



Recovering Tinbergen

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Abstract

From the long viewpoint of history of economics, the two most important contributions that Jan Tinbergen made to economics are surely the development of the first macro-econometric model and a general theory of economic policy-making. This paper explores these two innovations to recover why they deserve such recognition, analyses their technical and conceptual depths, and shows how they relate to the economic history of the period and his personal history. In the process, it becomes clear that they are not separate innovations, but, as Tinbergen recognised, involved the same logic; and as we can recognise, were driven by the same ambition to make economics usable in the world.

Keywords History of econometrics · Theory of policy · Jan Tinbergen

JEL Classification B2 · B3 · O21

1 Introduction

Jan Tinbergen epitomised what it meant to be ‘an economist’ in the middle of the twentieth century in using his scholarship in wider public domains beyond academia. But of course, Tinbergen was not typical. First, he was highly significant as an innovator in creating the technical changes that made economics a mathematical and statistical science. And second, he was an equally key, and influential, figure in that broad initiative creating an economics fit for policy-making in a wide range beyond the traditional and limited approaches of earlier times. These projects—of micro, macro, planning, and development policy—in the mid-century were associated with engineering kinds of practice and an engineering mentality both of which Tinbergen developed to a high degree. That is, he both lead in, and shared with his peers, the belief that economists were responsible for making their economies better places; that economies were amenable to ‘management’, and could be

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‘developed’; and these were necessarily both technical and normative projects. He was a major figure in the use of economics for the public good during a period when the profession and society at large believed that such usage was viable and trusted economists to do so responsibly. And he sought to embody the kind of scientific and public integrity that we hope to find in our economists working in the public domain.

During the final decades of the twentieth century, as formal modes of economics became dominant in the academic field, there was a partial retreat from such widespread commitments, now countered in the twenty-first century with new modes of public-policy engagement using field experiments, behavioural nudges, and so forth. As economists have found new ways to exercise their expertise, these recent changes provide a reflective surface against which to explore Tinbergen’s centrality within economics during his most productive years of the middle third of the twentieth century. I will pick up his two best-known contributions for discussion here: pioneering the first macroeconomic model, and creating the theory of economic policy. Separately these are both important contributions, but the depth of Tinbergen’s work on these problems remains unappreciated: they are both common knowledge, but at the same time their details are mostly forgotten and need recovering. Revisiting them to understand Tinbergen’s place in the history of our discipline also makes it clear they were not separate agendas for Tinbergen, but closely related in his ambitions for economists—both in developing analytical power and in using that for the public good.¹

2 Changing the Scientific Credentials of Economics

Economics underwent a very considerable technical revolution in the middle of the twentieth century, a revolution that involved the introduction of both mathematical languages in theory building, statistical data and methods in empirical domains, and modelling as a format for working out and ‘testing’ ideas on both fronts. While historians can date the beginning of these movements in the later nineteenth century in the work of Cournot, Jevons, Marshall, Walras, Edgeworth, Mitchell and a number of others (particularly on the statistical front), it is equally well established that this revolution was a slow burner. Indeed, it was not until after WWII, and perhaps as late as the 1960 and 1970s that economics in these forms came to dominate the journals. The middle years, 1930s–1960s, were critical to these technical developments and to their becoming mainstream.

It was in those years of the middle of the century that the leadership of the economics profession passed to the young men (and so few women) who pledged allegiance to the Econometric Society, with its associated journal *Econometrica*

¹ This account draws on my work on the broad history of twentieth century economics (2003), the history of econometrics (1990), and the history of mathematical modelling (2012) as well as an interview with Tinbergen (see Magnus and Morgan 1987). There is of course a much wider literature discussing Tinbergen’s work by both historians of economics (particularly by Boumans and Dekker) and economists reflecting on his legacy; a very small fraction is referenced in this paper.

(established 1933) to combine mathematics and statistics with economics.² Tinbergen was not a founder of the Society, but was an early participant and soon became recognised as a leading practitioner in this international movement. During the 1920s, he had undertaken government service (as a conscientious objector, in an alternative to military service) first in the Rotterdam State Prison and then, between 1927 and 1928, at the CBS (the Dutch official statistical office), where he became familiar with statistical business cycle data and its analysis. His thesis work began in 1928 with Paul Ehrenfest, on the mathematical analogy between economics and physics; and he returned to the CBS after finishing his thesis.³ So in the midst of the Great Depression, that strange combination of circumstances found Tinbergen already experienced on both mathematical and statistical fronts in economics.

In the late 1920s and through the 1930s (even into the 1970s), he was one of the most innovative developers of the econometric agenda across a range of economic questions and applied econometric topics from individual to aggregate issues. His econometrics of these years was extremely fruitful, prompting neat insights and elegant solutions to technical issues (the identification problem, dynamic modelling) and to substantive economic questions (technological unemployment), most usually worked out in specific pieces of applied work where the data and modelling provided an immediate demonstration of his ideas, his analyses, and his proposed solutions.⁴ This mode of working created exemplars which were much appreciated in the interwar period—for their relative simplicity and technical tractability proved incisive in treating the problems, analyses, and solutions of econometrics and they enabled the reader to recognise the general issues involved. This habit of creating and using econometric models to show generic problems of the data-theory conjunction and their solutions became a hallmark of Tinbergen's contributions. And his preference for working through exemplification continued to be fruitful, as Heckman's (2019) study of his development of the theory of hedonics suggests, but such later innovative work has often been overlooked.⁵ It was a mode of work increasingly overshadowed by the project of technical econometrics (as might be found in the Cowles Commission's work of the 1940s), which tended to focus on the mathematical formalization of such problems and their solutions, and so lost sight of the fact that the data from the world might not behave as assumed and expected.

² The Society was established to combine maths, stats, and economic theory, so that its house journal continues to cover that ground. By the 1960s, econometrics had come to be used only for the statistics-economic conjunction. Meanwhile, two other journals established in those interwar years became the 'holders' of statistical economics (*REStat*, from 1919) and mathematical economics (*REStud*, from 1933).

³ It is worth adding that Tinbergen's switch from physics to economics involved social and political commitments (see Dekker, forthcoming) rather than any a simple transfer of technical prowess from physics to economics (see Boumans 1993).

⁴ A selection of these innovative exemplar pieces of work was published in 1959, including the original version of Tinbergen's Dutch model of 1936, and another forgotten paper on technological unemployment (written with P. de Wolff, in 1939) recently re-covered and re-analysed by Rodenburg (2018).

⁵ Other seminal contributions from Tinbergen's work may also have been overlooked, to be recovered by historians.

Surely the most important bonfire that Tinbergen set for his part in the econometric revolution was building the first aggregate-level econometric model (of the Dutch economy), presented in 1936 and 1937.⁶ This was far from an obvious task for there was no recipe telling an economist how to do such a thing. The business cycle work of the 1920s—the dominant topic of aggregate economics of the period, had not yet been re-conceived as ‘macroeconomics’. Rather it involved both verbal theorizing (Wicksell, Hayek, and from almost every economist of note in the 1920s) and highly empirical data-mining. This was the ‘big data’ approach of its day, and methods of analysis were designed to reveal cyclical patterns of various lengths, and with different time relations. This practical research was undertaken at national level within the first set of substantial economic research institutes, namely ‘business cycle institutes’ (found—for example—in Vienna with Morgenstern, in Moscow with Slutsky and Kondratiev, in Berlin with Wagemann, and at Harvard with Persons) and reported in the early volumes of *RESStat*. Tinbergen was already familiar with this business cycle data, and their statistical analysis, from his CBS national service days. In contrast, the concept of a theoretical model that would offer ‘macro-dynamics’ had only just come to the table with Frisch’s development of a little dynamic model, the ‘rocking-horse model’, in 1933 (see Boumans 1999). Even with this start, and building on his own and others’ work in the late 1920s in developing the small dynamic ‘cobweb’ models of agricultural markets, there was no recipe for how to build a data-relevant, national level, macro-econometric, model, and as yet, no national income accounting data. Tinbergen did not inherit the recipe for our macro-econometric models, he created it. This is the standard story of how Frisch and Tinbergen together won the first Nobel Prize for economics for these creative, breakthrough, models.

But, this of course was the judgement of hindsight. Not the least, it is worth remarking that even the term ‘model’ was not in widespread usage; in Tinbergen’s 1936 account, it was barely used, and only in the 1937 version did it come into its own in the title of the first chapter. Considered at the time, it would be better to see Tinbergen’s innovation as creating an econometrics to explain business cycles, the organising, aggregate-level, concept of his day.

Tinbergen created his ‘model’ of the Dutch economy as a means to think through the problems faced by the Dutch economy in the 1930s, an economy that in 1935 was still suffering severe unemployment at the bottom of the cycle when many other economies were showing signs of recovery. It was also almost the only one of the industrialised economies left on the gold standard (as many others had already come off the standard, and effectively devalued). The task here was not to test extant theories of the business cycle, but rather to capture the features of the Dutch economy into a statistical data-based account to explore the mechanisms that caused the cycle. He described this process as one of “quantitatively stylizing” (1936) the phenomena of the business cycle—well known to him from his work at the CBS—into a set of individual equations. His choices made for each equation were a matter of

⁶ The paper was read to the Vereeniging voor de Staathuishoudkunde en de Statistiek in 1936, and was published in English in collection of 1959. A substantially revised version of the original paper (with additional material) was published in English in a French journal in 1937. This account, with the help of Marcel Boumans and Erwin Dekker, draws on them both.

using the statistical information to ascertain the bones of the structure of the separate economic relationships. This produced 24 equations in his 1936 account, made up (as he observed in 1937) of: definitions; institutional or technical relationships; and causal relationships which were to capture both direct causal claims, and deeper or indirect causal relations. These individual equations were estimated using data from 1923 till 1933, first with graphic methods to establish how the exact elements were actually involved in each relation, and then by hand/machine calculations to ascertain the parameters in those relations.⁷ This set of equations was needed for two purposes, equally critical to Tinbergen's agenda at the time, for policy questions followed on naturally from the analytical questions.

The first problem he faced was the economic analytical problem: to ascertain the dynamics of the system represented in the equations. This problem may not be obvious now, but at the time, it was not at all clear whether a verbal account translated into a causal process account in the set of equations would produce cyclical patterns over time and have a tendency to equilibrium. Knowing that the economic world was complicated (and thus required mathematical treatment), he was also aware that he needed to simplify his account by combining and grouping the individual estimated equations in order to obtain, or solve for, a system of relations—a workable model—that would reflect the causal mechanisms at work.⁸ Using his solved system model, he showed how retracing the past behaviour of the system would provide insight into the causes of the business cycle fluctuations in the Dutch economy for that period. Then, by extrapolating the model, Tinbergen checked the dynamics, or 'natural tendencies', of his system model by simulation (again by hand) to check its behaviour. In his 1937 account, the simulation showed cycles in the Dutch economy would be quite quickly damped, but with cyclical movements abroad, those Dutch cycles would be maintained. This was perhaps no surprise—rather it was consistent with the fact that the Netherlands was a small economy with a large external sector.

From here, Tinbergen moved, almost without drawing breath, into the second, the policy analysis problem: first examining the path of system when policy action indicated changes to additive terms (in his 1936 treatment), and then when such policy action created changes in the parameter values (in his 1937 treatment). The policies that he considered were those relevant to the Dutch economy in the mid 1930s depression, relevant given the policy recipes of the day, and relevant given the constraints of the situation of the day. These possibilities ranged across: 'public works' or investment; trade protection; rationalisation of industry; lowering of monopoly prices; wage reduction; employment plans; and devaluation. Simulations (in the 1937 account) to explore both equilibrium values of the system and extrapolations indicated that devaluation was the more obviously efficacious policy

⁷ For reference: Tinbergen's idiosyncratic graphic methods were a pragmatic approach to the huge time cost of calculating regressions on hand calculators; they are explained in Hendry and Morgan (1995). It was not until the mid 1950s that a macroeconomic model was calculated using a computer.

⁸ In his 1936 account, the system model consisted of 5 equations (solved down from 24 elementary equations) for simulation; by the 1937 version, it was 1 equation (solved down from 22 elementary equations).

for improving employment and income in the economy. (Calculating the policy response to imported cycles, and consequently the optimum policy in those circumstances was more difficult and opened up more questions.) Thus, Tinbergen's preferred policy to get the economy out of its rut—based on his testing regimes—was to come off the gold standard (with some additional labour plan), which indeed was the policy undertaken, though it is unlikely (Don 2019) that this was due to Tinbergen. His modelling project had been presented to the Dutch professional association of economists (who were by no means all receptive to his approach), and he was a young man with not much clout or reputation, certainly not sufficient to prompt such a definitive policy based on such an untried mode of argument that was extraordinary for the time.

We can see Tinbergen's work in 1936–1938 for the League of Nations, as a natural extension of his Dutch model work. This new project was much more broadly concerned with 'statistical testing' of the business cycle theories, which had been already surveyed and summarised by the League of Nations economics research programme team. The extent of his testing for his previous Dutch model had been limited. While his 1936 account used the terminology of "calculations" for his estimated individual equation results, and his 1937 version referred to these as "explanations" (using quote marks himself too), his 'testing' regimes were concerned only with the dynamics of his system. For the new project, his team carried out testing on an aggregate-level model that they built of the US economy (and later one of the UK economy). They did not try to test extant verbal theories as true or false. Rather, the project was to explore the possibilities of using statistical data to create a valid and useful statistical model, and so the notion of 'statistical testing' here asked something like: can econometrics offer a satisfactory and plausible approach to assessing theories of the business cycle? This was before the age in which statistical testing based on a probability framework had become properly developed (largely a post-Haavelmo, post-war, practice), and still before digital computers, so this was again all a hand/desk top calculation job.

Tinbergen and his team embarked on as intensive a regime of testing as could be imagined, surpassing anything done up to that point, and rivalling modern regimes of testing in range and consideration. In Hendry and Morgan (1995), we counted 17 different 'tests' of the US model. We divided them into three groups:

1. Tests of range of applicability: testing the model on other countries, on other times, or on other sub-periods;
2. Tests of the economics of the model in terms of: lag lengths, signs and sizes of coefficients, overall explanation, and stability of coefficients when other variable were included; and
3. Tests that might be termed technical tests of the model, of: linearity, collinearity, significance, measurement errors and forecasting performance.

The combination of tests here was unrivalled in the period, and in retrospect, we can recognise that Tinbergen and his group had laid out the range of tests that econometricians then went on to develop—in more formal frameworks—over the next

decades. It is also salutary to remark that while there were many economists that the League of Nations could have asked to survey the multitude of business cycle theories of the day (indeed, many economists of the day had a theory in hand, and many had written whole books on the topic)—there was really only one young, hardly-tried, hardly-recognised economist that the League of Nations could have asked to lead this statistical project. He was really the only economist of the day with the relevant combination of experience, expertise, and knowledge to guide the work into a successful outcome. And in doing so, Tinbergen set the testing agenda for econometrics, even if the majority of econometricians have remained unaware that his breadth of testing provided the ancestor for later testing regimes.⁹

3 Economics as a Policy Science and the Science of Economic Policy

There were two major realms of public economic action that dominate the period after WWII, and Tinbergen was deeply involved in the heart of them both: macro-policy making and development policy. It is important to stress that in this period, it was by no means unusual for an economist to be so involved, for these were the essential, and major, public policy problems for economists of his day. What was unusual, and perhaps has been overlooked in attention to the ground-breaking of his early econometric work, was his development of a generic theory of economic policy-making. This was not a commitment to a particular theory about how economies work, and on such basis, could be made to work better via policies. That was the kind of question that had been posed by Keynes: How does the economy work, and how should we frame policy to make it work better? Rather, Tinbergen's question was how should we design economic policy to achieve a given set of outcomes.

Tinbergen's 'theory' was first developed in a small booklet *On the Theory of Economic Policy* of 78 pages in 1952 and extended to a full account in *Economic Policy: Principles and Design* (1956, and many following editions). Through his work on the Dutch macroeconomic model, and analysis of Dutch policy issues, Tinbergen had seen that there were many policy possibilities, and which ones were viable depended on how the economic system worked and on the interrelation of domestic with international events. This experience was reconstructed into a general problematic as he became interested in fashioning a theory of economic policy-making that stretched from its design to its execution after WWII.¹⁰ But the link between analysis and policy remained extremely close, for he regarded "the logic of economic

⁹ Klein (1988), provides a salient account of the development of macroeconomic modelling and testing from Tinbergen's starting points.

¹⁰ Tinbergen credited Frisch (1949) with prompting his policy work, and certainly Frisch's memo opened up questions of how different problems to be solved, with different causal accounts of economy, ask for different policy responses using different 'instruments'. And, he also worked through a set of examples to make his points—a mode that Tinbergen also followed. But Frisch's text is best read as opening up the problem and providing hints for Tinbergen's work, rather than providing a conceptually furnished blueprint as Tinbergen was to do. In many ways Frisch's memo operates as an intermediary commentary between Tinbergen's policy work with his Dutch model of 1937 and his 1952 theory of policy.

policy [as] an inversion of the logic of economic analysis.” (1956, p 12) This major extension of his intellectual project makes more sense when we consider the local situation facing Tinbergen in the late 1940s and early 1950s.

Tinbergen grew up as an economist in the Great Depression, lived and worked through the war economy in The Netherlands, and became a key policy figure in the very difficult post-war re-construction period as Don (2019) recounts. As Tinbergen well understood, all economic policy actions involve making decisions with consequences for the well being of individuals, groups, and nation states—and so all decisions involve choices. At the same time, all decisions involve “considering economy policy at any moment as a coherent whole, [the] possibility of ‘partition’ [was] only exceptional.” (1956, p. 12), so that one could not separate out groups. In addition—for him—all policy solutions had to be negotiated through the democratic system, so both policy designs and decisions about actions were far from simply technical. Indeed, the whole foundation of the Dutch post-war planning mechanisms was the result of arguments about the nature and practices of economic expertise in a democratic society.¹¹ This was as true of post-war reconstruction (his immediate problem), as of policy making in a mature industrial economy (of the kind Keynes had developed), and equally of policy-making for economic development (the field he worked in later). And in the immediate aftermath of WWII, there was another broad background to which Tinbergen was very sensitive: the world was dividing into different ‘economic systems’: varieties of capitalism, socialism, and communism, with European countries developing various brands of what later became known as ‘social market’ economies. At this time, and well into the 1960s if not 70s, all these policy projects were gathered under the rubric of ‘planning’ a term embraced in both Eastern and Western bloc economic discussions, and which spanned from macroeconomic policy, to development projects large and small, and to the ‘central’ planning of the Eastern bloc economies.

Tinbergen’s sensitivity to these economic system divisions was deeply personal and political: his commitment to peace, to tolerance of other socio-economic systems, and to the integration of economies in ways that might overcome nationalism and prevent further wars, can all be found well developed in his economic writings in the post-war period. They can be connected back to his youthful, and highly developed, political and social convictions, and perhaps to his war experiences.¹² Historians of economics have often commented on the work experience of economists during WWII both in the field and back home, but have seldom reflected on the traumas these might involve and how they might have affected their scientific values (though these have occasionally surfaced in autobiographical accounts). It is significant in this context that, in his first extended treatment of his theory of policy in 1956, Tinbergen’s first stated aim of economic policy was the “maintenance of international peace”, coming before those of more conventional economic aims (full

¹¹ See van den Bogaard (1998), on the foundational debates for the CBS, and Don (2019) for a detailed account.

¹² I owe my understanding of this aspect of Tinbergen’s life and beliefs to Erwin Dekker (see Dekker, forthcoming).

employment, maximising per capita expenditure, monetary stability, etc.), followed by distributional issues, and personal economic freedoms.¹³ These value-commitments are clearly evident in Tinbergen's theories of policy-making—he wanted his theory to be relevant regardless of economic-system type, and his theory was designed broadly to cover whatever value system was in use or proposed. He preferred to avoid value judgements about what kind of system there should be because he hoped for integration of systems rather than exacerbating distance between them. Yet at the same time, his starting point was that all economic policy actions would have consequences for groups within society, so all choices were value choices involving questions of social justice. In the language of the debates of the time over optimum policy regimes, Tinbergen took the view that there were many Pareto optima because the economy was a set of people, and their activities, and dependent on particular socio-economic institutions, not an abstraction in formal languages,

If Tinbergen invented not the study of economic policy, but the theory of policy design and execution, what did this entail? It meant that his interests—and influence—were encompassing; he wanted “to supply a systematic survey of the various types of economic policy from simple every-day decisions to social and economic reforms” (1952, Preface). While micro, macro, and development issues might have looked like different questions and problems—they all were subject to policy action, and so fit for theoretical analysis under the same general problem of how to make economic ideas performative, that is, of creating economic change on the ground. This point is worth emphasising. Tinbergen understood the responsibility of the economist not just as one of design of policy, but of its enactment. His basic theory of instruments and targets, as often expressed: ‘Tinbergen taught us we need as many instruments as targets’, is a statement that might now seem both self-evident and even trite. But it is also misleading, for as he pointed out, a simple “one-to-one correspondence between targets and instruments evidently assumes a very special structure” (1952, p 31). Yet, as with many of Tinbergen's insights, the fact that a version of his idea could be conveyed with such rule-based simplicity gave it immediate impact and staying power, as was also manifest in his later work on problems of environmental policy (see de Zeeuw 2019). Yet, just as his original aggregate modelling and testing had exhibited the depth and range of considerations, so too the original theory of economic policy he proposed was itself dense and deep.

From his first policy book in 1952, Tinbergen outlined the difference between the ‘data’ of the system (referring to the non-target, knowns of the economic system that were not under the control of the planning authority), the potential ‘targets’ and ‘instruments’ appropriate for any particular policy design in a given system, and other variables that might, or might not be targets or instruments that system. Policy design depended critically upon empirical-statistical modelling of the relations in the system, as he had learnt from simulating his earlier Dutch model to figure out the effects of different policies. Such model usage would enable the economist to

¹³ De Zeeuw (2019) tells how in his later 1970s work on the environment, Tinbergen's ordering began with the environment, maintained the socio-economic well-being as second in line and finished with international peace.

explore how the policy execution would work out and so warn of potential dangers and problems in the policy design, while the interdependencies in the econometric model would also reveal side effects of those policy interventions. Thus, Tinbergen insisted on statistical model-based analysis in any policy design before policy action was taken. But his policy analysis also integrated, and showed the importance of, other more technical, aspects coming from his understanding of the then current econometrics. For example, he used the language and forms of structural not reduced forms in his policy design as he wanted to know not just the behavioural relations, but also the implications of any policy change for all those behavioural groups within the economy that were recognised in the modelling. Similarly, he used Herbert Simon's seminal work on identification which relied on recursive and block recursive systems to help frame the problem of interdependency. For Tinbergen this was not to solve a mathematically conceived identification problem, but in order to frame practical difficulties within the policy-design problem. It becomes easier to see that from this, that Tinbergen did not make two independent major contributions to economics.¹⁴ Rather, these two projects: economic system statistical modelling and economic policy design and making, were intimately connected, both as technical projects and as performative, ground changing projects.

Tinbergen's policy design work revealed and covered a range of important elements. For Tinbergen, as always, attention to the 'reality' of the problems of economic policy design and its execution made his 'theory' a sophisticated analysis of the terrain of using economics in the world, rather than a simple recipe to follow. Thus, he cautioned, policy design needed to take account of at least five major problems. First, there was the problem of inequalities between numbers of instruments and targets, not just in theory but also in practice, a point that would be revealed by the statistical model, particularly if instruments were non-independent (a problem that might have been assumed away in the mathematical model). Second, there might be a problem of the incompatibility of chosen targets, and of the possibilities of certain alternative instruments. Third, the economist needed to be aware of "boundary conditions". Such conditions were not just those of certain problems on technical grounds, but evident from "all the protests of reality" (1952, p. 15) due to the behaviour of data, and when certain projected effects of policy were deemed unacceptable on political grounds. Fourth, policy makers also needed to remember that the economy, and its data, were not stationary but changed as policy went on. And of course, fifth, policy design and action depended also—crucially—of the reliability of the model based on the data.¹⁵

Policy making was not an exact science, but a practical science requiring expertise. So, while Tinbergen's theorising involved some simple rules, his treatment embraced lots of difficulties, and depended upon knowledge built up through experience. And, as usual, he showed this in using and exploring statistical models, in 1952, working through two numerical examples of designs and their problems using cases from his experience at the Netherlands Central Planning office. His later text

¹⁴ This relationship was noted by Hughes Hallett (1989), who explored the contributions of Tinbergen and Theil to this joint project.

¹⁵ Tobin (1990), gives an extended account of the richness and depth of Tinbergen's theory of policy in discussing a number of these points.

(1956) expanded into an array of other extended examples using 21 models and investigating 37 different policy problems. Once again, he used these examples as exemplars: working objects which demonstrated the depth of his theories, and the difficulties of actually enacting policy, by considering the necessities of careful design based on knowledge of the actual system given in the statistical (i.e. measured) model relevant for that particular problem to be addressed. Tinbergen's theory of economic policy-making involved the same consideration of rules and practical analyses—whether the economic model and its problem was micro or macro, open or closed, dynamic or static, and national or international.

From this perspective, it is easier to see why Tinbergen's mind-set coming into development economics in the late 1950s and 1960s onwards appears largely to have been an extension of his economic policy science rather than a new intellectual direction. His writings on macroeconomic problems, reconstruction problems, centralized versus market systems, and development economic problems have all the same feel about them as well as the same approach. Yet his writings on development economics appear quite detached from the realities of development problems in a way that his earlier work on unemployment problems did not. This lack of engagement is particularly evident in comparison to many of those coming newly into economics with a public commitment just after WWII. Dedicated development economists of the early post war period, such as Albert Hirschman and Hans Singer, revelled in their experiences in the field (in their visits to local markets, and their experiences of the problem of small change), became involved in arguments over the role of agriculture, industry or infrastructure projects—all the while trying to figure out how to square the circle between development principles and realities in that field. In strong contrast, Tinbergen's writings remained determinedly technocratic. For Tinbergen, the planning office was his natural home from which he ventured easily—as international expert—only into government, international and political arenas.

What is significant in his approach, compared with others of his time, was his explicit commitment to institutions and processes of policy decision-making in his writings and advising work in economic development. This was both a political action plan and an analytical stance. He was heavily involved in the UN Development programme in the 1960s and 1970s, both at the level of many individual state plans, and at overall programme level. He helped originate and became the first Chair of the UN Committee on Development Planning in the mid 1960s, a body initially designed to monitor development and then in the 1970s, to plan development.¹⁶ In his many 1960s book on 'planning' (the generic broad name for policy making during the period shared by West and East) we see his commitment to both technocracy and institutions. In one of these books, he provided a detailed analysis of the planning processes, institutions, actors, and activities found in nineteen countries across

¹⁶ The Oral History Project (2007) of the UN Development Programme permits searching of their interviews, so that we can see both how Tinbergen 'showed up everywhere', but at the same time, there is a certain ambivalence in these interviews about how successful his initiatives on 'governance' were at this international level (oversight by UN bodies tended to be resisted by member governments). He was also a prime mover in creating the UN Research Institute for Social Development to pursue better measurements of development. See also Boumans and De Marchi (2018).

the East–West divide, some democratic and some not, some developed and some not, some large economies and some tiny.¹⁷ His general focus, in the 1950s through the 1970s, on co-operation, co-existence or integration, with his equal focus on the requirements for countries to choose their own socio-economic order, reflects not just the world he was living in, but also his own attitude of tolerance, desire for harmony, and a search for optimum economic orders.

4 Reflections

Weinberg and Galenson (2019) make the point that Tinbergen as a Nobel Prize Winner was an innovator in both conceptual and empirical fields. They are surely right, yet applying their associated labels of ‘deductive’ and ‘inductive’ to these modes of working seems inapt for Tinbergen. He was surely an innovator, but the salient characteristic of his work was not to straddle but to integrate such modes of work as a hallmark of his innovations. Unlike other young economists of his time who won that prize, such as the mathematical and econometric theorists: Samuelson, Koopmans, and Frisch, Tinbergen always dealt simultaneously with two different kinds of problems: integrating the mathematical formulation of the problem with the empirical material evidence of economics.¹⁸ But he never neglected the problem of knowing what the world might be like and how it might behave; he was never happy with assumptions unless he could also analyse the problem when they might not hold. His definition of economics began with people, and he often reminded us that every symbolic abstraction represented people in the economy, and that all our economics statistics come from their activity. He kept in mind that our economies were not stable (and as well as being dynamical systems, they were subject to change), and that above all, economists grappled with a field of unknowns and uncertainties. The lessons seemed to be that economists ought to be modest about their knowledge; Tinbergen however had a strong confidence that technical expertise, pragmatically used, was equal to the task of delivering both successful analyses and enacting successful policy regimes.

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¹⁷ See Tinbergen (1964), which is extremely rich in its survey materials of these 19 countries’ institutions of planning. The book was awkwardly titled *Central Planning* when in fact it was a volume about the role of the ‘central government’ in policy (ie planning).

¹⁸ Even Frisch, who sought his own solutions to empirical problems, and delivered critical accounts that Tinbergen respected, did not develop such usable and elegant solutions to the problems of integrating mathematical and statistical treatments.

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