Jan Tinbergen and the Limits of Expertise: Response to My Critics

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I am most grateful to the editors for inviting a discussion about my book *Jan Tinbergen (1903–1994) and the Rise of Economic Expertise* (2021), and I would like to thank Michele Alacevich, Mauro Boianovksy, Thomas Kayzel, Francisco Louçã, Mariana Martágua, Jon Murphy, and William Peden for their incisive and engaging comments. They have stimulated me to consider the broader implications of the rise of expertise, Tinbergen's moral and methodological choices, and the value of a biographical approach.

I. INTRODUCTION

It is clearly time to talk about experts and their expertise. But before we turn to that topic, it will be helpful to make explicit a methodological choice which remained implicit in my book; my choice of a specific interpretive framework to analyze Jan Tinbergen's life and contributions. Throughout the book, and especially in the final chapter, I have used Tinbergen's own aspirations of social engineering, moral leadership, and a mature socialism to evaluate his projects, and thus have suggested that he did not, and could never have, lived up to these demanding criteria. Michele Alacevich in his reflections, suggests that, despite all his achievements in economics and beyond, the book exudes a melancholic feeling because Tinbergen did not live up to his high-flying ideals. I agree. Stronger, I think there is something tragic in his efforts to live up to his own impossible standards. Tinbergen wanted to be more than an economist or expert, he wanted to be a moral leader.

Making this interpretive framework more explicit is helpful, especially when we contrast it with two other possibilities. The first will be obvious to historians and methodologists of economics, namely, whether Tinbergen made significant contributions to the discipline. He has made many. The Nobel Prize of 1969 highlighted his work on business cycles and, by
implication, his macro-econometric models. My book emphasizes his innovations in policy-decision models and draws attention to the theory of political-economic convergence and utility analysis and measurement (on the latter, see also Heilmann and Wintein 2021). Mauro Boianovsky, in his contribution to this symposium, makes clear that we should include growth theory to the list of contributions to economic theory (see also Boianovsky and Hoover 2009), and Michael Assous and Vincent Carret have analyzed his work on instability (Assous and Carret 2022). James Heckman (2019) has analyzed his labor market model as a precursor to hedonic pricing models. Marcel Boumans has previously drawn attention to his pioneering role in the use of models in economics (Boumans 1992). As an economist Tinbergen was no failure, not by a long shot.

Yet, one wonders if we should include Tinbergen on a list of the twenty-five most important economic theorists of the twentieth century. In hindsight, business-cycle research is more associated with the theoretical explanations of Wicksell, Hayek, and Keynes during the interwar period. The stabilization debate of the post-war period is a continuation of this theme, but mainly remembered for the debates between the monetarists and the (New-)Keynesians, with a cameo by Robert Lucas. Growth theory is directly associated with Solow, or else with the long-term dynamics analyzed in the work of Schumpeter or Kondratieff. Tinbergen’s contributions to the convergence debate were not seminal, and the measurement of utility failed. His work in welfare economics and hedonic models at best predated later seminal contributions but did not establish a new approach. Assous and Carret, rightly, present Tinbergen’s work on instability as a research program with great potential but acknowledge that it withered after 1940.

This leaves Tinbergen’s decision models, which occupy a key place in my interpretation of his work on domestic economic policy and development economics. These are of great importance if our interpretive framework is that of his contributions to the rise of economic expertise and economic policymaking; yet they can hardly be said to have seriously impacted (the development of) economic theory.¹ In my narrative about the rise of economic expertise, the decision models are a key contribution

¹ I sidestep here the more complex question of the significance of Tinbergen’s methodological innovations because these are not central to the contributions of this symposium. His use of models in economics and social science more broadly has received repeated attention by methodologists (for example, Morgan and Morrison 1999). His use of statistics in economics are put in perspective in the recent Nobel lecture by Guido Imbens (2022), and feature in Peden’s contribution, to which we turn below.
because they helped transform the relationship between economics and the state. Mark Blaug provides a simple definition of this new relationship:

Let governments decide their “objective function” defined in terms of the multiple ends or goals of economic activity; it is the task of economists to delineate the “possibility function”, the costs and benefits of alternative allocations of scarce means. (Blaug 1992, 128–129)

I hope to have convincingly demonstrated that this (imagined) relationship is not merely a result of the influence of Lionel Robbins and positivism on economics; rather, this influence was greatly facilitated by the decision models of Tinbergen and Ragnar Frisch.

So, we are left with the following scorecard—by Tinbergen's own standards: insufficient. As economic theorist: significant, but not outstanding. As economic expert: outstanding, and I would argue frequently overlooked. My book provides an interpretation of Tinbergen's life and work through a combination his own standards and the rise of economic expertise; it indeed says less about his contributions to economic theory per se. That choice was driven by the biographical approach of the project, but also by my belief that the history of economic expertise has been severely neglected. It is not always appreciated that many economists have contributed to it, but more importantly we have insufficiently considered whether the rise of economic expertise is, itself, desirable. I hope that my book demonstrates the relevance of the question and provides the start of a critical discussion of the economist as expert.

The choice of economic expertise as an interpretive framework and the analysis of the rise of economic expertise help explain why some of the contributors to this symposium sometimes disagree with my evaluations. They would have preferred the more traditional framework of contributions to economic theory, or they are more skeptical (or welcoming) of the rise of economic expertise.

In this response I will first turn to the topic of expertise and technocracy (section II). In the following section, I will reconsider the merits of Tinbergen's work in development economics (section III). Section IV reflects on the broader limits of (scientific) knowledge and the relevance of historical studies of economists and their lives.
II. Tinbergen, the Technocrat?

Thomas Kayzel makes a compelling argument that Tinbergen should, despite my arguments to the contrary, be regarded as a technocrat. He does so by contextualizing the notion of technocracy within interwar debates about science in the modern world, and the Synthesis movement in the Netherlands. He demonstrates that for the Synthesis movement, technocracy was not the rule by experts, but rather the use of expert knowledge to improve the leadership qualities of politicians. Kayzel rightly connects Tinbergen’s idea of leadership by example to the way he envisioned both his own role and the ideal role of political leaders in the modern world. In the process, he provides a deeper understanding of the elitist and anti-democratic strand in Tinbergen’s thought. In contrast to other socialists, scientists, and socialist scientists of his age, Tinbergen did not believe that scientific progress would have important emancipatory effects. Although he was a strong proponent of education, he remained wedded to a hierarchical notion of talent and ability, and thus, was convinced of the need for moral and political leadership. His own theoretical work and his activities as an expert can be understood as an attempt to contribute to this moral and well-informed leadership.

Kayzel is also correct in identifying ‘personalism’ as the type of moral conviction which best describes Tinbergen.² The protestant socialism which I describe in some detail in chapter three is further contextualized in Kayzel’s contribution, and I can only agree when he suggests that Dutch intellectuals, like Banning and De Vooys, were key influences on Tinbergen in this regard. But Kayzel also claims that these influences led to a consistent Weberian position in Tinbergen’s work. This version of Weberian science was value-neutral in the sense that it did not aim to realize certain socio-political values, it also sought to expunge the influence of values on the scientific method, though it would remain value-laden because it would be practiced by moral scientists with a Verantwortungsethik (ethic of responsibility).

There are parts of this description which I can only embrace. I repeatedly emphasize Tinbergen’s sense of responsibility and his quest for neutrality, for instance towards different political-economic systems or the different theories of the cycle. But I am less sure that it gave rise to a

² It is hard not to feel some embarrassment when somebody else characterizes the position of one’s protagonist so vividly, especially when one felt some, unjustified, restraint in capturing that personality. A small consolation is that I do mention personalism in a footnote (Dekker 2021, 34n3).
consistent position, or rather, a consistent set of choices and actions. A critical evaluation of Tinbergen’s work would not have a hard time identifying various instances in which values do enter his work. His internationalist outlook appears to be a clear instance of values entering his work and can be ‘explained’ through his position in the world, born in The Hague, city of international institutions, and citizen of a small open economy. Values might have leaked in, despite Tinbergen’s intentions to the contrary.

Even so, Tinbergen might have aimed for a value-free science combined with an ethic of responsibility, even if he did not achieve it. More interesting, therefore, is the question whether the morally committed scientist and the pursuit of rational science were not systematically in tension with one another. Kayzel, correctly, describes Weber as writing about the disintegrating forces associated with modernity. These give rise to tensions of different kinds, notably between morality and science which become increasingly separated. In my discussion of Tinbergen’s role before and during World War II, I seriously question whether Tinbergen did not strategically invoke value-neutrality when speaking up for values was too costly, during the occupation. The same values for which he, as a responsible citizen and scientist, had spoken up in the preceding years. Perhaps I was too cautious in the book, but I agree with Alacevich that Tinbergen is, in such instances, hiding behind the supposed neutrality of technocracy and failed to demonstrate moral leadership. Tinbergen’s decision to repeatedly advise authoritarian leaders, with whose politics he fundamentally disagreed, are even more problematic if we would like to ascribe a Verantwortungsethik to the Dutch economist.

More broadly, there appears to be no real safeguard in Tinbergen’s intellectual project to ensure that the result of rational science would not be used for morally and politically undesirable ends. Or that the policy tools would only be employed by ‘moral’ leaders. Tinbergen, and possibly the Synthesis movement by implication, appears to have no real answer to the problem of the use of new technologies for the destruction of humankind and the environment—this is a problem which led Tinbergen to become more skeptical about technological change with age (Tinbergen 1970, 1987). For Tinbergen’s project this issue was most tangible when it came to the use of his planning techniques. I suggest in the book that he should have been much more critical in thinking about which governments he should have helped to plan, since he would have known, both by his own standards and by any reasonable moral standards, that
planning would be used for undesirable ends.\textsuperscript{3} This was not a simple instance of bad judgment. Tinbergen engaged with both communist and fascist regimes, and in defense of his actions, appealed to commitments toward pure, apolitical neutrality, rather than to moral responsibility on his own part.\textsuperscript{4}

This apolitical defense can at best be characterized as vulgar Weberianism. One might suggest that this later work is not as closely linked to the Synthesis ideals, or that his choices in these later decades were influenced by his sustained proximity to political power. But the reason why I attribute such significance to the exchange with Van Cleeff over the Plan of Labor in 1935 is because I think it demonstrates that Tinbergen never quite lived up to the ideal which the Synthesis movement pursued. Or, to put it in other words, I don’t think he ever really found a way to reconcile the tension between morality and objectivity which Weber captured so vividly.

A special instance of the tension between moral ideals and the 'hard-to-control' effects of economic tools and techniques is that of the expertise institutions which Tinbergen founded, contributed to, or inspired. Mariana Mortágua and Francesco Louçã claim that the creation of contemporary institutions of policymaking and the role of experts in current affairs is no longer characterized by the Frisch-Tinbergen approach. More specifically, they suggest that current efforts to deregulate and liberalize the economy have brought back the “monstrosities” of the past, such as structural instability and deep economic crises (41). They do not specify which institutions they refer to, but a key argument of my book and subsequent work has been to demonstrate that modern economic expertise was institutionally established between (roughly) 1930 and 1970 (Dekker 2022).

Their detailed contribution adds further context to what motivated Tinbergen and Frisch to (attempt to) establish a position of authority for economic experts. Both economic as well as socio-political stability were important drivers for those who came of age during the interwar period.

The most important element of the Tinbergen-Frisch program was to improve policymaking to ensure that the economic collapse of the 1930s would not recur and the national and international political crisis of the following decade could be avoided in the future. This effort was

\textsuperscript{3} Here I accept for the moment that planning itself was not a technique with potentially harmful political and economic consequences (Hayek [1944] 2001).

\textsuperscript{4} He occasionally suggested that it was the moral responsibility of the scientist to talk to anyone, a position which in my opinion tends to collapse into pure apolitical neutrality.
successful and economic expertise came to occupy a key place in statistical bureaus, planning agencies, councils of economic advisors, and international organizations like the European Coal and Steel Community, the International Monetary Fund (IMF), the International Bank for Reconstruction and Development, and many others, sometimes adjacent to democratic institutions, other times replacing them.

There is interesting intellectual history underway which suggests that the institutionalization of (international) economic expertise began earlier (for example, Martin 2022). However, I disagree with Mortágua and Louçã, as well as various historians of neoliberalism, who suggest a strong break after the mid-1970s. Within policy orientation that break is apparent; in the simplest terms, it marks the shift from Keynesian macro-management to supply-side and innovation-driven policy (neoliberalism, if you insist). But my argument, which I could not fully develop in the book, is that this shift takes place within the mid-century organizations of economic expertise and is precisely so influential because the institutional infrastructure of expertise is already present. If we believe that this new policy orientation is misguided, it is at the very least mistaken to blame the neoliberal economists for it. The economists were there since the mid-century, and they had achieved their positions of influence and institutional prominence thanks to the efforts of an earlier planning-inspired generation.

What is more, the goals such as price stability and a growing economy, which have been pursued since the 1980s, are ultimately just variations of the pursuit of macro-economic stability and a full employment economy in the preceding decades. The idea that goals, say a 2% inflation target or a 4% unemployment target, can indeed be consistently achieved through policy instruments, such as monetary and fiscal policies, reflects the engineering view of the economy that Tinbergen and Frisch helped to popularize. That the instruments are now more frequently cast in terms of competition and deregulation does little to change the underlying mindset, the targets-instruments toolkit is precisely designed to be neutral with respect to instruments. To also expect a particular political direction from the experts who govern the economy is to expect them to be moral leaders, much as Tinbergen hoped they would be, but which he failed to ensure. That failure, in my interpretation, is not a coincidence but a natural result of unresolved tensions in his intellectual project.

I take Jon Murphy to be interested in these issues from a related angle. He treats Tinbergen and John Maynard Keynes as exemplary for different
models of expertise (cf. Maas 2014, chap. 5). The Keynes model is based on intuition, judgement, and personal authority; the Keynesian expert has a broad orientation and advises on both technical and political-moral issues. The Tinbergen model is based on economic models, statistical evidence, and impersonal scientific authority; the Tinbergenian expert advises on narrowly technical issues. The latter respects the strictures as Blaug laid them out. Murphy, however, moves beyond these ideal-typical models of expertise and connects them to the type of authority that experts exercise. In his more detailed typology he distinguishes between experts who seek (or have) jural authority, that is, the ability to make decisions. He contrasts these experts with those who merely seek to advise the decision-maker, such as the politician or the citizen. This results in four types of expertise which differ in the scope of their expertise (technical or also on values) and the type of authority they have (advisory or decision-making).

In Murphy’s elegant typology Keynes is an example of the broad expert who seeks decision-making authority. Tinbergen is characterized as the opposite, a narrow expert who seeks a position as advisor. Murphy does not provide examples of economic advisors of the other two types; broad but without decision-making authority or narrow with such authority. There are reasonable arguments to suggest that Tinbergen might be either. As we discussed, he aspired to be more than just a technical expert, so he was not always content with just narrow expertise. And the elitism emphasized by Kayzel is suggestive of a desire for a dominant position.

But Murphy is clearly talking about ideal-types, and the distinction between dominance and non-dominance is of considerable relevance and has been recognized by different economists (Buchanan 2000; Ostrom 2000). It captures the (ideal) attitude of the social scientist toward society and the democratic polity (cf. Dekker 2020). Yet, I think that to understand the institutionalization of economic expertise we need a somewhat different typology. For that typology I believe that the initial distinction in Murphy’s article between influence and authority is more useful.

In the preface of the book, I attempt to differentiate the influence (there incorrectly labelled ‘authority’) of thinkers like Keynes and Hayek, from the authority of economic experts, such as Tinbergen. The influence of the former is at the level of concepts and ideologies, while the authority of the latter is based on scientific techniques and their institutional position, for example as monetary authorities at a Central Bank or as
economic forecasters or planners at an organization like the Dutch Central Planning Bureau (CPB), which Tinbergen established. Rather than a desire for dominance, I think this distinction draws attention to the difference between institutionalized (political) positions of authority for economists and their indirect ideological influence.

Murphy suggests that Keynes was convinced that experts should have decision-making authority. And there are instances in which the British economist indeed had it. For instance, when Keynes was the British delegate at Bretton-Woods, where he occupied a position of institutional authority with a broad (values and technical) mandate. But we remember Keynes more for his ideological influence; his theories of economic (in)stability, uncertainty, and the role of the government in the economy. These ideas had considerable influence, although he developed them without formal or political authority. Hayek, who only briefly held a position as economic expert, at the business cycle institute in Vienna early in his career, is another good example of someone whose ideas had broad influence (values and technical), but he largely lacked governmental authority.

Tinbergen had some ideological influence in the Netherlands, particularly through his work on the Plan of Labor. But in the book, I suggest his main contribution was the institutionalization of expertise. To be able to achieve this Tinbergen had to narrow the authority of the (imagined) expert, precisely to increase their (potential) authority. Although there are exceptions, I think the rise of economic expertise was enabled by this claim of narrow technical expertise. Modern economic experts of the Tinbergen type did not rely on a claim of unique personal abilities, but rather on objective forms of technical competence. This claim enabled a new relationship of economists to the state, especially through the institutions of expertise such as the CPB or the IMF. The individual economist could, in special circumstances and through exceptional abilities, achieve a position of authority, as Keynes did, but this was the exception.

To analyze whether expertise limits or undermines democracy the intentions of economists, which Murphy emphasizes, are not unimportant. But more significant is whether their claim of expertise is recognized and institutionalized by the modern state. This can even happen when the intentions of the economists are more limited; as I suggest at the end of

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5 Murphy rightly observes that Keynesianism, the macro-management associated with the post-war decades, has little in common with Keynes' own convictions about the right type of expertise.

6 The episode in Chile is only a very partial exception (Caldwell and Montes 2015).
the chapter on fascism, we should also understand Tinbergen's success as a story of how the “state was able to mobilize modern economic expertise in its service” (Dekker 2021, 221). To me similar worries are relevant when he provides expert advice, and thereby legitimacy, in Turkey, Indonesia, Spain, and to Communist countries.

Another reason why it is precisely the institutionalization which should be studied is because there are good reasons to question Tinbergen’s self-presentation of the narrow scope of technical expertise. Keynes did not merely claim that he had broad expertise, but as Murphy demonstrates, he was also skeptical that purely technical advice was possible. There is by now an extensive literature on ‘values in science’ which suggests the same, and Kayzel rightly points to the values embodied by science according to Tinbergen: neutrality and internationalism. Studies of key individuals in social science can help us understand and contextualize how values are entangled with the activities and aspirations of the practice of science. But they should also make us aware that value-neutral science is at best a normative goal, not an empirical description of economics or expertise in action. Economists are well aware that efficiency is itself a value, and that technical advice on the most efficient solution ignores question of equity. More broadly the institutionalization of expertise embodies values such as rationality and neutrality in the decision-making process, which might themselves be questioned. I hope that my biography of Tinbergen illustrates that social science is practiced by individuals who are part of the societies they study, which requires the kind of reflexivity explored in my chapter on the Lucas critique.

Tinbergen was not prone to much self-reflection or that kind of reflexivity. But as I demonstrate in the book, he did arrive at a distinctly non-engineering definition of economics: “The problem of comparing alternatives [sic] forms of organization of economic life constitutes the problem par excellence of economic science” (Dekker 2021, 246). I have elsewhere made the unlikely link to the work of James Buchanan, but the definition by the pioneer of public choice is strikingly similar: “The true purpose [...] is to design alternative legal structures and to evaluate their potentialities in enhancing efficiency in the exploitation of mutual advantage” (Buchanan 1982, 175). There is, however, one important difference. Buchanan presents his definition as the right understanding of the science of political economy, which he differentiates from economic science which takes the current institutions and technology as given and studies prediction and control in this ‘given’ environment. Political
economy, Buchanan argues, cannot do without imagination of alternatives as well as a degree of normative commitments which help us evaluate the expected outcomes under different institutional rules.

Tinbergen’s legacy of economic expertise through institutions like the CPB and the Turkish State Planning Organization is primarily technocratic, thus Kayzel is right in historicizing the notion of technocracy and in highlighting the specific meaning the term carried in the Netherlands, circa 1935. But with Buchanan’s distinction at hand, and Blaug’s influential notion of technocracy in the back of my mind, I am going to stick to my claim that Tinbergen was not a technocrat. The Synthesis movement aimed, like Tinbergen, at synthesizing science and morality, not rule by technical experts.

Tinbergen realized the limits of a science of prediction and control, of only optimally engineering the current economy, possibly because his decision models enabled exactly that. Alacevich and Boianovsky are correct in their suggestion that the question whether Tinbergen is a technocrat is not merely a question of either/or: he can have been that and more. That is also what Buchanan suggests with his distinction between economics and political economy. There is a place for economics, like there is place for technocrats. Economic science is: “Positively valuable to governmental agents, business firms, and private individuals” (Buchanan 1982, 179). But we must not forget that it is a part of the broader normative and imaginative project of political economy: experts can be no better than the political-economic structures of which they are part. Tinbergen’s later work might have failed to have much influence, but Tinbergen’s books from 1970 onwards sought to demonstrate that there are alternatives.

III. Tinbergen and the Theory of Economic Development

Mauro Boianovsky presents a fundamental challenge to my account of Tinbergen’s work in development economics. He suggests that I downplay Tinbergen’s theoretical contributions and wrongly conclude that there is an emptiness at the core of the Dutch economist in this area. More generally, he suggests that it was common for economists of the period to be concerned with both the theory of economic development as well as policy design. He also, rightly, corrects several technical inaccuracies of my discussion of growth models.

Boianovsky is right that Tinbergen was interested in the theory of economic growth. He published his seminal paper on the subject in 1942,
and when he had the opportunity to invite an economist to give the first series of the (François) De Vries lectures in the Spring of 1963, he invited Robert Solow, which resulted in *Capital Theory and the Rate of Return* (Solow 1963). Although several of Tinbergen’s former students have suggested to me in conversation that the book on economic growth by Henk Bos and Tinbergen came about without much input from the latter, Boianovsky correctly observes that this book is certainly influenced by the vision of Tinbergen, including the idea that a model, Harrod-Domar in this case, can have ‘didactic’ functions, and that free trade is a foundation of development economics. Tinbergen’s interest in growth and development is also evident from his early engagements with the theory of human capital (and education planning). I believe, furthermore, that Boianovsky’s claim that Tinbergen was closest to neo-classical economics in his views on growth and development is correct.

But there is the question of the forest and the trees. Tinbergen made both small and large theoretical contributions in a wide variety of fields. My book does not discuss all of them—that would have been a different book, one more closely tied to the history of economic theory and less to the history of economic policy and economic history. This choice was driven by the considerations I laid out in the introduction: I think his legacy in the latter is more important than in the former. Moreover, Tinbergen spent most of his time at the intersection of policy and economics, not in contributing to economic theory. And he made his career choices, always seeking positions as expert, accordingly. So, when Boianovsky writes in his final paragraph that Tinbergen “deployed his neoclassical background” to further “the formalization of economics”, I must object (96). Formal economic models were a means for Tinbergen, never the goal. There is much more work of Tinbergen to explore, including theoretical innovations I neglected; but a good look at his overall scientific output between his earliest known publication in 1924 and the year of his Nobel Prize in 1969, totaling 896 publications, demonstrates that his major focus was on policy design, not theory.\(^7\)

And I wonder whether Boianovsky and I are ultimately not much closer than it appears at times from his critical review. Toward the end of his article, he cites Niehans who reached a similar assessment about Tinbergen’s work as I did: it was the theory of policy that stood out. Boianovsky presents Bruno’s characterization as at odds with mine, but the idea that Tinbergen’s development economics was an adaptation of his

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7 See https://www.erwindekker.com/bibliography-tinbergen.html.
Economic Policy: Principles and Design (1956) is identical to my argument in the chapters on development economics. Tinbergen’s 1956 book and its successor the Design of Development (1958) are based around the decision models and largely agnostic about the theoretical models and econometric models which are used as the background for these decision models.

I understand Boianovsky to argue that the structure of the decision models must be based on a theory of the economy, as well as an econometric estimation of the relevant relationships. This is true, and we might differ somewhat in our assessment of the commitment of Tinbergen to a particular underlying theory of development. Boianovsky suggests there is a commitment to a neoclassical growth model, while my analysis of Tinbergen’s practice at the CPB and later in his development economics emphasizes the pragmatic decisions which Tinbergen was willing to make about the underlying model, depending on policy needs and data availability. In Turkey, for instance, he used the Harrod-Domar growth model in the construction of the first Five Year Plan.

Alacevich’s critique of my chapters on development economics is nearly diametrically opposed to that of Boianovsky. He suggests that I am too kind to Tinbergen: not only was there an emptiness at the (theoretical) core of his development economics, but Tinbergen’s analysis “barely skimmed the surface” (81). Alacevich finds himself in the company of contemporary critics who suggested that Tinbergen lost nearly all interest in diagnosis and focused only on cures (Dekker 2021, 299–300). Although it seems Alacevich wants to go further and suggest that there was hardly a cure either. He might well be correct. I do, however, think that Alacevich is somewhat unkind to Tinbergen’s aspirations as a moral guide in Shaping the World Economy, and that Tinbergen’s domestic work at the cradle of development aid were inspirational to students and citizens. But I fully agree with Alacevich that in this work, perhaps even more than in the more technically oriented books on planning, he loses all sight of “radical uncertainty and ignorance” (83).

I have written elsewhere with some admiration about the humility of the Viennese Students of Civilization (2016), who recognized the limits of their own knowledge and rational scientific knowledge more broadly. The development economist Hirschman, about whom Alacevich (2021) has written wonderfully, was widely praised for an appreciation of local economic circumstances, inevitable tensions, as well as the paradoxes of life. I don’t think that my research about Tinbergen has provided me with
sufficient insight into the reasons why so few social scientists manage to recognize the limits of scientific knowledge and human knowledge more broadly. Tinbergen’s sense of responsibility and the duty he felt to do something as part of the intellectual elite, certainly drove him to look for solutions rather than problems, answers rather than questions. This is ironic given that his mentor, Paul Ehrenfest, was known as the Socrates of Leiden.

IV. LIMITS OF KNOWLEDGE

William Peden does not locate Tinbergen’s views on certainty and uncertainty in moral and political convictions, but instead in epistemological convictions. His original argument demonstrates a surprising overlap between Tinbergen and Keynes on induction and the constructive role that econometrics can play in generating economic knowledge. Although Keynes expressed skepticism about the econometric approach of Tinbergen and the way he evaluated his findings, Peden demonstrates that Keynes primary argument was about the way that Tinbergen combined evidence and not the (partially) inductive approach he had adopted. Keynes formulated his arguments at a time when the alternative sources of knowledge were institutional, historical, and moral, but Peden interestingly connects the debate to contemporary discussion about the combinations of different types of evidence, such as randomized control trials, econometric studies, and surveys.

Peden makes a compelling case that Tinbergen’s advanced methodological position on testing was influenced by debates in physics during the 1920s. Those same influences, however, also made him skeptical of probabilistic models, because he remained wedded to a deterministic view of the world. Einstein, whom Tinbergen met several times, famously refused to accept the probabilistic view of the world associated with certain interpretation of quantum mechanics (‘God does not play dice’). Tinbergen believed the same and was therefore (initially) skeptical about the probabilistic revolution set in motion by Trygve Haavelmo, as well as the models of the business cycle by Frisch, which relied on external shocks as Mortágua and Louçã remind us.

But what struck me most about Peden’s contribution is that the foregrounding of probability and statistics in the discussion of Tinbergen’s League of Nations model moves the practice of modeling to the background, and I think unjustly so. Models played a key role in the scientific practice of Tinbergen. They mediated at the intersection of the verbal
theories of the business cycle and the statistical evidence which was slowly accumulating in the 1930s. Models again played a key role at the intersection between the policy goals of governments and the knowledge about the economy in his policy work in the 1950s. ‘Tinbergen: Model Economist’ was ultimately too corny as title for the book but captured something essential about his work. Models were at the heart of his work, and Tinbergen also hoped that models could guide. A guiding science of models, with himself, as moral model, at the helm, was what he envisioned.

That ambition is not easily captured in probabilistic terms. One might claim that Tinbergen had optimistic priors. But as he expressed it in several interviews, he was not optimistic, not convinced that things would get better. But he was hopeful, as it was his duty to keep on trying to improve matters. The nuanced historical literature on models in economics does better justice to such ambitions (Morgan 2012; Boumans 2014; Sugden 2000). Economists have, for instance, relied on models of perfect competition and rational choice as normative benchmarks and have sought to make the world function in accordance with the model, rather than the other way around (Garcia-Parpet 2007). Similarly, Tinbergen practiced modelling not merely to capture the world, but also to steer it, to demonstrate what it could be, such as he did explicitly in his models of convergence between capitalism and socialism. I think that his idea that the economy could not be predicted by economic models, but that it could be engineered in the right direction, should be understood similarly. He was aware of the inherent instability of the economy but believed that modern economic policy could make it stable. His models did not merely demonstrate that the economy was stable, but rather that it could be made stable.

That practice is probably not a fully defensible philosophical position, nor a purely scientific methodology. But it is how Tinbergen, and others, have used economic knowledge in practice. It was how he made sense of the world and attempted to change it. It might also help explain why his models sometimes misled and blinded him to the limits of his own knowledge and the limits of experts’ ability to engineer society. Some of these dangers are discussed in philosophy of science as issues of inductive risk (Douglas 2000).

Yet, I believe that they are also an instance of human folly and of scientific hubris. The tensions and creative uses of models are interesting, precisely because they are examples of science in action. They are the type
of uses of knowledge which we might come to understand better through historically informed methodological discussions as well as biographies of practitioners of economics: scientists, experts, policymakers, and business leaders. It is an important task, because science, at least for the foreseeable future, will be practiced by humans.

REFERENCES


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