years later (Vermeire *et al.*, 1999).

Two impossibly brief mercury case studies help to illustrate the fuzziness of knowledge and uncertainty surrounding toxicology. In the later-1960s, Sweden found itself beset by mercury contamination throughout the country's water systems. The story is complex, but after an initial recommendation proposed that an acceptable exposure to mercury through fish consumption rested around 0.5 parts per million, that limit was subsequently doubled in order to preserve the integrity of the fishing industry. Lake Vänern fish already carried concentrations of 0.7 mg/kg (Egan, 2013). In a more macabre expression of the speed required in ascertaining crisis discipline-specific knowledge, a case of widespread mercury poisoning occurred in Iraq in 1972, the result of improperly used and distributed mercury-treated grain. Amidst the chaos and calamity, the Iraqi case provided a critical opportunity to measure mercury exposures on human subjects. Whereas the Swedes measured mercury content in fish, the new evaluations could be rendered more precise by disregarding the first 10-fold protocol, effectively by eliminating interspecies uncertainty factors—getting rid of the middle-fish. Put another way, where Lehman and Fitzhugh were addressing uncertainty factors as part of a qualitative analysis of potential risk, data derived from Iraq could engage a more accurate approach, not fully quantitative, but reducing the precautionary guesswork. Here was living data that could eliminate the obstacles associated with uncertainty. As a result, numerous national and international agencies - the World Health Organization and the US Food and Drug Administration foremost among them - descended on the provinces surrounding Baghdad, and collected data from mercury victims. Crass opportunism, perhaps, but crisis disciplines frequently required haste and creativity in establishing data. These studies subsequently served as the cornerstone for numerous national and international recommendations for acceptable mercury exposure for the next 25 years (Bakir *et al.*, 1973; Clarkson, 2002).

Thumbnail sketches of the Center for the Biology of Natural Systems and reference dose debates within toxicology circles offer insight into crisis disciplines in practice. But, in effect, there are two chapters to the history of crisis disciplines. On the one hand, the organic production of scientific communities committed to addressing specific environmental problems—on local or global scales—sprang up to meet the social or political need for scientific information. Their work required speed and intellectual flexibility, working across many areas of specialization at once.

The second chapter charts a self-reflexive awareness that the establishment of these formal and informal crisis disciplines constituted a recognizable trend and that new methods of interrogating the environmental crisis could help take regulatory decision-making off the back foot. Where toxicology, conservation biology and the science of the total environment targeted discrete problems, frequently in a vacuum, new crisis disciplines emerged to engage the environmental crisis more holistically. Risk analysis, futurology and systems modelling—each maybe sections of a larger anticipatory science—embraced a much wider series of problems and pushed social and scientific discourse toward practices that promised a more resilient future.

Philosopher Ian Hacking (2003) locates the birth of professional risk analysis to 1969. The very end of the 1960s christened the professional risk analyst, which supposes an important degree of self-awareness. At the same time, Chauncey Starr - an engineer typically credited for founding risk analysis as an academic discipline - published an important and popular paper on risk in *Science*. Indeed, Starr's essay belongs alongside Soulé's and Weinberg's as a seminal work on crisis disciplines. Starr offered a quantitative method of measuring of cost-benefit analysis. The research was still in its infancy. Starr conceded that his risk analysis could not yet distinguish 'what is "best" for society from what is "traditionally acceptable"' (Starr, 1969, p. 1232). But this became the project of the 1970s, as risk experts sought to quantify all manner of technological threats as a means of anticipating hazard and prioritizing danger. This became a boon to the insurance industry - and how its actuaries measured the costs of natural and technological disasters - whose influence increased markedly over the decade.

And if risk analysis and management became professionalized streams of a new crisis discipline, the public conversations they inspired provided ample context for a cultural re-visioning of risk throughout the developed world. The professional interest in quantification failed to translate to the public as accessibly as many crisis disciplines (which may have had as much to do with who was primarily interested in quantifying costs and benefits). By the 1980s, the sociologist Ulrich Beck chastised management experts for the contempt they showed toward public perceptions of risk (Beck, 1992). Risk perception - the public response to potential hazards - rarely aligned itself with the experts' calculations. Or, rather, here was an example of Weinberg's trans-science in action. The hard data needed to be softened with psychological, sociological and cultural contexts. Risk analysis could interpret generic risks, but not whether individuals or groups of individuals should accept them or not. Because of a relative breakdown in communication, public perceptions of new environmental pollutants gravitated towards increased fear over the course of the 1970s, climaxing with a culture of toxic fear in the USA in the 1980s (Egan, 2014).

Risk was inherently predictive. A concomitant interest in futurology -at once grossly distant from 'science' and intimately linked both in influence and inquiry - is also a key feature of science in the 1970s. In describing futurology in 1972, Herman Kahn observed that 'discussing the future is necessarily an art and not a science in the usual sense, though many disciplines are enlisted in a common, integrated effort of analysis and speculation' (Kahn and Biggs, 1972, p. 1). The synergy with Soulé's definition for crisis disciplines is striking. Forecasts, projections and future scenarios were everywhere present during the decade. The first World Future Research Conference was held in Oslo in 1967. By 1980, the World Future Society consisted of more than 50,000 members. Alvin Toffler's bestselling *Future Shock*, from 1970, warned of the pace at which historical change was accelerating. The inevitable 'information overload', as he called it, anticipated society's inability to cope with the speed of change (Toffler, 1970). In many respects, futurology might have been the most influential of all crisis disciplines. True to Soulé's definition, futurology was an inchoate collection of specialists merging around a singular type of problem (Seefried, 2014; Andersson and Rindzeviciute, 2015). And if astrology enjoyed heightened popularity and intelligence agencies contracted psychics, futurology also acquired a more rigorous methodology (Connelly, 2010).

I don't know whether to include the international Club of Rome's work around *The Limits to Growth* as a part of a larger trend in futurology, or whether their work in systems dynamics constitutes its own crisis discipline. *The Limits to Growth* was the first -and most widely read - work that integrated systems dynamics and computer modelling in its predictions of the environmental future. For its proponents, systems dynamics provided society with a clearer perspective of the origins, significance and interrelationships between the myriad components that made up the environmental crisis. While it represented a gross departure from the kind of on-the-ground scientific work conducted by the Center for the Biology of Natural Systems and those engaged in other crisis disciplines, the Club of Rome's 'invisible college' of experts in policy, economics, and the natural and social sciences were also contributing to survival science. The central message from *Limits to Growth* was that the human ecological footprint was growing at an unsustainable rate. Population, food production and resource extraction were straining the planet's carrying capacity to breaking point. There were limits to growth and humankind was in danger of exceeding them (Meadows, Randers and Meadows, 1972).

Such future shock wasn't all doom and gloom, however. Perhaps the most influential futurologist was Herman Kahn, who became famous for his descriptions of a 'winnable' nuclear war in the 1950s and 1960s. If the Club of Rome warned about ecological limits to technological growth, Kahn embraced the opportunities presented by new and future technologies to overcome the traditional boundaries set by the ecosphere. He actively challenged the apocalyptic warnings of environmental collapse from neo-Malthusians such as Paul Ehrlich and painted a more optimistic reading of the environmental future and the human capacity for ever-expanding wealth and affluence (Kahn, Brown and Martel, 1976). Kahn's work engaged systems every bit as much as the Club of Rome's. He was an early adopter of Monte Carlo sampling in the crafting of future scenarios, and relied heavily on electronic computers to do so. And while systems-thinking was not new, its relationship with computing - through linear programming and game theory - grew markedly during the 1970s. Nevertheless, modelling scenarios earned a place in survival science as more and more computer specialists turned their efforts to environmental issues in the 1970s and more environmental scientists acquired an appreciation for what systems thinking could do for their predictive analyses.

One recurrent point of focus for future studies and for survival science in general was the state of the environment and its potential for sustaining civilization. If crisis disciplines changed the nature of science and how it approached the environmental crisis, it is important to stress that the environmental crisis provoked new ways of looking at the physical environment. In 1964, Swedish scientists discovered that seed grain laced with mercury-based fungicides had poisoned birds and livestock across the country. While mercury pollution would become an especial intersection for science, environment and policy the world over in the 1970s, it was Swedish scientists who hypothesized that mercury in water systems could constitute a massive catastrophe. And, most importantly, they thought to look for it (Egan, 2008). The cultural awareness of environmental hazard was strong in Sweden. In the immediate aftermath of the Swedish mercury scare, Sören Jensen identified the ubiquity of PCBs in humans and in animals. And in 1967 Svante Oden raised widespread awareness about acid rain (Rothschild, 2014).

Another component of the new crisis disciplines was their problem-solving nature. Perhaps in light of the mounting information overload identified by Toffler and others, this was not such a surprise. Much of the new work was reactionary in nature—trespasses into nature had already occurred, and scientists scrambled to resolve the environmental problem while simultaneously trying to understand the nature of the hazard. Such efforts were also subsidized by special funding. In the USA, the Nixon Administration pushed the National Science Foundation to develop a new stream of support for scientists engaged in 'Research Applied to National Needs'. The short-lived programme (it was closed in 1978) fostered more applied research and triggered concentrated efforts to resolve such socially relevant problems in the 1970s as pollution, transportation, energy and other urban and social difficulties.[3]

This paper deliberately skirts the subject of expertise. In crisis disciplines there was a radical transformation of what expertise meant in modern science and how expertise was used to advance knowledge and justify policy decisions. Practitioners of crisis disciplines were still regarded as experts, and many acted as public intellectuals, even if they were disinclined to claim definitive knowledge. Tellingly, Soulé pointed out, in crisis disciplines, 'tolerating uncertainty is often necessary' (1985, p. 727). Uncertainty is a critical component of crisis disciplines - and of survival science more broadly. Further work needs to illuminate the place of uncertainty in science, especially as it pertains to ideas about expertise and authority (Nowotny, 2016). And I suspect that the most fruitful work in this vein will concentrate on the 1970s. In the aftermath of the turbulent 1960s, uncertainty impressed itself on all manner of social and scientific endeavour. Part of this might derive from the unravelling of the unquestioned authority of the military-industrial complex that imposed itself during the incipient years of the Cold War. But in itself, survival science - or, rather, its practitioners - engaged in a concerted effort to shift the audience for their work from cloistered specialists to the public and policymakers with varying degrees of success. Making sense of how risk analysis, futurology and systems modelling accepted and accounted for uncertainty in the 1970s helps to explain much of the cultural anxiety of the 1980s.

On some level, crisis disciplines or survival science merely constituted tacit acknowledgment that science and society were inextricably linked. Asking trans-scientific questions demanded that science come into conversation with economics, politics, values and forms of local knowledge. While such a dynamic arguably weakened the authority of scientific expertise, it was hailed in some corners as a democratization of science (Ravetz, 1996).

But what of historical significance? I submit three lasting outcomes of crisis disciplines and their activities in the 1970s.

1. Gave voice to the environmental crisis as a crisis of civilization.
2. Irrevocably altered scientific praxis.
3. Developed a new, vernacular science, which transformed the public understanding of science as well as the public participation in science and politics.

Lest this sound too triumphal, this third point requires further analysis and complication. In spite of the explicit attempts to create a more vernacular language for science information and practice in order to better encourage public participation, the irony of a growing science illiteracy has been the dominant trend in North America and in much of Europe just as a more accessible science was made available. Science (traditional and survival) remains walled off from many aspects of public life, sequestered from the overwhelming majority of the population (Shapin and Schaffer, 1985, p. 343).

It is conceivable that our proximity to the 1970s - indeed, to the entire timescape after World War II - continues to obscure our interpretation of the period. That 'shock' or 'crisis' should seem such defining characteristics could well be a function of our contemporary analysis of a history that continues to live with us: a past that has not yet passed, our interest fuelled by 'the latest catastrophe' (Rousso, 2016). But in survival science, we might read rumblings of not the latest, but the last catastrophe and its epistemological origins in the decade under question. From our vantage point in the early decades of the 21st century, the emerging Anthropocene - a new epoch driven by humanity's rapacious appetite for environmental collapse- is informed not just by history, but also by science. Rising waters, marching deserts, diminishing resources and disappearing species are wont to leave a more lasting impression on the earth's crust and on human history than shifting borders and fluctuating markets.

And, here, let me submit that climate science is a crisis discipline with its myriad cooperations between meteorologists, oceanographers, geophysicists, biologists, physicists, mathematicians, geologists and other specialists. 'Until the 1980s', note J. R. McNeill and Peter Engelke:

> discussions of anthropogenic climate change had been confined largely to the scientific community. There had been some political awareness and media coverage during the 1970s, but the issue was too new and abstract to receive much of a hearing. Moreover, the scientific consensus about warming was relatively weak. But the 1980s were a watershed decade, as scientific agreement about anthropogenic warming strengthened and the issue became political for the first time. (2014, p. 429)

So: it is possible that the survival science of the 1970s- if, indeed, we might be able to pull such a thing into clear, historical resolution -serves as an important first chapter in a brief epistemological history of the ecological crisis that is likely to define the human condition throughout the 21st century. Combined with the social ramifications of an aging global population and fracturing economies, we might find that human societies are increasingly looking over the precipice and into the abyss. New scarcities and vulnerabilities - the languages of sustainability, resilience and the Anthropocene - are forefront in the scientific discourse of the present. But their mainstream origins are evident in the emerging survival science of the 1970s: a science upon which our survival might very well rest. A history that should be heeded.

NOTES

1. Versions of this paper were presented in September 2015 at a workshop titled '1970s: Turn of an Era in the History of Science?' in Aarhus, Denmark, and at a symposium in Lugano, Switzerland, titled 'Environmental Justice, "Collapse"', and the Question of Evidence'. I am grateful to convenors of both conferences and to the participants for their questions and feedback. I am especially grateful to Matthias Heymann for his thoughtful insights on an earlier draft of this piece. Some minor overlap - in theme and content - exists between this essay and a previously published piece, 'Confronting Collapse: Environmental Science at the End of the World', *Intervalla* 3 (2015). I reuse a small section of that work here with permission from the editors of *Intervalla*.
2. Commoner's proposal was clearly outside the mainstream of scientific inquiry for the time. The Public Health Service had never attempted to develop a comprehensive research programme on the environment and, in the end, of the 10 centres it hoped to support only the Center for the Biology of Natural Systems would ever receive funding.
3. RANN receives passing mention in a few studies that treat the history of science in the American 1970s, but it deserves more thorough analysis and interpretation.

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