

Social Engineers Changing the World: Tinbergen and Frisch's Framing of Economics

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I. INTRODUCTION

In this impressive biography of Jan Tinbergen, Erwin Dekker (2021) describes this pioneer of modern economics as being both “the most important economic ‘bureaucrat’ of the twentieth century” and “one of the greatest idealists the economic profession has ever known” (xvi, xvii). As paradoxical as this may seem, it is a very accurate summary of Tinbergen’s life. As we move through the more than 400 pages of the biography, this tension between the pragmatic public intellectual and the rigorous academic emerges as defining the career and the heritage of the Dutch physicist who became an economist for the sake of social justice. Indeed, the synthesis of his life’s contribution was his “institutional awareness”—or the placing of “science at the service of the state”—where he developed and experienced a new approach to the “theory and technique of governance” (11, 13). He was a proud social engineer, what the younger and impressed colleague, Paul Samuelson, would call a “humanist saint” (418).

The influence of Paul Ehrenfest—Tinbergen’s supervisor for his PhD in theoretical physics—is described in some detail, including how he paved the way for the transition of his student to economics (see also Boumans 1992; Jolink 2003). That transition was not uncommon, for as Tinbergen pointed out: “I was not the only one who, in that period, switched from the physical sciences to economics. We had quite a lot of ‘migrants’ [...]. Our choice was in part a reaction to the Great Depression”

AUTHORS’ NOTE: Unless otherwise indicated, the letters are quoted from the Frisch Archive (Oslo Library and University), the Tinbergen Archive (Erasmus University Library), and the Schumpeter Archive (Harvard University). We are thankful for the suggestions of an anonymous referee and the editors, emphasizing that the errors are our own.

(Tinbergen 1991, 277). His example was replicated by younger physicists, such as Koopmans, who explained to his older colleague that:

I seem to be taking the same route as you have done in the past: although in principle I find physics a beautiful field, but I am too concerned with the social problem to be able to devote myself completely to theoretical physics. I therefore consider the possibility to use the mathematical development I possess in the study of economic and statistical problems. (Koopmans to Tinbergen, July 18, 1933)

However, Tinbergen did not simply take up economics, for he also assumed a mission in that discipline, which was inspired by his ideals: “My choice of democratic socialism, my ideal of European federalism, and my priorities for the Third World all have that source or inspiration [his Protestant creed]” (Tinbergen 1991, 277). This social engineer pursued these ideals throughout all his life. Indeed, the changes in his journey were determined in coherence with such ideals, and this is why he “led a new movement after the Second World War that turned instead to the study of centralized economic planning that placed minimal reliance on advanced statistical techniques” (Epstein 1987, 9) or “consciously decided to leave econometrics behind him in the 1950s to focus on the problem of development” (Dekker 2021, 421). Tinbergen described this metamorphosis in a clear way towards the end of his career: “Also I think that forecasting is not the most important function of economic science. The most important function rather is to search for the most desirable policy, including the choice of institutions” (Tinbergen 1992, 255). His consistency was built on a specific concept of progress.

In reflecting on Dekker’s biography, we discuss the context of Tinbergen’s evolution, including his change(s) of focus on how to engineer social progress, first conceiving econometrics as a tool for business cycle research and planning and then abandoning it for development economics, as compared to that of the closest of his colleagues, Ragnar Frisch. Frisch was eight years his senior, and as Dekker recounts in some detail, Tinbergen was impressed by his energy when they first met at the inaugural European conference of the Econometric Society, in Lausanne, 1931 (the “soul of the conference”, as Tinbergen described Frisch [Dekker 2021, 85, 111]). During the next four decades they shared the ambition to create econometrics and suffered a similar disillusion with its progression, and they searched for feasible techniques for adequate collective and institutional decision-making. This shared ambition stemmed from a common

understanding of the need to promote development economics as a concrete instrument for growth in what was then called the 'Third World'. Our contribution corroborates Dekker's approach in that sense, providing notes and arguments on Tinbergen's and Frisch's parallel evolutions in order to discuss the bifurcations and paths they chose through their careers, highlighting their convergences and divergences. We believe that it is appropriate to situate their contributions to economic science in the context of their long-standing cooperation, friendship, and mutual reinforcement, and that the history of their interaction is one of the defining processes of twentieth-century economics.

The next section overviews this enduring cooperation and the world views that sustained it. Section three explores some of the avenues that they pursued as far as planning is concerned and then, briefly, as they worked on development policies. Section four discusses how they both employed mechanical analogies in representing the economy, and section five argues that each later abandoned econometrics for similar reasons. A difference is noted in relation to Dekker's contribution concerning the assessment of the heritage of these founders of econometrics. The sixth section concludes.

II. SOCIALISTS IN BOTH THOUGHT AND ACTION

The friendship and intellectual cooperation between Frisch and Tinbergen were the topic of a previous and fairly comprehensive study by Dekker (2019); as such, this section merely reflects on the evolution of their bond in order to discuss, in the next sections, their respective views of economic and social progress.

It has been noted that their political views, as well as their religious and humanitarian beliefs, were very close. Tinbergen joined the youth organization of the Dutch Social Democratic Labor Party before he was barely twenty years old, motivated by his rejection of the atrocious living conditions of the working class. Although Frisch never became a member of a party, he became involved in some political campaigns. Frisch and Tinbergen shared the same type of left-wing ideas and, in particular, they both rejected the anti-democratic movements of the time and the tragic path towards war. They were also both moved by the need to break the business cycle, when they witnessed the sufferings imposed by the Great Recession. Accordingly, their personal views and scientific motivation were profoundly interconnected.

Unlike Frisch, Tinbergen was directly involved in the political and ideological debates of the period at that time, as a result of his involvement with the Labor Party (his first paper, which was published in 1925 in a Social Democratic magazine, discussed Marxism and the labor theory of value [Dekker 2021, 71]). However, they both had in common the driving ambition to bring about a socialism that would distribute wealth and provide for the needy. In a letter written by Tinbergen in 1928, he announced the agenda he would later try to pursue at the Central Planning Bureau: “I deeply hope that we will play an active role, and will turn into an active community of socialist social-engineers. Socialist in both thought and action” (quoted in Dekker 2021, 69). Frisch certainly felt the same.

This activist inclination led the young Dutch economist, who had been a conscientious objector during the First World War, to try and apply his abilities to the service of peace and to persuade his peers to follow the same path (as a conscientious objector, he had always been suspicious of military alliances and later opposed the creation of NATO [Dekker 2021, 351]). In a frequently quoted letter written to Frisch on March 20, 1936, he suggested the publication of an Econometric Society manifesto in opposition to the upcoming war. He included a draft for that purpose, which opened with the statement that “econometricians feel it is a first duty to raise their voices against the tendencies leading to the largest wholesale destruction of human welfare: the war”. Although Frisch supported Tinbergen’s concern, he preferred not to involve the Society, and Tinbergen conceded.

During the early part of his life, Tinbergen witnessed the turning of sympathies of Hendrik de Man, a towering figure that was the leader and theoretician of the Belgian Social Democrats, wielding considerable influence among the Left of Central Europe. De Man conceived and divulged a national economic plan, which became a model for the Dutch Social Democrats, as well as for parties in other countries, which impressed the young Tinbergen. The impact of this program and its achievements eventually led Tinbergen to later on undervalue the tsunami of anger amongst democrats created by De Man’s support for the Nazi occupation of Holland, for whom he was the puppet prime minister from 1940 to 1941. De Man was not the only case of a major political swap to the victors during the first stages of the Second World War, and this was a major blow for the democratic resistance movement. As Dekker indicates, Tinbergen’s difficulty to condemn the hero of his youth was not a momentary bias since, “inspired by Tinbergen”, his son-in-law later wrote a book “urging

for a reevaluation of Hendrik de Man” after the end of the war (Dekker 2021, 344).

During the passage of time, two further differences emerged between Tinbergen and Frisch, both of which highlight the influence of their local intellectual or institutional environment. The first difference, which is the most enduring one, was motivated by the construction of the European institutions, since Tinbergen welcomed the European Economic Community (EEC) as being a progressive movement. He was invited to deliver the Wicksell Lecture at the Stockholm School of Economics in 1963 and, on that occasion, he chose to favor the inclusion of Norway and Sweden to the EEC, which at that time was already discussed (yet, Sweden only joined 32 years afterwards and Norway never did). On the contrary, when the Norwegians rejected joining the EEC in a referendum in 1972, Frisch commemorated the outcome, having actively participated in the ‘no’ campaign (Louçã 2007; Dekker 2019). In addition, Tinbergen, who was involved in some scientific work in Turkey, also argued for the admission of that country as a member of the EEC (Dekker 2021, 331).

The second difference concerns their attitude toward some emerging political issues. As Frisch kept away from the temptation of getting involved in diplomacy, he was more outspoken than his friend. Instead, Tinbergen explored his ability to convince his institutional audiences of the adequacy of his own views, and this eventually explains differing attitudes between the two economists. The case of Spain was a telling example, resulting from Tinbergen being offered an honorary doctorate from the Francoist Bilbao University just a few months after being awarded the Nobel Prize. He accepted and travelled to Bilbao, not only to attend the ceremony, but also to deliver some lectures in that region, in which he was publicly challenged by anti-fascist students. During the same year of 1970, a group of French Cepremap researchers issued an open letter calling for a boycott of the meeting of Econometric Society that was to be held in Barcelona the next year in reaction to the fact that 16 nationalist Basque activists were on trial in Burgos at the time, all of whom risked a death penalty (six were later effectively sentenced to death, to be commuted to long periods in prison). Frisch was quick to respond to the call and, on January 8, 1971, he wrote to Gerard Debreu, the then president of the Society, to say:

I inform you that I will not attend the next European meeting of the Econometric Society if it takes place at Barcelona as scheduled. The

international public opinion would interpret our presence in Barcelona as an implicit support of Franco's regime, which is responsible for the scandalous trial of Burgos. I therefore ask the Econometric Society to change the place of the meeting to another country.

Debreu rejected the call and Frisch did not travel to Spain.

Notwithstanding these differences, the two friends shared a deep commitment to a common agenda. The parallel of their frequently converging and rarely diverging lives is of note, whereby they followed the same scientific agenda and concurrently moved in the same directions, as their visions of the world were part of a shared commitment to economics as a science capable of addressing real social problems.

III. SOCIAL ENGINEERS AT WORK

Both Tinbergen and Frisch perceived econometrics as the detailed analysis of business cycles and were suspicious of the general use of probabilistic concepts, in a field where Tinbergen excelled in empirical research, while Frisch preferred to look for formal models of cycles (Louçã 2001, 2007). Simultaneously, they both investigated the cardinal measurement of utility, as they conceived utility as a cornerstone concept in economics. For Tinbergen, "marginal theory of value is the equivalent in economics to relativity theory in physics" (Dekker 2021, 40-41). Though, for Frisch, utility measurement was required for the definition of a social preference function that Tinbergen did not think was attainable. However, this bridge demonstrated the common desire to understand the business cycle and to propose a way of addressing its dangers in order to prevent the impoverishment and social devastation provoked by crises. As Frisch put it in 1950:

In order to define precisely the problem, I consider as my point of departure the economic situation as it existed in the thirties. Massive unemployment in most countries led to a monstrous situation. Amidst abundance, buying power decreased. Food and other means of consumption were deliberately destroyed, while people prayed. This experience leads to a simple but fundamental conclusion: the need to prevent those monstrosities. No solution to any economic problem is admissible unless it satisfies such a condition. (Frisch 1950, 475-476)

For Frisch, the solution to economic crisis was planning, the alternative being economic chaos (Frisch 1931). Frisch and Tinbergen's self-attributed mission in economics was to open new avenues for growth in order to avoid unemployment and deprivation.

A simultaneous agenda for social engineering, which was also developed as part of the Econometric Society, was proposed in the US by the Cowles Commission. As explained by Jacob Marschak, who was appointed the director of the Commission in 1942, "I hope we can become social engineers; I don't believe we are much good as prophets" (Marschak 1941, 448). Social engineering was the primary function of the Cowles program (Epstein 1987, 50) and as Lawrence Klein, one of its proponents, would claim in his reminiscences of the 1940s, "we members of the Cowles Commission were seeking an objective that would permit state intervention and guidance for economic policy" (Klein 1991, 112). As Mirowski noted, "in the immediate postwar era, Cowles was ground zero of Walrasian market socialism in America" (Mirowski 2002, 242). Yet, the Cowles program failed and structural estimation of a Walrasian type of system of simultaneous equations was abandoned in the late 1940s (Epstein 1987, 64, 110). The failure of this thinking could have been anticipated by both Tinbergen (as he considered the Walrasian system to be static by definition and thus inadequate to model economic evolution) and Frisch (who, more radically than Tinbergen, thought that these estimation procedures would not be able to uncover the structural economic relations). In this sense, it was the Frischian-Tinbergian approach to decision models that endured and went on to become a field of action at a time when other economists resorted to abstract modeling.

Tinbergen and Frisch also shared a curiosity about other related topics, a relevant example being the study of long-term economic fluctuations, a lasting fascination for both. Early in his career, Tinbergen crossed paths with his fellow party member Sam de Wolff and reviewed his book on long waves (Tinbergen 1929), noticing that a parallel line of research was being carried in Russia at the time: "Research on long waves is still in an initial stage, and it is mainly in Moscow [meaning Kondratiev] that valuable work has been done on this subject" (Tinbergen 1929, 858 authors' translation). Shortly after, in 1933, Tinbergen was invited by Van Gelderen, one of the leaders of his party, to be part of the drafting of a Plan (Jolink 2003, 130). Van Gelderen was another enthusiast of the long wave hypothesis and the young professor was certainly well aware of his contribution to this area of econometrics. In his book for the League of

Nations, Tinbergen also used the concept of long waves to define the dating of sub-periods (Tinbergen 1939, 42). Indeed, much later, in 1987, he wrote a preface to a book on the issue (Tinbergen 1987b). Frisch shared the same notion of long waves in economic evolution and expressed it since 1927 (Louçã 1999; Freeman and Louçã 2001).

Uncovering the secrets of the business cycle was a demanding task, and in the 1930s it was obviously on the top of the economic agenda. For that, the two economists, more than others, succeeded in proposing new methods, both in theoretical models and in technical instruments in statistics. This is how they came across the tools of mechanics and the concept of a mechanism.

IV. FROM THE DENIAL OF MECHANICS TO THE USE OF THE MECHANISM...

When approaching the analysis of business cycles, Frisch and Tinbergen came from different points of view. Tinbergen, the previous physicist, had soon concluded from successive failures of analogies between economics and thermodynamics that these were not helpful guides. He challenged Paul Ehrenfest on this subject, as his supervisor had explored such analogies and reported to Schumpeter, quite enthusiastically, that he had found “several points of contact” between economics and thermodynamics (Ehrenfest to Schumpeter, May 2, 1918). In the same sense, he later insisted to Tinbergen about the need to explore those bridges (Ehrenfest to Tinbergen, November 29, 1927). Tinbergen himself published a paper in 1928 on the analogy with the principles of the conservation of energy (Dekker 2021, 76-77) and, more conclusively, his PhD thesis investigated the transfer of concepts from physics to economics, albeit this was a “limited transfer”, as Boumans put in the title of his book (Boumans 1992). In any case, Tinbergen moved away from the analogy when he concluded that it was useless, as his subsequent studies would demonstrate. Instead, Frisch, the economist, was fascinated by physics and its major achievements, such as mechanics:

We all have our peculiar way of working, and I, for one, never understand a complicated economic relationship until I have succeeded in translating it either into a graphical representation or into some mechanical analogy. (Frisch to Schumpeter, July 5, 1931)

This had a major implication on his choices of modeling (Louçã 2007).

The question was discussed for instance when several papers were submitted to *Econometrica* that raised the question of such analogies and

Frisch, the journal's editor, consulted his colleague on the issue. In one case, Tinbergen wrote to Frisch rejecting a paper that developed an analogy with physics:

I am rather skeptical about its value; so I am in general concerning analogies between physics and economics. I never saw one that did not, more or less, force economic phenomena into a form that is not characteristic to them. I still must see the first important result of these analogies. (Tinbergen to Frisch, September 26, 1934)

On October 25, Frisch replied that “I notice that you are somewhat skeptical about Creedy’s paper, but that you do not quite make objections to accepting it for *Econometrica*”. This was a benign conclusion that favored his own choice. Later the same year, the two economists discussed another paper on an analogy, in the case with the Law of Conservation of Energy. Once again, Tinbergen rejected the paper, as “I cannot see it is very useful to economics until better examples, giving really new insight, are given by him” (Tinbergen to Frisch, December 24, 1934), while Frisch accepted it, stating, “with regard to the application of mechanical analogies, I think I believe a little more about them than you do. But of course, there must not be any ‘mechanical’ application of mechanical analogies” (Frisch to Tinbergen, January 11, 1935). The rhetorical precaution did not hide their opposing conclusions.

These differences notwithstanding, Frisch and Tinbergen shared a common ground that was not centered on strict analogies from particular processes studied by physics, although it was inspired by it. That common view was based on the more abstract concept of a mechanism (Boumans 1992; Jolink 2003), an explanatory device used to represent the economic relation, which dominated the first phase of their careers, when they were focused on understanding the business cycle. The notion of mechanism—and not specific analogies with mechanical processes—was the intellectual engine behind their research on the matter, and this had two different implications.

The first implication was that the mechanism became their standard representation of the structure of the economy, meaning that its description should be based on a determined system of equations. Yet, in order to impose movement on such a system, ‘external influences’ should be imposed, such as impulses or shocks, as Frisch established in his to-be famous paper on impulse and propagation systems (Frisch 1933) and Tin-

bergen further emphasized in his annual survey for a 1935 issue of *Econometrica* (Tinbergen 1935, 241–242). Although Tinbergen requested further clarification from Frisch on his paper (“its economic foundation is not clear in every point” [Tinbergen 1935, 271]), which inaugurated the business cycle model that would influence the following generations of theories of economic oscillations, he also framed his views on the basis of the analogy of a pendulum (or, as Frisch also put it, a rocking horse). In that case, the mechanism would be the pendulum itself (or the wooden horse), which would dissipate the exogenous ‘impulses’ that provide the energy for the movement. This was a telling metaphor, but poorly constrained the notion of the mechanism to an equilibrating system, and Frisch engaged in a long discussion with Schumpeter about the nature of such a mechanism, as his concept could not explain oscillations, but only their fading out (Louçã 2001).

The second difficulty for both economists was that the notion of an impulse lacked a clearly defined, realistic counterpart. In effect, those impulses would not be part of the mechanism, but instead the result of non-explained exogenous sources of energy impinging on the structure. In the same survey, Tinbergen wrote—showing some hesitation—that “frequently, the impulses present themselves as given initial conditions of the variables—comparable with the shock generating a movement of a pendulum—or as given changes of the data entering the equation” (Tinbergen 1935, 242). Neither economist favored a stochastic conceptualization of the shocks.

Both the mechanism, to be described by a system of deterministic equations, and the impulses, to represent possibly unexplained shocks, were the common points of departure of their concept of business cycles. Their further work on the role of these two systems ended up counterposing their approaches to what was to become standard econometrics.

V. ... AND TO THE REJECTION OF EPSILONIST EXERCISES

The presumption of the existence of a mechanism that generates the economic processes directed Tinbergen in his research on statistical inference, namely the famous League of Nations books on business cycles. He emphasized that “it is the object of analysis to identify and to test these direct causal relations” (Tinbergen 1939, 8), and added that this would lead to measuring stable relations with constant coefficients, as:

Theory always means reducing variable things to constancy. [...] Describing phenomena without any sort of regularity or constancy behind them is no longer theory. An author who does not bind himself to some “laws” is able to “prove” anything at any moment he likes. But then his is telling stories, not making theory. (Tinbergen 1940, 80)

Frisch did not agree with his friend on this point and subsequently produced the most challenging critique of Tinbergen’s 1939 book as it emerged directly from the headquarters of the econometric camp. In fact, he denied that autonomous relations or relations that are independent of institutional or policy changes could be identified (Frisch [1938] 1995). As a consequence, when Keynes was leading “a ferocious campaign to discredit the activities of Tinbergen”, arguing for a “statistically realistic economics”, and even suggesting to call this area “Realistic Economics” (Epstein 1987, 142), Frisch did not come to the defense of Tinbergen (Dekker 2021, 238, 246).

Tinbergen certainly knew that his view of testing causality was controversial, not only from his conversation with Frisch, but also because by that time he was corresponding with Johan Åkerman on Yules’ reservations regarding the extension of the application of correlation calculus. Åkerman stated that this calculus was a “dangerous weapon which might lead to an oversimplification of the setting of the problem”, since “causal connections in the domain of natural science are of a different character than causal connections in the domain of social sciences” and because recurrence is not so frequent in social life (Åkerman to Tinbergen, February 12, 1938). Nevertheless, Tinbergen persisted with his point of view and his book on cycles for the League of Nations opened an avenue for different methods of estimation, even if not fully understanding the nature of the implicit mechanism.

The analysis of the other dimension of the explanation, namely the impulses, proved to be even more difficult. When testing his League of Nations model, Tinbergen acknowledged Frisch’s suspicion about the ‘classical method’ of R.A. Fisher, for which:

The probable average magnitudes of those differences [“erratic component” or “disturbance”] are derived from the assumptions that the disturbance in subsequent time intervals are to be considered as “random drawings” drawn from the “universe” of all possible values of these disturbances. [...] [Instead] Professor R. Frisch, in his treatment of these problems, does not use the concept of an unknown “universe” from which the “sample” is drawn. He considers *every* variate as being

built up of a systematic part and a disturbance. The relations assumed between the variates are supposed to hold good exactly between the systematic parts, and the regression coefficients in these relations are called the true coefficients. (Tinbergen 1939, 28, 29–30)

In any case, Frisch decided not to follow the probabilistic turn in econometrics, which became dominant after Neyman convinced Haavelmo to adopt the “classical method” on probability in 1940 approximately (Duo 1993, 129). He would sometimes evoke a disposition for considering the stochastic interpretation of the ‘error’ but could not hide his own preference for the explanatory power of the deterministic mechanism, minimizing the theoretical status of the eventual shocks. As he stated in a lecture in Japan in 1960, “of course, I am all for a thoughtful stochastic theory, but it must be formulated in such a way that you can express a hypothesis about the data generating mechanism” (Frisch 1960, 10; see also Bjerkholt and Duppont-Kieffer 2011).

In his League of Nations piece, Tinbergen noted that analytical difficulties would be not only multicollinearity or the difficulty to determine lags, but also “the possibility that disturbances do not follow a simple statistical law of distribution” (Tinbergen 1939, 24). In any case, almost 50 years later on, Tinbergen not only came to emphasize his own past doubts, but also noted the eventual overestimation of the mechanism:

It [Frisch’s 1933 rocking horse model] was only a theoretical model and *I did not understand the role of the shocks as well as Frisch did*. But I think he was perfectly right, and of course one could indicate some of the exogenous variables playing the role of shocks. The most natural ones would be harvests or crops, and in fact they move as a random series. But there were other shocks as well. Too little effort has been made to identify which were the most important shocks in certain concrete cases. Theoretically, it was a very important concept. [...] On the other hand, *I think that what interested economists most was not the shocks, but the mechanism generating endogenous cycles, and it might very well be that we have overestimated the role of the mechanism*. Maybe the shocks were really much more important. This problem has never been solved, because the War came along and after the War we were not interested in business cycles anymore. (Tinbergen 1987a, 125; our italics)

Although he did not elaborate in detail on the subject to the best of my knowledge, Tinbergen mistrusted the traditional interpretations of the

‘error’ or ‘impulse’ or ‘stimulus’—the semantic variance is already a program per se:

The error term is introduced as a catchall for less important independent variables and for measuring errors of both the dependent variable and the independent variables. [...] Essentially, the introduction of an error term is a second best setup and in a way a *testimonium paupertatis*. (Tinbergen 1990, 201; on this, Louçã 2004)

Curiously enough, this topic was not sufficiently noted at the time of the Keynes-Tinbergen debate on the League of Nations volumes, perhaps given the difficulties of interpretation of the variables by the modeler himself. Keynes expressed the intuition that something was not clear with the residual, as he discussed the ‘statistical alchemy’ of the book:

Prof. Tinbergen finds room for outside explanations in the ‘residual.’ It follows that, in certain cases, the larger the residual, the more accurate the analysis will be. The more important the outside explanations are, the larger the residual ought to be. But does he not, in general, judge the accuracy of his analysis by the smallness of his residual? (Keynes 1940, 155)

And he had a point.

Several decades after the above-described debates, Tinbergen and Frisch again discussed the very same topic, just to pour scorn on those econometricians who had been transformed into technicians of numerology or sages of unrealistic approaches. Frisch fired the first rounds, writing in his chapter for the Harrod *Festschrift* that “epsilontologists” were nurturing “playometrics”, engaging in “engineering data” instead of delivering real statistics, which resulted in “too many of us often used too much of our time and energy on the study of the keyholes in northern Iceland in the first half of the thirteenth century”, and “piling up queer assumptions” (Frisch 1970, 161-162, 163, 165). In his typically more moderate style, Tinbergen added that “the desire to obtain high correlations gave birth to a species of econometricians called correlation hunters, and this species is sometimes rightly ridiculed” (Tinbergen 1991, 278). That was the epitaph of the past journey of these two economists in the field of econometrics.

For that reason, after the Second World War, both Frisch and Tinbergen moved to development economics as the appropriate domain for

planning, using decision models as the instruments for the economic expert and the social engineer. Haavelmo did exactly the same, abandoning econometrics at about the same time, also turning toward central planning (Epstein 1987, 128). In his own country, Tinbergen exercised an influential role as the director of the postwar Dutch Central Planning Office, a task he carried out up until the Summer of 1955. Similarly, Frisch played an important role in Norway, counseling the government and other institutions regarding the definition of goals and policies. Once again they both ended up on the same journey, with Frisch working with the governments of India and Egypt, and Tinbergen with those of India, Indonesia, and Turkey (Dekker 2021, 261ff.), with both being significantly involved in establishing the foundations of major national economic plans.

In spite of all resistances, the two social engineers persisted with their stance on political-economic decision-making and planning in those countries, but they were to live long enough to testify that these emerging leaders of the developing countries only immersed themselves in inconsequential efforts at planning that came to nothing. Nasser, Nehru, and Sukarno, or the heirs of Atatürk for that matter, who consulted either Frisch or Tinbergen, or both, all failed to prevail in their early efforts to design and follow ambitious plans, which were subsequently downgraded to mere management tools, or even fell into oblivion. Development policies, conceived by Frisch and Tinbergen as departments of planning, were later submerged in most of these countries by neoliberal options.

In his book, Dekker aptly analyzes how Tinbergen's vision of economic expertise carried his brilliant career and was cherished by his colleagues, as it did also for Frisch; but Dekker also recognizes that Tinbergen's contribution fell out of the standard in economics. Yet, he states, the heritage persists:

Tinbergen did not merely help turn economics into a quantitative empirical science through his work in econometrics. He helped turn economic policymaking into a quantitative domain in which instruments are manipulated to achieve policy goals[, although] many of them [the institutions of economic expertise] do not neatly fit into Tinbergen's vision of the role of economic experts. (Dekker 2021, 428)

The professionalization of economics has certainly been a multifaceted process, and considerable influence is to be attributed to those mathematically inclined economists who cherished quantification and led the early drive to econometrics. But the creation of contemporary institutions

of policy making, and the role of experts in the current affairs, were driven by the rejection of the approach of the first generation of econometricians, namely Tinbergen and Frisch. Policy goals are typically defined as the reverse of what they hoped for, as the functioning of markets reigns supreme, precisely what delivered in the past the ‘monstrosities’, leading to crises, a process that moved the opposition and inventiveness of the economists we are here studying.

VI. CONCLUSION: BREAKTHROUGHS AND FAILED PATHS

When Frisch and Tinbergen received the 1969 Bank of Sweden Prize in Economic Sciences in Memory of Alfred Nobel “for having developed and applied dynamic models for the analysis of economic processes” (Royal Swedish Academy of Sciences, n.d.)—the first such prize to be attributed in economics—both had in fact already moved away from econometrics for a long time. Nevertheless, it is beyond doubt that they had pursued the same path since the early thirties, a path that had taken them into the field of econometrics in the first place: understanding, modeling, and estimating the business cycle with the objective to tame it. Indeed, what changed was that managing the cycle evolved into the prioritized definition of policy instruments, true to the function of planning, where the social engineers—‘socialists in thought and action’—were to find their home.

Never tiring from attempting to convince his peers, Frisch made one last effort to present his concept of an econometrics based on planning at a 1963 seminar at the Vatican: “What I am going to present to you today is in all humility a frontal attack on a ghost that has been haunting all of us for the last generation. [...] The ghost is human nature itself”. He continued:

Therefore, the social challenge, facing us as economists and social engineers, is to help the politicians work out an economic system built upon a set of incentives, under the impact of which the economic activity will be satisfactory from the viewpoint of the economy as a whole, even if the behavior of many individuals is essentially selfish. We must find a means of circumventing the human obstacle to human progress. (Frisch 1963, 1198)

That would be the task at hand: with the objective to vanquish “unenlightened financialism” and to define the “preferences regarding the results to be obtained in the nation as a whole, or in the world”, in order to define

a “quantitative decision model”, including the creation of necessary institutions (Frisch 1963, 1199, 1203). This provoked a storm at the seminar and triggered the ferocious opposition of Maurice Allais, among others. By that time, Tinbergen was already uninterested by these quarrels.

As much as Frisch and Tinbergen influenced the agenda and the course of early econometrics and economic thought throughout the first half of the twentieth century, they were both unsuccessful in their effort to mobilize a new generation of planners following their approach. In effect, Tinbergen—the pragmatic public intellectual and the rigorous academic—and Frisch—the builder of the econometric edifice—both gloriously failed to define the future of their science, a fact that both acknowledged, and standard econometrics became a province they would eventually hardly recognize.

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