Modelling innovation as toxic (techno-economic) positivity

Some consequences of SPRU's attack on "The Limits to Growth"

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ABSTRACT

The Science Policy Research Unit (SPRU) at the University of Sussex played a significant role in establishing innovation studies as a field and in establishing innovation policy as a framework for governmental thinking. But the SPRU's first book was not directly about innovation; it was "Thinking about the future: a critique of the Limits to Growth" (1973). The "Limits to Growth" (1972) had been an attempt by researchers at MIT, funded by the Club of Rome and the Volkswagen Foundation, to quantitatively falsify the idea of endless growth on a finite planet. The SPRU's response—later reprinted with the title "Models of Doom" outside the UK—was one of many that framed "Limits" as overly pessimistic. This paper considers the impact of this work on the developing fields of innovation research and policy. It takes a critical ethnostatistics approach to the modelling practices deployed by these two very different groups of professional social scientists. It focuses on two methodological moves made by the SPRU researchers. First, this paper shows how the SPRU arguments established a fetish for data precision—a standard that the MIT team rejected, but one that carried on through the SPRU's further work into innovation research and policy. Next, it discusses how the SPRU researchers (role)modelled mathematical faith in socio-technical change. This was more than a techno-optimist (or Promethean) stance. It established a norm of toxic positivity around questions of technology, innovation, and the environment. These two methodological moves—fetishizing data precision while asserting toxically positive Prometheanism—became cultural memes that carried forward from this debate into innovation policy, modelling, and statistical practices. In short, the pro-innovation econometrics developed for this debate by the SPRU researchers had a lasting impact on the epistemic culture of innovation studies.

Keywords: Degrowth; Ethnostatistics; Methods; Sustainability; System Dynamics; Techno-optimism.

Proposal Submitted 31 July 2024, Article Received 11 January 2025, Reviews Delivered 3 June 2025, Revised 31 July 2025, Accepted 10 October 2025, Available online 17 December 2025.

Acknowledgements: Thank you to Rosana Icassatti Corazza and Ronald Cancino for their insightful reviews that greatly improved this work. This paper also benefitted greatly from early-stage feedback at the 2024 European Association for the Study of Science and Technology conference (Amsterdam) and the 2nd Novation Forum. Thank you to Cornelius Schubert, Sebastian Pfotenhauer, and Lee Vinsel for particularly insightful comments and questions at those conferences. This research was supported by the Rath Professorship in Entrepreneurship at Acadia University.

INTRODUCTION

Histories of innovation highlight the centrality of the *Science Policy Research Unit* (SPRU) at the *University of Sussex*. The writings of Christopher Freeman especially, but also many other SPRU researchers, played a significant role in establishing innovation studies as a field of research and in establishing innovation policy as a distinct framework for governmental thinking about economy and society (Fagerberg *et al.*, 2011; Fagerberg *et al.*, 2012; Fagerberg *et al.*, 2013; Soete, 2019, p. 89). Research at the SPRU shaped innovation models toward a systems approach and normalized statistical measurements of innovation. But before all this, the SPRU began building its reputation—and establishing some field-defining norms—with its first book: *Thinking about the future: a critique of the Limits to Growth* (Cole *et al.*, 1973b).

The *Limits to Growth* (Meadows *et al.*, 1972) was an attempt to quantitatively falsify the assumption that endless growth is possible on a finite planet. It was prepared by a team of researchers at the MIT's management school: Donella Meadows, Dennis Meadows, Jørgen Randers, and William Behrens III. They used a new computer-driven "system dynamics" approach to quantitative modelling developed by Jay Wright Forrester (1973). This work was sponsored by the *Club of Rome*—"an informal organization" of "scientists, educators, economists, humanists, industrialists, and national and international civil servants" who shared a "conviction that the major problems facing mankind are of such complexity and are so interrelated that traditional institutions and policies are no longer able to cope with them" (Meadows *et al.*, 1972, p. 9-10). The group felt a great sense of urgency over these challenges and chose to proceed with limited data and an imperfect model. As the authors of *Limits* explained, "We hope that it will lead thoughtful men and women in all fields of endeavor to consider the need for concerted action now if we are to preserve the habitability of this planet for ourselves and our children" (Meadows *et al.*, 1972, p. 12). And, despite its methodological density, the work did find a very wide readership that extended well beyond research and policy circles[1] (see Forrester, 1989). It sold over 10 million copies in 30 languages (Ramage and Shipp, 2009, p. 109). It also garnered tremendous criticism—especially from economists.

The SPRU response was one of many that presented alternative (political) assumptions and modelling (see also Herrera, 1977; Kahn *et al.*, 1976; Maddox, 1972; Nordhaus, 1973; Vermeulen and de Jongh, 1976). Like others, the SPRU team criticized the assump-

[1] Propelled by coverage in news and popular media—apparently including an article in eight million copies of *Playboy* magazine (Forrester, 1989).

tions of the MIT model and its statistical imprecision. More importantly, the SPRU critique also advanced the view that sufficient socio-technical progress can overcome—or at least significantly and continuously delay—any natural limits. For this reason, they characterized *Limits* as overly pessimistic about the future. The SPRU book was then quickly reprinted for an international audience as *Models of Doom…* (Cole *et al.*, 1973a). This helped characterize the MIT/Club of Rome as doomsayers. Others joined that chorus (see discussion in Higgs, 2014). And the doomsday rhetoric was "effective in blunting the impact of the critique of growth and remains popular with think tanks and economists today" (Higgs, 2014, p. 54). The SPRU was one of several groups that advanced the "optimistic" view that human ingenuity will conquer any limits to growth.

Kerryn Higgs (2014) and Elodie Vieille Blanchard (2010; 2015) have both examined the impact of this early SPRU research within the broader *Limits to Growth* debate. In this paper, I reverse that line of inquiry. I am interested in the impact the *Limits* debate had on the SPRU and on the developing fields of innovation research and policy. Taking a critical ethnostatistics approach (Gephart, 1988, 2006), I examine the methodological norms that were advanced by SPRU researchers in this debate. I argue that the pro-innovation position SPRU researchers developed for *Models of Doom*—and several related follow-up books (i.e., Encel *et al.*, 1975; Freeman, 1992; Freeman and Jahoda, 1978; Miles, 1975)—had a lasting impact on the epistemic culture (Knorr-Cetina, 1999) of innovation studies.

Present-day SPRU researchers, Adrian Smith and Adrian Ely (2025), have recently written about the lasting impact of the *Models of Doom*. Their paper (Smith and Ely, 2025) focuses on questions of post-growth and post-colonial justice for the "Third World" or Global South. The SPRU and the Bariloche Foundations (Herrera, 1977) both criticized *Limits* for failing to acknowledge global inequities. Smith and Ely (2025) argue that the SPRU's hopes for global justice were later dashed by the rise of neoliberalism. What remained was the SPRU team's "shared hope in the ability of technology to decouple economic growth from environmental collapse" (Smith and Ely, 2025, p. 2). They say: "this hope has been prominent in policy ever since" (Smith and Ely, 2025, p. 2). And that is where this paper comes in. I am interested in the consequences of the SPRU's excessive techno-optimism on innovation research and policy. As Hickel and Kallis (2020) point out, "there is no empirical evidence that absolute decoupling from resource use can be achieved on a global scale against a background of continued economic growth" (p. 469). Nonetheless, present-day innovation studies continue aspiring toward techno-fixes (Pansera and Fressoli, 2021; Sharma *et al.*, 2025).

Unfortunately, it is now taken-for-granted that "innovation stretches the limits to growth" (Pansera and Fressoli, 2021, p. 381). Mario Pansera and Mariano Fressoli (2021) have said that "this view has become normalized and it is today a matter of fact for most governments and international institutions" (p. 381). Like Pansera and Fressoli (2021), I believe that "untangling innovation from growth is key to imagine a post-growth era" (p, 380). But while they credit this entanglement to "the revival of Schumpter's ideas in the 1980s" (Pansera and Fressoli, 2021, p. 381), my work here traces it back a decade earlier. In the *Limits* debate, "innovation-as-endless-growth" became embedded in methodological practices and disciplinary norms.

These methodological norms have had far reaching effects. Christophe Cassen and Béatrice Cointe (2022) examined changes in global computer models and found that, beginning in the *Limits* debate, "technological change progressively became a parameter in models as more elaborate methodologies were developed to simulate it" (p. 610). Similarly, Michael Keary (2016) said that "environmental modelling, powered by models of technological change, is now the most significant locus of technological optimism," and "…worryingly, green thought has not caught up" (p. 8). It is therefore surprising that "the STS literature engages with the politics of technology but does not adequately engage with the *Limits* to Growth debate" (Sharma *et al.*, 2025, p. 218). This debate was where "innovation-as-endless-growth" became an epistemic point-of-departure for future socio-economic research and policy.

My own epistemic point-of-departure for this paper is a place where the intellectual histories of innovation (Godin, 2011, 2012, 2017, 2019, 2020) meet the methods of critical organizational history (Durepos *et al.*, 2021; MacNeil, 2024). I begin with some discussion of what is meant by "epistemic culture" (Knorr-Cetina, 1999) and explain how a critically- and historically-conscious approach to "ethnostatistics" (Boje *et al.*, 2006; Gephart, 1988, 2006; Helms Mills *et al.*, 2006) can help to surface scholarly norms from statistically dense texts. After describing my approach, I provide an overview of the debate between the MIT and SPRU research teams. I briefly describe the modelling methods that were used for *Limits* (Meadows *et al.*, 1972) and then the alternative methodological assumptions contained in the SPRU response (i.e., Cole *et al.*, 1973a; Cole *et al.*, 1973b). This is followed by specific discussion of the two key methodological memes (i.e., on-going disciplinary practices) that I identified within the SPRU critique of *Limits*. First, the SPRU researchers established a fetish for data precision. Next, they ignored that norm and (role)modelled mathematical faith in socio-technical change. I present each of these methodological moves in turn.

I conclude with the broader point that *Models of Doom* was an important early boundary-setting contribution to innovation research and policy. Although the resulting books are not known as contributions to innovation studies, they are traces of the earliest work at the SPRU. The SPRU researchers were sharpening their analytical tools, building their research group's reputation, and establishing new disciplinary norms. This is more than an instance of techno-optimism. The legacy of this debate is a research/policy field concerned with social and environmental justice but burdened by toxically positive assumptions about innovation, economic growth, and planetary limits.

MY APPROACH

INTELLECTUAL HISTORIOGRAPHY

Benoît Godin (2020) longed for innovation to have many more historians and noted the limitations of a field dominated by econometrics (Godin, 2017). His work examined the historical development of ideas about technological innovation (Godin, 2019, 2020; Godin and Vinck, 2017b), the establishment of innovation studies as a discipline (Godin, 2012, 2014), the development of theoretical models to explain innovation (Godin, 2006, 2017; Godin and Lane, 2013), and the standardization of innovation surveys and measurements (Godin, 2002, 2005). While Godin's approach arose from the history of science and technology, mine comes from training in critical organizational history (Durepos *et al.*, 2021). Critical organizational history (COH) drew on the postmodern turn in history and then followed the amodern (or "new materialist") turn in STS (see Durepos and Mills, 2012). It is heavily influenced by authors such as Derrida (1996), Deleuze and Guattari (1987), Haraway (1988), Latour (1987), Law (1994), and Mol (2002). Durepos, Shaffner, and Taylor (2021) describe COH as "a theoretically informed historicized approach to understanding how and why we come to be where we are in contemporary organized societies" (p. 449). Many contributions to COH are acutely and reflexively concerned with method (e.g., Durepos and Mills, 2012; Hartt *et al.*, 2020; Williams, 2021). In prior work, I have demonstrated how a COH approach can disrupt the taken-for-granted methodological assumptions of innovation research (MacNeil, 2024). There, I re-examined the history of theoretical models—i.e., the linear (push/pull), chain-linked, and systems models of innovation. Here, my focus is on specific mathematical modelling practices.

ETHNOSTATISTICS

Karin Knorr-Cetina (1999) wrote about the cultures of different scientific disciplines. She examined the scientific practices in high-energy physics and in microbiology. This provided a lesson on how two very different disciplinary traditions (i.e., epistemic cultures) make knowledge in two very different ways. Laboratory ethnographies like this have been fundamental to understanding contemporary scientific practices as socio-material enactments. This new materialist perspective has since been adapted to the study of past management and organizational theories and practices (Durepos, 2015; Durepos and Mills, 2012). In this paper, I push it further by focusing on past practices of statistical modelling. Following Knorr-Cetina (1999), I am not interested in identifying the best or most highly developed scholarly practices. I am interested in understanding how different knowledges arise from different norms.

My work was informed by ethnostatistics: a methodological approach from organizational studies focused on the cultural practices around numbers (Gephart, 1988, 2006). Robert Gephart (2006) defined it as "the empirical study of how professional scholars construct and use statistics and numerals in scholarly research" (p. 417). It is a sparsely used method, with very limited use in innovation statistics (e.g., Kilduff and Oh, 2006). Elsewhere, I have argued that ethnostatistics might provide new ways to (re)consider the problems of standardized quantitative methods and measurements in innovation research (see MacNeil, 2024, Chapter 7). It provides a different way to build on the existing critique of innovation numbers (e.g., Gault, 2018, 2020; Godin, 2002, 2005). For this paper, it also provides a new way to disentangle notions of "growth" and "innovation" (cf. Pansera and Fressoli, 2021). It allows me to focus on the ways that "growth" and "innovation" became entangled in past practices of quantitative econometric modelling.

My primary focus is on the debate that occurred across three books: *Limits*... (Meadows *et al.*, 1972), *Thinking*... (Cole *et al.*, 1973b), and *Models*... (Cole *et al.*, 1973a). Interestingly, these books describe the in-person meeting that was held between the MIT and SPRU research teams prior to their publication (see Cole *et al.*, 1973a, pp. 216-240; Meadows *et al.*, 1973). The reprint of the SPRU text (Cole *et al.*, 1973a) was also published as a special issue of the journal *Futures* (see Streatfeild, 1973). Both the reprinted book and the special issue contain a sharp response to the SPRU from the MIT team. Nonetheless, I approached all these texts as mere traces of past statistical practices. Not having access to a time machine means that I cannot access the real past. I can only access the ways these authors described their work. I can only produce a situated understanding of past practices through my reading of the traces that remain.

To supplement my understanding of the primary texts, I also examined several other directly related texts (i.e., Encel *et al.*, 1975; Freeman and Jahoda, 1978; Miles, 1975), other unrelated texts from and about the SPRU during this period (i.e., Fagerberg *et al.*, 2011; Freeman, 1974; Soete, 2019), the authors' later reflections on this debate (i.e., Freeman, 1992; Meadows and Robinson, 1985; Meadows *et al.*, 2018; Meadows *et al.*, 2004), a documentary film about *Limits* (i.e., Enrico, 2013), and other sociological and historical investigations of the debate (i.e., Bloomfield, 1984; Higgs, 2014; Smith and Ely, 2025; Vieille Blanchard, 2010; Vieille Blanchard, 2015). Across these sources, my focus was on understanding how the SPRU researchers were constructing their arguments and using mathematical ideas. I was looking for evidence of the epistemic culture(s) swirling around the SPRU-side of this scholarly debate.

OVERVIEW OF THE DEBATE

LIMITS TO GROWTH

Work on *The Limits to Growth* (Meadows *et al.*, 1972) began when Forrester attended a meeting of the Club of Rome in Switzerland on June 29-30, 1970 (Lane, 2007). Forrester was an accomplished engineer and engineering professor who had transferred into MIT's management school shortly after it was established in the 1950s (Ramage and Shipp, 2009). In addition to his technical inventions, Forrester is credited with the development of system dynamics (Lane, 2007)—an approach to "modelling" or simulating the interaction of elements in a complex system. At first, this method was applied to the simulation of industrial dynamics, such as supply chains and industrial production processes (Forrester, 1961). Forrester had also recently applied his method to questions of "urban dynamics" (Forrester, 1969). At the Club of Rome meeting, he promoted the use of his method for understanding the Club's many interrelated social and environmental concerns. Given the enthusiastic response, he sketched his mental model of "world dynamics" on the flight home. His original sketch is reproduced in Lane (2007). With some refinement, Forrester is said to have produced the first computer model in a few days. He called this model "World 1." Within a month, he presented a second version to Club of Rome members who came to visit him at MIT. With funding from the Volkswagen Foundation, a team was hired to advance the model further, led by Forrester's former PhD student, Dennis Meadows. However, it was Dennis' wife Donella Meadows—with her PhD in biophysics from Harvard —who would become the lead author of *Limits* (see Ramage and Shipp, 2009).

The book presented results from a model the authors called "World3." The model was illustrated, similarly to Forrester's original sketch, as a nodes-and-links diagram of many interrelated social, economic, and environmental forces (on fold-out pages 102 and 103, in Meadows *et al.*, 1972). Data was attached to each node (e.g., population, birth rate, etc.) and each link represented a mathematical relation between factors/forces in the model. This was not a linear diagram; the model included many complex interrelations. The authors explained that "our world model was built to investigate five major trends of global concern—accelerating industrialization, widespread malnutrition, depletion of non-renewable resources, and a deteriorating environment" (Meadows *et al.*, 1972, p. 21). And they described their work as:

> A preliminary attempt to improve our mental models of long-term, global problems by combining the large amount of information that is already in human minds and in written records with the new information-processing tools that mankind's increasing knowledge has produced—the scientific method, systems analysis, and the modern computer. (Meadows *et al.*, 1972, p. 21)

Meadows *et al.* (1972) argued that expressing their work as a formal mathematical model meant that their assumptions about the world became more precise and "open to inspection and criticism by all" (p. 22). They also argued that the use of a computer allowed the team to "extend our intuitive capabilities so that we can follow the complex, interrelated behaviour of many variables simultaneously" (Meadows *et al.*, 1972, p. 89). This was at a time when there was growing excitement over the use of computers across social science.

The numerical results of the model are presented in over 200 pages of detailed charts, tables, and text. In broad strokes, the MIT team's conclusion was that "if the present growth trends…continue unchanged, the limits to growth on this plant will be reached within the next one hundred years" (Meadows *et al.*, 1972, p. 23). They pointed to others who had already come to similar conclusions. And they asserted that "we do not expect our broad conclusions to be substantially altered by further revisions" (Meadows *et al.*, 1972, p. 22). Nonetheless, Meadows *et al.* knew these numbers and this model would be quickly subjected to intense scrutiny.

The MIT team had held pre-publication meetings with at least two other research groups—SPRU and Hudson—that did not completely agree with their methods or conclusions (see discussion in Cole *et al.*, 1973a; Kahn *et al.*, 1976). And so, *Limits* is full of warnings and caveats. The authors warned that "these graphs are *not* exact predictions of the values of the variables at any particular year in the future. They are indications of the system's behavioural tendencies only" (Meadows *et al.*, 1972, p. 93, emphasis in original). They also admitted that,

> The model we have constructed is, like every other model, imperfect, oversimplified, and unfinished. We are well aware of its shortcomings, but we believe it is the most useful model now available for dealing with problems far out on the space time graph. (Meadows *et al.*, 1972, p. 21)

Nicolas Georgescu-Roegen (1975) later suggested that this embrace of imperfection is what triggered the fiercest criticism of *Limits*. The analytical approach used by the MIT group resembled econometrics, but it did not meet those epistemic standards. "From all one can judge, it was this fact that irked economists to the point of resorting to direct or veiled insults in their attack" (Georgescu-Roegen, 1975, p. 364). Meadows *et al.* proceeded, nonetheless. They asserted that decisions were being made everyday that could have long-term repercussions for the "world system"—and they argued that those decisions "cannot wait for perfect models and total understanding" (Meadows *et al.*, 1972, p. 22).

MODELS OF DOOM

Researchers at the SPRU invited Dennis and Donella Meadows to visit the University of Sussex in July 1972—only months after the publication of *Limits*. There was "a valuable and constructive discussion" (Cole *et al.*, 1973b, p. vi). The Meadows provided the SPRU team with technical reports to accompany the analysis in *Limits*. Dennis Meadows left the meeting with the impression that the SPRU team generally agreed with *Limits* (see Enrico, 2013). But the SPRU team left these conversations "convinced that they [MIT] would prefer frank and professional criticism to flattery" (Cole *et al.*, 1973b, p. vi). Within less than a year, the SPRU published its edited volume of papers criticizing the MIT model. In less than a year again, this was reprinted as *Models of Doom* (Cole *et al.*, 1973a). Both editions of the SPRU response (Cole *et al.*, 1973a, 1973b) contain the same 14 chapters authored by a team of 13 researchers. The reprint (Cole *et al.*, 1973a)—which also appeared as a special issue of the journal *Futures* (see Streatfeild, 1973)—added a 24 page response from the authors of *Limits*. All this text contains evidence of the dramatically different approaches to "modelling" valued by the two project teams.

In his introductory chapter, Freeman sums up the SPRU response by saying "the criticism is extensive, and sometimes severe" (Cole *et al.*, 1973b, p. 5). Harvey Simmons, a Canadian political scientist who worked on the project as a Visiting Fellow at the SPRU, wrote more vividly that the MIT team's "apocalyptic visions of the immediate future are tempered by the glittering image of utopia barely discernible through the fire and brimstone that rages in the historical foreground"—he then quickly noted that "this is not to denigrate the beliefs of the Forrester/Meadows school in any sense" (Cole *et al.*, 1973b, p. 207). Indeed, the SPRU team seems to have generally agreed on the fact that some real physical limits to growth do exist. However, they concluded that the real challenge facing the world was one of insufficient socio-technical development—especially in the Global South (see discussion in Smith and Ely, 2025).

The SPRU team expressed concern that global policy makers might not work toward the necessary socio-technical change after reading the "doomsday" predictions within *Limits*. In one of his chapters, Keith Pavitt noted that "people tend to believe predictions and their conclusions and policy recommendations tend to creep into the collective psyche" (Cole *et al.*, 1973b, p. 156). Elsewhere, Pauline Marstrand and Craig Sinclair worried that attention might be "drawn away from what are urgent, and still soluble problems, and diverted into speculation upon an imaginary race against time between 'life' and 'global asphyxiation'" (Cole *et al.*, 1973b, p. 88). And Marie Jahoda, who chaired the SPRU project team, worried that reading *Limits* might "restrict, not to say paralyze decision makers now in taking actions on current urgent problems, actions which in their total impact may well contribute the most powerful negative feedback loop to falsify the doomsday curves" (Cole *et al.*, 1973b, p. 215). At the same time, she worried that her own team's criticism might similarly disarm policy makers. She wrote that the SPRU's assertion "that Forrester and Meadows have gone wrong must not lead to the conclusion that we can sit back and relax in the best of all possible worlds" (Cole *et al.*, 1973b). She was disagreeing with the construction of the MIT model but agreeing with the need for action.

Yet somehow the SPRU researchers could not come to their own agreement on whether *Limits* was in favour of socio-technical change. In one section, Craig Sinclair wrote that the MIT approach "explicitly rejects the possibility that improved social control mechanisms may diminish the risk of disasters" (Cole *et al.*, 1973b, p. 181). Like others on the SPRU team, he seems to have felt that *Limits* ought to have assumed socio-technical change would occur impulsively in response to the system-level threats. Many passages of the SPRU text argue that socio-technical change was missing from *Limits* and ought to have been built into the math. But later, Harvey Simmons wrote that "Forrester and his men" were "stressing that political solutions will be needed to prevent world disaster" (Cole *et al.*, 1973b, p. 193). And William Page wrote that,

> …they are not attempting to predict the future, but to show the possible consequences of present trends and relationships continuing without drastic change. Indeed the message of most of the doomsday authors is not that forecasts are necessarily expected to materialize—but that they could do so if appropriate action is not taken now. (Cole *et al.*, 1973b, p. 172)

Here, Page appears to have been more sympathetic to the *Limits* approach and model than the others on the SPRU project team.

Most members of the team found that corrections to the MIT numbers and assumptions would dramatically change the results. Pavitt explained: “there will not be a crisis of the form described in the models, if historical trends [in social, political, and technical change] continue” (Cole *et al.*, 1973b, p. 153). And to summarize the SPRU conclusions, Freeman said, “…in general we do not believe that the physical constraints are quite so pressing as the MIT team suggests. We do not accept their enthusiastic endorsement of zero growth as the ideal for the world” (Cole *et al.*, 1973b, p. 10). The SPRU team saw too many problems with the modelling presented in *Limits*. While Freeman certainly expressed strong environmental values during his career (e.g., Freeman, 1992; Freeman, 1996), and concern for the environment was expressed in various parts of the SPRU text, other key values separated the MIT and SPRU project teams.

When the prominent Marxist David Harvey soon entered the *Limits* debate, he explained the conflict between MIT and SPRU in simple terms. He said, “the difference between the Meadows model and the Sussex team’s refashioning of it is largely due to the pessimism of the former and the optimism of the latter” (Harvey, 1974, p. 271). These pessimism/optimism labels were later refined when Herman Kahn’s Hudson Institute entered the fray with their own modelling (Kahn *et al.*, 1976). They constructed a table outlining different positions that had already been taken within the *Limits* debate. On the pessimistic extreme they positioned the MIT team and its supporters under the heading, “the convinced neo-Malthusians” (Kahn *et al.*, 1976, p. 10). It had become common shorthand to criticize *Limits* by invoking the old theories, classist politics, and modelling errors of Thomas Malthus (who wrote about population growth almost 200 years earlier). However, this was an oversimplification of the MIT perspective (Vieille Blanchard, 2015). Meanwhile, on the optimistic extreme, Kahn *et al.* (1976) positioned the SPRU in a group they called “the technology and growth enthusiasts” (p. 10). This highly optimistic perspective was focused on an “unlimited pie” where technology “solves almost all problems” (Kahn *et al.*, 1976, p. 10). By and large, the Hudson team ascribed to this position themselves. They said, “Man has always risen to the occasion and will do so in the future despite dire predictions from the perennial doomsayers who have always been scandalously wrong” (Kahn *et al.*, 1976, p. 10). For the critics at the SPRU and Hudson, this pattern of “rising to the occasion” was a fundamental oversight in the mathematical modelling of *Limits*.

ANALYSIS

My analysis further complicates the distinction between "optimistic" and "pessimistic" modelling assumptions. In the following short sections, I will describe two methodological differences between the modelling teams at the SPRU and MIT. My focus is on identifying the emergent scientific norms at the SPRU from their written arguments and differences in technical approach. First, I will describe how that the SPRU's work on *Limits* fetishized data precision. Then, I will show how the SPRU's work (role)modelled faith in socio-technical change. I will argue that this was more than simple techno-optimism; *Models of Doom* initiated a toxically positive disciplinary norm.

NORM 1: FETISHIZATION OF DATA PRECISION

The SPRU authors were justifiably critical of the numbers presented in *Limits*. Reading it fifty years later, one cannot help but bristle at the simplistic approach to certain calculations. Yes, we have better computers now. But the assumptions driving the math were also very rough. In my first year of grad school, I wrote a mediocre term paper showing how different approaches to population modelling produce wildly different estimates. I learned to account for changes in cohort-based birth rates, geographic variations in population change, and migration effects. But I found none of this in *Limits*. So, I can see how people with more training in econometrics would be struck by the lack of precision in the MIT modelling.

In retrospect, there is no doubt the basic projections in *Limits* were off by considera-ble magnitude. The MIT team projected a global population of 7 billion by 2002 (Meadows *et al.*, 1972, p. 38). The world population was approximately 6.27 billion at that time (World Bank, 2023). To make up the difference, the world would have needed at least two more United States; the total US population is (now) approximately 341 million (UN DESA, 2024). Or, put another way, the population estimate in *Limits* was off by about eight years: the global population reached 7 billion in 2010 (World Bank, 2023). Note, however, that the SPRU team was also incorrect in its population predictions. In the SPRU's chapter on population forecasting, William Page suggested the MIT figures would be underestimated. He argued that past attempts at long-term population forecasting have all been wrong. After reviewing past approaches to population modelling, he concluded that "it is impossible to know with certainty and accuracy a country's population over the long term future" (Cole *et al.*, 1973b, p. 172). He expected the MIT numbers would eventually prove to be just as inaccurate as past population forecasts. He was right. Anyone can now confirm that the projections in *Limits* were wrong. But those numerical outputs of the *Limits* model were not the primary concern at the SPRU.

Like other critics, the SPRU authors emphasized concerns about the data that supported and underpinned the structure of the World Model. This was most firmly stated by Cole and Curnow, who wrote: "since no firm empirical evidence is available, any relationship assumed in the model must be arbitrary" (Cole *et al.*, 1973b, p. 127). Here, they were focused on the relationships between pollution, agricultural production, and industrial production. Their word "arbitrary" was a clear rhetorical exaggeration; the SPRU team certainly agreed that relationships existed between these forces. But they disagreed with the MIT group about the *volume* of "persistent pollution" can be "absorbed" by the planet (Cole *et al.*, 1973b, p. 82). And so, they were not trying to say that the causal relationship was arbitrary. Rather, Cole and Curnow seem to have been making the point that the MIT team had not provided the empirical evidence normally expected when specifying an econometric model. This point recurs throughout the SPRU critique. The economist William Nordhaus (1973) took this further in his own response to *Limits*, saying: "without an accurate model there is no assurance that systems dynamics is better than mental models; the main result is a spurious and misleading precision" (p. 1157). In other words, they were saying that the precision of data moving through the model is unimportant if the relationships between elements of the model are not specified precisely and empirically.

The SPRU took this opportunity to call for better data. In summarizing problems with the MIT model, Cole and Curnow explained, "the database for the model is inaccurate, but not through any fault of Meadows and his colleagues... It is hoped that the MIT work will stimulate the collection of better statistics" (Cole *et al.*, 1973b, p. 109). This call for better statistics is resonant throughout the SPRU critique. The SPRU would go on to encourage and support the development of precise[2] datasets on sociotechnical change, especially through Freeman's relationship with the OECD (Fagerberg *et al.*, 2011; Soete, 2019). But the SPRU's concerns about data precision frequently sidelined important points in *Limits*. For example, Sinclair's chapter on environmentalism argues that pollution should be addressed as only a local concern, since there was no precise data on pollution at the global level. He argued that "there are so few reliable data on pollution... that attitude and judgment inevitably play a very large part in determining the assumptions that are made" (Cole *et al.*, 1973b, p. 176). And, with respect to the possible long-term climactic effects of pollution, he said, "even where modern predictions can be made for future physical levels of pollutants, technical uncertainty as to the effects of these is often such that firm recommendations cannot be made as to safe levels" (Cole *et al.*, 1973b, p. 180). Again, the SPRU team was agreeing that pollution is harmful. But they were also suggesting that the MIT group might be underestimating the amount of pollution that can be 'absorbed' or

[2] Fred Gault (2018; 2020) has shown that the resulting datasets are imprecise in their measurement of innovation.

otherwise mitigated by natural and socio-technical responses. They were suggesting that some indeterminate amount of pollution might be acceptable, and they were criticizing the lack of precision in the MIT estimates. There was no urgent need for global action but rather a need to collect more precise data.

The response to this from the MIT group was sharp. Meadows *et al.* accused the SPRU of "choosing to attack the straw man of perfection" (Cole *et al.*, 1973a, p. 221). They said that "instead of waiting for perfect models, society must work to construct and implement the best models possible today" (Cole *et al.*, 1973a, p. 73). And they explained the SPRU's obsession with precision as being driven by "a modelling philosophy that is directed primarily toward precise, short term prediction and is based on a reductionist view of the world" (Cole *et al.*, 1973a, p. 220). The MIT team clearly did not share the same cultural assumptions about modelling. They were attempting to provide an anti-reductionist model. They wrote: "the emphasis is meant to be not on the equations or the intricacies of the model, but on what it tells us about the world" (Meadows *et al.*, 1972, p. 23). They were trying to focus readers on the forest, not the trees. And yet, many critics pointed out that *Limits* was "very sensitive" (Vermeulen and de Jongh, 1976, p. 29) to small changes in those numerical details.

The SPRU's concerns about data precision cast doubt over the Meadows *et al.* (1972) conclusions. These same concerns also helped position the SPRU as a centre for expertise in the construction of datasets. As noted, Freeman already had a strong reputation and significant influence in shaping official statistical practices at the OECD (Godin, 2005). In the SPRU's next publication—the one that really launched its reputation for innovation studies—Freeman (1974) included a lengthy appendix on data measurement standards. That appendix would be dropped from later editions of *The Economics of Industrial Innovation* (Freeman, 1982; Freeman and Soete, 1997). And yet, interest in precise measurement would persist at the SPRU. The SPRU became tremendously influential in defining standards for data on science, technology, and innovation (Godin, 2005). In this way, the fetish for data precision became part of the disciplinary culture of innovation studies.

NORM 2: TOXIC TECHNO-ECONOMIC POSITIVITY

The teams at the SPRU and MIT also had fundamentally different assumptions about how technological innovation should enter these models. Neither team was invested in the term "innovation" at that time. Instead, both teams wrote interchangeably of economic, social, political, and technological change. They both tended to emphasize the latter—the technology. But they were not in agreement about how it should be specified in a model like this. In his introduction to the SPRU critique, Freeman explained that "technical changes are at the heart of our differences" (Cole *et al.*, 1973b, p. 10). He argued that "the MIT group is underestimating the possibilities of continuous technical progress" (Cole *et al.*, 1973b, p. 10). His colleagues Cole and Curnow repeated and broadened this sentiment: "the absence from the models of certain adaptive economic technological and social feedback processes is considered to be particularly suspect" (Cole *et al.*, 1973b, p. 108). Jahoda also concluded that the MIT model was missing "purposeful adaptive processes which continuously occur in the real world through political, social, economic and technological actions" (Cole *et al.*, 1973b, p. 213). Throughout their work, the SPRU team argued that adding such processes to the model would radically alter the results. For example, the SPRU team felt that adding technological change would mean that "any physical limits to agricultural production recede beyond the time horizon of the model" (Cole *et al.*, 1973b, p. 56). And, adding it across all dimensions of the model "has the effect of indefinitely postponing the catastrophes which the model otherwise predicts" (Cole *et al.*, 1973b, p. 10). The results of this technology-optimistic math lead Freeman to suggest that R&D—i.e., technological innovation—could serve as an "insurance policy" against the risk that the world might crash up against the limits to growth (Cole *et al.*, 1973b, p. 12). This was clearly not the view of Meadows *et al.* (1972).

Meadows *et al.* provided a different perspective on innovation in their reply. They asserted that they had not forgotten to account for technological innovation in their model (see Meadows *et al.*, 1972, pp. 48, 63). More clearly than any other text, the 30-year anniversary edition of *Limits* explains how certain technologies were built into the 1972 model: "health care, birth control, agricultural improvement, resource discovery and substitution" (Meadows *et al.*, 2004, p. 209). Other technologies were treated differently:

> They include resource efficiency and recycling, pollution control, unconventional increases in land yield, and land erosion control. When we first built the model, we didn't consider these technologies so established that they were technically proven and readily adopted by anyone in the world who could pay for them. We therefore programmed them so they could be activated as a discontinuous step at any simulated time that seemed reasonable to the model user. For instance, one could assume that the entire world would make a major commitment to recycling in 2005 or concerted effort against pollution in 2015. (Meadows *et al.*, 2004, p. 209)

Here, we see the specific difference in thinking that led to dramatically different mathematical approaches. The MIT team chose not to assume that technological innovation would be automatically triggered. In their response to the SPRU, they said, "we view technology as socially determined, discontinuous, infinitely varied, and delayed" (Cole *et al.*, 1973a, p. 233). They also elaborated their own model of technological innovation (Cole *et al.*, 1973a, p. 232)—and it had the kind of feedback mechanisms that would eventually appear in Kline and Roseberg's (1986) chain-linked model. The MIT team was also keen to point out that technology is not always a positive force for people or the planet. Overall, the evidence confirms that the MIT team had thought extensively about innovation.

So, it is on this basis that the MIT team criticized the SPRU team for its simplistic treatment of technological change. They said: "we cannot view it [technological change], as the Sussex group consistently does, as a cost free, purely beneficial, miraculous force that can repeal natural laws and rollback physical limits indefinitely" (Cole *et al.*, 1973a, p. 237). Somewhat sarcastically, they pointed out that it is unsurprising how exponential growth becomes possible when you introduce "exponential technologies" into the model (Cole *et al.*, 1973a, p. 226). It had been the SPRU's conclusion that human potential can "bend the imaginary exponential growth curves to gentler slopes than overshoot and collapse" (Cole *et al.*, 1973a, p. 211). Summarizing this SPRU position, Jahoda wrote that "man's inventiveness in changing social arrangements is without limits even if not without hazards" (Cole *et al.*, 1973a, p. 215). In this way, the SPRU was arguing that the MIT team should have built faith in technology into the model.

Thirty years later, the remaining members of the MIT team would continue to reject this argument, saying that,

> It is true we did not include in the original World3 model technological progress at rates that would automatically solve all problems associated with exponential growth in the human ecological footprint. That was because we did not—and still do not—believe such tremendous technological advance will occur by itself, nor through the unaided operation of 'the market.' (Meadows *et al.*, 2004, pp. 203-204)

In other words, they saw no place in their model for this kind of faith in technical change. Indeed, they remained concerned about mathematical renderings of this faith. Meadows *et al.* (2004) said,

> For many economists technology is a single exponent in some variant of the Cobb-Douglas production function—it works automatically, without delay, at no cost, free of limits, and produces only desirable outcomes. No wonder economists are so rapturous about its potential to solve human problems! (Meadows *et al.*, 2004, p. 210)

The SPRU team was not exclusively full of economists. And Freeman only briefly mentions a Cobb-Douglas function in his response to *Limits* (see Cole *et al.*, 1973a, p. 77). Nonetheless, these arguments speak to the epistemic differences between the two camps. The SPRU and MIT teams could not agree because they viewed—and modelled—technological change differently.

This aspect of their disagreement would lead the MIT team to being labelled as anti-technology. Meadows, Randers, and Meadows would later say, “if we suggest that technology or markets have problems or limits, some will consider us to be heretics, and they will say that we are anti-technology” (Meadows *et al.*, 2004, p. 205). Indeed, Kahn *et al.* (1976) expanded on the arguments from SPRU and further characterized *Limits* as anti-technology. They described the MIT team’s “neo-Malthusian” position as the polar-opposite of any “technology and growth enthusiasts” (Kahn *et al.*, 1976, p. 10). This contrast established a straw man position: the group from MIT became the adversaries of technology-because they were adversaries of unbridled growth. Notice the entanglement of technology and growth. Also notice the irony. The *Limits* team was situated in one of the USA’s most important institutions for technology. The project was initiated by Forrester—a highly accomplished engineer who held patents on key computer and defense technologies (Lane, 2007; Ramage and Shipp, 2009). The MIT team was not anti-technology (Vieille Blanchard, 2015).

The MIT team was not even pessimistic. In a documentary film about their work and this debate, Dennis Meadows and Bill Behrens both express the surprise they felt at having been called pessimists. Behrens says, “At that time we were all optimists. Every single one of us believed that the research that we were doing and the book that we were writing was a prescription for optimism” (Enrico, 2013, starting at 27:10). Indeed, this self perception is confirmed in footage from the *Limits* book launch event in 1972. Donella Meadows said, “I’m very hopeful in this country that the mechanisms do exist for this kind of cultural change. In fact, I think it’s already happening” (Enrico, 2013, starting at 26:15). And so, I have no doubt that the MIT team was hopeful and optimistic that *Limits* would inspire innovation. It was spurious to call them pessimistic.

Conversely, I would argue that the SPRU team was advocating a toxically positive perspective on innovation. Toxic positivity is an extreme form of positive or optimistic thinking that denies negative realities and emotions. In the late 1970s, Neil Weinstein (1980) studied a phenomenon that he called “unrealistic optimism about future events” (p. 806). Later, Jack Halberstam (2011) called this “toxic positivity” (p. 3). It is a cultural norm where people who express negative emotions or concerns are quickly redirected to focus on the bright side (Ehrenreich, 2009; Halberstam, 2011). This is the kind of pushback Meadows *et al.* (1972) received from the SPRU and then from others, like the Hudson group. The negative effects of growth they had highlighted were sidelined by arguments and mathematical adjustments that positioned technology as an overwhelmingly positive cure. The father of ecological economics, Nicholas Georgescu-Roegen, sarcastically caricaturized this aspect of the SPRU’s work:

> Should we run out of some resources, we will always think up something, just as we have continuously done since the time of Pericles. Nothing, therefore, could ever stand in the way of an increasingly happier existence of the human species. (Georgescu-Roegen, 1975, p. 360)

For their part, the SPRU team admitted that some of their changes to the modelling "could be criticised as being over-optimistic" (Cole *et al.*, 1973a, pp. 130-131). They argued that this over-optimism was a justified antidote to the pessimistic assumptions in *Limits*. But it was not.

In hindsight, present-day SPRU researchers have noted that their predecessors' techno-optimism, "has not aged well" (Smith and Ely, 2025, p. 237). Consider some of the specific technologies that the SPRU team presented as justification for their mathematical optimism. They asserted that "it is not unreasonable to expect that within 30 years a breakthrough with fusion power will provide virtually inexhaustible cheap energy supplies" (Cole *et al.*, 1973a, p. 103). Fifty years later, we still do not have workable fusion power. And, "the running joke is that engineers have been saying it's a decade away for about six decades now" (Hickel, 2020, p. 145). But the SPRU team argued that "should this breakthrough take longer, pessimism would still be unjustified" because we would still have "nonconventional hydrocarbons" to extract (Cole *et al.*, 1973a, p. 103). This is where the SPRU arguments became the most environmentally toxic.

A page and a half of the SPRU text is dedicated to the possibilities (and technological challenges) of developing the Alberta oil sands, similar heavy oil deposits in Venezuela, oil shales, "and the conversion of coal to oil and gas" (Cole *et al.*, 1973a, pp. 103-104). In hindsight, we know that the extraction of these nonconventional hydrocarbons has been extremely damaging to the planet (Rosa *et al.*, 2017). We could excuse the SPRU team for not knowing this in the 1970s; hydrocarbon projects like these have long been the subject of rhetorical promises and green-washing (Katz-Rosene, 2017). But the key point remains: the SPRU team was excessively optimistic about the promise of technology to drastically extend growth. They were using specific technologies like fusion power and nonconventional hydrocarbon development as exemplars of other promising, yet-to-be-developed technologies. These examples were support for the argument that some indeterminate future socio-technological changes should be assumed within the model. Jason Hickel caricatured arguments like this, saying:

> It's like jumping off a cliff while hoping that someone at the bottom will figure out how to build some kind of device to catch you before you crash into the rocks below, without having any idea as to whether they'll actually be able to pull it off. (Hickel, 2020, p. 128)

My own conclusion will draw fewer laughs. This is because I am less concerned with the specifics of the SPRU's techno-optimism. I am more concerned with the lasting normative effects.

Much has been written about techno-optimist (or sometimes "Promethean") arguments (e.g., Danaher, 2022; Hornborg, 2024; Keary, 2016; Königs, 2022; Krier and Gillette, 1985). John Danaher (2022) has summarized a variety of these techno-optimist positions, and their criticisms. In simple terms, he defines techno-optimism as "the view that technology, when combined with human passion and ingenuity, is the key to unlocking a better world" (Danaher, 2022, p. 2). This definition is consistent with the perspective expressed by the SPRU team in *Models*. They argued that technology cannot fix all ills (Freeman and Jahoda, 1978, p. 389), but that "sustainability could be achieved by reorienting the R&D system and through major institutional change" (Stern and Valero, 2021, p. 2). Danaher (2022, pp. 3-4) says that all varieties of techno-optimism and pessimism involve taking a "stance" or committing to a viewpoint like this. But what happens when that viewpoint is translated into statistical techniques and methodological norms?

Others have already noted that the SPRU were first to take a techno-optimistic, "Promethean," or "cornucopian" stance against *Limits* (see Icassatti Corazza *et al.*, 2015; Keary, 2016; Vieille Blanchard, 2015). This was expressed in both writing and mathematical modelling. Others would join the chorus with slightly different optimistic perspectives. But I agree with Krier and Gillette (1985) that, overall, the techno-optimist critiques of *Limits* were about more than a commitment to exponential growth and innovation. These critiques also involved ignoring the degree to which some innovations are bad for people and the planet (Krier and Gillette, 1985). Retrospectively, some might call this a "pro-innovation bias" (Godin and Vinck, 2017a). But the term "innovation" had not yet entered the conversation. Indeed, this was only the beginning of the scholarly efforts that would establish innovation studies as a discipline (see Fagerberg *et al.*, 2013; Godin, 2017; MacNeil, 2024; Soete, 2019). As I have said from the outset: the SPRU's work here was the groundwork for long-term organizational and disciplinary norms. So, I propose that this was something more than a one-time stance or bias. This was toxic positivity. Before its first publications about innovation, the SPRU was establishing arguments and mathematical techniques that made complex, techno-critical, degrowth perspectives appear irrational. The norm would be to model innovation from only a positive perspective.

CONCLUSION

This paper has contributed to the intellectual history of innovation studies by examining the methodological practices that were developed and employed in the early days of the Science Policy Research Unit. The SPRU is well-known for its members' many positive contributions to innovation theory and policy (see Fagerberg *et al.*, 2011; Soete, 2019). Less consideration has been given to the SPRU's participation in the *Limits to Growth* debate. Present-day SPRU researchers, Smith and Ely (2025), recently looked at this with an eye for its impacts on the international politics of technology. But most of what has been written situates *Models of Doom* (Cole *et al.*, 1973a, 1973b; Streatfeild, 1973) as the techno-optimist turning-point in the *Limits* debate (Higgs, 2014; Vieille Blanchard, 2010; Vieille Blanchard, 2015). None of this is well studied in STS (Sharma *et al.*, 2025). In this paper I have argued that the SPRU team's work here helped establish norms for the emerging fields of innovation research and policy. I will now conclude with some discussion of my methodological limitations (and contribution), the intellectual contribution of this paper, and future research directions.

METHODOLOGICAL LIMITATIONS AND CONTRIBUTION

I took a critical ethnostatistics approach to this key moment in innovation scholarship. As one excellent reviewer pointed out, ethnostatistics (Gephart, 1988, 1997, 2006) is a scantly used approach and my work here extends its scope considerably. Ethnostatistics was developed within organization studies but has had limited use even there. A few critical management scholars have deployed it to problematize the assumed objectivity of organizational statistics (e.g., Boje *et al.*, 2004; Helms Mills *et al.*, 2006; Smith *et al.*, 2004). At the intersection with innovation studies, Kilduff and Oh (2006) used ethnostatistics to surface the assumptions inside four conflicting analyses of the same medical innovation diffusion data. And in my book, *Observing Dark Innovation* (MacNeil, 2024), I fused ethnostatistics with autoethnography to understand neoliberal norms of innovation quantification. There, I criticized ethnostatistics for often employing the "god trick" (Haraway, 1988) of providing seemingly distanced description of 'others' statistical work. Indeed, that classic criticism of ethnography could be applied to this paper. But I have attempted to remediate this aspect of ethnostatistics by injecting critical reflexivity. This is why my voice and politics surfaced frequently throughout this paper. By now it should be clear that this project was motivated by personal ecological values, a post-growth positionality, and a concern for deconstructing counterproductive disciplinary norms (see also, MacNeil, 2024). In this way, my approach to ethnostatistics is closer to other styles of sociological quantification studies (see Lippert and Verran, 2018). It differs in its direct focus on statistical methods as evidence of cultural norms.

To some extent, I have inverted the research question that was asked forty years ago by Brian Bloomfield (1984). In one chapter of his PhD thesis, Bloomfield compared the modelling approaches of the SPRU and MIT teams to better understand norms in the emerging field of system dynamics. His thesis was written before the term "ethnostatistics" was coined. But he called his work "ethnographic" (p. 85) and he was interested in the "professional cosmology" (p. 85) of "system dynamicists" at MIT. In my case, I was interested in the professional norms that were being established by the early innovation scholars at the SPRU. In both cases, he and I have relied on published traces of past scholarly activity. No one is able to produce ethnographic "thick description" (Geertz, 1973) of the past without access to a time machine. We can only work from traces. This is where a critically- and historically- conscious ethnostatistics reveals its greatest limitations, but also its utility. It cannot provide a full ethnographic description of the past, but it is useful for the deconstruction of long-standing methodological/disciplinary norms.

INTELLECTUAL CONTRIBUTION

In this paper I found that both the MIT and SPRU teams agreed on the fundamental fact that planetary limits exist. However, they fundamentally disagreed about how these limits should be approached. I have examined two aspects of their disagreement.

First, I explored how the SPRU team criticized the lack of data precision in *Limits*. For many dimensions of the MIT model, the SPRU team noted the lack of high-quality data and dismissed the surrounding arguments. I have suggested that this amounted to a fetish for data precision. The fetishization of data precision extended beyond this debate as the SPRU was quickly developing influence and a reputation for building datasets and data standards. But my conclusion here should not be read as an argument for statistical anarchy or the abandonment of all modelling. It is only a further call for methodological humility (Law and Singleton, 2005, p. 350; MacNeil, 2024, p. 147). No degree of data precision can build a time machine or crystal ball.

I have also described how the SPRU team was simultaneously arguing that *Limits* ought to have taken a very imprecise approach to socio-technical change. The SPRU arguments helped falsely position the MIT team as anti-technology. They made the exaggerated argument that technology had been left out of the *Limits* model. The SPRU's techno-optimist arguments were then presented as a more realistic approach. And in their own math, the SPRU team demonstrated that the limits to growth will extend beyond our current time horizon if we assume greater technological advancement. This approach made any natural limits fade asymptoticly into the future. Here, they were advocating mathematical faith in technological change. There are a variety of terms that might apply here, such as "techno-optimism" (Danaher, 2022; Königs, 2022; Krier and Gillette, 1985), "techno-solutionism" (Sætra, 2023), or "Prometheanism" (Hornborg, 2024; Keary, 2016). But these existing labels are focused on researchers' attitudes or stances toward technology. These are positions one might take in a debate. The concept of "toxic positivity" (Halberstam, 2011) shifts our focus toward a cultural norm that shapes other people's attitudes or stances. It defines the realm of possible and acceptable attitudes, stances, and arguments. I see this as an important new shift for post-growth thinking in STS and critical studies of innovation. I am proposing the concept of "toxic techno-economic positivity" as a critical interpretive key that may help further disentangle innovation from growth.

What I am not trying to do here is attack the SPRU. The team that wrote *Models* agreed that "exponential growth of population and industrialization on a finite planet cannot continue indefinitely" (Cole *et al.*, 1973b, p. 22). And so, I am not saying that the SPRU team meant to undermine the main point of *Limits*. There is no doubt about their deep concern for people and planet. Chistopher Freeman was a life-long environmentalist (Stern and Valero, 2021). I believe that he and the entire SPRU team wanted readers to 'focus on the positive.' They wanted their readers to have faith in human capacities for positive change. This is laudable. But this also had unintended, toxically positive effects. It set the stage for disciplinary norms focusing almost exclusively upon the 'bright side' of technological innovation. It suggested to scholars, policy makers, and other readers, that they need not worry about the limits to growth if they instead focus on the promise of technological innovation.

FUTURE DIRECTIONS

Both *Limits* and *Models* clearly state that mathematical modelling is a cultural and political activity. In *Models*, Freeman said, "it is essential to look at the political bias and the values implicitly or explicitly present in any study of social systems. The apparent detached neutrality of a computer model is as illusory as it is persuasive" (Cole *et al.*, 1973b, p. 7). I agree. In this paper, I have demonstrated how insights into the culture of innovation modelling can be surfaced by using a critically- and historically-conscious approach to ethnostatistics. This method has future potential for critical studies of innovation.

There are also several lines of future inquiry that arise from this paper. Because I was focused on disentangling the innovation-growth dyad, I did not explore the MIT team's arguments that the SPRU researchers had taken a very anthropocentric view of technological change. This can be found in the MIT response appended to Cole *et al.* (1973a) and in Meadows *et al.* (1973). I also did not explore concern from the SPRU over inequities between the Global North and Global South. This was the primary focus of another detailed critique of *Limits*—by the Argentinian Bariloche Foundation (Herrera, 1977). The SPRU critique of *Limits* had also included multiple claims that the MIT team was working in the interests of the global elite. This concern for global inequities does carry forward into future research at the SPRU. Smith and Ely (2025) have recently published an excellent contribution along this line of inquiry. More work is needed in decolonizing innovation studies.

My concern here has been with the ways that innovation research and policy came to believe the mantra: "not to innovate is to die" (Freeman, 1974, p. 256). In *Models*, the SPRU researchers expressed this belief in both a mathematical and a planetary-existential sense. I have shown how they argued against the *Limits* to Growth by pointing to the lack of precision in the mathematical modelling. But I have also shown how they also ignored this desire for precision and advocated mathematical faith in technological fixes. Unfortunately, this helped redirect attention from very real planetary limits—which, admittedly, the MIT team had miscalculated. *Models of Doom* suggested that scholars and policy makers could redirect their attention to the bright spots of technological innovation. I have called this redirection toxic techno-economic positivity, and I have argued that it persists today inside the epistemic culture of innovation research and policy.

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