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Economic Modeling in Rawls: The Original Position

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Abstract: Critics of Rawls's *A Theory of Justice* frequently envision his original position as containing a human consciousness. Thus, the restrictions Rawls introduces for this 'individual'—the lack of particular circumstantial and personal information—is considered a potential problem. The very ways in which Rawls circumscribes the knowledge available in this position is thought to compromise the personhood of the individual there, and hence as well the conclusions reached (that is, Rawls's two principles). This paper will argue that, on the contrary, the lack of full personhood is a critical part of Rawls's modeling strategy, and that Rawls borrowed this particular sense of modeling from economics. It is well known that Rawls worked to verse himself in economic theory, and it is difficult to overlook its use in *Theory*. It will be argued that it is through parallels with economic reasoning that Rawls's original position model can be most fruitfully understood.

Keywords: Rawls, original position, economics, model, Mill, Kant, rationality, *homo economicus*

JEL Classification: B20, B31, B40, H30

I. INTRODUCTION

Who are the individuals in Rawls's 'original position'? A reader's answer to this question frequently determines a judgment on Rawls's entire system. In the original position, Rawls posits that individuals are stripped of much of their individuation, to achieve a certain end: "One excludes the knowledge of those contingencies which sets men at odds and allows them to be guided by their prejudices" (*A Theory of Justice*, 19).¹

¹ This and all subsequent references to *A Theory of Justice*, abbreviated as 'TJ', will be to the original edition (Rawls 1971).

Author's Note: I want to thank David Levy for the initial inspiration to work on Rawls, as well as direction towards particular sources. I must also thank Thomas Scanlon for helpful criticism, and encouragement. And the referees for this journal deserve particular mention for comments which dramatically added to the quality of the presentation.
The problem is: after one is deprived of knowing one’s individual history, awareness of social position, wealth, idea of possible prospects, race, and sex, is there sufficient self left to make reasonable choices about social and moral guidelines? On the face of it, it is perplexing. Are human beings behind the veil of ignorance real enough that we feel persuaded that their decisions would be our own?

This paper will argue that this line of questioning is fundamentally misconceived. However, there are two separate contexts to which such a claim may apply: the Rawls of A Theory of Justice (and before), and the Rawls of Political Liberalism. Rawls clearly had misgivings about the level of reliance on the rationality claim, and many of the critical building blocks utilized from economics, by the time of PL. Such refinements of the theory could be considered advances, or not. The focus here will be on Rawls previous to PL, primarily the Rawls of TJ. Even within TJ, however, there exists a troubling variety of terms of description. Rawls discusses the original position in two largely contradictory ways. One side of his presentation invites our participation, our projection of ourselves behind the veil—a kind of humanizing of his system. But another type of description—which Rawls takes some pains to emphasize is primary—is that the entire original position, including the ‘individuals’ situated there, is engineered. It is, essentially, a machine designed to generate a certain sort of output. This mechanistic sense of how the original position functions has proven an enormous stumbling block for critics. One dimension of the problem for critics is that Rawls developed this mechanistic formulation—which he had from his undergraduate days—through his close study of economics. This paper will attempt to make the argument that the original position is mechanical (Rawl’s in-

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2 It is likely that this cut-off is too severe. The lectures in Justice as Fairness: A Restatement, which are from the 1980s, do appear very much in sync with A Theory of Justice.

3 This and all subsequent references to Political Liberalism, abbreviated as ‘PL’, will be to the expanded edition (Rawls 2005).

4 “I use the distinction between the two parts of the original position which correspond to the reasonable and the rational as a vivid way to state the ideas that this position models the full conception of the person. I hope that this will prevent several misinterpretations of this position, for example, that it is intended to be morally neutral, or that it models only the notion of rationality, and therefore that justice as fairness attempts to select principles of justice purely on the basis of conception of rational choice as understood in economics or decision theory. For a Kantian view, such an attempt is out of the question and is incompatible with its conception of the person” (PL, 306n21).

5 For instance, T.M. Scanlon favors the Rawls of TJ (personal correspondence). For a compelling overview favoring the later Rawls, see Audard (2007).
tention), and examine how the parallels with economic theory make this more easily seen.

One predisposition that places us in a position to see Rawls’s idea more clearly is to envision the original position as a model. Models in economics have an occasionally shocking level of abstraction. The pushback against modeling in economics is similar to what we see in studies of Rawls; the individual (*homo economicus*) fails to tally with anything like our introspective view of ourselves. The modeled person lacks dimension. He or she also is assumed to act in isolation—decisions are assumed independent from those of others. How can the model be informative—how can it pertain to science—while misrepresenting human nature so completely? Such questions should be familiar to readers of and commentators on Rawls. His modeled ‘person’ is also stripped of critical dimensions and decides in a state of isolation. Yet these harsh caricatures—models—in economics, though being adjusted at the margin in various ways, have survived. Their survival is tied to what they offer the theorist. Economies are vast and complicated entities, and simplifications are required to add coherence to even a single element in the process. The complications in social ordering and morality—the issues Rawls faces—are also hugely complex. Rawls appreciates how models work in economics, and it is our perception of that appreciation that enables us to see his original position in the terms he intends.

II. USE OF MODELS

The approach of this paper—the use of neighboring disciplines to add dimension and insight to one another—is a technique that Rawls himself utilized. This is, of course, part of a larger argument about the importance of economic thinking in Rawls generally. Rawls explicitly invokes economics throughout *A Theory of Justice* and *Justice as Fairness: A Restatement* (2001). And he also argues in his lectures for the overlap of economics and political philosophy being a fruitful one. For instance, in his chapter on Hume’s “Of the Original Contract”, Rawls discusses the advantages of this dual attack:

Still, since 1900 the tradition [of utilitarian analysis] has divided into two more or less mutually-ignoring groups, the economists and the philosophers, to the reciprocal disadvantages of both; at least in so far as economists concern themselves with political economy and so-called welfare economics, and philosophers with moral and political philosophy. (Rawls 2007, 162–163)
It is important to realize that one of Rawls’s early inspirations—Mill’s utilitarianism—predates the schism mentioned in the quote. Mill was, we know, both a social theorist, facing the questions which interested Rawls, as well as the most famous economist of his era. And Mill had his own definition of ‘economic man’ (homo economicus). That definition centered on seeing individuals as wealth-accumulators. “Mill thus constructs a homo economicus but does so fully aware that his artifice is an ideal type which rarely has its exact counterpart in the world of reality” (Spiegel 1983, 380). Mill explains this in more depth:

No mathematician ever thought that his definition of a line corresponded to an actual line. As little did any political economist ever imagine that real men had no object of desire but wealth, or none which would not give way to the slightest motive of a pecuniary kind. But they were justified in assuming this, for the purposes of their argument; because they had to do only with those parts of human conduct which have pecuniary advantage for their direct and principal object; and because, as no two individual cases are exactly alike, no general maxims could ever be laid down unless some of the circumstances of the particular case were left out of consideration. (Mill [1967] 2006, 327)

Rawls, looking for a path into his problem, was certainly aware of these simplifications and abstractions Mill describes. They would provide an example of, and demonstrate the potential benefits of, this sort of modeling of the individual. And Rawls’s explorations of economics would bring him into contact with even more severe economic modeling. By the time one reaches the neoclassical view of homo economicus present in Knight (as opposed to the classical one in Mill), one finds that the purely economic actor is not human at all, but in his mechanical responses is more akin to a slot machine!6

In his economic reading—and Rawls footnotes over forty economists in TJ alone—Rawls would have come across a great deal of such model-making. Mary Morgan summarizes this tendency in 19th century economics:

But these were all model men compared to the rich descriptive portrait we find in other works of social science. Each model man was made to reduce the complexity of dealing with all human feelings

6 This is Knight’s actual phrase. For a more in-depth look at Knight’s influence on Rawls, see Coker (2021).
and emotions and actions that flow from them and, at the same time to focus the attention on the explicitly economic aspects of man’s behaviour. This sequence of model men was the nineteenth-century economists’ answer to the problem of dealing with human behaviours in a scientific way. (Morgan 2012, 164–165)

This tendency towards abstraction would only become more extreme in the contemporary economists with whom Rawls was also familiar. And Rawls himself, as we will see, will take great liberties with the notion of ‘rich descriptive portraits’ that some readers will expect to animate his notion of individuals behind the veil.

Before moving to the particulars of the argument, it might be useful to briefly touch on Rawls’s use of economics as a reference point. Such references, no matter Rawls’s reliance on them, tend to be excluded from critics’ summaries of his arguments. For instance, when Rawls considers just rewards for labor, his thinking and terms of analysis are economic:

It is easy to see, however, that this is not the case. The marginal product of labor depends upon supply and demand. What an individual contributes by his work varies with the demand of firms for his skills, and this in turn varies with the demand for the products of firms. An individual’s contribution is also affected by how many offer similar talents. There is no presumption, then, that following the precept of contribution leads to a just outcome unless the underlying market forces, and the availability of opportunities which they reflect, are appropriately regulated. (TJ, 308)

More startlingly, the reference point in discussing politics is again economic:

The ideal procedure is further clarified by noting that it stands in contrast to the ideal market process. Thus, granting that the classical assumptions for perfect competition hold, and that there are no external economies or diseconomies, and the like, an efficient economic configuration results. The ideal market is a perfect procedure with respect to efficiency. (TJ, 359)

By the next page we will find out that politics, of course, is not a perfect procedure. Discussing the original position, he once more centers it on economics:
I now turn to matters of detail. Note first the similarity between arguments from the original position and arguments in economics and social theory. The elementary theory of the consumer (the household) contains many examples of the latter. In each case we have rational persons (or agents) making decisions, or arriving at agreements, subject to certain conditions. (Rawls 2001, 81)

The reference is to constrained maximization, a basic technique in economics. He will go on to discuss the ability to predict the economic actor's choices. He soon turns to the need to avoid depending on external "psychological hypotheses or social conditions not already included in the description of the original position" (82). He elucidates the idea by discussing an economic example. The passage continues:

Consider the proposition in economics that the agent for the household buys the commodity-bundle indicated by the (unique) point in commodity-space at which the budget line is tangent to the (highest) indifference curve touching that line. This proposition follows deductively from the premises of demand theory. The necessary psychology is already included in those premises. Ideally we want the same to be true of the argument from the original position. (82–83)

The list of examples could be extended considerably, but these should serve to establish two points. The first is simply Rawls's familiarity with, and use as a reference point of, economics. His recourse to economics as a frame of reference, and as a technique of reasoning, is frequent and continuous. Rawls, to a surprising extent, sees into his problems through an economic lens. The second is how economics, by fashioning the terms of analysis—by its assumptions—is able to point to outcomes. It is this sense of modeling that helps us see how Rawls understands his original position. The restrictions on decision-making in the original position are extreme. These restrictions have caused many of Rawls's commentators to balk, insisting that violence has been done to critical components of human nature. But in the context of economic modeling that Rawls saw so plainly, restrictions/assumptions dispense with fidelity to reality as a standard, and instead are a closed system with the outcomes they produce. Rawls insists that he constructs the original position exactly to achieve/predict a particular outcome—his principles. Yet the full implications of his model understood purely in those terms remains a challenge to conceptualize.
III. ‘INDIVIDUALS’ IN THE ORIGINAL POSITION ARE NOT INDIVIDUALS

Rawls is specific in describing his theoretical procedure as modeling. Answering the charge that the hypothetical agreements in the original position would have no ‘significance’, he argues:

In reply, the significance of the original position lies in the fact that it is a device of representation or, alternatively, a thought-experiment for the purpose of public- and self-clarification. We are to think of it as modeling two things. (Rawls 2001, 17)

He then details those two things: fair conditions of agreement and “acceptable restrictions on the reasons on the basis of which the parties, situated in fair conditions, may properly put forward certain principles of political justice and reject others” (17). Even more explicitly:

In using the conception of citizens as free and equal persons we abstract from various features of the social world and idealize in certain ways. This brings out one role of abstract conceptions: they are used to gain a clear and uncluttered view of a question seen as fundamental. (8)

Or, keying off the parallels with economics (partially quoted earlier):

The proposition follows deductively from the premises of demand theory. The necessary psychology is already included in those premises. Ideally we want the same to be true of the argument from the original position: we include the necessary psychology in the description of the parties as rational representatives […]. As such, the parties are artificial persons, merely inhabitants of our device of representation: they are characters who have a part in the play of our thought-experiment. (83)

Rawls is here sculpting what he calls individuals merely to produce a certain result. This sort of paring away of the unhelpful, the intractable, and the irrelevant characterizes much of economic thinking. Here is Hal Varian’s introduction in his Microeconomic Analysis, where he discusses equilibrium:

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7 Here is a perfect example of Rawls’s ambiguity of presentation. In TJ as well, he states how in the original position “principles of justice should be chosen under certain conditions”. But later in that same paragraph, he says, “the ideal outcome would be that these conditions determine a unique set of principles” (TJ, 18). In this later refinement, it is no longer individuals choosing, but conditions determining.
The second analytic technique that we will use in our study of microeconomic behavior involves the study of equilibrium. At its broadest level, equilibrium analysis can be viewed as the analysis of what happens to an economic system when all of the unit’s behavior is compatible. Thus we will typically not be concerned with the analysis of an economic system when some firms or consumers find their actions thwarted.

The focus on equilibrium analysis is not due to the belief that equilibrium is necessarily more important than disequilibrium, but rather that the analysis of behavior in disequilibrium is substantially more difficult. (Varian 1984, 1)

Here is a classic economic assumption, seeking an answer or conclusion through a radical simplification of circumstances, and letting those simplifications determine theory. It could be, and has been, argued that excluding situations in disequilibrium essentially excludes economic situations altogether. There is pushback in economics against how relevant equilibrium analysis is for real-world applications. But economic theory is held together by a web of such models, mathematical and otherwise. Morgan indicates that modeling of a technical sort began to dominate economics in the latter half of the twentieth century. But in the more general sense in which we are considering it here, it has been central from at least the time of Adam Smith. It represents a group of keys that economists try in every lock. And its ascendancy as a self-consciously used methodology parallels the time of Rawls’s early exposure to it. By then it “had become the accepted mode of reasoning in economics in the sense that it became ‘the right way to reason [...] what it is to reason rightly’” (Morgan 2012, 13–14). Economists in the 1950s and '60s were beginning to address questions that interested Rawls, bringing with them a mode of thinking closely linked to their training in this intensive modeling.

This has all been somewhat hypothetical and general so far. How would it look in terms of a specific critique of Rawls’s position? In his *Liberalism and the Limits of Justice,* Sandel (1982) argues that the results derived from the original position have a certain reflexive character. The resulting final product is one “of dual dimensions, and in this it’s the key to our account”, Sandel argues, and continues:

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8 Focusing on this particular issue in Sandel should not imply there are not interesting insights elsewhere in his study!
For what issues at one end in a theory of justice must issue at the other in a theory of the person, or more precisely, a theory of the moral subject. Looking from one direction through the lens of the original position we see the two principles of justice; looking from the other direction we see a reflection of ourselves. If the method of relative equilibrium operates with the symmetry Rawls ascribes to it, the original position must produce not only a moral theory but also a philosophical anthropology. (Sandel 1982, 48)

The passage in Rawls that is mentioned as the springboard for this view is the section in which Rawls discusses going in and out of the original position, as a kind of check on what we are thinking there (TJ, 20). This movement is at least partly to evaluate the reasonableness and strength (or weakness) of the principles generated. Rawls argues that we wish these principles to be as weak as possible but still answer to our considered moral judgments. They must also be acceptable to others. But this process is to modify the original position itself; if we cannot reach ‘significant’ judgments, then we should adjust the conditions under which we deliberate.

Sandel’s assessment of what should be happening here requires a jump in reasoning. He takes this experimental back-and-forth Rawls describes as implying that we are judging the evaluative self as well. It is not just the deliberative procedure that is in play, it is the sense of ourselves as actual individuals behind the veil. There is, as he puts it, an anthropology involved.

We must be prepared to live with the vision contained in the original position, mutual disinterest and all, prepared to live with it in the sense of accepting its description as an accurate reflection of human moral circumstance, consistent with our understanding of ourselves. (Sandel 1982, 48–49)

Rawls, though, does not intend this ‘vision’ to be ‘an accurate reflection of human moral circumstance, consistent with our understanding of ourselves’, in anything like the sense that Sandel wishes to understand it here. Sandel will go on to argue that surely we cannot be content to have this “individual” so cut off from society and its effects; “it rules out the possibility of a public life in which, for good or ill, the identity as well as the interests of the participants could be at stake” (62). The reason we are not compelled by such an argument is that it has disconnected from Rawls’s text altogether. Rawls might argue that such things have already
had their effect, and have been incorporated in our deepest moral intuitions. But for our purposes Sandel’s error is to take the model for a depiction of an actual individual in some critical way. And as often as Rawls uses the word model to describe his constructs, Sandel (and many other critics) tellingly fail to follow suit. Models are functional, and they will be unrealistic to the degree necessary to achieve that functionality. This should instill a degree of caution in evaluating them. And standards of ‘richness’ and/or verisimilitude would likely be particularly fraught starting points. Rawls is elusive here; he utilizes models in a manner economists take as standard, but which philosophers frequently attempt to understand on other grounds. The result, in criticism such as that offered by Sandel, is that the argument misses its subject. (Economists, unfamiliar with many philosophical premises, also often transpose Rawls’s arguments along lines more compatible with their training, with similar results.)

IV. CAN THE ORIGINAL POSITION BE SIMULATED IN EVERYDAY LIFE?

Much of the discussion so far has been on the portrait of the ‘individual’ behind the veil, and to what standards of realism or completeness that portrait should be held. But modeling is two-sided: it sacrifices the verisimilitude of the agent, to achieve a predictable outcome. The outcome, in that sense, is the driver; the assumptions, and the agent, are molded to whatever degree necessary to achieve that outcome. Rawls is direct in indicating that this is how he sees the original position—as a situation designed to output certain principles. Yet Rawls places the original position in a variety of contexts. While economics is clearly a central one, the inclination to place it in relation to Kant is present in TJ, and continues through the various refinements he introduces later. How does, or does, the mechanistic model conform to a Kantian connection? Rawls suggests that the noumenal self from Kant finds its corresponding entity in the ‘individual’ behind the veil. This claim of linkage puts stress on the system. The argument in TJ is that ‘acting autonomously’ in Rawls’s theory plays out in the nature of the original position. The Kantian (noumenal) self would not be influenced or controlled by circumstance in choosing moral boundaries and priorities; the original position suitably isolates the decision from these elements. Yet this way of arguing

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9 Argued in (TJ, 251–257).
10 Rawls also links the veil of ignorance to Kant, saying it is "implicit" in Kant's ethics (TJ, 140–141; §40).
might seem to place an actual self behind the veil, something that Rawls argues explicitly against elsewhere. One way to understand this difficulty is to recall Rawls's use of—in his economic parallel—of a psychology being ‘built in’. “The proposition follows deductively from the premises of demand theory. The necessary psychology is already included in those premises” (Rawls 2001, 83). In the original position, Rawls implies the similar inclusion of a ‘necessary psychology’. Just as the ‘individual’ in demand theory in economics is pared down to her dimension as ‘consumer’, Rawls's agent is likewise pared to only those elements required for ‘moral choice’. That this agent, as in economics, is not actually a person implies a certain interpretation of Kant’s noumenal self. In “Kantian Constructivism in Moral Theory”, Rawls ([1980] 1999) does discuss ‘moral persons’ behind the veil. But the terms appear unchanged from TJ; Rawls claims that “I am concerned with the parties in the original position only as rationally autonomous agents of construction” (308). These are not actual persons: “The rational autonomy of the parties in the original position contrasts with the full autonomy of citizens in society” (308). Later he reiterates that the parties “are rational agents of construction” (315). We must leave it there. We fall back on “at the basis of the theory, one tries to assume as little as possible” (TJ, 129). But certainly Rawls's engagement with Kant was involved and ongoing.

Rawls's quotation above—concerning the ‘necessary psychology’ being built-in—does require a clarification of how Rawls intends ‘prediction’ in his theory to be understood. The classic paper on prediction in economics is probably Friedman ([1953] 1966), where Friedman famously separates reasonable assumptions from accurate predictions, insisting that only the accuracy of the prediction, not the reasonableness of the assumptions, are to guide us in selecting a model. Rawls, in a book (TJ) awash with citations from economics, glaringly fails to cite this paper. This should reinforce our sense that Rawls’s use of prediction is distinct from Friedman’s, and from econometric usage generally. Friedman’s cavalier attitude toward assumptions doesn't match up well with Rawls's carefully deliberated original position. Rawls's mention of demand theory, perfect competition, and of tangencies of curves speaks of

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11 Taylor (2014) argues similarly from later Rawls ([1980] 1999). Taylor discusses the second moral power of rationality, defined by Rawls as the “capacity to form, to revise, and rationally to pursue a conception of the good” ([1980] 1999, 312): “Thus, Rawls's second moral power of rationality which unites deliberative rationality with creative self-authorship, is simply a variation on the contemporary concept of personal autonomy. [...] This self-authorship links rationality (so understood) to Kantian moral autonomy, which Rawls endorses in Theory, §40” (Taylor 2014, 154–155).
a different type of parallel. In those instances, the level of abstraction is severe. There is no ‘self’ for the individuals in those scenarios, there is only ‘outcome’. This is clearly what Rawls aspires to for his theory. As an example, one might imagine being a consumer facing a certain market price. We enter imaginatively into the role of the consumer, yet the construction exists outside this entering, and does not depend in any way upon it. This is the proper way to understand the original position. We “can by deliberately following the constraints it expresses simulate the reflections of the parties” (TJ, 120). But despite our negative or positive reaction during this simulation, the structure of the original position and the principles it generates remain unchanged.

But there is adjustment, or flux, present in how the individual is assumed to arrive at reflective equilibrium. The process there is matching and modifying moral dispositions; we search for weak premises, then test them to see how thoroughly they account for “considered convictions” of justice. These are then adjusted against one another—changing the premises to account for previous convictions, or modifying our existing judgments (20–22). Although Rawls is not entirely clear, he does say we are not generating the principles directly, but are “altering the conditions of the contractual circumstances” (20). We are deciding on the circumstances of the original position; the principles are then generated by those circumstances. This process serves, ultimately, to remove the individual, and replace her or him with as close to a pure procedure as Rawls can construct. This is the “procedure familiar in social theory”. Decisions made will be “strictly deductive” (119). Rawls asserts that the output of these particular principles is the only possible result of his original position, as configured. Whether this is an acceptable way to proceed is a valid question; but doubting the intention distances us from the argument of the text. Rawls states that the machinery he introduces cannot be supposed in our ordinary lives. Such attempts to “simulate the original position in everyday life” will be impeded by “our various propensities and aversions” (147). That we will fail to inhabit the original position simply by casting our minds there illustrates the need for the model. We can move towards the mindset of the original position, without arriving successfully. If we view the original position incorrectly, as an environment inhabited by an actual full consciousness, then we should find our confidence in the system’s results undermined. Yet this is not the case: “But none of this affects the contention that in the origi-
nal position rational persons so characterized would make a certain decision. This proposition belongs to the theory of justice” (147).

V. MECHANISTIC MODELS IN RAWLS’S EARLY WORK

We were dismissive of Rawls connection with economic prediction above, in relation to Friedman. But there is a sense in which that needs to be revisited. Rawls, in progressing towards the formulation in TJ, did in some sense utilize the idea of prediction as test. In an early graduate school paper, “A Brief Inquiry into the Nature and Function of Ethical Theory”, Rawls (1946) postulated that moral philosophers should search for a model which could be described as a ‘reasoning machine’. These machines would be:

Systems of definitions and axioms such that when fed determinate input regarding the sorts of familiar moral choices with respect to which we can noncontroversially distinguish competent from incompetent judgments, they yield theorems or moral principles, that provide sufficient reasons for, and thereby render intelligible to us, all and only the competent judgments. (Reidy 2014, 13; much of what follows is indebted to this paper)

How predictive this all is in a Friedman sense is not completely clear, but it does have a version of ‘data’—the difference between competent and incompetent noncontroversial moral judgments— which would allow a reasoning machine candidate to fail. Reidy goes on to make a connection with Frege’s use of a similar ‘machine’ to distinguish valid inferences, and outlines a connection with the Vienna Circle and positivism. What logical positivism asserts that is helpful here is its narrowing of what counts as meaningful: either analytical statements (for example, tautologies) or synthetic statements, which can be verified or falsified by evidence (Caldwell 1982, 13). This eliminates meaning in metaphysical statements, and avoiding metaphysical options was a relative constant across Rawls’s various iterations of theory.

It is interesting, and significant, to see a mechanistic design at the heart of Rawls’s deliberations so early. Additional evidence from the archives is presented by Galisanka (2019). From a 1962 lecture, “Nature of Political and Social Thought and Methodology”, we find: “Ideally, we want an account which reduces to zero the need for the other person to rely on his intuitive hunches as to how we will judge” (quoted in Galisanka 2019, 183; underlining in the original). In that lecture, the idea
of a machine is still present, even as the material for TJ is taking shape. That machine performs similarly to the much earlier 1946 version, in that the theorist should search for principles that, when built “into a machine”, would produce our own moral judgments when “given the facts and information which we regard as relevant” (quoted in Galisanka 2019, 182). One immediately noticeable change in transitioning to TJ, however, regards the role of information. These earlier mechanistic constructs relied on information—in 1946, more information improved results, without qualification. By 1962, the information is that “which we regard as relevant”. By the publication of TJ, the hugely distorting, and hence dangerous, role of information takes center stage. The original position is the response to this realization. The mechanistic function which outputs the two principles is adapted from earlier versions. The example of economics becomes crucial to the idea of rationality, and establishes parallels for how the automatic response of Rawls’s procedure might work. Rawls envisions all these parts as critically interdependent:

The notions of the basic structure, of the veil of ignorance, of a lexical order, of the least favored position, as well as of pure procedural justice are all examples of [simple concepts that can be assembled to give a reasonable conception of justice]. By themselves none of these could be expected to work, but properly put together they may serve well enough. (TJ, 89)

Rawls’s use of the phrase ‘reasoning machine’ for these earlier versions of his deliberative process makes unavoidable what should have been clear from the later work. The deliberative process should be understood as largely automatic, and mechanical. It is a model, in the sense in which we have been examining it in economics. This understanding of the deliberative position—so boldly presented in the early work—is refined and expanded in TJ. A somewhat lengthy quote covers several topics:

One should note also that the acceptance of these principles is not conjectured as a psychological law or probability. Ideally anyway, I should like to show that their acknowledgment is the only choice consistent with the full description of the original position. The argument aims eventually to be strictly deductive. To be sure, the persons in the original position have a certain psychology, since various assumptions are made about their beliefs and interest. These as-
sumptions appear along with other premises in the description of this initial situation. But clearly arguments from such premises can be fully deductive, as theories in politics and economics attest. We should strive for a kind of moral geometry with all the rigor which this name connotes. Unhappily the reasoning I shall give will fall far short of this, since it is highly intuitive throughout. Yet it is essential to have in mind the ideal one would like to achieve. (121)

Rawls’s inability to fully implement his ‘moral geometry’ does not imply that we should lose track of it, as an ideal. He reminds us of this, it would seem, because he suspects this is what we might tend to do—lose track of it. There is a ‘certain psychology’ present as well. We might well be tempted to focus on that, and synthetically inflate its dimensions, rather than see it merely as an approximation of his actual model: moral geometry. But we should also remember how psychological assumptions were mentioned when Rawls’s discussed demand theory (above). In that context they implied no attempt at verisimilitude; nor should they have such an implication here. Recall as well Mill’s use of geometry in his example. There, no line as defined by mathematicians had any connection to something that might exist in the world. That does not mean geometry cannot have real-world implications. In an obvious example, geometry can aid in such things as bridge building (Hands 2001, 23). For Rawls such geometry is clearly a continuation of the automatic nature characterizing his earlier ‘reasoning machine’. In these terms, what is in the original position is something, more than someone. There could be any number of these somethings, any number of original positions. (“We may conjecture that for each traditional conception of justice there exists an interpretation of the initial situation in which its principles are the preferred solution” [TJ, 121].) Rawls has constructed the one which will output his desired principles.

It is unclear when Rawls began reading economics seriously; Reidy is working in much of the above with a paper Rawls wrote in 1946. If this predates his familiarity with economics, Rawls is establishing a number of viewpoints and working practices that he will discover dramatically overlap such ideas in economics. Rawls’s reasoning machine is clearly designated as such to avoid criticism for failing to resemble actual per-
It is interesting, on the one hand, to chart the evolution of Rawls’s system as it changes shape and pivots on different concerns. But it is also of interest to see consistent preoccupations and solution-types visible at widely varying times. One such idea is how his system differs from intuitionism. Rawls struggles to get around intuition as a general position and fallback as early as his doctoral dissertation. There, his search for “principles [that] are like functions in mathematics” would result in principles “so precise that they are ‘mechanically followed’, or used by consciously applying the rules without appeal to intuition’ (Rawls 1950; quoted in Galisanka 2019, 79). By 1971 Rawls is still working to circumvent a position he admits “may be true”. He argues in TJ that his own system succeeds against intuitionism at least on one count: it narrows the scope of inquiry. In his system “we have asked a much more limited question and have substituted for an ethical judgment a judgment of rational prudence” (TJ, 44). Rational prudence will, suitably situated, reduce (or eliminate!) the variety of ethical positions inherent in moral judgment based on intuition. But in both accounts precision and consensus are target goals.

His argument from a linguistic parallel has a similarly precise and mechanistic feel:

There is no reason to assume that our sense of justice can be adequately characterized by familiar common sense precepts, or derived from the more obvious learning principles. A correct account of moral capacities will certainly involve principles and theoretical constructions which go much beyond the norms and standards cited in everyday life; it may eventually require fairly sophisticated mathematics as well. This is to be expected, since on the contract view the theory of justice is part of the theory of rational choice. (47)
come) will determine. And rationality, as Rawls indicates, bonds the system’s functionality all the more firmly to economics.\footnote{“We suppose that the parties are rational, where rationality (as distinguished from reasonableness) is understood in the way familiar from economics” (Rawls 2001, 87). Also, “moreover, the concept of rationality must be interpreted as far as possible in the narrow sense, standard in economic theory, of taking the most effective means to given ends” (TJ, 14).}

We have seen that Rawls’s use of models, though (perhaps) extreme and perplexing by philosophical standards, was anything but in the context of positivism and economics. Critics, however, sometimes have difficulty refraining from rushing in. Nagel, for example, criticizes Rawls’s original position in terms of its excessive disconnectedness.

The model contains a strong individualistic bias, which is further strengthened by the motivational assumptions of mutual disinterest and absence of envy. These assumptions have the effect of discounting the claims of conceptions of the good that depend heavily on the relation between one’s own position and that of others. (Nagel 1975, 9)

Rawls would approve of this assessment (focusing on disinterest), seeing it as a critical component of his system rather than a criticism. What Nagel here considers a too individualistic foundation to the original position is merely a blind alley. He commits the error of imagining different individuals, with differing needs and inclinations, in the original position. Not seeing Rawls’s model—a term Nagel does use!—in its proper form distracts from more pertinent analysis. But even critics seeking to justify Rawls’s original position often fail to see Rawls’s use of models clearly. Samuel Freeman could stand for these critics. Freeman defends Rawls with the example of a mathematician. For Freeman, the mathematician need not “keep in mind particular facts about their personal lives in order to successfully” solve a problem (2007, 160). Only portions of their mental abilities—divorced from much of their personal history—need be operational. But the example puts pressure on the vulnerable point: are the ‘givens’ operating behind the veil similar to those in play for mathematics? The parallelism does not, in fact, seem to hold. Critics might respond to this defence by saying: ‘Yes, it is exactly how these situations are not parallel that is informative. Moral underpinnings do not have the freestanding existence that mathematical theory does. Removing the personal history building blocks that fashion our beliefs creates problems’. But this point-counterpoint argument also misses the
essential character of the original position, which makes the exchange moot.

One can bypass these questions altogether of course by counter-theorizing that the entire construct of the original position is unnecessary, as Scanlon (1982) does. And it is not that all commentators have failed to follow Rawls when he discusses figures in the original position as idealizations. Nussbaum, for instance, does use the word “model”, and calls the individuals behind the veil “imaginary” (2015, 5, 17). Similarly, O’Neill recognizes the “no actual persons take part in or count in the Original Position” (2015, 60). But these recognitions still seem to stop short of the radical usage Rawls sometimes has in mind. His critics almost never discuss (or is it actually never?) his models in the mechanistic terms in which Rawls first conceived of them. This gives us perspective on arguments about how literally we are to interpret the text; that is, about its rhetorical nature. Laden (2014) speculates that Rawls utilized the language (in TJ) that would most clearly convince an audience of “utilitarian mathematicians and economists as well as moral philosophers”. This called for the deployment of a “formal apparatus” that was not, in fact, “essential to the argument” (67). This is an interesting viewpoint, but neglects to account for how those very formal features, designed to convince, were downplayed, or overlooked altogether. This, combined with the consistency of Rawls’s usage of mechanistic elements in the previous decades, leads us to believe that Rawls utilized this formal treatment even though it would disconcert. Whether we are comfortable with it or not, it forces us to be outside the system, largely to observe it rather than participate in it. Adam Smith famously exhorts us to exercise a Stoic vision of others, to value those distant from us the same as we value those near at hand. But he knows this to be impossible. We naturally favor friends and family over faceless and distant strangers. Rawls would have us view his conclusions as dispassionately and abstractly as possible—to evaluate them without consciousness of our own position, sex, race, or advantages. This is also impossible. Rawls often prefaces sentences with the word ‘ideally’. He states that when making moral decisions we often strive for the sort of objectivity and generality that the original position represents in the extreme. In that sense, we are asked to extrapolate beyond our possible experience. This distances us from the procedure, but the outcomes become proximate in the practical sphere. We can then ask: Do these outputs, emerging from this abstract cauldron, align with our deeply held beliefs? And
are they apt to be politically useful? These are elusive and difficult questions. But we miss them, and much else, if we get ensnared, stumbling over issues of the verisimilitude of ‘individuals’ in the original position.

A somewhat unusual parallel might prove informative. Morgan in her models book devotes a section to models as caricatures.\textsuperscript{16} Since her book is about modeling in economics, the brief look at caricatures is meant to lend some insight into what is going on with economists when they use models. Her visual example is the caricature of Louis Philippe in the middle of the nineteenth century by Charles Philipon. The caricature is that of a pear, with only a few lines to indicate that a face should be imagined on the fruit. The caricature had bite because the word for pear in French has the connotations of fathead, or dupe. And since it was the King he was caricaturing, Philipon was arraigned in court. For his defense he drew four images, the first being a portrait of the King, and the last being his pear caricature, with the intervening two being stages of abstraction. His defense was two-fold: (1) the first portrait of Louis Philippe, though quite accurate, had no indication that he was the King, and (2) really, the caricature looked like a pear, not so much like any particular person. The short story is he lost his case. But his pear won the war, as citizens across France scribbled it on buildings and fences with the obvious intention of ridiculing the monarch (Morgan 2012, 157–164).

For our purposes, we realize that the pear—while not losing touch with its origin in the face of Louis Philippe altogether—is also something other than a portrait of a person. It models, you could say, a single dimension of its subject—his fatheadedness. This otherness is the bridge that Rawls’s critics have trouble crossing. When one describes a person in a certain situation as ‘imaginary’, the unreality of the situation does not imply that we’ve given up on conceiving of the person as someone dimensionally similar to ourselves. Imagined scenarios can partake of sufficient realism to make them compelling. It is a leap of a different sort when a person becomes a pear, or an economic actor becomes a slot machine. Such transformations are driven by their end result; it is the insult of the pear, or the automatic nature of the slot machine, that

\textsuperscript{16} Gibbard and Varian (1978) discuss economic models as caricatures in a slightly different way. For them, when a new “feature of the world” assumes centrality in a model, “the representation of the feature is not so much an approximate description of the feature and its place in the world as it is a caricature. By that we mean not only that the approximation is rough and simple, but that the degree of approximation is not an important consideration in the design of the model” (Gibbard and Varian 1978, 673).
determines how the ‘person’ winds up in the model, not the strength of the connection back to real individuals. If economic actors are modeled as wealth maximizers (Mill’s concept), then it isn’t just that we are focusing on individuals in a market context. In that context someone might well be motivated by charity, or compassion, or any number of conflicting impulses. But the model screens those out. It aims at a *result*. Rawls maintains that his model of the original position works in a similar fashion. We begin with the outcome, and then puzzle out what dimensions of human reasoning would generate that outcome, and then isolate them. Thus, individuals behind the veil are more than (or actually less than) imagined; they are simply aspects. Can aspects alone come to conclusions about principles of social morality? Well, they had better be able to, because that is the entire motivation behind their distillation. The charge against Rawls in this area might be that those particular aspects fail in their mission, and generate something other than Rawls’s two principles. The charge cannot be that those aspects display a certain disqualifying lack of verisimilitude.

This level of abstraction in economics frequently involves assumptions about the environment in which actors operate as well. The perfection in ‘perfect competition’ obviates individual initiative. Knight believed that, in the real world, actors were searching, and in a state of becoming, without fixed preferences to strictly order their behavior. Yet his *homo economicus* model occupied the opposite extreme.

> With uncertainty absent [...] it is doubtful whether intelligence itself would exist in such a situation; in a world so built that perfect knowledge was theoretically possible, it seems likely that all organic readjustments would become mechanical, all organisms automata. (Knight 1921, 268)\(^{17}\)

In the actual world, a variety of motives and concerns can motivate individuals in identical circumstances. In such a situation, one’s economic prediction might attain Mill’s ‘most of the time’ standard. But Rawls seeks unanimity, as he tells us.\(^{18}\) Unanimity is a very mechanistic result.

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\(^{17}\) We should recall here that Rawls was an exceptionally attentive reader of Knight. See footnote 5 (above).

\(^{18}\) “Moreover, if in choosing principles we required unanimity even when there is full information, only a few rather obvious cases could be decided. A conception of justice based on unanimity in these circumstances would indeed be weak and trivial, but once knowledge is excluded, the requirement of unanimity is not out of place and the fact that it can be satisfied is of great importance. It enables us to say of the preferred conception of justice that it represents a genuine reconciliation of interests” (*TJ*, 141–142).
There is no room for a variety of personal motives. And there is no place for a probability calculation that varies according to the participant’s attitude toward risk. Rawls rules those considerations out. He preserves only those aspects of evaluation that will generate his desired outcome. In actual coalition-building, attaining unanimity has one huge virtue, and one huge cost—both obvious. The cost is convincing everyone down to the last misanthrope that your program is preferred. The advantage is there is no coercion in enforcing the policies approved—they are what everyone wants. These two costs—decision and coercion—were graphed against each other in a book by Buchanan and Tullock, that Rawls read, footnoted in *TJ*, and with the authors of which he initiated a correspondence. Buchanan (who advanced the constitutional perspective found in the book further on his own) and Rawls each felt their programs had significant commonality in their types of model-solutions, and in their assessment of the dangers these models helped avoid.

But how to achieve the virtues of unanimity, absent the costs? One technique which most likely won’t work is simply to have a freewheeling discussion. Rawls and Buchanan share a strong joint influence in Knight, and Knight strongly opposes the notion of arriving at anything resembling what he terms ‘truth’ through the act of persuasion. Rawls repeats these arguments from Knight in *TJ*. Knight himself circles this problem of unanimity without making headway. The impasse for Knight is based on a lack of faith in everyday citizen thought—and this problem draws him towards a world of experts. Rawls plunges deeply in the other direction; he wants a system to output principles which determine noncontroversial judgments of right. In what Rawls calls the ‘science of ethics’ (a phrase Knight also uses) in his early work, there is room for honest disagreement; such disagreements are in good faith, and qualify as reasonable. His system is designed to root out positions not held in good faith, positions whose motivation is pointedly self-interested, or based on power or class or general group-bias. Rawls needs principles he can utilize against such opinions, but the more abstract principled views he

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19 The book is *The Calculus of Consent* (1962). The book was the spark to form a group—the Committee on Non-Market Decision-Making, later the Public Choice Society. Rawls was invited to, and attended, their second annual meeting. For a selection of the Rawls/Buchanan correspondence, see Peart and Levy (2008).

20 I am using the word ‘noncontroversial’, from Rawls’s early work, as a shorthand term. In *TJ* he describes the same idea more elaborately: “We can note whether applying these principles would lead us to make the same judgments about the basic structure of society which we now make intuitively and in which we have the greatest confidence” (*TJ*, 19).
seeks and these everyday muddying concerns would seem to be intertwined. Modeling is Rawls's method of disentanglement.

VI. CONCLUSION

It is possible to see models in two different ways: (1) as a world unto themselves, and (2) as a stylized version of the actual world. Each of these must have some connection to the actual world to be of interest, but their connection is not the same. Morgan sees these as distinct (Morgan 2012, 30–37). But it should be possible to see them as degrees of abstraction as well. In a sense, model builders use the level of abstraction required to order the model’s world, and to secure the outcome the model was designed to examine or produce. In economics, perfect competition would be a failed model if only ‘a large number’ of buyers were price takers. If some really possessed bargaining power, the point of the model disappears. In other words, if the model were less extreme, and more realistic, it would become pointless. As it is, it has heuristic value, and is a graph in almost every principles textbook. It is important to keep this sort of reasoning in mind when examining Rawls’s original position specification. Failure to see ‘ourselves’ in the original position may seem a telling criticism, but in fact such criticism points to a misunderstanding of Rawls’s system. The original position has a test for success: it must output principles that coordinate with or predict our judgments of noncontroversial moral questions. In the mind of the model builder, this requirement demands a level of severity. If, for instance, the ‘individual’ or process in the original position were to have memory of its wealth level, this could be considered a gain in ‘realism’. We would be more apt to recognize ourselves in this position. And shouldn’t this greater ability to associate one’s personal reality with the model construction add to our assurance about its relevance? Even more critically, we might want particular memories concerning how we formed, or solidified, some of our moral predispositions. Perhaps we feel, with these and other additions, we would gain confidence about the ability of the construct to output the same principles it did under the alternative specification. If this were so, however, we would have competing—different—theories of justice. By not selecting them, when he could have, Rawls implies these alternate models would prove less robust than his own. We must suppose he considered these ‘fleshier’, more accurate versions of a deliberative self/process, and found they failed to output the required principles. Why abstract more than one
needs to? The process he gives us is exactly the process that performs as required—meeting, as it were, its design brief. And we have every textual reason to suppose, despite it having ‘a certain psychology’, that the decision-process in the original position can most successfully be understood as a sort of mechanism. To ignore this, and desire that the process doing this work were more like us, is to misunderstand Rawls’s program in a fundamental way.

Morgan suggests that philosophers have problems with the concept of models for a number of (well-founded) reasons. She mentions particularly that there is “concern about the status of the representation” (2012, 33). What does the model ‘mean’ if it is something other than the world? Sugden discusses the lure of instrumentalism, where “the ‘assumptions’ of a theory, properly understood, are no more than a compact notation for summarizing the theory’s predictions; thus the question of whether assumptions are realistic or unrealistic does not arise” (2002, 117). For Rawls, this would seem to imply looking to applying principles to the noncontroversial moral situations. Or, as Rawls phrases it in TJ, the output must align with certain convictions. And “these convictions are provisional fixed points which we presume any conception of justice must fit” (TJ, 20). This total scrapping of the model’s link to reality—except through the predictive tie—makes the terms of description in models somewhat misleading. In economics as well, calling the units in the model ‘individuals’ necessarily creates a tension. We have a tendency—across disciplines—to imagine them, at least in certain ways, like ourselves. We want to have some grip on the nature of their ‘status of representation’. Because we intend the predictive outcome of the model to apply to individuals, the natural inclination is to envision the model in individual terms as well. Rawls, like economists, necessarily blurs our picture by talking of the model both as a complete abstraction, and as some version of an individual. We focus on this latter representation not just because it is familiar, but also because it gives us another way to grasp the theory. Models as total abstractions are the creatures of their creator; they have purpose without dimension. And in the sense that they are a black box, they resist our criticism. As critics, we find this an obstruction. We want to choose our interpretation of what the model is, in order to bypass this impediment.

Perhaps we need to be more cautious than this, however. It is simply being argued that, in light of a natural tendency to dress our models as people, we should be cognizant of how this can work against, rather
than aid, our better understanding. Rawls points on several occasions to the tension we’re discussing; this is from *Political Liberalism*:

As a device of representation, its *abstractness* invites misunderstanding. In particular, the description of the parties may seem to presuppose a particular metaphysical conception of the person [...].

I believe this to be an illusion caused by not seeing the original position as a device of representation. The veil of ignorance, to mention one prominent feature of that position, has no specific metaphysical implications concerning the nature of the self; it does not imply that the self is ontologically prior to the facts about persons that the parties are excluded from knowing. (*PL*, 27)

Rawls then introduces the comparison with role playing. When acting the role of Macbeth or Lady Macbeth, we shouldn’t be thinking we *are* plotting nefariously in Scotland! We must keep in mind what the exercise is about: “Trying to show how the idea of society as a fair system of social cooperation can be unfolded so as to find principles specifying the basic rights and liberties and the forms of equality most appropriate to this cooperating, once they are regarded as citizens, as free and equal persons” (27).

In conclusion, it might be fitting to examine another thinker who has strongly influenced both philosophy and economics—Thomas Hobbes. If we can talk about Hobbes’s ([1651] 1968) ‘state of nature’ as a model, it offers some contrast with Rawls’s. In Hobbes, it is the situation that is modeled, but you are supposed to imagine yourself in that situation, fully formed. Hobbes’s model also fails the realism test (as, we have been arguing, almost every model does). In the real world, individuals bunch into groups or tribes, and the warfare is external. Internally, within-group, there is relative harmony. Hence Hobbes’s model is ‘incorrect’. Rawls’s model would seem to ask less of us—we don’t need to imagine a new exterior environment. But in certain ways it asks much more. It asks, at the least, for us to enter a mode in which we are not fully ourselves. More accurately, though, it asks of us to become a slice of reasoning behavior, and not really be ourselves at all. Models, to function, can be extreme. Rawls requires his system to generate very specific and powerful conclusions. And to accomplish this his model, despite its nominal parallels with merely sequestered individuals, is at core more extreme than most.
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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 revaluation 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-Tinberian 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, 85 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 (Erhenfest to Schumpeter, May 2, 1918). In the same sense, he later insisted to Tinbergen about the need to explore those bridges (Erhenfest 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 sceptical 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 Ataturk 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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Reading Tinbergen Through the Lens of Max Weber

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I. INTRODUCTION
With *Jan Tinbergen (1903–1994) and the Rise of Economic Expertise*, Erwin Dekker (2021) has written the biography of Jan Tinbergen that he deserves. As a figure both important for the development of the economic sciences and immensely influential on Dutch politics, Dekker's book is not the first assessment of Tinbergen's life and work. But where previous authors have focussed on one aspect of Tinbergen's thought—for instance, as scientific expert, party ideologue, or inventor of the first macroeconomic model, and often reducing Tinbergen to a specific archetype, like technocrat, calculative engineer, or dreamy idealist—Dekker takes all these aspects and paints a complex picture.¹ Tinbergen was both technocrat and idealist, neutral scientific scholar and party ideologue, and thus cannot be reduced to any ideal-type.

One of the myths Dekker seeks to dispel in particular is the idea that Tinbergen was a pioneer or that he embodied modern economics. As Dekker describes, Tinbergen is best situated on the break between 19th-century political economy and modern (neoclassical) economics. On the one hand, Tinbergen was early on an adherent of the marginalist, like Jevons, Walras, and Menger, who laid the foundations for post-war economics. Tellingly, Tinbergen's first articles were on utility curves when his political outlook was still strongly influenced by Marx. On the other hand, Tinbergen was, with regard to the goal of economics, much closer to 19th-century political economy. He shared with the German Historical School of Schmoller, Brentano, and Sombart, and the Austrian School of

Schumpeter, Mises, and Hayek, an interest in the relationship between political order, law, and economics (11, 159).

Another major figure that Dekker mentions in this context is Max Weber. As Dekker suggests, Tinbergen can be seen as “the ideal Weberian scientist” (205). Like Tinbergen, Weber was manoeuvring between the aims and methods of the Historical School and marginalism. Also, Tinbergen adhered to Weber’s prescription of the neutral scientist, abstaining from any value commitments in his scientific work. That latter point might be surprising, as Tinbergen was very much also a politician with strong personal convictions. Moreover, his work as scientific expert sought to combine these professions of the scientist and politician. How could Tinbergen be both the ideal Weberian scientist and still combine politics with science?

Dekker argues that Tinbergen escapes this paradox by posing the economy as a separate sphere (206). Tinbergen imagined himself to be a social engineer that could manage the (almost mechanical system of) the economy on the basis of the inputs from politicians. But he refrained from saying anything about the desirability of those inputs. And indeed when looking at his publication of the first macroeconomic model of the Dutch economy, it is clear that Tinbergen takes a neutral position with regards to many policy proposals circulating at the time for counteracting the recession (Tinbergen 1936). Yet, I would advance a different reading of Tinbergen’s mixing of science and politics. Like Dekker, I see Tinbergen as a scientist adhering to the Weberian ideal. However, I think that Weber’s distinction between the vocations (Beruf) of the scientist and politician is less clear cut than often assumed. Weber allowed a role for values in science. These were, however, to be internalised, forming the personal creed of the scientist, not the shared values that constitute a community or political movement. As I will argue, Tinbergen found a way for his personal convictions, as a scientist, to play a role in his politics without violating the Weberian dictum.

In support of my interpretation, I will place Weber’s writings on the vocations of the scientist and politicians in their historical context and connect this context to the interwar political and cultural environment in the Netherlands in which Tinbergen made his first important contributions as an economic planner. My intent is to use Weber’s lectures on the vocations of the scholar and the politician as a lens through which multiple themes in Tinbergen’s life and work can be viewed. This will allow me
to highlight and connect aspects of Tinbergen’s thought that otherwise remain obscure.

It is not my intention to reduce Tinbergen’s life and work to the philosophy of Weber. Rather the aim is to explore the philosophical questions that are entangled in Tinbergen’s thought. Dekker himself connects throughout the book Tinbergen’s intellectual development to larger issues in the philosophy of economics. For example, to the issues of how economic expertise relates to public economic knowledge and liberal democracy (249–258, 393–395). In that sense, my commentary is not intended as a critique of Dekker’s work but rather as a continuation of his philosophical reflections and an exploration of what other readings are possible.

II. THE HISTORICAL BACKGROUND OF WEBER’S VOCATION LECTURES

In 1919, Max Weber delivered two famous lectures in Munich, which can be summarised in one sentence: scientists should refrain from pursuing a higher (political) purpose, for instance a vision of the good life or a political creed, while politicians should refrain from doing science (Weber 2020a; see also 2013). Weber’s image of the scientist as politically neutral would become dominant after the Second World War, but it was, at that time, still highly contested. It would not be overstating the case to say that Weber deliberately provoked his audience with his comments (Der- man 2012, 48). Therefore, it is worthwhile to give a brief background as to what led Weber to make his famous defence of the separation of science and politics.

Weber was part of the tradition of the German Historical School in Economics and was a member of the Verein für Socialpolitik founded by Gustav Schmoller, Gustav Wagner, and Lujo Brentano. The Verein and Historical School were famous for their social commitments, strongly advocating for social reforms in the late 19th century. At the same time, as is well documented, Weber felt attracted to the main rivals of the Historical School, the Austrian School in Economics, headed by Carl Menger (Maas 2014, 34; Kolev 2018; Callison 2022). Menger did not only disagree with the Historical School in terms of methodology, favouring a stricter conception of economics as concerned with means-ends rationalisations of individual actors, but he also disagreed with the political activism of the Verein für Socialpolitik (Grimmer-Solem 2003, 249–267; Klooster 2022). Menger, who had a strong conservative outlook as compared to the liberal-progressive inclined Schmoller, Wagner, and Brentano, not only
denounced the social reforms advocated by the Verein but went further by arguing that economics should refrain from direct involvement in politics if it wanted to be scientific.

Connected to the methodological and political skirmishes of the Austrian School and Historical School was also a question of the national and international focus of economics. The Historical School propagated the research of economics in a national and comparative context. Each national economy developed differently according to national customs and laws, even if universal developments could be discerned in a comparative framework (for example, industrialisation, or the emergence of the proletariat). The Austrian School, in comparison, had a more universal outlook, focussing on economizing behaviour regardless of national context. Or as Dekker has suggested in his earlier work, they focussed on civilisation rather than nation, that is, the level of culture and morality shared by Western peoples beyond the confinement of ethnicity or nationality (Dekker 2016).

It was this clash between a nationalist and universal outlook that would mark interwar scholarly thought on the relation between science and politics. Weber delivered his famous vocation lectures against the background of a major change in the identity of the Western sciences. Science before 1914 was conceived of as a nationalistic affair. Although international scientific congresses were an important part of knowledge exchange, these congresses were conceived of as a meeting of nations, similar to a diplomatic conference (Somsen 2016, 2021). In the nationalistic conception of science, the political commitments of the scientists had been relatively straightforward. In this ideal—probably best embodied by the Wilhelmine era of state-funded sciences—scientific breakthroughs were victories of the nation, and science contributed to the strength of a nation (Wise 2018). Each scientist represented their own specific nation and its interests.

By the 1900s, however, this conception of science started to shift. Rising tensions and animosities between the European powers made the collaborative efforts between nations increasingly difficult. And with the outbreak of the First World War, the nationalistic identity of science came to a dramatic end. After the War, many of the scientific community feared that science in service of nationalism had strongly contributed to the war effort, and was consequently responsible for the many horrors committed by new war technologies, such as mustard gas (Somsen 2016).
In the demise of the national identity of science a new scientific persona of the international scholar was born (Daston and Sibum 2003). This entailed a commitment to the international scientific community rather than a national one. This international outlook rhymed well with another international movement emerging at the beginning of the 20th century, the peace movement. This international commitment to peace formed a strong antidote to science in the service of war technology. Unsurprisingly, internationally oriented scientists, like J.D. Bernal and Joseph Needham, combined an international outlook with a strong commitment to peace (Hobsbawm 2006). As Dekker shows, it was this international conception of the scientist that, together with the international peace movement, instilled a strong international outlook on the young Tinbergen, clearly displayed in his convictions that only international collaboration could solve the economy’s problems and that international solutions required international, rather than national, institutions (Dekker 2021, 273).

The new international persona of the scholar raised one important political question, of which Weber was acutely aware, namely, to what kind of politics was this international science aligned? Weber was a firm nationalist, albeit a liberal one. In the chaos ensuing Germany’s defeat in World War I, he urged his student to fight for the German nation, notoriously arguing that Germany should take the territories lost to Poland back (Palonen 2001). Still, he understood modernity as a fracturing force (which I will explain in detail below), shattering any overreaching values, ideologies, or culture that could form the basis for the legitimacy of the nation state. Modernity had caused a plurality of values, and the issue seemed to concern which values science should adhere to in lieu of any natural overreaching national-cultural one. The new international persona of the scientist invited scholars to seek new overreaching ideologies to guide the scientific endeavour. This led figures like J.D. Bernal, Joseph Needham, Dirk Struik, and Otto Neurath to argue that this new international politics, to which science would be a service, could only be socialism (Alberts 1994; Somsen 2008; Sandner 2009). In contrast, Weber gave a different answer. He argued that scientists in modern times should refrain from any political ideology or commitment (Weber 2020c, 22–23).

This led Weber to the formulation of his famous thesis on the value-neutrality of science. For Weber, the increased rationalisation of society had led to a divergence between the types of questions science could pursue and the questions concerning the meaning of life. Science could tell humankind how to control its environment, Weber argued in Science as
*Vocation*, but not to what ends this newfound control should be put. Anyone who thought differently was misleading themselves. Such weariness of overreaching goals was also on display in his other Munich lecture, *Politics as Vocation*. There, Weber, as a liberal, warned against the idea that the state and society should commit to one particular worldview. For Weber, modern societies were plural, and it was up to the individual to choose among “the multiple gods” which ones they wished to serve (Weber 2020c, 36). Science, Weber stressed again, could not tell individuals which ends to pursue, only how. It was up to the individual unbound by science to decide what the ultimate ends were.

At first glance, Tinbergen seems to go against Weber’s dictum, as throughout his whole career he sought to combine politics and science. Moreover, Tinbergen put moral convictions at the centre of his economic research, seemingly disagreeing with Weber’s idea that modern sciences only revolved around increased instrumental control. His aim was “to make economics a moral science again”, as Marinus van der Goes van Naters put it (Dekker 2021, 215). At the same time, Tinbergen agreed with Weber that the rationalisation at the centre of modernity was a fragmenting force that made overreaching ideologies impossible. Moreover, as Dekker describes, in his famous work for the league of nations, Tinbergen’s research took on a very political neutral character, simply checking for the viability of different theories of business cycles (159–160). Did Tinbergen embody the Weberian scientist or the complete opposite?

Part of the answer lies in the fact that for Weber values did ultimately play a crucial role in science (Shapin 2019). Not as the goal, nor as something to be derived from the scientific endeavour, but in the form of the personal commitment scientists had to show towards their scientific work (Weber 2020c, 24). The academic world was harsh, Weber warned the Munich students, and if one went into science with the expectation to find wealth, political power, or the meaning of life, disappointment was inevitable. Instead, the scientist had to internalise their hopes for scientific endeavour. As no rewards of a scientific career were to be expected, it was up to the personal commitments of the scientist to persist. If one wished to commit to the “impersonal gods” of the sciences, one could decide to do so (Weber 2020c, 32). The most important point was to be true to one’s commitments.
III. PERSONALISM

One of the central focuses of Dekker's biography of Tinbergen is his adherence to personal moral convictions. Although Dekker does not use the term ‘personalism’ to describe Tinbergen’s worldview—the notion perhaps too vaguely associated with the works of Henri Bergson and in the Dutch context with the theologist and Labour Party member Willem Banning—I would argue that the notion encapsulates quite well how Tinbergen combined politics and science; this strongly resonates with what Weber called for in the personal commitments of the scientist and the politician. Personal character was at least central to how Tinbergen balanced his public commitments as a scientist and political advisor.

As Dekker argues, already from his teenage years, Tinbergen engaged in politics out of a personal calling, fostered by the religious remonstrant morals he grew up with. In Dekker's narrative, Tinbergen experienced his engagement with science and politics almost as a task given by God. Citing Matthew 25:15 “to every man according to his ability”, Tinbergen understood this task as his responsibility to use his talents to the best of his ability for all of mankind (92). The ordinance of one's profession was thus based on what was given by God and the personal commitment to these dispositions. In Weber's terms, it was a personal choice to act under the ultimate gods of science.

Tinbergen's abandonment of Marxist socialism, in which the collectivisation of the means of production stood central, in favour of a more community and spiritual-based socialism can be described as personalist. As Dekker recounts, Tinbergen was in the 1920s deeply involved in the youth movement of the Social Democratic Worker's Party (Arbieders Jeugd Centrale, AJC) (44). The AJC professed a form of socialism based on the creation of a new moral consciousness of the worker. It promoted virtues of frugality and abstinence as well as the cultivation of new sensibilities. Its members were encouraged to abstain from liquor and smoking, make long hikes, and partake in creative activities such as dancing and singing. The goal was not to give the working classes ownership over their means of production but to create a new kind of human. A human being morally prepared to take up the challenges of the modern world. The ultimate aims of socialism were to create a new community through the cultivation of one's personality.

The spiritual inclinations of the AJC seems at odds with Weber's rejection of new forms of spiritualism or vitalism (Weber 2020c, 38). Still, the move toward the cultivation of personal virtues is in line with his idea
of science as vocation. This should be read in conjunction with his views on rationalisation and modernity. Weber is famous for his Brutus-faced confrontation of modernity: one cannot stop rationalisation, one cannot go back to the enchanted world of yesterday, and one cannot re-spiritualise the earth (Derman 2012, 122). Despite Weber’s conviction, many interwar scholars sought to synthesize the rationalised world with a new sense of community. Jan Goudriaan spoke, for example, of the spirit of democracy consisting of “something soft and tender, the love of humanity” and “something steel-hard […] the piercing intellect”, and that “the combination of these two qualities […] is the first necessity for the betterment of this society” (quoted in Boumans 1989, 230).

Such sentiments were more broadly shared in the Dutch context. As David Baneke describes, most prominent among them were the scholars associated with the journal Synthesis, who, true to their name, sought the synthesis of the hard calculative mind of science with the thoughtful and feeling heart of culture (Baneke 2008). One of its most prominent adherents was the engineer Isaäc Pieter de Vooys, who argued for the poetic understanding of reality, a combination of the arts and sciences, which allowed engineers to “think creatively under scientific control” (quoted in Baneke 2008, 108–109). The manner in which the Synthesis movement thought to achieve this reconciliation between creative thinking and rational control was by using science for social ends, or to be more specific, to use science to foster a new community spirit. Clearly, the Synthesis movement went against Weber’s dictum of separating politics from science, however, as I will argue below, their personalist interpretation of how science and politics should be combined provided for Tinbergen a model of how to be a neutral scientist while at the same time being a politically engaged expert.

Dekker only briefly focuses on Tinbergen’s involvement in the Synthesis movement—Tinbergen wrote a couple of entries to a Synthesis encyclopaedia on economics—but his personalist conviction does match the general outlook of the movement (81). For example, Tinbergen’s involvement in the design of the Plan of Labour in 1934 can be understood in light of the search for a new community spirit. Although Tinbergen’s task was to provide the scientific underpinnings of the Plan, the aim of the Plan was not intended as a policy document to be simply implemented by the government. Rather, it aimed to inspire a political movement, to show that an alternative to the current politics was readily available. Through festivities and parades, the Plan was intended to foster a new political
community, in which the people could join in the jubilance of a more socialist future. Tinbergen’s contribution to the plan (in the form of the first macroeconomic model of the Dutch economy) was scientific research in service of a communal spirit, very much in line with the aims of the Synthesis movement as professed by De Vooys.

IV. TECHNOCRACY

De Vooys was a proponent of technocracy. One of the reductions that Dekker seeks to dispel is the one-sided image of Tinbergen as a technocrat. According to Dekker, Tinbergen’s motivations in politics were foremost inspired by personal values and not by any scientific worldview (105–106). However, the notion of ‘technocracy’ underwent a shift in meaning after the Second World War and we should be wary to not project our contemporary meaning of the term back into the past. For De Vooys, ‘technocracy’ did not so much denote the rule of scientists as justified by their expertise, but rather the use of expert knowledge to enhance their political leadership qualities. In the former sense, it might be justified to call Tinbergen a technocrat.

To understand this notion of technocracy, it is informative to go back to Weber’s lecture on the vocation of politics. Weber was infamously a proponent of a charismatic leader that could navigate modern parliamentary politics between the Scylla of bureaucracy and the Charybdis of ‘horse-trading’ interest group politics. The basis of such good leadership was character, more specific, a personal sense of responsibility (Verantwortungsethik), the ability to act pragmatically in dangerous situations, without shifting the responsibility for the outcome of one’s acts elsewhere (Weber 2020b, 96). This form of ethics was in contrast to a principle-based form of leadership (Gesinnungsethik), where absolute convictions would dictate political action.

In light of Weber’s description of the ideal leader, one can easily categorize Tinbergen’s politics as a form of Verantwortungsethik. As Dekker rightly points out, Tinbergen was never the hopeless utopian dreamer

2 Through a discussion of a letter exchange between Tinbergen and his fellow socialist engineer Ed van Cleeff, Dekker stresses the illiberal character of the Plan of Labour propaganda (118–123). In contrast, I would like to stress the anti-fascist goal of the Plan of creating a popular front against fascism. The Plan marks the shift in focus of the Social Democratic Worker’s Party from the working classes to the people in general. See Kayzel (2021, 120–121).

3 Later associations with the ‘Führerprinzip’, together with his nationalists convictions, has strongly tainted Weber’s advocacy for strong leaders (see Löwith 1981; Mommsen 1984).
Although he was a very principled man in his private life, priding himself on personal frugality and never owning a car, Tinbergen was mostly a pragmatist when it came to politics. He was always aware of the concrete restrictions of the political landscape in which he was operating. Dekker seems to imply that he was too pragmatic, as he was willing to cooperate with non-democratic regimes in order to realise his own economic ideas, as was the case with the consulting work he did for the regime in Turkey (334–335). How then did Tinbergen integrate his strong moral convictions with his pragmatism? The answer was that in Tinbergen’s conception, the ideal leader would lead by example. Showing that his ideal economic order was possible, indeed viable, even if global politics were far removed from this ideal, was the first step in changing the minds and hearts of the people and national leaders (361). In other words, he showed how a responsible leader could make the best of their circumstances and could inspire a new community spirit. This was a very personalist idea of good leadership, in which the creation of new communities stood central.

Tinbergen’s own leadership by example was strongly based on his own technical abilities. A good example is his book *International Economic Integration* (1954), which presents a model of the decision-structure of the world economy. The aim of this model was not to be a representation of the global economy. Nor was it a blueprint to be directly implemented, as Tinbergen realised the model was too ambitious. Rather it was a method to think through the major issue of international trade and the relations between industrialised economies and developing economies. It had to inspire the national leaders to think of economic issues beyond the national economies and to take steps in the direction of more international cooperation. His hope was that the European Economic Community (founded three years after the publication of Tinbergen’s *International Economic Integration*) could embody the ideal that was theoretically spelled out in his theoretical work. True to the Synthesis ideal, Tinbergen’s theoretical writings attempted to inspire new international communities through the clarity of scientific reasoning. The technical skill to separate realistic options from mere wishful thinking was indispensable. In that sense, Tinbergen was a technocrat in De Voogs’ definition of the term: a political leader that utilised scientific knowledge to boost their

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4 This is explored by Dekker in more detail in some additional articles on Tinbergen and the international economic order (Dekker 2019, 2020).
own ability as a leader. In Tinbergen’s case, this meant using technical knowledge to lead by example; or, to show examples from scientific knowledge in one’s leadership.

V. RATIONALISATION AND HISTORICAL DETERMINISM

One of the puzzles that Dekker presents to his readers is how the determinism of rationalisation was to be reconciled with Tinbergen’s person-alist-voluntarist politics (13). As Dekker emphasises, Tinbergen remained wedded to the conception of modernity as an unstoppable force of progress and rationalisation all his life. At the same time, Tinbergen adhered to Goudriaan’s dictum, that “the world will become what we make of it” (quoted in Dekker 2021, 105). I would argue that analysing Weber’s thought again might shed light on how Tinbergen overcame this paradox. Weber’s answer to this problem was straightforward enough. Faced with an impersonal and cold rationalised world (a disenchanted world), the scientist and politicians were forced to internalise the wishes and hopes they had for a meaningful existence on earth, converting them into personal commitments toward the ends of their own choosing. Many scholars after Weber, however, thought that the answer was to be found in giving science substantive, normative goals, like the Dutch Synthesis movement proposed.

According to Dekker, Tinbergen strongly associated the concept of rationalisation with the emergence of scientific management (Taylorism). Scientific management meant the further division of labour into the most possible elementary task which would be controllable by the manager of the firm, resulting in a more ordered and efficient process. With business becoming more ordered, rationality would take a more central place in everyday life (104). Optimistically, Tinbergen (under the pseudonym Jan Dirks) argued in his very first article for De Socialistische Gids (the Socialist Guide) that competition was starting to become less and less important regarding the question of production (Dirks 1929). The economy as a whole became organised on the basis of rational principles, resulting in trusts, cartels, and monopolies. Like many other planners of the period, Tinbergen saw the increased ordering of the economy as a development that would make a (centrally) planned economy the logical next step. The question was whether this planned economy was to the benefit of the capitalist or that of the worker.

5 This form of technocracy is also implied by Dekker, I would argue (see 106).
Tinbergen realised that a planned economy did not necessarily entail a socialist economy. Moreover, he was not blind to the negative consequences of a rationalised economy. Rationalisation alienated workers from their product and made exploitation by capitalists easier. Already in his very early work, he attempted to determine how the utility gained from increased efficiency would be distributed amongst the workers (Tinbergen 1931). Did increased productivity leads to more happiness? He worried about the psychological effects of lay-offs due to increased efficiency on the fired worker. Even if the worker was to find new work, the negative experience of being fired would undermine the willingness of the labourer to work in the long term.

Rationalisation was an inherent part of modernity but with dire consequences. How could one counter the alienation of the worker without denouncing rationalisation tout court? The solution was that the future was determined by rationalisation but that rationalisation did not determine one possible outcome. Instead different rationalised futures where possible. Modernity could still be steered towards a better outcome for the workers, and it was the task of scientists to steer modernity. Although Weber’s definition of rationalisation differed from the one associated with Taylorism (Weber’s definition was much wider in scope and firmly associated with a specific Western form of rationalisation), it shared its emphasis on instrumental control. Weber argued that scientists had to accept this instrumental form of rationality. At the same time, it was up to the modern human to decide to what ends instrumental reason had to be utilised. In present-day terminology, by applying a strict distinction between epistemic values which guided rational science and non-epistemic values which provided humankind with ideals for ethical and political life, Weber’s definition of rationality was not wholly deterministic. As long as non-epistemic values could determine the goals of science, politics could decide on the course of the future, even if that future was always rationalistic in nature.

This conception of rationalisation and modernity as both inescapable but not deterministic resonated with other philosophical accounts of science and modernity of the time. Famously, Max Horkheimer and Theodor Adorno would later argue that rationality was not a neutral mean but rather determined what ends humankind would pursue (Horkheimer and Adorno 2002). They worried that rationalisation would not allow for a plurality of values, as Weber had argued, but would subsume political life under the stringent rules of scientific rationality. In such a scenario
rationalisation would indeed become deterministic. How to escape this fate? For the Synthesis movement the answer was to put the non-epistemic values front and centre again in science, dissolving the strict distinction between science and politics that Weber had drawn.

One of the models for such a combination of non-epistemic values with scientific practices was Edmund Husserl’s *The Crisis of the European Sciences* (1970). In this work, Husserl diagnosed a crisis of science stemming from the fact that scientific rationality, that is, a science dependent on formal axioms and abstract rules, had lost any connection with the ‘life-world’. The original question that had instigated scientific endeavour was lost in the world of abstract logic that science employed. If rationalisation was to be employed towards the benefit of the worker, it was imperative to combine rational science with non-instrumental politics in the form of finding a community spirit (van Lunteren and Hollestelle 2013). The Synthesis movement followed a similar recipe, making the search for community spirit central to the scientific endeavour. Tinbergen’s own hope that modernity could be steered towards a socialist future should be read in this context.

The search for a new community that the Synthesis movement and Tinbergen adhered to seems antithetical to what Weber was advocating. Still, Tinbergen found a way to combine this search with his commitments to value-neutral science in a Weberian vein. Embedding scientific rationalisation in a broader cultural sense of community meant for Tinbergen that community spirit should be internalised as a personal conviction. Scientists could display this spirit through their political leadership. It was, in other words, a personalist solution, in which the personal commitments of the scientist were the mediator between the cold impersonal rational world and the human value-based community.

It is in this light, I would argue, that Tinbergen’s technocracy should be understood. One could not, under Weberian preconditions, use science to arrive at communal values, but one could use science to boost a sense of community that stemmed from personal convictions through leadership. Morality should enter science neither as an ultimate goal, nor as something that science dictates, but as the personal morals of the scientist in the pursuit of science and politics. Scientific practice was, in that sense, not value-laden, but the reasons to pursue science were. The *scholarly persona* of the scientist could link the value-neutral products of science to the social aims of science without mixing epistemic and non-epistemic values in scientific or political practice. Rationalisation still
dictated the development of science regardless of pre-established values, but its fruits could be utilised for political ends, which in Tinbergen’s case was to inspire a community.

Present-day philosophers of science have, after Weber, argued that the value-neutral ideal of science is not possible and that non-epistemic values always play a role in scientific practices (Longino 2004, Douglas 2009, Harding 2015). I do not want to suggest that Tinbergen’s personalist view provides the answer to how the value-neutral ideal is still attainable in light of these later criticisms. Still, Tinbergen’s thought provides a historical instance of how scientists have attempted to resolve this tension between politics and science, which has so far received little to no attention.  

VI. CONCLUSION

As the second half of the title of Dekker’s biography implies, the story of Tinbergen’s life is the story of how economic expertise arose. Dekker cites Timothy Mitchell’s argument that economic expertise could only gain prominence once the economy was established as a separate sphere (Mitchell 2002; Dekker 2021, 207). Dekker argues that Tinbergen already positioned such an autonomous sphere in his macroeconomic models of the 1930s. Although I do think that Mitchell’s argument rings true and the rise of the economy as an autonomous sphere is a crucial part of the story Dekker tries to tell, I would argue that a different reading of Tinbergen’s expertise (especially in the interwar period) is possible. By focussing on the theories of Max Weber and the Weberian elements in Tinbergen’s thought, I have attempted to show that Tinbergen’s interwar expertise, although presented as neutral and focussed on the economic sphere, was embedded in broader cultural and political concerns. The aims of his expertise went beyond the economy and dealt with the fate of the modern world in total.

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6 For an interesting comparison on how W.E.B. Du Bois dealt with similar issues concerning the value-neutral ideal of science, see Bright (2018).


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Ambiguity of Superiority and Authority: An Analysis of the Keynes-Tinbergen Debate

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I. INTRODUCTION
The Keynes-Tinbergen debate is perhaps one of the most formative debates in modern economics. Tinbergen’s (1939) report on business cycle theories for the League of Nations was criticized internally by Dennis Robertson, Gottfried Haberler, and Ragnar Frisch, as well as externally. The most famous, and scathing, critique came from John Maynard Keynes (1939). Keynes's critique of the report focused primarily on the technical aspects, but various commentators have placed the debate within larger contexts. Marcel Boumans (2019) argues that Keynes and Tinbergen were discussing epistemological issues as well as technical. Erwin Dekker (2021, 186–187) places the critique within a larger debate on the purpose and politics of the League of Nations. Keynes was disappointed that Tinbergen's report was politically neutral. Conversely, Tinbergen saw the neutrality as a feature of the report. The controversy has an additional angle that is hinted at by Dekker but deserves to be made explicit: the Keynes-Tinbergen debate represents a fundamental difference on the role and authority of experts in the policy-making process. Both Tinbergen and Keynes saw an essential role for the government to plan the economy. They differed in how the expert serves such planning. Underlying the debate over politics and statistical methods was a fundamental difference in opinion about what authority experts had in their role as experts as well as how dominant experts should be in the political process of planning.

1 'Planning' as used by both Tinbergen and Keynes encompassed a broader notion than the 1930’s socialist notion of planning revolving around output. For Tinbergen and Keynes, the goal of planning was to provide stability, rather than choose various output levels. See Dekker (2021, chap. 10; in particular pages 227-234).

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The political nature of the expert was different for Tinbergen and Keynes. For Tinbergen, the expert was technical and neutral. Planning organizations like the Central Planning Bureau (CPB) were to be “a more anonymous type of expertise that depended not on the personal political and economic qualities of the head of the bureau, but rather on the methods employed by the CPB” (Dekker 2021, 230). Expert advisors were equals to other participants in the decision-making process (232). Experts would “not aim for some optimal policy that would have been scientifically satisfactory but opted for an approach that focused on the feasibility of particular goals”; goals that were determined by lawmakers rather than experts (235). The expert was to tell lawmakers what they could realistically expect or enact, but not tell them what their goals ought to be. The expert and their advice was to be positive, neutral, and value-free; the expert’s authority did not convey any special considerations beyond the technical.

For Tinbergen, the expert was a passive advisor when it came to choosing policy goals. Technical expertise was important, but what mattered more was the need to analyze the current situation as simply as possible to better understand what was possible or desirable (223). To that end, Tinbergen sought to develop an optimal decision-making structure that included choosing policy goals (225). Policy goals would ultimately remain in the hands of lawmakers and the expert’s job was to determine what policies were feasible (228–229).

For Keynes, the expert was not a passive, co-equal advisor in a team effort, but rather an intellectual superior to the non-expert. Keynes saw the authority of the expert extending over other considerations beyond just the expert’s narrow silo due to his expertise and education. The expert not only explained what levers lawmakers could pull, but what their goals ought to be. The economist in particular was better situated to deal with the “general problem of organisation of resources as distinct from the particular problems of production and distribution which are the province of the individual business technician and engineer” (Keynes 1932; emphasis in original). For Tinbergen, the expert was different from the policymaker and both were placed in a larger decision-making institution. For Keynes, the expert should be actively and dominantly involved in the policymaking process, helping to determine what policy ought to be. The expert’s personal, political, and moral values should infuse their advice. The expert should not remain neutral. Thus, I follow Ross B. Emmett (2017) who argues that Keynes’s turn from laissez-faire to managed
economies was not about the adoption of technocratic methods chosen in a democratic format, but rather more directed by a highly educated elite few who could set the agenda for government action (Keynes 1927). Keynes saw the expert playing a different role in planning than Tinbergen: that of policymaker, enlightened thinker, and partisan politician (Clarke 1988, 94). For example, in responding to an article written by former French Minister of Foreign Affairs Gabriel Hanotaux for the Guardian series *Reconstruction in Europe*, Keynes declared with rhetorical flourish:

No! The economist is not king; quite true. But he ought to be! He is a better and wiser governor than the general or the diplomatist or the oratorical lawyer. In the modern overpopulated world, which can only live at all by nice adjustments, he is not only useful but necessary. (Keynes 1978b, 432)

This statement is not meant to be taken literally, but rather is meant to be indicative of Keynes's ideal of a politically active and encompassing expert. The ordinary politicians “have ears but no eyes” (427) and thus must be led to do what is necessary. Individuals could be directed by “animal spirits” and predictable irrationalities. Thus, it fell to a highly-educated elite to guide everyone else and protect the “crust of civilization” (Toye 2000, 140; Emmett 2017, 75). Keynes, anticipating Thaler (1992), treated economics as a prescriptive science as much as a positive one.

Initially, Keynes felt that persuasion and changing public opinion would be necessary (Johnson 1974, 100; Keynes 1978a, 35). Over time, however, Keynes “came to think of himself more as the economic scientist, the technician, the mechanic who is called in to fix the machine when the self-starter is broken” (Johnson 1974, 100). Persuasion may still be desirable, but deference to the expert was preferable. After all, Keynes thought “political intuition and judgment, of the type he possessed” was needed to inform courses of action (Dekker 2021, 186). He believed that political parties needed to be “sufficiently autocratic” in order to avoid “ill-advised movements in the direction of democratising the details of the party platform” (Keynes 1978a, 295–296). Economic issues were not

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2 There is a subtle irony that ‘Hydraulic Keynesianism’ (Coddington 1976, 1265) has come to describe the use of neutral experts pulling levelers to chart an economy given Keynes's concerns about irrationality and judgment. Hydraulic Tinbergenism would be a more apt moniker.

3 Keynes explicitly rejected dictatorship, even by the technocrat. See his essay “The Economic Consequences of Mr. Churchill” (Keynes 1978c, 438–439).

4 Johnson (1974, 100) describes Keynes as one who “in his own opinion, he was always right”. Clarke (1988, 80) similarly discusses Keynes as seeing “his [political] aspirations
just economic, but political and moral as well (Keynes 1927, 1978a). The economist’s political and well-cultivated moral values had to inform their judgment. Since the economist was an expert, they should be educated to possess superior value judgments as well in terms of arts, politics, and social justice (Keynes 1978a, 303–305). The economist would be one of the most important groups of experts moving forward (Keynes 1932).

The political underpinnings of Keynes’s implicit theory of expertise seem to have driven his criticism of Tinbergen’s report to the League of Nations. Dekker (2021) argues that Keynes’s negative reaction to the Tinbergen report could be read as “a response to the type of activity the League of Nations sponsored” (186). That the Report was primarily of technical matters rather than any commentary on the political issues the League and its member nations faced was a disappointment to Keynes (Keynes 1939; Dekker 2021). Keynes’s perception of politicians as figures who argue over silly formulas knowing they are silly (Keynes 1978b, 212), “charlatans who manipulated the public with their propaganda” (Johnson 1974, 100), as well as people who needed to be led to the right conclusion (100), suggests that Keynes saw the neutrality of the report as a dereliction of Tinbergen’s duty as an advisor to provide the best possible course of action using proper judgment. The expert needed to be politically active, not merely passively giving advice or developing models policymakers would not know how to use.

Tinbergen and Keynes had differing understandings of the authority of experts. How far outside their discipline does the authority of the expert extend? Should experts expect nonexperts to defer to them or help decide what their goals ought to be? The Oxford English Dictionary defines authority both as, “power or right to give orders, make decisions, and enforce obedience; moral, legal, or political supremacy”, and “power to influence the opinions of others, esp. because of one’s recognized knowledge or scholarship; authoritative opinion; acknowledged expertise” (Oxford English Dictionary 2022a). The first definition indicates dominance whereas the second involves influence. Keynes seems to understand the authority of expertise through the first definition while Tinbergen understands the authority of expertise through the second definition. Keynes saw the authority of the expert extending into the right to make decisions or dominate the process—especially for “ill-understanding voters” (Keynes 1978a, 295)—given the experts supposed superiority

as the “true destiny of New Liberalism”. Keynes certainly seemed to have a high opinion of his opinions.
of values and insights. Tinbergen saw the authority of the expert as having the ability to influence the others in the decision-making process, but were not the dominant figure. Understanding the ambiguity of ‘authority’ will further our understanding of their disagreement, as well as enhance discussions of expertise in general.

To better analyze the ambiguity of authority in expertise, I will rely on Adam Smith’s discussions of superiority and authority in The Theory of Moral Sentiments ([1759] 1984). Smith has a dual concept of superiority in one’s relationships, one jural and one comparative (Diesel 2020). Authority for Smith was similarly ambiguous. Depending on what meaning of superiority one used, it implied a different meaning of authority.

II. JURAL AND COMPARATIVE SUPERIORITY

For Adam Smith, there are two jural relationships we engage in. ‘Jural’ refers to that which is “pertaining to natural or positive right, or to the doctrines of rights and obligations” (Black’s Law Dictionary, n.d.). The first of these jural relationships is the equal-equal relationship (Diesel 2020). The second is the superior-inferior relationship. Smith implies the distinction throughout The Theory of Moral Sentiments, though it is clearest in his section “Of Justice and Beneficence”. Smith opens the section with a discussion on beneficence as a virtue that “cannot be extorted by force” (Smith [1759] 1984, 78). To extort one to perform a beneficent act would “still be more improper” than the lack of beneficence (79). If a man did not give his benefactor his due, then “his benefactor would dishonour himself if he attempted by violence to constrain him to gratitude, and it would be impertinent for any third person, who was not the superior of either to intermeddle” (79; emphasis added). The italicized clause suggests there is another class of individual who could compel benefits or transfers.

A few paragraphs later, Smith makes the distinction between jural equals and jural superiors abundantly clear. First, he reiterates the point that “even the most ordinary degree of kindness or beneficence, however, cannot, among equals, be extorted by force” (80; emphasis added). But then he addresses the caveat he mentioned earlier: “A superior may […] oblige those under his jurisdiction to behave […] with a certain degree of propriety to one another” (81). Smith goes on to list various laws of “civilized nations” (81) that compel benefit, such as obliging parents to take care of children. Thus, we have two types of relationships between individuals: equal to equal, where benefit cannot be compelled, and superior
to inferior, where benefit can be compelled (Diesel 2020). The former are our relationships between each other *qua* individuals. The latter is our relationship to the sovereign.\(^5\) The jural superior has jural authority: the rightful and proper power to compel and coerce. The jural superior is the dominant member of the relationship, and the jural inferior's duty is, generally, to obey.

The authority of the jural superior to compel behavior is not limited to simply matters of services and transfers. Rather, the sovereign, is entrusted with the power not only of preserving the public peace by restraining injustice, but of promoting the prosperity of the commonwealth, by establishing good discipline, and by discouraging every sort of vice and impropriety; he may prescribe rules, therefore, which not only prohibit mutual injuries among fellow-citizens, but command mutual good offices to a certain degree. (Smith [1759] 1984, 80)

To this end, the sovereign must use infuse their decisions with value judgments of what constitutes ‘vice and impropriety’. In other words, the jural superior could overrule the value judgments of individuals under their authority and compel them to practice certain virtuous behaviors. For Smith, this jural power was limited; it had to be used with “the greatest delicacy” and executed “with propriety and judgment” (80). However, jural power did extend to various economic regulations. For example, though it is a “manifest violation of natural liberty”, Smith supported banning certain bank notes and building party-walls to prevent fires (Smith [1776] 1981, 324).

Smith uses ‘superior’ in a different sense as well. Within the equal-equal relationship, there can arise superiority. This form of superiority develops not from a position of power, but rather a judgment. One may be recognized as comparatively superior due to “excelling by some standard of judgment” (Diesel 2020, 111). Furthermore, individuals that continue to excel may become influencers for that particular area. For example, Smith held up Milton as an example of “sublime” poetry (Smith [1759] 1984, 123). Milton became a standard by which future poetry was judged, and his opinions carried great weight: “Thereby John Milton serves as a sort of authority on poetry” (Diesel 2020, 112).

\(^5\) I use ‘sovereign’ as Smith does to refer to whomever is making the political decisions. That could be a king, a town council, a legislature, etc. Thus, ‘sovereign’ refers to both the individual and the decision-making process, in much the same way ‘King in Parliament’ refers to the legal operation of the British government and not simply to the monarch (Dicey [1885] 1982, 3).
What is important to note is the comparative superiority of Milton does not carry with it jural authority; the comparative superior is not the dominant member of the relationship. Milton influenced (and influences) poetry, but he could not compel anyone to accept his opinions. Likewise, his expertise was circumscribed to just the area he was comparatively superior. Milton’s authority was limited to poetry and did not extend to, say, mathematics. This is not to say that the expert should not have opinions on something outside their domain. Rather, just because Milton has superior knowledge and judgment on poetry does not imply that he also has superior knowledge and judgment on mathematics. Milton could have and provide opinions outside of poetry, but he could not compel anyone to accept them. Milton was a comparative superior, not a jural superior. His comparative superiority did not imply jural superiority. Likewise, his comparative authority (authority that influences) does not imply jural authority (authority that dominates).

Additionally, comparative superiority did not imply superior values. For example, poets and other “fine writers” were often petty in the eyes of Smith (Smith [1759] 1984, 125–126). They would often “depress the reputation” of other writers they disliked, treat one another with “disrespectful kindness” or prevent new members from arriving on the scene (125). One’s superiority in values was not connected to one’s expertise. A comparative superior could possess superior values, but they do not have superior values simply because of their comparative superiority in a given field.

Adam Smith had a polysemous understanding of superiority and that influenced the kind of authority the superior had. There is jural superiority which conveys jural (compulsive) authority. Then there is comparative superiority which conveys comparative (influential) authority. However, these polysemous characteristics get mixed up when discussing expertise.

III. COMPARATIVE AND JURAL EXPERT SUPERIORITY
Experts tend to be defined as superior, at least in terms of the knowledge they possess. For example, Socrates argued for his own superiority in education, and those of generals in military matters, due to their wisdom (Xenophon 2013). Recent literature classifies experts as those with “inherent information advantages” (Radzveick and Moore 2011, 103) or “superior skills and intuition” (Kang and Kim 2022, 577). Koppl (2018), who provides an extensive literature review on experts and expertise, notes that “experts are usually defined by their expertise” (37), a definition
which is supported by the *Oxford English Dictionary*: “A person regarded or consulted as an authority on account of special skill, training, or knowledge; a specialist” (*Oxford English Dictionary* 2022b).

Given the ambiguous nature of superiority, what ‘superiority’ means in expertise is a source of confusion. In a broad sense, experts are generally perceived comparatively superior to non-experts. By some standard of judgment, the expert is seen as superior. That standard of judgment may be from a degree-granting institution (such as a university bestowing a degree on an individual), the relative prominence of the individual (Bikhchandani, Hirshleifer, and Welch 1992; Murphy 2022), or how successful they are perceived to be (Smith [1759] 1984). Regardless of how the judgment is formed, the expert is ‘superior’ in the comparative sense and thus possesses comparative authority as well. Tinbergen treated experts as superior in the comparative sense in their role as experts. Experts were to be politically neutral and offer strictly technical advice; value judgments outside of their area of expertise were neither warranted nor desirable when advising. The expert’s job was to help determine if a chosen course of action was feasible, or how to make it so (Dekker 2021, 235).

The evidence of their superiority came from their technical prowess in their field. To the extent the expert was a jural superior for Tinbergen, it was because they were part of a larger jural superior body; their expertise in and of itself did not convey them jural superiority. The relationship to the non-expert remains an equal-equal relationship.

Some take the comparative superiority of experts to imply, or at least should imply, jural superiority as well. The dominance of experts in the relationship assumed by theorists is rarely explicit, but often implicit. The expert, by virtue of the fact they are an authority in the field, possess superior values and insights to non-experts. Thus, the expert ought to be actively involved in setting policy and infuse their values into their decisions for the non-expert. In an extreme sense, the expert is not part of the decision-making process; they *are* the decision-maker and the non-expert must simply follow their advice. The jural superiority of experts mindset is akin to what Koppl (2018, 189–192) refers to as the “rule by experts”. Non-experts have no recourse to push back against the advice/decisions of the expert. Keynes’s argument that experts have the proper right to choose the virtues aimed at by policy goals—specifically “social justice and social stability” (Keynes 1978a, 305)—is an example of experts possessing jural superiority (and thus jural authority) by virtue of their
comparative superiority, although Keynes did not believe this superiority to be absolute.

Table 1 represents the taxonomy between superiority and authority in expertise I have developed thus far.

Box I represents a conception of expertise that grants one superiority in technical and value judgments and that superiority carries with it jural dominance. I argue that Keynes’s view of expertise belongs in Box I given the expert’s active role in determining policy and their duty to promote certain partisan political and social values regardless of what the non-experts may wish; that the expert is the dominant member in the relationship.

Box II represents a conception of expertise where the expert has technical superiority, but that does not imply superior value judgments as well. However, the expert is still the dominant member of the relationship. Box III has a conception of expertise granting technical prowess, but not superior values and the expert is not dominant in the relationship. Tinbergen, with his conception of the expert as a technical specialist belongs in Box III. Many equal-equal expert consultations, such as doctor visits, fall into Box III; experts may offer an opinion, but it is up to the non-expert to judge the opinion for themselves. Finally, Box IV represents a view of expertise that confers technical and value superiority, but not dominant jural authority. Many religions fall into this category where spiritual leaders have both technical expertise (for instance, how to conduct rituals) and superior values.

It is important to note that, with both Keynes and Tinbergen, the expert had a role as part of a jural superior entity: the sovereign. Neither saw the expert qua political advisor as a jural equal to the citizen. The key difference between Keynes and Tinbergen, and why they occupy different boxes, is the degree of dominance during the policy-making process. Tinbergen believed expert advisors were politically neutral equals to the other elements of the sovereign in the policy decision-making process and ought to be passive; taking certain political goals as given and working to establish policy that fits those goals. Political opinions were important socially, but did not serve a role for the advisor in his role. Keynes, on the other hand, often argued the expert possessed (or ought

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6 While it is important to note that Tinbergen did see a role for the expert within the larger sovereign body, his conception of the expert qua expert did not convey to him special jural authority. Thus why I place Tinbergen in Box III as opposed to Box II.
to possess) superior values and ought to be active in determining political goals, not passively taking policy goals as given. The expert as an advisor should be actively involved both in positive technical decisions and in normative value decisions (Fitzgibbons 1991).

In earlier writings, Keynes condemned the moral neutrality of the technical advisor; he saw the coming debates about the future of society “not round technical questions, where the arguments on either side are mainly economic, but round those which, for want of better words, may be called psychological or, perhaps, moral” (Keynes 1927, 50). Economic questions were inseparable from political questions (Keynes 1978a, 295). Technical questions could be important for smaller matters, but for economists to be successful in their “chief task of […] distinguish[ing] the Agenda of the Government from the Non-Agenda” (Keynes 1927, 40), a restatement of the moral and political character of economics was necessary (Emmett 2017).

For Keynes, solely technical analysis could run into issues of interpretation without some sort of moral/non-statistical framework to guide it:

To apply these [statistical] methods to material, analyzed in respect to the circumstances of its origin, and without reference to our general body of knowledge, merely on the basis of arithmetic and those characteristics of our material with which the methods of descriptive statistics are competent to deal, can only lead to error and delusion. (Keynes 1973, 419)

It is only with this expanded knowledge beyond the technical, those of superior moral virtues, that the expert could be truly effective in their role. Science required both technical prowess and properly calibrated judgment. Pure neutrality could not exist if the expert was to be effective in their role as advisor.

Keynes saw the role of the expert, and the economist in particular, as key for setting the ‘Agenda’ of the government (Keynes 1927). That task
was too important to be left to the politicians alone; they must be guided. For example, Keynes praised American President Franklin Roosevelt for letting him be guided by experts and his willingness to be open to new ideas (Keynes 1978c, 305–309). Indeed, Keynes idea of the worldly expert seems to have caused annoyance among his Treasury Department colleagues (Johnson 1974, 101).

Tinbergen, however, took political neutrality and passivity to heart. While he certainly had his political opinions, he did not let them influence his role in providing expert advice. His report to the League of Nations had no discussion of its political role: just statistical business cycle analysis. When the Netherlands was occupied by Germany in World War II, Tinbergen and the Central Bureau of Statistics cooperated with the occupying government (Dekker 2021, chap. 9). His ideas were often used by authoritarian leaders to further their own agendas (Dekker 2021). Tinbergen accepted awards from all types of governments and offered advice to any who would listen. The expert was much more passive and apolitical for Tinbergen. The expert was an important, but not dominant, figure in the Tinbergen conception of expertise.

IV. CONCLUSION

Understanding the difference between jural superiority and comparative superiority helps us understand the dynamic of the Keynes-Tinbergen debate over the League of Nations report more fully. The debate was not simply about technical methods studied by Tinbergen. By placing the debate within the framework of a larger political debate on the League of Nations, Dekker (2021) helps us better understand the context and Keynes’s seemingly over-the-top reaction to the report. But I argue the debate is a microcosm within an argument that has been raging for centuries over the ambiguity of the words ‘superior’ and ‘authority’: What does expert ‘superiority’ mean? What sort of ‘authority’ does that superiority convey? By using Diesel’s (2020) elaboration of Adam Smith’s discussion of superiority and social relationships, I believe we have a language that helps clarify the underlying debates on expertise. Without understanding the ambiguous nature of superiority and authority, participants will talk past each other. Dekker’s reorientation of the Keynes-

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7 It does not appear this was a willing collaboration, as Tinbergen struck a deal to try and keep some independence for the Central Bureau of Statistics from the German government and he was forced into hiding toward the end of the war.
Tinbergen debate around the League of Nations helps make the differing understandings of expertise for the two economists much clearer.

Indeed, the conversation surrounding Tinbergen’s League of Nations report seems to be two brilliant men talking past one another. While Keynes’s (1939) original critique was on both statistical and non-statistical methods, Tinbergen responds to the non-statistical questions in statistical terms, preferring to focus on statistical residuals rather than the matters of judgment Keynes was discussing (Tinbergen 1940). Boumans (2019, 284) notes “each world view was so different that they misunderstood each other profoundly”. Included in their worldviews were different conceptions of the expert and different understandings of the words ‘authority’ and ‘superiority’ as well as how politically active and dominant the expert should be. Both were operating on a fundamentally different conception of what authority and superiority the expert possesses in his role as an expert and advisor. These differing conceptions influenced the theories and methods each developed and continue to infuse policy discussions to this day.

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Jan Tinbergen’s Fallacy: Economic Expertise as an A-Political Endeavour

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I. A Story of Disconnections

Jan Tinbergen was a stubborn optimist. Yet Erwin Dekker’s (2021) biography of him exudes a feeling of melancholy. The reason for this apparent paradox lies in the fact that Tinbergen was an optimist by design. An inveterate planner and a believer in enlightened government, the rule of experts, and pragmatic solutions driven by high-flying idealism, Jan Tinbergen emerges from this book as a person stubbornly committed to making the world a better place, and yet somewhat unable to connect with it. In a sense, this is a dramatic proposition, as Dekker suggests by repeatedly discussing Tinbergen’s multiple dogged attempts at building bonds with others. Tinbergen did so from a cultural and moral perspective, with the mix of progressive Christian humanism and socialist ideas he developed in the socialist youth movement (AJC). Politically, he always privileged dialogue over ideological conflict. Finally, he showed this attitude also in his work as a civil servant, from his elaboration of the 1935 Plan of Labor in the Netherlands to his collaboration with India in the 1950s and 1960s. In other words, I am struck by Tinbergen’s consistent failed attempts at making human and cultural connections, and I wonder whether it was not actually his vision of planning and control that thwarted his efforts to engage more fully with an unpredictable and messy world.

One major achievement of this most informative intellectual biography is the evenhandedness with which Dekker shows Tinbergen’s tragic mix of aspirations to do well and poor results. Yet, if the reader, following Dekker, sees the immense gap between these two poles, Tinbergen arguably did not. Professionally, at the individual level, Tinbergen succeeded—his expertise was highly sought-after and he was awarded the first Nobel prize in economics in 1969. Dekker obviously follows the up-
ward professional trajectory of Tinbergen—a history of growing responsibilities, broadening intellectual influence, and increasing centrality in policy-making debates both nationally and internationally. But he also develops a second narrative that shows the problematic face of this history of professional success: a naïve faith in good sentiments and often empty rhetoric, coupled with top-down, even authoritarian, visions of technocratic rule and social and political engineering.

Consider how Tinbergen tried to shape his own cultural, moral, political, and public persona. For example, the socialist youth movement that he joined as a very young man (along with a roster of future Dutch social-democrat politicians) exalted the construction of socialism through the will and determination of the socialist vanguard to follow austere and spartan norms of life. Their love of primitivism and pagan rites was typical of those years and often characterized other modernist cultural movements as well. But the outright absence of any serious analysis of political economy and class relations relegated the movement to the naïve fringes of the socialist world. As Dekker notices, the AJC was frequently mocked by older members of the party for its utopianism and prescientific socialism (48–49). In fact, it was not only the older generation that found those choices quite infantile and out of touch with a reality in which Fascism and Nazism were seizing power through the exercise of widespread political violence and with the support of industrial interests. Many young European socialists in the 1920s and 1930s used explicitly political means to slow the advent of Nazi-fascism and to advance a socialist agenda: a far cry from the camping and dancing Dekker so effectively describes.

Indeed, Dekker shows that even as he matured, Tinbergen would continue to remain aloof from the necessity of taking a hard stand, even in the face of Nazi-fascism. Though he was an outspoken pacifist and before the war petitioned in favor of hosting refugees in the Netherlands, during the war Tinbergen maintained good relations with the Nazis, kept his prominent job at the Central Bureau of Statistics, and continued to publish extensively. In a 1944 book on the various national responses to the Great Depression, Tinbergen discussed Germany’s economic policies “without a single word about the massive mobilization and war industry of the 1930s” (201). As Dekker concludes, “Tinbergen increasingly started to decouple the economy from other domains”, in an attempt “to turn ‘the economy’ into a purely technical domain, quite distinct from other political or ideological domains” (205–206). Apparently, Tinbergen “believed that in order to solve the economic and social problems of the day, we
had to overcome ideological and political approaches, and focus on what worked” (206-207). But this was either terribly naïve or disingenuous, especially when dealing with the Nazis. It is possible, as Dekker has recently hypothesized, that Tinbergen pretended to believe that he could disregard ideological differences and discuss economic matters as if it was business as usual because of cowardice (Dekker 2022). In his book, Dekker rightfully avoids easy and superficial judgments, and it is not for the reader to become an armchair moralist. But one is allowed, I think, to observe the gulf that Tinbergen himself created between his own words and deeds.

II. Tinbergen the Technocrat

In the pages on the Nazi occupation of the Netherlands as well as in other pages, Dekker does an admirable job in showing the increasingly narrow parameters of a field of analysis and a specific methodology ever more impermeable to external inputs and influences (192–221). This process of delimitation was deeply interrelated to the transformations in the economic role of the state, on one side, and the role of the economists in government, academia, and public opinion, on the other. It is on this point that I am in disagreement with Dekker or, at least, I stress different elements of the analysis. In particular, I am unconvinced by Dekker’s attempt to treat Tinbergen’s passivity in the face of Nazi-fascism as distinct from the emergence of the figure of the ‘a-political’ expert. Dekker writes:

A focus on Tinbergen’s involvement in the war, the compromises he was forced to make, and the dubious choices he made quickly loses sight of the bigger story. That bigger story is the fact that the war, if anything, accelerated the development of the state as the active manager of the economy, and the quantitative economist as the ultimate expert. (210)

That bigger story, one could add, was the result of unavoidable war economic mobilization and the concomitant growth of bureaucracies, routinizations, planning, and so on. In a sense, the war created the need for the economic expert, and the economic expert therefore came into existence.

But at the same time, economic experts emerged as a self-proclaimed a-political, non-ideological, and non-partisan figure precisely by deciding that they could conveniently remove political considerations from their analyses. Though this was convenient, it was not realistic. After all, it is
not possible to discuss Germany’s economic recovery in the 1930s without an analysis of war mobilization. Not only does allegedly a-political expertise have deeply political repercussions; it also tips the scale of political debate, by delegitimating political positions in the name of some mythical technical neutrality.

The technical and allegedly a-political economic expert that Tinbergen imagined is described reasonably well by the word ‘technocrat’. Tinbergen has often been described as a technocrat, but Dekker disagrees. As he writes, “we are now in a good position to correct that view” (106). To distinguish him from the typical character of the technocrat, Dekker refers to the leaders of the past that inspired Tinbergen. Individuals such as Henry Ford, Robert Owen, and the Dutch socialist politician Floor Wibaut, Dekker notes, “were certainly not technocrats. [...] These men were able to combine moral leadership with action, and it was that combination that [...] Tinbergen admired” (106). Yet Dekker downplays the technocratic element in Tinbergen by discussing a completely different point, that is, what it is that defines elites. As Dekker writes, “what sets the elite, the leaders, apart from the masses [...] is primarily a set of ideals and their knowledge of socialism, not their technical knowledge” (106). But here we must use caution. Firstly, ‘technocratic’ and ‘elite’ are not interchangeable terms. And second, in the passage quoted above, Dekker is discussing Tinbergen’s socialism in the early 1930s, which by necessity limits his discussion of the term ‘technocrat’. Rejecting Tinbergen’s characterization as a technocrat on the basis of his being a visionary member of the elite is a non sequitur. That Tinbergen was fascinated by men of vision who exerted roles of leadership in their times does not mean that he was not a technocrat.

Indeed, Dekker himself acknowledges that “there are clear technocratic elements” in Tinbergen’s work (106). And, as Dekker reminds us in several passages, Tinbergen aimed at eliminating any political dimension from his expert knowledge of economic issues and planning techniques. Moreover, he positively and consciously operated to make his expertise as highly influential as possible. In a very interesting chapter on the role of experts and policymakers in economic models, Dekker writes:

Although Tinbergen’s intentions were to make economic policy work for everyone in society, he did so without involving society. Even though he had put the policymaker, himself, into the model with his decision models, he had still elevated the expert far above the rest of society. (255–256)
Fifty pages later, Dekker seems to reach our same conclusion: “Development planning [...] was a purely technocratic project for Tinbergen” (306). So one question that arises naturally from this discussion is why Dekker seems so resistant to use the term technocrat for Tinbergen. Would it not be more convincing to say that he was not exclusively a technocrat? Tinbergen was an elitist and a technocrat.

Admittedly, any discussion about definitions is interesting only up to a certain point. If I insist on it, it is because this dispute is closely connected to the more substantive point discussed at the beginning of this section, namely, Tinbergen’s role in Nazi-occupied Netherlands, or, more in general, the role of the technocrat in twentieth-century political economy. It is my impression that Dekker tends to separate the individual from his role. Tinbergen, the man, is separated from Tinbergen, the apolitical expert. I do not believe this separation is possible or heuristically useful.

The increasing separateness between Tinbergen, the expert, and society at large is visible in his growing dissatisfaction with processes of democratic deliberation. Dekker shows this very clearly. In a private company, Tinbergen argued, democracy is limited by the fact that it is management that carries responsibility, not the workers. In politics, democracy has to be limited lest group interests become excessively powerful. Tinbergen’s solution to this conundrum, Dekker writes, “was telling: we needed more experts—in this instance, independent ‘general’ experts, who were skilled at weighing group interests and pursuing the general interest” (345). Tinbergen criticized democracy more explicitly in a short book from the mid-1960s on central planning: “Experience has shown that for most if not all developing countries parliamentary democracy does not work as a system of governing a country”, and “even in some developed countries [...] the system did not work very well” (356). Indeed, as he put it, “all this shows that for a good form of government, a number of decisions must be left to the elites” (356).

III. TINBERGEN AND THE DEVELOPMENT QUESTION

Tinbergen’s planning approach also informed his work on development, and I was not surprised to see that in more and less developed countries, alike, he was unable to understand the realities he was confronting. As Dekker concludes, for all his interest in development economics and development planning in non-Western countries, Tinbergen “never came to grips with human, social, and cultural diversity” (370). Yet an attempt at
making sense of this diversity is arguably what characterized the most interesting and fruitful works by other development economists.

Dekker offers a very interesting discussion of Tinbergen’s contribution to development economics. As he notices, while the major debates in the discipline focused on the process of economic development and in particular on its causes, obstacles, and prerequisites (to use concepts fashionable at that time), “those issues are peripheral in Tinbergen’s writings” (288). His most famous three-stage planning model focused on policymaking, not theories of economic growth. This, Dekker writes, “created a curious emptiness at the core of [Tinbergen’s] development work” (288). Development, for Tinbergen, was a matter of creating the necessary institutions to facilitate processes of policymaking through careful planning. When in 1955 he wrote *The Design of Development*, a document for internal use at the Economic Development Institute of the World Bank used to train administrative officers from less developed countries (it was published in 1958), he highlighted the need for a “coordinated and coherent plan” and for “a harmonious program” among different institutions (quoted in Dekker 2021, 289, 290). As he maintained, particular attention should be devoted to developing “the optimum pattern of organization”, for “general programming has to supply a bird’s-eye view of the pattern of future development of the country”, and “the aim is to arrive at a framework of figures for the possible development of the economy” (quoted in Dekker 2021, 290, 291).

Dekker writes that “the emphasis was on techniques not theories” (292), and about another book from that period, he argues that it “did not excel in diagnosis, nor in a theory of economic development, but it excelled in showing how we could get from A to B” (300). As I read Tinbergen’s works on development issues, this conclusion somewhat misinterprets and inflates Tinbergen’s contribution to the discipline.

Indeed, in the same years in which Ragnar Nurkse (1953) published in-depth analyses of patterns of capital accumulation, investment, and consumption in less developed countries, W. Arthur Lewis (1954) developed a highly sophisticated and enormously influential model of economic development with unlimited supplies of labor, and Albert Hirschman (1958) construed a critique of balanced economic growth through the discussion of linkages and inducement mechanisms, Tinbergen’s work barely skimmed the surface. One would search in vain for any original or at least interesting discussion of theories or processes of policymaking in less developed countries. Tinbergen did not provide theories,
as Dekker writes, but he did not provide techniques either. Entire publications or reports could proceed from beginning to end with the tone and level of analysis depicted by the following example:

The essential problem for each country is to find out in which fields its comparative advantages lie. As a rule they will be related to geographical factors such as mineral deposits, quality of the soil, climate and transportation facilities. Particular comparative advantages will then show themselves in low costs of certain raw materials and of transportation. In certain cases a particular skill of the population may add to the advantages. (Tinbergen 1958, 23)

Indeed, it is not difficult to understand why Dekker highlights the “emptiness at the core of [Tinbergen's] development work” (288).

Dekker suggests that Tinbergen was increasingly focused on “providing a vision” that others would fill with data and techniques (296). It is certainly possible, as Dekker writes, that Tinbergen aimed at “position[ing] himself as a moral guide, not an engineer providing more technical skills” (297). Yet his publications from this period were not really “visionary” analyses, but often superficial documents with only limited usefulness (297). “Virtually all reviewers”, Dekker notes, “expressed skepticism” (299).

With the same conviction with which I disagree with Dekker on the reading of Tinbergen's works on development, I found Dekker's discussion of the institutional dimension of development planning fascinating. Dekker offers a masterful discussion of how processes of institution building are consubstantial to theoretical analysis and processes of policymaking, and how economic organizations, think-tanks and research institutes connect knowledge production and policy processes. From this perspective, Dekker offers a number of very interesting examples on the inherent tension between the two poles of free and independent research, on one side, and relevance for policy, on the other. When research institutions have strong, close connections to political power, the independence of research may easily suffer. At the same time, research institutions that are very distant from political power risk irrelevance. By following Tinbergen's role in a number of national and international organizations, Dekker provides a very valuable analysis of how research institutions relate to political power, and of how economic knowledge takes shape not in the vacuum of intellectual research, but in the reality of research laboratories.
IV. THE UNAVOIDABLE REALITY OF UNINTENDED CONSEQUENCES
Towards the end of the book, Dekker writes that “for Tinbergen there was no invisible hand” (411). He believed in conscious planning and organization, the only means to manage a world that did not include harmony among its natural characteristics. If one wanted harmony, and the book shows without any doubt that Tinbergen wanted it badly, one had to pursue it consciously and rationally. “Progress, stability, and peace necessarily had to be organized”, Dekker writes, “and [Tinbergen's] intellectual effort is best understood as an attempt to bring that about” (411).

On the surface, this is not particularly problematic. Much of the ‘spontaneity’ of human interactions is predicated on prolonged efforts at institution building to create the social space to make interactions possible in the first instance. And as we know all too well from personal experience and observation of political, social, economic, and military conflicts, progress, stability, and peace often require strenuous efforts to be organized. But Tinbergen's view implies more than this. Adam Smith’s invisible hand is a famous example of the role of the unintended consequences of purposive action in social processes. No matter how carefully we plan, the realm of possible and actual outcomes far exceeds our imagination and planning abilities. Yet Tinbergen's goal, Dekker tells us, was that of “reforming the economy so that it would become a determinate and predictable system” (224). To be precise, Tinbergen did not believe in predictability as forecasting (in another passage, Dekker states that “Tinbergen was skeptical of attempts to predict the course of the economy” [152]), but certainly he saw planning as a way to give the economy a well-defined structure, and the expert a firm control over it. By setting for himself such a rigid and ambitious goal, however, Tinbergen also curtailed his own abilities to negotiate radical uncertainty and ignorance. Reading Dekker's exhaustive and well-researched book, I began to understand the melancholy of a thinker whose separatist disciplinary convictions about the world prevented him fully from joining it.

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Tinbergen on the Theory and Policy of Economic Development

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I. SETTING THE AGENDA
Erwin Dekker’s Jan Tinbergen (1903–1994) and the Rise of Economic Expertise (2021) is a tour de force. It seeks to establish that Tinbergen’s main contributions to economics consisted in new techniques for the design of economic policy, not in the formulation of new approaches to economic theory and econometric modelling. As the title of the book indicates, Dekker develops throughout his book the view that Tinbergen played a key role in the rise of ‘economic expertise’ as a central dimension of the economists’ activities in the economic policy realm. Although Tinbergen shifted the focus of his analytical effort from developed (particularly his home country, The Netherlands) to underdeveloped countries, he remained, according to Dekker (262), primarily a policymaker. Hence, Tinbergen’s (1958) volume on development planning—written for the International Bank for Reconstruction and Development—extended to developing countries the approach to economic policy modelling he had put forward in his 1956 book, Economic Policy. They both featured the word ‘design’ in their titles.

Dekker’s (288–289) main claim—regarding Tinbergen’s participation in the new field of development economics that emerged in the post-war period—is that his take was “unique”, in the sense that the prevailing concern with theories of growth and development was “peripheral” to his work on decision models. Dekker is aware that, in principle, a framework for development planning should be based on a theory or model of the development process. However, he argues that Tinbergen’s new framework was not linked to a specific development theory, but compatible with a whole set of them—just like in Tinbergen’s previous work on business cycles, when Tinbergen supposedly did not commit to a particular theory of economic fluctuations. Instead, the assumption behind

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Tinbergen's development planning model was that “certain key policy-making institutions [...] were in place” (288).

Although carefully stated, Dekker's argument for Tinbergen's 'uniqueness' among development economists should be taken *cum grano salis*. The division between the theory of economic development on one side and applied development planning on the other was not at all conspicuous in the burgeoning development economics at the time. Arthur Lewis, for instance—who put forward in 1954 his seminal theoretical model of capital accumulation under unlimited labour supply (see Boianovsky 2019a)—contributed two books on development planning (Lewis 1949, 1966; see Tignor 2006). In Latin America, the United Nations Economic Commission for Latin America (ECLA, known as CEPAL in the region), produced in the mid-1950s, under the leadership of Raul Prebisch and Celso Furtado, an influential document on development planning that attracted the attention of development economists worldwide (UN 1955).

Osvaldo Sunkel, a young member of CEPAL, took advantage of his 1953–1955 European study tour to visit Tinbergen in The Netherlands and inform him of CEPAL's new approach to planning (Boianovsky 2019b). Another prominent development economist, Albert Hirschman (1958, 1963), studied carefully the formulation of economic development policy in theory and practice, although from a distinct perspective (see Chenery 1959 on the similarities and differences between Tinbergen and Hirschman in that regard).

On the other hand, Tinbergen did contribute to theoretical development economics, often with an eye to its implications for development policy. Indeed, Tinbergen's 'uniqueness' as a development economist resided rather in his concern with formal modelling and the quantitative or econometric dimension, which set him apart from most of other pioneers in the field at the time (Hollis Chenery was one of the very few exceptions). Whereas growth economics, especially in Solow's (1956) hands, emerged as part of the increasing formalization of the economic discourse in the post-war era, development economics tended to move in the other direction, in part because of the difficult task of modelling economic divergence and international asymmetries between poor and rich countries. However, Tinbergen (1942), unlike other development economists, had contributed a path-breaking neoclassical econometric growth model that anticipated some central aspects of Solow (1956).
Tinbergen’s 1942 paper did not attract a large readership, as it was published in a German journal during the War (it was translated only in 1959) and it did not discuss steady-state solutions as clearly as Solow. However, it was a key paper in the context of the shift of Tinbergen’s agenda from the developed full-employment economies (tackled in the 1942 paper) to the underdeveloped economies beset by permanent or structural unemployment—a feature already pointed out by Rosenstein-Rodan (1943), among others—examined in his 1958 book and some of his papers around that time.

Dekker (269) mentions that crucial transition in passing, without discussing it in any detail. Unfortunately, his treatment is marred by some inaccuracies, as the statement that neoclassical growth theory was “pioneered by Harrod and Domar” (291)—when in fact they advanced a Keynesian model of growth and fluctuations featuring unemployment. The so-called ‘Harrod-Domar growth’ model was an adaptation by development economists (including, e.g., Tinbergen 1958) of the original formulation to the study of capital-constrained developing economies (see Boianovsky 2018). Accordingly, the capital-output and saving ratios became central—called ‘instrumental variables’ in Tinbergen’s system—to the strategy of development planning. Moreover, Dekker’s (198) description of Tinbergen’s (1942) Cobb-Douglas growth model as assuming a “constant proportion of labour and capital” is incorrect—such constant proportion is a property of the steady-state solution (in the absence of technical progress), not of the production function.

Again, that contrasted with the Tinbergen-Solow neoclassical model’s result that the rate of economic growth is determined by the (exogenous) rate of technical progress, under the assumption of diminishing returns to capital accumulation—not by “saving and technology” as stated by Dekker (199). Tinbergen’s investigation of theoretical models of development and growth culminated in the book with his Dutch colleague and former student Hendricus Bos, Mathematical Models of Economic Growth (1962).

Dekker (263–264) describes how Tinbergen led an outstanding research team of growth mathematical economists and planners in The Netherlands in the 1950s, but refers to his book with Bos (1962)—the first ever handbook of its kind—only in a footnote. Among other topics, this book discussed the determinants of the optimal rate of growth and saving, a subject Tinbergen (1956b) pioneered in a paper that confirmed his distinctive status as a theoretician and model-builder among post-
war development economists. Tinbergen’s distinctiveness was reinforced by his critical reaction to Paul Samuelson’s (1948) famous ‘factor-price equalization theorem’, which is not mentioned by Dekker. Tinbergen (1949) was the only development economist who criticized Samuelson’s powerful theorem—which predicted convergence of the remuneration of workers and other productive factors across countries under free trade—in its own mathematical terms. Samuelson’s theorem—as read by development economists, Tinbergen included—was in apparent contradiction with perceived international economic asymmetries (see also Tinbergen 1979, 342; Boianovsky 2021). Tinbergen’s engagement in criticism of Samuelson’s trade theorem reinforces the technical/theoretical dimensions of his work as a development economist, which Dekker tends to downplay.

II. FROM GROWTH TO DEVELOPMENT

Dekker (197–199, 202–203) provides an insightful account of the context of Tinbergen’s (1942, [1942] 1959) article on growth, produced during the Nazi occupation of The Netherlands. Under the Nazi regime, “business cycles were declared a thing of the past” (197), which prompted Tinbergen to shift the focus of his research at the Dutch statistical institute toward the study of long-term growth. Tinbergen’s (1984, 315–316) recollection of that episode claimed, instead, that his research on economic growth did not result from an imposition by Nazi occupation, but from the fact that—as The Netherlands was cut off from outside world—he had “plenty of time” to reflect upon issues left open by his work on business cycles carried out in the 1930s. One of those issues was the distinction between cyclical short-run fluctuations and long-run trends featuring full employment, around which business cycles took place. As a “check on the non-Nazi attitude” of the editors of the Weltwirtschaftliches Archiv, Tinbergen (1984, 315n2) quoted in his 1942 article a “considerable number of Jewish authors”. Dekker’s account and Tinbergen’s recollection are significantly different, though not necessarily incompatible with one another.

Tinbergen (1984, 316) described his 1942 growth model as a theory of economic development in “embryonic state”, a sort of “prelude” to development theory. The only theoretical influence Tinbergen ([1942] 1959, 187) acknowledged was Gustav Cassel’s ([1918] 1932, chap. 1, sec. 6) pioneer investigation of the “uniformly progressing state”. Despite—or perhaps because of—the relatively small impact of Tinbergen (1942)
on the literature, he would often claim its originality as the first ever growth model, encompassing both theoretical foundations and statistical testing—which was his meaning of economic “models” (see Tinbergen 1979, 347; 1967, 231). Under the assumption of a Cobb-Douglas aggregate production function with disembodied technical progress—with productive factors paid their marginal products under perfect competition—Tinbergen (1942, [1942] 1959) found that, for Germany, United States, France, and the United Kingdom over the period 1870–1914, the long-term rate of growth of per capita income was 1.5%, determined by the rate of technical progress. That period was selected because output was then arguably decided by the supply side (the production function) instead of aggregate demand—although there has been some controversy among economic historians regarding the period 1873–1896 (see Saul 1969). Tinbergen came back to that model in his 1962 book with Bos, when they discussed its steady state solution. Tinbergen and Bos showed formally that the capital-output ratio is constant in the steady state (Tinbergen and Bos 1962, chap. 3), a feature of Solow’s (1956) formulation as well.

Tinbergen’s visit to India in 1951, invited by P.C. Mahalanobis to participate at a conference in New Delhi, had exposed him to widespread poverty in underdeveloped countries and led to a substantial shift in his research agenda, as recalled by Tinbergen (1984, 316–317) and documented by Dekker (chap. 12). Tinbergen’s 1953 article about India’s Five-Year Plan, published as the leading article of the first issue of the Indian Economic Journal, marked his transformation into a development economist. Dekker (269), unfortunately, mistakenly gives “Journal of Indian Economics” as the journal title and omits Tinbergen (1953) from the bibliography. It is noteworthy that Tinbergen (1953, 2n2) referred to his 1942 German article as a source on estimates of the long-run growth rate, a result he then applied beyond the sample of countries examined in that article.

After distinguishing between capital widening (capital accumulation accompanied by an increased labour force) and capital deepening (an increase of the capital-labour ratio), Tinbergen (1953, 3) argued that, on the basis of the empirical Cobb-Douglas production function, capital deepening had a relatively small impact on output. That is related to the assumption of a diminishing marginal productivity of capital, a key postulate in the neoclassical growth model. Moreover, Tinbergen (1953, 4) expressed scepticism about the notion of the incremental capital-output
ratio deployed in Mahalanobis’s formulation of the Indian Five-Year Plan, as it ascribed the whole increase of output to capital accumulation, without accounting for the influence of “knowledge”. Nevertheless, Tinbergen, hesitantly, endorsed the Five-Year Plan’s emphasis on capital accumulation, which, according to the logic of his 1942 model, should be able to bring about an increase of the level of income per capita, but not of its permanent growth rate.

However, by the time he published his 1958 book on The Design of Development, Tinbergen’s original neoclassical approach to growth gave way to a reliance on a fixed-coefficient model associated to the so-called Harrod-Domar growth model as perceived by development economists. Tinbergen never abandoned his neoclassical roots—as shown, for instance in his emphasis that planners should give preference to labour-intensive activities (not to capital-intensive industries as stated by Dekker 269) in India and other developing countries beset by capital scarcity (see, e.g., Tinbergen 1958, 26). His neoclassical credentials were also displayed in his support for Heckscher-Ohlin trade theory and in his misgivings about protectionism, unlike many other development economists at the time (see Tinbergen 1958, 51–52; 1968; 1984, 323). Indeed, in their concluding chapter Tinbergen and Bos (1962, 113–114) criticized what they called “very unorthodox ideas”—by the standards of neoclassical mainstream economics—in economic development theory and policy.

As Dekker (197, 307) shows, overpopulation was a main element of Tinbergen’s take on underdevelopment, although (like Lewis) he was no Malthusian. Solow (1956, 90–91) had argued that, by changing some assumptions about the determinants of population growth, his neoclassical growth model was able to generate multiple equilibria and explain a poverty trap (see Boianovsky and Hoover 2014, 204). Tinbergen too introduced overpopulation in his own neoclassical framework, but under another guise. The problem, from Tinbergen’s perspective, was that, due to a very low capital/labour ratio, the marginal productivity of labour could fall below subsistence, even if the average productivity of labour was above that level. Under those circumstances, if minimum wages were kept above or at subsistence by trade unions etc. (as Tinbergen 1958 expected to be the case), that would introduce a wedge between the marginal productivity of labour and real wages, with ensuing unemployment of a ‘structural’ sort.
Tinbergen (1958, 76–78) discussed that wedge as the most important form of “fundamental disequilibria” that characterized the economy of developing countries, where part of the population could not be gainfully occupied for “lack of complementary means of production: land and capital”. The Swedish economist Knut Wicksell—whose influence on Tinbergen’s decision to give up physics for economics in the 1920s is mentioned by Dekker (70)—had put forward that hypothesis in some detail at the beginning of the 20th century (see Boianovsky and Trautwein 2003, 422–423). Moreover, Tinbergen’s discussion of ‘fundamental disequilibria’ makes clear how his contributions to development economics fit into a long neoclassical tradition coming from Wicksell, unlike Dekker’s account.

III. Optimality and Accounting Prices

As put by Tinbergen (1984, 116), his 1951 visit to India made “visible” to him the capital scarcity typical of underdeveloped poor economies. The Harrod-Domar growth model, featuring just one scarce productive factor (capital), provided a ‘didactic’ way to discuss capital accumulation, and, by that, a natural starting-point for Tinbergen and Bos’s (1962, chap. 2) analysis. It was in that context that the book addressed what Tinbergen perceived as the “main problem” of development economics: the determination of the “optimum rate of development” (Tinbergen and Bos 24–31, 115; the term ‘development’ is here used in the sense of ‘growth’). Tinbergen (1956b) had provided the first formal treatment—followed by his 1960 *Econometrica* article—of that difficult analytical issue since Frank Ramsey’s famous 1928 essay on optimal saving. Indeed, by the late 1950s and until the late 1960s the theme of optimal growth attracted much attention, now in the context of the Solovian version of neoclassical growth modelling and of Dorfman, Samuelson, and Solow’s (1958) turnpike theorem, together with some incursions by non-neoclassical economists such as Roy Harrod (see Boianovsky and Hoover 2014, 212–214; Boianovsky 2017).

Bent Hansen (1969, 332)—in a passage quoted approvingly by Boumans and De Marchi (2018, 232) and apparently endorsed by Dekker—argued, on the occasion of Tinbergen’s Nobel Award, that the Dutch economist “took little part in the discussion of topics like optimal growth rules, turnpike theorems and dynamic efficiency”, which he saw as of little relevance for development planning. While Tinbergen was generally concerned with the practical relevance of economic theorems,
it is hardly accurate to describe him as eschewing optimal growth and related topics. True enough, apart from a section in Tinbergen and Bos (1962), his 1960 article was Tinbergen’s last contribution to that field, but he did follow developments that took place after that, some of them led by the well-known economist Tjalling Koopmans, his countryman and “intimate friend” (Dekker 2021, 193) since the 1930s (see Koopmans 1965). This is well-illustrated by Tinbergen’s 1969 Nobel Lecture on the role of models in economic analysis, which referred to optimizing “dynamic models” for infinite time periods, of the kind put forward by Edmund Phelps and Koopmans, as belonging to the “really fundamental features of economic science” (Tinbergen 1969).

One of Tinbergen’s (1958, Annex 2) main innovations was his emphasis on the role of “accounting prices” as dual variables in the design of development planning (see also Tinbergen 1956a, 181; Tinbergen and Bos 1962, 41–45, with reference to Qayum 1960). Such a concept, deployed by Tinbergen and other development economists like Chenery in the 1950s, has become better known by the term ‘shadow prices’, which became widespread in connection with the literature on linear programming at the time—the terminology and notion of ‘shadow prices’ in fact go back at least to Hicks’s 1939 Value and Capital. Tinbergen’s deployment of ‘accounting prices’—a term that became influential in the 1950s due to Tinbergen’s usage in development economics—reflected his discussion of the ‘fundamental disequilibria’ characteristic of developing countries, in which capital and foreign exchanges are undervalued, while labour is in excess supply (see Chenery 1959). The neoclassical case, featuring a production function with high substitutability between factors—e.g., the Cobb-Douglas production function—implied that production factors were paid their marginal productivities (that is, ‘accounting prices’), unlike the prevailing disequilibria conditions of developing countries. Samuelson (1970, 751) singled out Tinbergen as one of the “sophisticated planners” who advocated the application of “shadow prices” or “accounting prices” to labour, capital and imported goods in developing countries.

Tinbergen chaired the United Nations’ (1960) report on development planning. The report assumed a long-run stability of the capital-output ratio (at a value around 3), seen as based on solid empirical grounds, “one of the most useful parameters with a fair degree of stability” (UN 1960, 11). Around that time, the stability of that ratio was listed as one of Nicholas Kaldor’s famous ‘stylized facts’ of economic growth. Tinber-
gen did discuss in passing the restrictive assumption that output is a linear function of capital only. In general, output should be treated as a function of capital and labor and the changing relation between them. To base the projection of national output on the (stable) capital-output ratio implied a “certain type of technical change in the relevant future” (UN 1960, 11). He did mention in passing the neoclassical production function as an econometric model alternative to the adapted Harrod-Domar approach, which could be “usefully applied to some countries”—as he had done in his 1942 model of growth in developed economies (UN 1960, 11n1).

Tinbergen did not deal, in his book with Bos and other sources, primarily with development policy, but “with models that can be used in designing such a policy” (Tinbergen and Bos 1962, 2). The hard core of development planning consisted of mathematical models with empirically estimated coefficients, used through a succession of stages from macro to micro levels. Moreover, growth models themselves did not imply anything about their use by planners, in the sense that “widely different policy devices may sometimes be obtained with the same model” (Tinbergen and Bos 1962, 47). Hence, Tinbergen’s overall focus on model building—in economic theory and econometrics in general—also showed in his approach to development planning, as he attempted to build development policy on the grounds of theoretical and applied models, not just decision models.

IV. SEARCHING FOR THE ECONOMETRICS OF DEVELOPMENT
In the early 1960s, Tinbergen (1961) contributed a methodological paper on “development theory” to the Festschrift in honour of Johan Åkerman (Hegeland 1961)—a volume often cited mostly due to Samuelson’s chapter on “A New Theorem on Nonsubstitution”. Tinbergen’s 1961 “econometrist’s view” of economic development is not mentioned by Dekker, even though it represented his main attempt to explain how he believed economic asymmetries between poor and rich countries should be approached. Tinbergen (1984, 321–322) confirmed the importance he attached to this essay in his 1984 autobiographical reflection on his career as a development economist, in a section called “A Philosophical Interlude: The Role of Environment in Its Widest Sense”. By then, it was clear to Tinbergen that the points raised in his 1961 chapter had, against his expectations, neither been answered nor acquired any priority in development economists’ agendas.
Tinbergen’s ‘econometrist’s view’ argued for development economics as based on solid empirical foundations. He was critical of Baumol’s notion of the “magnificent dynamics” of classical economists, Harrod and Schumpeter, as it lacked strong connections with measurement (Tinbergen 1961, 57; 1979, 347). Tinbergen traced the beginnings of the “scientific” era of growth and development economics, with its mix of theory and measurement, to the Australian economist Colin Clark and to his own German article, both published in 1942 (Tinbergen 1961, 57; 1979 347). As pointed out by Dekker (274–275), Clark’s (1942) statistical analysis of international inequalities in the world economy made a big impression on Tinbergen. However, as Tinbergen (1979, 348) acknowledged, his and Clark’s theories were “in fact different”. A main difference, one may surmise, was that Tinbergen (1942)—just like Solow (1956)—could not satisfactorily explain international divergences of growth rates.

As it has gradually become clear to development economists, there are essentially two ways to explain economic divergence between nations. Per capita income convergence for countries with the same parameters was a corollary of the neoclassical growth model with diminishing returns to capital, as elaborated by Tinbergen (1942), Solow (1956), and Swan (1956). From that perspective, steady-state income divergence resulted from differences in parameters (‘fundamentals’) such as saving rates and population growth, since the general state of technological knowledge was supposed to be the same across countries.

In the alternative view of underdevelopment as a ‘coordination failure’, advanced by Paul Rosenstein-Rodan, Ragnar Nurkse, and some other development economists in the 1940s and 1950s, countries with the same fundamentals can move along divergent paths. The latter notion—which was behind the ‘Big Push’ development policy so influential at the time and occasionally mentioned by Tinbergen—was partly based on the assumption of increasing returns, unlike the neoclassical growth model. Tinbergen and Bos (1962, 36–37) referred in passing to some analytical hurdles posed by increasing returns—such as the presence of negative profits if firms under perfect competition charge prices equal to marginal costs—which would be fully solved by the 1980s only, when economists learned how to model economic growth with increasing returns.

Tinbergen’s (1961, 49) econometric program may be understood as a suggested investigation of differences in “fundamentals” across countries, which he expressed as the influence of the “environment” formed
by “non-economic parameters”. It is clear from Tinbergen (1961, 49) that he was critical of what he perceived as a lack of attention to measurement by development economists—he referred to Rostow’s (1960) influential concept of development according to stages—able to bring “theory and observation together”. That would help to formulate development theories as “refutable hypotheses” (Tinbergen 1961, 50). The equations explaining the aggregate volume of production should include as well, apart from the amount of productive factors (as in Tinbergen 1942), “environment variables” (formed by indexes indicative of climate, institutions and the state of technology) and “psychological or ‘racial’” characteristics (Tinbergen 1961, 53).

The reference to ‘race’, repeated in 1984, set Tinbergen apart from most—but certainly not all—development economists, as it reminded of the 18th and 19th century notion of ‘national character’. Tinbergen (1961, 55) suggested as well that the volume of savings and population growth, both treated as exogenous in his 1942 model, should be explained in terms of economic variables. By the end of his 1961 essay, Tinbergen (1961, 58) claimed that “it is by no means belittling the work done by [the pioneers] when I conclude that there is a real need for a concerted programme of econometric research in the field of development theory”. However, his plea never went beyond a suggested call, with no real impact on the field or even on Tinbergen’s own research program after that.

In his assessment of Tinbergen’s work as an economist, Niehans (1990, 383–384) stated that although Tinbergen became world famous mostly as a missionary for development planning, his “lasting contributions to economic science [...] were in other areas”, particularly the theory of economic policy. This seems to be broadly compatible with Dekker’s biographical account. However, other commentators, such as Bos (1970) and Bruno (1984), have stressed instead the scientific character of Tinbergen’s contributions to development economics through several formal models, some of them discussed above. Hence, according to Bruno (332), Tinbergen’s ‘unique’ contribution to development economics lies in the adaptation of his 1956 Economic Policy: Principles and Design, a by-product of his work at the Dutch planning office, to the subsequent Design of Development. A formal development plan should be mainly based on a theoretical construct or ‘model’ combined with applied empirical content. Searching for a theory of economic development able to
inform development planning was part and parcel of Tinbergen’s en-deavour, even if not always successfully.

Dekker’s quest for Tinbergen’s ‘uniqueness’ as a development economist has not reached its goal, in the sense that, against Dekker’s claim, Tinbergen did not participate in a supposed separation of development economics between ‘theoretical’ formulations and ‘applied’ planning.¹ Nevertheless, Dekker’s extended research on Tinbergen as a policymaker has opened new vistas on how theory and policy were intertwined in Tinbergen’s agenda. Tinbergen’s distinctiveness as a development economist—as compared to the rest of the field in the post-war period—was related to the role of his neoclassical background as revealed particularly in his concern with ‘optima’ throughout his long career as a development economist (see Stone 1964; and, for an illustration, Tinbergen 1968). However—unlike Jacob Viner, Gottfried Haberler, and Peter Bauer in the 1950s, who argued for classical and neoclassical views of development based on the working of the market and opposed development planning and the notion of market failure in general (see Little 1982, chap. 4)—Tinbergen deployed his neoclassical background as a key element of his contributions to development planning and development economics as a whole as part of the formalization of economics, even when advancing his ‘econometric’ plea for the study of economic development.

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¹ “There was something unique to Tinbergen’s approach to development. Whereas many of the other ‘pioneer of development’ were concerned with theories of economic growth and development, those issues are peripheral in Tinbergen’s writings. [Instead,] Tinbergen built decision models and institutional models of how to plan” (Dekker 2021, 288).


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Probability and Statistics in the Tinbergen-Keynes Debates

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I. INTRODUCTION
Erwin Dekker’s *Jan Tinbergen (1903–1994) and the Rise of Economic Expertise* (2021) provides a rare treasure in the history of economics: from a single volume, we can obtain a sense of both Jan Tinbergen's worldview and historical context. The worldview was egalitarianism, optimistic about scientific expertise, and hopeful that internationalism could avoid the horrors of the First World War, which occurred in Tinbergen’s formative years. His historical context was the early years of several parts of modern economics, including development economics, macroeconomics, environmental economics, welfare economics.

Dekker details Tinbergen's major influence across all of these areas. I shall consider just one instance of Tinbergen's wide-ranging impacts. He was a foundational figure in the development of macrodynamics: the study of systemic changes in an economy over time, such as business cycles. Tinbergen’s *Statistical Testing of Business Cycle Theories* (1939a, 1939b) published by the League of Nations and his earlier study (Tinbergen, 1936) were the first large-scale and rigorous formal excursions into macrodynamics.

Like any highly original science, Tinbergen’s research attracted criticisms, including from future Nobel Prize winners (Friedman 1940; Haavelmo 1943). By far the most famous critique was by John Maynard Keynes (1939, 1940). In brief, Keynes argued that (1) Tinbergen failed to identify and satisfy the conditions for using correlation analysis, (2) he used this technique badly, and (3) he selected an unpromising subject matter: business cycles. In short, Tinbergen was employing “black magic [...] a branch of statistical alchemy” (Keynes 1940, 156) with almost no significance for the tumultuous business cycle theory debates of the 1930s and 1940s. Keynes endorsed statistical reasoning in economics, but...
only to estimate the parameters that are important in particular theories, not to test them.

Chapter 8 of Dekker’s book adds to our understanding of the Tinbergen-Keynes debates, especially their origins within internal League of Nations discussions, which included Keynes. Dekker reveals that there was a persistent misunderstanding, because Keynes interpreted Tinbergen as stridently presenting a finished (or nearly finished) method to solve major macroeconomic debates. Meanwhile, Tinbergen took an optimistic but modest view of his research as a preliminary excursion into testing business cycle theories, yet his confident rhetoric in the 1939 volumes belied his private circumspection.

Furthermore, Dekker explains how Tinbergen’s research for the League of Nations, as well as Keynes’ response, took place within a wider socio-political context. At the time, economics was still strongly divided into separate schools. Most business cycle theories were associated with a particular country: there were various American, Austrian, British, French, German, Norwegian, and Swedish schools of thought. Tinbergen saw his macroeconometric project as an attempt to combine insights from each approach. Yet, as Dekker explains, Tinbergen’s decision was not purely scientific. His internationalist approach to theory was parallel to his internationalist values: just as he hoped to break down national barriers in economics, so Tinbergen yearned to break down the barriers to international coordination and peace (165–172). Meanwhile, Dekker points out how Keynes’ scepticism and rhetoric also seems to have been partly political. By the time of Keynes’ critique, the technocratic and institutional approach favoured by the League of Nations had clearly failed to create a stable international order. The League of Nations would continue for six more years, but it was insignificant. In Keynes’ view, the economic research that the League had produced through Tinbergen was also insignificant. For Keynes, the future was in the hands of greater leaders, partnered with men of intuitive brilliance, like himself (186–187).

In addition to this contextualisation of the Tinbergen-Keynes debates in the history of international politics, Dekker also provides us with their context in the development of macroeconomics. Tinbergen’s research was a high point in a much larger econometric movement in the Inter-War period. Dekker points out the considerable irony that, in hindsight, subsequent syntheses of Tinbergen and Keynes would overshadow Keynes’ objections. While Keynes was harshly pessimistic about Tinbergen’s methods, many Keynesians (especially Americans) shared Tinbergen’s
optimism. Hence, they sought combinations of (a) Keynes’ macroeconomic perspective and (b) Tinbergen’s ambitions to add statistical methods to macroeconomists’ toolkit. With many intermediary vicissitudes, this synthesis survives and thrives in macroeconomics (163–164).

Dekker’s biography focuses on the difficult challenge of explaining Tinbergen’s worldview and wider historical context to a modern audience. One aspect of this worldview was Tinbergen’s robust aversion to discussing philosophical issues explicitly (415). Even in his replies with Keynes, Tinbergen focused relentlessly on the technical challenges, such as whether Tinbergen had tested his estimates using multiple subperiods, rather than the philosophical issues. Consequently, Dekker understandably puts aside the latter in his discussion. Focusing on them could mislead us about Tinbergen’s view of the controversy. As Tinbergen saw it, the controversy primarily consisted in technical debates arising from Keynes’ misinterpretations and his ignorance of the relevant econometric literature (Magnus and Morgan 1987, 129–130).

Another reason that could be given for Dekker’s focus is that many scholars have already explored the philosophical aspects of the Tinbergen-Keynes debates. For example, Anna Carabelli emphasises the importance of Keynes’ A Treatise on Probability (1921) for understanding the sources of Keynes’ criticisms (Carabelli 1988, chap. 10). She argues that Keynes’ ontological assumptions made him sceptical that many statistical methods were applicable in economic contexts. The phenomena of economics are characterised by instabilities arising from human nature, which lacks the stability of natural materials that have deterministic features (like mechanical systems) or stable long-run probabilities (like radioactive half-lives). In contrast, Tinbergen had a physics background, and tended to assume—as a defeasible conjecture—that the statistical methods which had been so successful in physics would also be applicable in economics (Dekker 2021, sec. 4.3).1 Similarly, Mary Morgan has explored the distinct roles that Keynes and Tinbergen saw for statistical inference in economics: Keynes granted that one could statistically estimate very local facts, such as an economy’s price level or its fiscal multiplier over a brief period. Tinbergen advocated a further theoretical role of refuting macroeconomic theories or even indicating ways of synthesising them (Morgan 1990, 124–125). To summarise the main differences, Keynes and Tinbergen differed on:

1 See Lawson (2003) for a concise discussion of Keynes’ ontology and its role in both A Treatise on Probability and his explicitly economic works.
(1) The applicability of methods from physics to economics;
(2) the role of statistical inference in economics;
(3) the adequacy of Tinbergen’s chosen statistical methods, even for the modest roles envisaged by Keynes; and
(4) whether the techniques for correlation analysis methods (a key part of Tinbergen’s study) in economics were improving. In particular, Keynes was sceptical that these methods had fundamentally improved since the period he was actively researching the topic, which was the long development period of A Treatise in Probability, prior to 1921 (Dekker 2021, 186–187).

In this book symposium contribution, I shall seek to supplement the insights provided by Dekker by discussing some underexplored philosophical aspects of the Tinbergen-Keynes debates. Like much of the literature, Dekker focuses on the differences between Tinbergen and Keynes. I do not dispute that there were differences, but I shall discuss some important similarities. I shall also connect these debates to contemporary methodological issues. Additionally, the literature on the debates has tended to be pessimistic about (or uninterested in) the possibilities of integrating insights from Keynes into econometrics, while still retaining Tinbergen’s ambitions for the subject.

In the first two sections, I discuss their shared views on the objectivity and nature of statistical evidence. I shall then explain how recent developments in statistical methods and the philosophy of science may help econometricians to address one of Keynes' fundamental criticisms—the problem of amalgamating evidence in macroeconometrics. Thus, it may be able to synthesise (at least partly) Tinbergen's ambitions with Keynes’ cautions. Overall, my discussion will illustrate how, by examining the similarities as well as the insights of both economists, we can understand their individual perspectives better and their relation to modern debates.

II. OBJECTIVITY
A common view in contemporary philosophy of statistics is Subjective Bayesianism, according to which statistical reasoning is based on arbitrarily chosen probabilities (Ramsey 1990; Howson and Urbach 2006). In this section, I shall explain how Tinbergen and Keynes both differed from this subjectivist position regarding statistical research.

Tinbergen understands statistical practice as performing tests that will estimate relative frequencies (the rate at which some event, property, characteristic, event, and so on occurs in a class of things) with a degree of reliability we judge to be sufficiently high for some practical or
scientific problem. Frequentists quantify ‘reliability’ in terms of the long-run frequencies of error in a statistical inference. Economic theories are tested through using them to guide the development of empirical models. For example, if a business cycle theory $T$ implies that interest rates have an important effect on investment under certain conditions, but statistical evidence in estimating a model suggests that the effect is small, then that is evidence against $T$ (Tinbergen 1940, 142–143). Thus, Tinbergen’s approach is an early example of frequentist econometrics. His revolutionary contribution was to demonstrate the possibility of testing dynamic macroeconomic (macrodynamic) models. These models are about macroeconomic changes over time, like business cycles. Moreover, he pioneered the approach of using a wide variety of tests (Morgan 1990, chap. 4).

In frequentism, the main subjective element is the choice of acceptable error rates—the long-run frequency of errors in a test. For example, in significance testing, given a particular model, rejection threshold (‘significance level’), and dataset, the evidential significance of a test is determined by objective factors, such as (a) the likelihood of the data given the model and (b) the methods used to sample the data. By ‘objective’, I mean that it is not determined by scientists’ arbitrary opinions.

Which error rates should we choose? This decision is not determined by frequentist methodology, but it is also not arbitrary. For instance, confidence interval testing is incoherent if we set our standards too low. In particular, contradictory estimates can all exceed a confidence level that is set too low. A confidence level is $\gamma = (1 - \alpha)$, where $\alpha$ is the test conditions’ maximum long-term rate of random error for inferring a particular type of confidence interval. The confidence interval is an approximate

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These errors can be random or systematic. Random error is error due to chance, such as selecting an unrepresentative sample. There is random error even in an unbiased sampling process. It is distinct from systematic error, which is due to a problem with the testing set-up, such as a biased selection process. If we are observing bats in a forest and estimating their relative frequency per acre, we can mitigate the risks of random error with larger samples, but if we choose to make our observations during the day, then we will still make systematic errors, because our sampling process is biased against seeing bats.

See Mayo (2018) for a contemporary and sophisticated methodology of frequentist statistics.

In more detail, the maximum long-run rate of error for inferring a type of confidence interval is the approximate relative frequency at which we would make errors if we used such samples for inferences. If we made the inferences enough times, the proportion of our inferences that are mistaken would settle around this frequency $\alpha$. When it settles, we have reached the long-run. Some error might be unavoidable (it depends on the facts about what we are studying) but we can mathematically determine a maximum level that the long-run error rate cannot exceed.
estimate of a parameter $\theta$. If we set $\gamma$ at 50% or lower, then it is possible that both a statistical hypothesis (such as ‘$\theta$ is in the interval 0.8 to 0.9’) and its negation (‘$\theta$ is not in the interval 0.8 to 0.9’) can have maximum error rates above $\gamma$. To use an extreme case, if $\gamma = 0$, then $\alpha = 1$, meaning that we are willing to make estimates with a long-term error rate of 100%. Thus, by this ‘standard’, we could infer both statistical hypotheses and any statistical hypothesis that disagrees with them, and our inferences would still meet our ‘standards’ for avoiding long-run errors. To avoid incoherence, statisticians must set $\gamma$ high enough to avoid the possibility of incompatible hypotheses exceeding $\gamma$. Typical conventions for $\alpha$ include 0.05 and 0.01, but any value below 0.5 will avoid incoherence, because the maximum probability of an error in inferring a type of hypothesis $H_1$ is $1 - P_{E}(H_d)$, where $P_{E}(H_d)$ is the maximum possible error rate for inferring the disjunction of alternatives to $H_1$. Thus, if $\alpha > 0.5$, then $P_{E}(H_d) < 0.5$. The relevant error for any alternative to $H_1$ will be less than $P_{E}(H_d)$. Hence, $\alpha > 0.5$ makes it impossible to estimate both a statistical hypothesis and a contrary hypothesis when using confidence interval estimation.

Keynes was far more active in complex philosophical controversies than Tinbergen, which makes Keynes’ epistemology more comprehensive and yet also harder to interpret. Keynes developed a detailed theory of probability and statistics in *A Treatise on Probability* (1921). However, the extent to which his ideas had changed by 1939 is a matter of considerable controversy. Curiously, the Tinbergen-Keynes debates are rarely used as exegetical evidence in this controversy.\(^5\)

Keynes (1921) interprets probability as an objective evidential relation between propositions. Thus, just as ‘All men are mortal and Socrates is a man’ has an objective relation of deductive implication towards ‘Socrates is mortal’, so ‘95–100% of men are mortal and Socrates is a man’ has an objective evidential relation of partial support in favour of ‘Socrates is mortal’. This partial support is not deductive, because the premise could be true and the conclusion false, but Keynes argued that it has a weaker, non-deductive form of logical implication. Keynes used this interpretation of probability to formally analyse a wide range of non-deductive types of reasoning, like induction and analogy. In the latter part of Keynes (1921), he also applied it to statistical methodology. Thus, in 1921, Keynes believed that evidential relations in statistics are objective.

\(^5\) There are some exceptions, such as Rowley and Hamouda (1987), Brady (1988), and Marchionatti (2010).
In 1922–1926, Keynes was criticised by his close friend, Frank P. Ramsey (1990). The exegetical controversies concern the extent to which Keynes altered his views in response to Ramsey. These controversies involve the interpretation of several difficult texts, including Keynes’ *The General Theory of Unemployment, Interest and Money* (1936) and his obituary for Ramsey (Keynes 1972, 335–339). Very roughly, there are two popular positions in this dispute, which I shall call (a) the Continuity interpretation and (b) the Recantation interpretation.

According to the Continuity interpretation, Keynes’ views about probability and statistics were more or less unchanged by Ramsey’s critique (O’Donnell 1989; Runde 2003, 52; Brady 2017; Davis 2019, 88). According to the Recantation interpretation, Ramsey convinced Keynes to modify his views fundamentally, including abandoning objectivity (Bateman 1987; Gillies 2006; Raffaelli 2006). To see how good scholars can disagree about on this topic, even though Keynes is rarely an obscure writer, consider this passage from his obituary for Ramsey:

Ramsey argues [against me] that probability is concerned not with objective relations between propositions but (in some sense) with degrees of belief, and he succeeds in showing that the calculus of probabilities simply amounts to a set of rules for ensuring that the system of degrees of belief which we hold shall be a consistent system. Thus the calculus of probabilities belongs to formal logic. But the basis of our degrees of belief—or the a priori probabilities [...]—is part of our human outfit, perhaps given to us merely by natural selection, analogous to our perceptions and our memories rather than to formal logic. So far as I yield to Ramsey—I think he is right. (Keynes 1972, 338–339)

The problem is that the scope of ‘so far’ is unclear (Fitzgibbons 1998, 163). It certainly involves the previous sentence: Keynes agrees with Ramsey that we learn prior probabilities from our innate nature, rather than from an intuitive grasp of platonic conceptual relations. How much more does it include? Does it include Ramsey’s denial of probability’s objectivity? Furthermore, arguably much of the paragraph was consistent with Keynes’ earlier views anyway, such as the role of the probability calculus.

The Tinbergen-Keynes debates help to resolve this controversy. Tinbergen claimed that econometric tests can prove a theory to be incorrect.

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6 There is also the Subjectivist interpretation by Anna Carabelli (1988, 2021) and Donald Moggridge (1992, 623) who argue that Keynes was always similar to Ramsey and therefore had little, if anything, to concede.

7 Rules for inferring probabilities from other probabilities.
Keynes argued that, outside of special circumstances, this is wrong. However, he does not appeal to the subjectivity of evidential relations (Keynes 1939, 559–560). Such an appeal would have made Keynes’ task easier. If we are free to adopt almost any probability distribution as our initial degrees of belief, then talk of ‘proof’ in statistics is completely out of place, even in an extended non-deductive sense. For example, there are probability distributions where a high proportion of Heads in a sample of coin tosses is evidence in favour of its bias towards Heads, but also probability distributions where it is irrelevant, and even some in which such evidence confirms that the coin is biased against Heads! On Ramsey’s view, all of these probability distributions are equally permissible. Keynes does not even mention this shortcut, indicating that he still regarded evidential relations in statistics as objective. In general, throughout Keynes’ criticism of Tinbergen, he apparently adopts an objective conception of evidence—it is the relation of a hypothesis to our evidence and background knowledge that determines a test’s evidential force (or lack thereof).

Of course, Keynes may have been trying to rely on premises that his contemporary economists believed—Subjective Bayesianism was still very rare in 1939. However, Keynes enjoyed controversy and had no fear of advocating unpopular positions. The Tinbergen-Keynes debates were a perfect opportunity for him to explore the implications of a Ramseyan view, if he had adopted it. Thus, the texts in the Tinbergen-Keynes debates support the Continuity interpretation, though they do not end the debate. Overall, it seems that although Tinbergen and Keynes had different theories of statistical evidence (frequentist and non-frequentist) both agreed on the objectivity of evidential relations in econometrics.

It is worth emphasising that ‘objectivity’ in this sense still leaves a wide scope for rational disagreement in economic debates. That two people should agree on the evidential relations between a theory $T$ and statistical evidence $E$ does not imply that they will agree on whether we should believe $T$, believe that $T$ is false, or be agnostic about $T$, even if they both believe $E$. As Keynes details in *A Treatise on Probability*, scientific evidence is a three-place relation between theories, evidence, and background information (assumptions, knowledge, and so on). For instance, disagreements about the econometric assumptions that economists make in a particular context can block agreements on the pertinence of some statistical evidence for a macroeconomic theory, even among rational economists. Thus, this sense of ‘objectivity’ does not have the absurd implications that controversies in economics are easily
resolved, nor that economic reasoning has no role for intuition and subjective judgement.

III. THE PROBLEM OF INDUCTION

Some commentators have linked Keynes’ views on probability and statistics, including in the Tinbergen-Keynes debates, to the Problem of Induction (POI). I shall argue that this is misleading, given the traditional understanding of the POI, and that the usefulness of inductive reasoning in economics is actually a salient area of common ground for Tinbergen and Keynes. First, I shall explain the POI. Second, I shall argue that Keynes did not view it as a problem for economics. Third, I shall argue that Tinbergen, like Keynes, adopted a sophisticated form of inductivism about economics. Thus, on a major issue of dispute in the methodology of economics, Tinbergen and Keynes were on the same side.

Dating the origins of the POI is beyond this article’s scope, but it was most influentially presented by David Hume ([1740, 1748] 1975). There are many interpretations of it, but the fundamental core is clear. Hume contends that inductive arguments are not deductively valid. Inductive arguments have (a) premises known directly or indirectly by observation and (b) conclusions about unobserved objects or properties. Given that (now) uncontroversial logical fact, it is not clear how they are otherwise justified. In other words, how the premises can support the conclusions? On the traditional interpretation of Hume, the POI is a sceptical paradox: we have an instinct to trust induction, but Hume thought that we cannot justify this instinct (Russell 1912, chap. 6; Stove 1973; Stroud 1977, chaps. III–IV; Stove 1982, chap. IV).

Sheila Dow interprets Keynes as raising the POI to criticise Tinbergen (Dow 2004, 552). She interprets Keynes as an inductive sceptic, in the sense that he regarded inductive reasoning as a convention (Dow 2009, 2010; see also Andrews 1999). There are weak versions of the POI where Dow is certainly correct. If one interprets Hume as just saying that induction is not deductively valid, and thus involves some special risks, then Keynes agreed with Hume. However, that position is not sceptical enough to be an objection to any but the most overconfident econometrician.

On stronger interpretations of the POI, Keynes’ position against Tinbergen actually presupposes that the POI is not an issue for economics. Dow identifies a passage by Keynes as echoing themes from Hume:

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8 For economists with opposing views, see, for example, Peter Boettke (1997), Murray Rothbard (2004), and perhaps Ludwig von Mises (1949; but see Linsbichler 2017).
The most important condition is that the environment in all relevant respects, other than the fluctuations in those factors of which we take particular account, should be uniform and homogeneous over a period of time. We cannot be sure that such conditions will persist in the future, even if we find them in the past. [...] The main prima facie objection to the application of the method of multiple correlation to complex economic problems lies in the apparent lack of any adequate degree of uniformity in the environment. (Keynes 1939, 566–567)

Granted, this is a problem involving induction, but it is not Hume’s POI. Hume raised a problem of knowing about the wider ‘environment’ beyond what we have observed, whereas Keynes is claiming that our observations of instability in economic phenomena are representative of that environment: we know that economic phenomena are such that Tinbergen’s methods are unreliable beyond ephemeral time spans and particular economies. Consequently, Keynes’ criticisms of Tinbergen suggest that he had a thoroughly inductivist view: induction can and does contribute to economic knowledge.

What about Tinbergen? Some of his remarks may suggest an anti-induction position. Tinbergen says that econometric testing cannot prove a theory to be true, but it can prove it to be false (Tinbergen 1939a, 12). Taken in isolation, this remark corresponds with Karl Popper’s position about statistical inference (Popper [1980] 2002). Popper’s approach to statistics was based on his belief that the POI is unsolvable. Against Keynes and others, Popper argued that science’s logic is purely deductive. A key step in Popper’s research programme to establish this deduction-only view of scientific evidence was to argue that truly scientific theories are falsifiable, in the sense that they could be logically inconsistent with some data. Statistics present a challenge to Popper’s methodology, because no statistical estimate is ever deductively inconsistent with our data: even $T_1 = ‘At least 99% of human births are male’$ is consistent with what we know, because we might just have observed extremely atypical subsequence thus far. Popper proposed that we set conventional rules to

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9 See also Carabelli (1988, 185ff). Keynes tends to assume that heterogeneity and instability go together. If heterogeneity is something like the stochastic relevance of many conditions to a random variable’s value and stability is something like a high standard deviation for that random variable, then these are mathematically distinct. However, Keynes might be right that, as an empirical fact, these are correlated in economics. The instability of economic—as opposed to many natural—phenomena was a crucial part of Keynes’ methodology. It was why he regarded economics as a ‘moral science’ rather than as analogous to the natural sciences, especially physics (Keynes 1978, 296–300).
“regard” data that is extremely improbable given a theory as if it falsifies that theory (Popper [1980] 2002, 194).

However, Tinbergen’s views are not Popperian. First, it is important to note that there is no logical asymmetry between verification and falsification in a statistical context. Both rejecting $T_1$ and accepting a hypothesis like $T_2 = \text{‘Less than 99\% of male births are male’}$ require non-deductive reasoning, because our evidence neither deductively entails nor refutes either hypothesis. Second, Tinbergen (1939a, 12) says that there are senses of “verification” that econometrics can achieve, though he does not specify them; he apparently means what contemporary philosophers call ‘confirmation’, which is evidential support such that the evidence may prove the hypothesis or (more typically) provides weaker evidence in favour of it.

Given this logical symmetry between verification and falsification, what rationale could Tinbergen have for distinguishing them? Dekker points out an intriguing subtlety in Tinbergen’s prospectus for econometrics. Tinbergen was pessimistic about econometric models’ capacities to make accurate predictions. Nonetheless, he was optimistic about the possibility of synthesising theories of the business cycle, by using econometrics to test the adequacy of rival theories’ claims, and then combining the insights of the remaining ideas (Dekker 2021, 152–153, 178). Thus, his ambitions for econometrics required the exclusion of false predicted relationships. Given such a refinement of the rivals, theoreticians could construct a synthesis from a narrower, more robust set of theories. Therefore, econometrics had a negative role in Tinbergen’s macrodynamic programme. In contrast, Tinbergen did not think that econometrics alone can warrant the (provisional) acceptance of a business cycle theory for either practical or scientific purposes. Other theoretical reasoning is necessary. A theory must survive the filter of testing, but the overall evaluation of its plausibility is beyond econometrics.

Where did Tinbergen acquire these fairly sophisticated views on induction, falsification, and statistical inference? It is hard to say, because he tries to avoid philosophical discussions wherever possible. However,

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10 In this sentence, by ‘logical’, I mean deductive logic. Standard statistical norms are sometimes called the ‘logic’ of statistics, but they are separate from what I mean. Many statisticians sharply distinguish verification and refutation, saying that statistically significant result is evidence against the null hypothesis $H_0$, but failure to reject is not evidence for $H_0$, and similar claims (Fisher 1974, 16). These norms are open to dispute, but they are compatible with what I say in this article. I thank an anonymous referee for raising this point.

11 Aside from estimating parameters.
placing him in historical context provides some indications. Tinbergen first encountered scientific methods (at a tertiary education level) studying physics at the undergraduate and postgraduate levels (his PhD combined physics and economics) at Leiden University from 1921 to 1929. The 1920s were an exciting and uncertain period in the history of physics. The Newtonian theories were no longer tenable. The exemplar of an empirically successful and theoretically rigorous scientific theory had fallen just as quickly and dramatically as the great land empires of Germany, Russia, Turkey, and Austro-Hungary. Yet, despite some stunning successes, the revolutionary theories of Einstein and others were far from well-established, not least because the fall of Newtonianism had dramatically demonstrated the fallibility of science. In this period, the fallibility of induction was prominent in the minds of educated people. David Stove has noted this effect in the philosophy of science (Stove 1973, chap. 7) yet it also created a new sense of modesty in physics. Despite the comparative stability of physics since the fall of Newtonianism, this modesty survives to some extent even today. In this atmosphere, even a physicist as averse to philosophy as Tinbergen could easily pick up a relatively subtle methodology of induction: it is fallible, but it is also capable of providing some evidential support. Since Tinbergen tends to assume that the methodology of physics is also applicable to economics, it is plausible that his practice in economics inherited this approach to inductive inference.

Therefore, while positive evidence and negative evidence are logically the same in statistics, it was negative evidence that was important for Tinbergen’s aims. His focus on falsification is pragmatic, rather than Popper’s epistemological motivation from the POI. Overall, inductivism in economics is common ground for Keynes and Tinbergen.

IV. THE DEBATES AND EVIDENCE SYNTHESIS: AN OPTIMISTIC NOTE

One of the deepest methodological issues that Keynes raised is now known as evidence synthesis—how to amalgamate information from multiple methods (Keynes 1939, 561). Tinbergen argued that, although there

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12 It is representative of Keynes’ philosophical sophistication that he developed a modest theory of induction prior to the fall of Newtonianism: he developed the key ideas of A Treatise on Probability in the 1900s and early 1910s, before the main revolutions in physics. It is striking to contrast Keynes’ stress on induction’s fallibility against almost all of his prominent predecessors—John Stuart Mill, William Whewell, Immanuel Kant, and other less influential figures in 19th century philosophy of science. It is even more striking given that Hume’s ideas about induction were not prominent, at least outside Cambridge (Russell 1912, chap. 6), when Keynes began his work on induction.
may be significant non-measurable economic phenomena that cannot be directly handled by his testing methods, the effects of these phenomena can be taken into account by using our background knowledge about them (Tinbergen 1939a, 11) and he gave particular examples of how he had incorporated events like major strikes (Tinbergen 1940, 143). Tinbergen thought that, in some contexts, this incorporation would improve the analyses’ accuracy. However, Keynes noted that Tinbergen’s method for the identification of these exogenous factors was through the size of the residual: the amount of variation that the model did not explain in a correlation analysis. Yet the size of the residual was also Tinbergen’s criterion for evaluating the analysis’s accuracy. So should we (a) discount models that take our background knowledge into account because of their large residuals or (b) take them more seriously due to their greater expected accuracy? Tinbergen’s methods become ad hoc and perhaps incoherent at this point (Keynes 1940, 155).

The underlying issue is that Tinbergen lacked a systematic and plausible methodology for evidence synthesis. This is not a problem if there is just one type of evidence. For example, if we can estimate a parameter via confidence intervals and only confidence intervals, from disjoint samples, then it is straightforward to combine the evidence into a single estimate.

Unfortunately, when the evidence is heterogenous, then this simple picture falls apart. As Keynes noted, this problem occurs in economics. It also occurs in other sciences. For example, in pharmacology, evidence about the risks and safety of a drug often comes from many sources: randomized controlled trials (RCTs) but also surveys, case series, cohort studies, and so on (De Pretis, Landes, and Osimani 2019). Combining evidence from across these different types is a considerable challenge.

However, recent developments in Bayesian methodology have made progress in evidence synthesis for many sciences, including medicine (Sutton and Abrams 2001; Sweeting et al. 2008; Walach and Loeff 2015; Landes, Osimani, and Poellinger 2018; De Pretis, Landes, and Osimani 2019; De Pretis, Landes, and Peden 2021) psychology (Scheibehenn, Jamil, and Wagenmakers 2016; Heck et al. 2022) and economics (Ades et al. 2006; Baio 2012; Jackson et al. 2019). Using the tools of Bayesian statistics, it seems possible to combine evidence in a systematic way, at least in many contexts. Therefore, it may now become possible for econometricians to respond to Keynes’ critique far more systematically than Tinbergen could hope in 1940.
Unfortunately, Bayesian statistics presupposes prior probabilities—probabilities before learning one’s evidence. According to Bayesianism, hypotheses can have probabilities given some evidence. These probabilities are determined by a combination of (a) the probability of the evidence given the hypothesis, (b) the prior probability of the hypothesis, and (c) the prior probability of the evidence. While (a) is often determined mathematically, (b) and (c) are not. So where do they come from? The ‘Problem of the Priors’ has been a persistent and fundamental criticism of Bayesianism (Venn 1876; Nagel 1939; Glymour 1980; Mayo 2018). In brief, worry is that the choice of priors may be subjective and therefore unscientific. In contrast, the methodology used by Tinbergen was frequentism; it is fundamentally based upon the objective error rates of tests, not prior probabilities of hypotheses (see section II).

Both frequentism and Bayesianism have their strengths: the former in avoiding priors, the latter in its tools for evidence synthesis. It is hence unsurprising that philosophers and statisticians have been interested in combining these methodologies. For instance, some contemporary philosophers (Kyburg and Teng 2001; Kyburg 2006; Williamson 2010, 2013) have proposed an intriguing compromise, which I shall call Objective Bayesianism. According to this approach, Bayesian reasoning takes precedence over frequentist reasoning, but only when the ‘priors’ involved are derivable from background knowledge about the phenomena. These ‘priors’ will be imprecise approximations of the true frequencies, derived from sources like confidence interval estimates, maximum likelihood estimation, or well-supported stochastic theories. Hence, when our background knowledge about phenomena is weak, then frequentist testing will tend to be suitable. We lack evidence-based priors in such situations, but we can fall back on the purely mathematical facts about error rates that frequentists utilise. However, when we can have Bayesian priors based on evidence rather than subjective opinion, then we can use Bayesian reasoning—in the sense of using these priors for techniques like Bayesian evidence synthesis methods.

Thus, it may be possible to systematically address Keynes’ point if our background knowledge is sufficiently rich. When interpreted as a constructive challenge, rather than a dogmatic rejection of econometric testing as such, then Keynes’ criticisms offer useful guidance for the further development of econometric methodology. Of course, Objective

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13 This label is not quite right for Kyburg and Teng, for reasons that are unimportant here.
Bayesianism has its critics (for instance, Howson and Urbach 2006). Additionally, it has not yet been applied to econometrics. As Keynes noted, there are many epistemological and substantive conditions that must be satisfied before probabilistic methods are applicable to a problem. For example, we might know that we are making assumptions about the homogeneity of our subject matter that we know are wrong, such as that the variables involved are not autocorrelated. The persistent disagreements in economics, especially macroeconomics, do not suggest an optimistic view. Nonetheless, it is worth exploring.

V. CONCLUSION
Keynes must have been surprised and confused by Tinbergen: a young economist who claimed to be testing macroeconomic theories in 1939. Even more puzzling must have been the League’s apparent endorsement of Tinbergen’s research. Keynes would have been justifiably worried that, with World War II beginning, he would be unable to participate for long in the debates about this new method. It would have been tempting to be angry with Tinbergen and attack him superficially. Yet Keynes admired him as an economist and a person (Patinkin 1982, 229–230). He limited himself to only occasional rhetorical flourishes, like describing Tinbergen’s work as ‘alchemy’. Furthermore, as we have seen, Keynes’ criticisms were far from superficial. The Tinbergen-Keynes debates remain both historically and philosophically important. While the macrodynamic programme in econometrics that Tinbergen initiated has made tremendous progress, few are sanguine about it. The philosophical issues raised by Keynes remain controversial. Furthermore, the foundations of econometrics remain obscure. What are the relevant ‘probabilities’ in econometrics? Degrees of belief, error rates, or something else? How should econometrics be synthesised with other types of evidence in macroeconomics? As I have shown in this commentary, the Tinbergen-Keynes debates remain a thought-provoking means to approach these issues.

Additionally, the reactions to Tinbergen’s work by economists other than Keynes have been largely neglected by philosophers of economics. Dekker describes several (chap. 8) and one could add the reactions of Milton Friedman (1940) and Trygve Haavelmo (1943), which are underexplored in the philosophy of economics. Dekker’s book is an excellent

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14 A variable like national income or inflation is autocorrelated if its value at \( t \) is correlated with its value at \( t + n \), for some period \( n \), which could be three months, a year, two years, and so on.
introduction to these controversies, because it gives us a comprehensive impression of Tinbergen’s own underlying ambitions and the historical context of his efforts.

Dekker portrays Tinbergen as a passionate man of action, with technocratic tendencies, who sought to use his expert status to achieve ambitious and idealistic goals: achieving a stable and peaceful international order that would end or at least greatly mitigate the suffering caused by business cycle fluctuations, substantially alleviate world poverty, and combine rival economic schools into consensus syntheses. In accordance with this practical orientation, Dekker notes how Tinbergen was highly averse to taking stands on philosophical questions (415). Nonetheless, I have explained how Tinbergen’s views in his clash with Keynes do have important philosophical aspects. I have also argued that their agreements are philosophically interesting, as well as their disagreements. We can also use these debates as gateways into the philosophical views of these economists. Furthermore, the methodological issues raised in the Tinbergen-Keynes debates continue to be important for the philosophy of science, especially the philosophy of statistics. Developing methods that can achieve Tinbergen’s aims, while respecting the insights of Keynes’ critiques, remains an obdurate but exciting challenge for economists and statisticians.

The literature on the philosophical aspects of the Tinbergen-Keynes debates has understandably focused on their differences, since these are what is important for interpreting the cause of Keynes’ criticisms and the details of Tinbergen’s responses. However, as I have argued in this article, there are also important similarities. For example, it is possible that the similarities were one reason why Tinbergen did not really engage with the epistemological (rather than technical) aspects in Keynes’ attack, in addition to Tinbergen’s aversion to philosophical controversy (sections II and III above). Additionally, when we see that Keynes attitude to Tinbergian econometrics can be rationally reconstructed as principled methodological caution, rather than as a purely ideological or a fundamental opposition, then we can see prospects for econometrics that combines the insights of Keynes with the aspirations of Tinbergen (section IV). Finally, by supplementing Dekker’s discussion with these philosophical points and references to a few key texts in the literature (like Carabelli 1988; Brady 1988; O’Donnell 1989; Morgan 1990) and others, I hope that I have helped readers gain a deeper understanding of the Tinbergen-Keynes debates.
REFERENCES


Press.


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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. 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. 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

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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 iden-
tifying various instances in which values do enter his work. His interna-
tionalist 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 econ-
omy. Values might have leaked in, despite Tinbergen’s intentions to the
contrary.

Even so, Tinbergen might have aimed for a value-free sci-
ence combined with an ethic of responsibility, even if he did not achieve
it. More interesting, therefore, is the question whether the morally com-
mitted scientist and the pursuit of rational science were not systemati-
cally 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 sci-
ence which become increasingly separated. In my discussion of Tinber-
gen’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 sup-
posed neutrality of technocracy and failed to demonstrate moral leader-
ship. 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 hu-
mankind 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 govern-
ments 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. 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.

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

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

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 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.

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**Abstract:** In her recent book, *Faces of Inequality* (2020), Moreau aims at developing a normative account of discrimination that is guided by the main features of anti-discrimination law. The critical comment argues against this methodology, indicating that due to indeterminacy relative to their underlying normative principles, central anti-discrimination norms cannot fulfill this guiding role. Further, using the content of such norms to guide ethical discussions is likely to be misleading, as it reflects evidentiary considerations that are unique to the legal context. The critical comment’s claims are developed based on a close examination of indirect discrimination (or disparate impact) norms, and, as such, have wider implications for ongoing moral and political debates that are heavily influenced by the content of these norms.

**Keywords:** discrimination, anti-discrimination law, equality, disparate impact

**JEL Classification:** D63, J14, J15, J16, J71, K31

**I. SITUATING MOREAU’S DISCUSSION WITHIN ANTI-DISCRIMINATION JURISPRUDENCE, LEGAL THEORY, AND THE PHILOSOPHY OF DISCRIMINATION**

Ethical and political discussions of discrimination—that is, the wrongful differential treatment of people, in a way that is related to their membership in some socially salient group, such as their race or gender (Lippert-
Rasmussen 2013, 13–46)—are heavily influenced by and intertwined with legally oriented discussions of the phenomenon. This is hardly surprising, considering that many of the attempts to confront discrimination have taken place in legal venues, where legal anti-discrimination norms provide a framework in which discrimination claims are mounted and evaluated. What is surprising to many, however, is that the normative underpinnings of anti-discrimination jurisprudence—including its underlying assumptions about the nature of discrimination and the reasons why it is wrong—remain, to a significant extent, obscure and controversial.

Anti-discrimination legislation and constitutional provisions commonly express a general commitment to equality, but do not specify any more particular interpretation of this value that can guide judgments on whether given instances of group-based differential treatment constitute wrongful discrimination. Historically, then, anti-discrimination jurisprudence has evolved through judicial decisions determining whether particular instances of group-based differential treatment coming before courts violate these general provisions; on the basis of such concrete, case-based determinations, courts have then formulated rules and standards that designate certain classes of cases as instances of illegal discrimination (De Burca 2012).

Presumably, the inclusion of certain cases of group-based differential treatment within the purview of anti-discrimination norms reflects, at least in part, the judgment that they are instances of wrongful discrimination. However, in many of the accompanying judicial discussions, the grounds for such judgments have remained unspecified; nor has the continuous process of developing anti-discrimination law included any direct and systematic examination of normative questions about discrimination. And while the legal-theoretical literature contains several projects whose aim is to specify normative rationales or principles that could explain or justify existing anti-discrimination law, these have been confronted with the intractable nature of this now extensive body of jurisprudence. Ranging over a variety of diverse domains, including employment, healthcare, and the provision of goods, and comprising numerous, versatile legal sources and materials (Khaitan 2015, 2–3), existing anti-discrimination law is arguably too complex and internally inconsistent for such

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1 This definition slightly diverges from Moreau’s (2020, 3, 7); however, I focus here on cases that fall strictly within both definitions, and on groups and types of wrongful discrimination that are (relatively) widely and consensually considered to be core examples of the phenomenon.

2 These debates are ongoing and have not reached a consensus (Khaitan 2015, 6–7).
normative underpinnings to be specified. Furthermore, several components of anti-discrimination law—including some of its prominent features—are the subject of ongoing, heated legal and political controversies about their appropriate interpretation (De Burca 2012, 3–7; Khaitan 2015, 2–5; Collins and Khaitan 2018, 4–12). The philosophical-egalitarian literature, on the other hand, has remained largely isolated from these legally-oriented discussions of discrimination, and only recently started examining ethical questions associated with the phenomenon (Lippert-Rasmussen 2013, 4).

Against this background, Moreau’s recent book, *Faces of Inequality: A Theory of Wrongful Discrimination* (2020), takes a fresh, integrative approach to the topic. Similar to many recent philosophical discussions, the book aims at developing a normative account of discrimination, that is, a set of claims about when and why instances of group-based differential treatment constitute wrongful discrimination. Diverging from both recent philosophical and legal-theoretical discussions, however, Moreau maintains that such a normative account should accommodate both moral intuitions about discrimination and the content of existing anti-discrimination jurisprudence. More precisely, while she permits the book’s normative claims to ultimately diverge, to an extent, from those that can be gleaned from existing jurisprudence, Moreau maintains that ethical discussions of discrimination should at least start from, or be guided by “some of the basic ideas about discrimination given to us by the law”, or the law’s “widely shared features” (13–14). Further, she maintains that the resulting normative account must accord with these features, arguing that failure to do so amounts to a serious inadequacy flaw that provides a strong reason to reject such an account (13–14, 27–28).

Based on this methodological approach, along with the general normative premise that the wrongness of discrimination is tied to a violation of the (abstractly defined) value of equality, Moreau develops a pluralistic account of discrimination: she argues that discrimination is wrong when and because it (1) unfairly subordinates people, or (2) interferes with deliberative freedoms to which they have a right, or (3) deprives the victims of some basic goods (11).

Arguably, difficulties accompanying attempts at specifying the normative underpinnings of anti-discrimination jurisprudence as a whole loom at such a project as well. Further, one may question the prospects

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3 Some examples include Lippert-Rasmussen (2013); Eidelson (2015); Hellman (2008); and the many other accounts discussed in Lippert-Rasmussen (2017, chap. 6–14).
of accounting for both the normative nature of discrimination and anti-discrimination law at the same time, considering the gaps between them. Particularly, anti-discrimination laws in liberal societies do not aim at regulating *all* instances of wrongful discrimination (as Moreau notes about discriminatory actions performed by individuals in their interpersonal interactions, 211–247), while presumably reflecting goals and considerations other than targeting wrongful discrimination as such. Hence, there are reasons to suppose that the content of anti-discrimination law does not neatly align with or reflect the normative principles making discrimination objectionable.

Since these difficulties have been noted in the literature (Khaitan 2015, 1–9; Moreau 2020, 12–23, 27), however, I focus here on an often overlooked difficulty with using the content of anti-discrimination law to guide ethical discussions of discrimination, or requiring that normative accounts of discrimination accord with the law’s prominent features. I will argue (in section II) that even when ignoring the difficulties associated with gleaning clearly-specified normative principles from anti-discrimination law in its entirety, and focusing instead only on one prominent, clearly-defined anti-discrimination norm—namely, the norm pertaining to indirect discrimination (or disparate impact)—this does not result in any clear enough guidance for developing equality-related normative accounts of discrimination. This is because the norm’s content is not determinate enough relative to this task. Further, I will argue (in section III) that the norm’s content reflects, to a significant extent, considerations pertaining to the process of adjudicating the factual aspects of legal claims of discrimination. Hence, using the norm’s content to guide ethical discussions, or requiring that normative accounts of discrimination match its content is likely to mislead us in developing accurate normative claims.

Before proceeding, some clarifications are due. I will assume here, with Moreau and prominent views in the literature and public discourse, that the wrongness of discrimination is tied with a violation of equality, abstractly defined (4–9). Normative accounts of discrimination should, then, specify more concrete equality-related reasons why discrimination is wrong, or when and why instances of group-based differential treatment constitute wrongful discrimination, over and above this general association with inequality (24). My argument will be that anti-discrimination law cannot provide significant and adequate guidance for this particular task.
II. INDIRECT DISCRIMINATION NORMS ARE INDETERMINATE RELATIVE TO THE EQUALITY-RELATED REASONS MAKING DISCRIMINATION OBJECTIONABLE

Indirect discrimination norms (IDN) are a central feature of anti-discrimination jurisprudence in many countries and jurisdictions (Moreau 2020, 13–18; Hepple 2006, 608–609). Their original formulation has been set forth in the landmark U.S. Supreme Court case *Griggs v. Duke Power Co.* (401 U.S. 424 [1971]). There, a company that had previously openly discriminated against African-Americans changed its policy when a legal prohibition on discrimination took effect. Instead of openly proclaiming that African-Americans will not be considered for certain jobs, the company instated an eligibility requirement of passing a standardized competency test, whose implementation led to the rejection of a large number of African-American applicants (whereas White employees already occupying the relevant positions were not asked to take the test). Based on these background facts, the Court introduced a rule on which policies, laws, or practices that are ‘facially neutral’—that is, that do not contain explicit reference to people’s group identity, and do not openly proclaim any discriminatory aim such as excluding or disadvantaging members of a certain group—are to be considered unlawfully discriminatory if they satisfy two conditions: (1) they lead to a ‘disproportionate disadvantageous’ (or ‘adverse’) effect on members of groups protected by anti-discrimination norms; and (2) this outcome is not reasonably connected to a legitimate goal of the policy (IDN are often distinguished from direct discrimination norms, which prohibit the *explicit* designation of people belonging to certain groups for disadvantageous differential treatment). Influenced by the *Griggs* decision, many jurisdictions outside of the United States have introduced legal norms with similar formulations, where they are routinely used to adjudicate claims of discrimination in a variety of different domains.\(^4\)

\(^4\) For a description of the process of adopting these norms in the European Union and the United Kingdom, and how they are generally used there (along with brief references to other jurisdictions), see De Burca (2012, 4–5, 11–12); Hepple (2006, 607–616); and Collins and Khaitan (2018, 1–4). The case law, that is, the body of judicial judgments implementing these general norms by using them to decide concrete cases (which together with the norms’ general formulation constitutes a part of anti-discrimination law) is, of course, extremely extensive and detailed, and there is much diversity, and sometimes inconsistency, in the way IDN are interpreted and applied in particular cases (see, generally, Collins and Khaitan 2018, 3). I cannot describe this body of jurisprudence here—for recent surveys of EU and UK indirect discrimination case law, see Connolly...
Which equality-related normative principle(s) can plausibly justify a determination that a given policy is illegally discriminatory (and presumably, wrongfully discriminatory), under this formulation of IDN? Before examining this question, it is important to address a preliminary hurdle relating to a longstanding controversy about the norms’ appropriate interpretation. Some legal authorities and scholars maintain that IDN should be interpreted solely as an evidentiary tool, to be used in legal proceedings to ‘smoke out’ cases where discriminators motivated by inequitable attitudes use ‘facially neutral’ tools (such as standardized tests in employment contexts) to avoid legal liability for excluding or disadvantaging members of certain groups (as most likely occurred in Griggs). Advocates of this more restrictive interpretation maintain that IDN should only apply to cases where satisfying the mentioned conditions indicates an underlying ‘discriminatory motivation/aim’—presumably, the presence of things such as group-based animosity, prejudice, or objectionable stereotypes that have motivated or influenced the policy’s design or implementation. Conversely, those advocating for a more expansive interpretation of IDN maintain that, at least in some cases, showing that the outcome of a certain policy satisfies the said conditions should be enough for it to fall under these norms’ purview, even in the absence of an indication of discriminatory attitudes (Collins and Khaitan 2018, 25–28; Primus 2003, 518–536).

These competing legal-interpretive approaches—often associated with broader political and ideological orientations—are commonly taken to align with or reflect competing views about what makes discrimination wrong. Thus, contrast is often drawn between ‘motivation-oriented’ or ‘subjective’ views of discrimination, vs. ‘outcome-oriented’ or ‘objective’ views (see, for example, Rutherglen 2006). What is commonly overlooked in these broader debates, however, is that both approaches are compatible with a wide range of views about what makes discrimination wrong. Thus, applying both interpretations to real-life examples of differential treatment would lead to designating a wide range of cases as unlawfully discriminatory, and those would plausibly be objectionable (when they are) for a variety of equality-related reasons. Further, when examining particular instances of plainly objectionable discriminatory policies that

(2022); and Tobler (2005). For general descriptions of how IDN are implemented to adjudicate concrete claims of discrimination in the United States (here again, indicating their diverse and sometimes inconsistent and controversial usage) see Ricci v. DeStefano (557 U.S. 557 [2009]) and Texas Department of Housing and Community Affairs v. Inclusive Communities Project, Inc. (576 U.S. 519 [2015]).
would normally be designated as unlawful under each approach, often-
times this particular judgment could be grounded in several equality-re-
lated normative principles. Hence, even assuming that this controversy
was settled and one interpretive approach would prevail as reflecting the
normative nature of discrimination more accurately, this would only take
us so far in restricting the range of normative accounts that are compati-
ble with the content of IDN.

To see this, let us focus on the second (‘outcome-oriented’) interpre-
tative approach (which appears more compatible with Moreau’s substan-
tive views, 9–10), in its narrow application within the context of standard-
zized tests used to screen out job candidates. Suppose that using a certain
standardized test that is not reasonably connected to a legitimate busi-
ness aim screens out a disproportionate number of African-American can-
didates (say, relative to their number among the pool of candidates). Sup-
pose also that inegalitarian attitudes do not underlie or influence the pol-
icy design or implementation in any way. Such a policy would be design-
nated as unlawfully discriminatory based on its outcome, according to
this interpretive approach. But what are the equality-related reasons why
it may plausibly be regarded as wrongfully discriminatory?

One claim that plaintiffs or purported victims can raise in such a sce-
nario is that the policy is not compatible with equality of opportunity: re-
jected candidates can plausibly complain, for instance, that the reliance
on a written test denies members of groups that tend to underperform in
such tests an equal chance of freely competing against other candidates
(say, based on on-the-job performance). But another way of accounting
for the moral objection to such an outcome is based on the claim that
certain benefits—say, positions in certain governmental institutions, or
highly competitive positions for artists and athletes—should be awarded
based strictly on some appropriate distributive principles, such as merit,
reward for previous achievements, or compensation for hard work. Yet in
other domains where the same formulation is routinely used—for in-
stance, in adjudicating claims of discrimination in health or housing—
such claims do not seem appropriate, whereas other equality-related
claims do. Plaintiffs or purported victims may claim, for instance, that the
distribution of healthcare resources or housing resulting from an identi-
fiable policy does not conform to principles of need-based distribution,
or does not give priority to the worse off; and that because it burdens
members of a certain group, such a policy discriminates against them.
All of this illustrates, then, that many different normative commitments may underpin a legal judgment that a certain policy is unlawfully discriminatory, under this outcome-oriented interpretation of IDN. All of these egalitarian principles and claims may, presumably, be used as a basis for developing general accounts of discrimination—all of them compatible with IDN, that is, with one of the law’s prominent features. Moreover, considering the versatility and complexity of contemporary anti-discrimination jurisprudence, the range of these accounts is likely to be even wider if the law in its entirety is to be considered. This indicates that existing law does not significantly constrain the range of equality-associated normative accounts available to the ethicist, and that requiring that such accounts accord with its prominent features (at least, where IDN are concerned) does not provide significant guidance as to their content; rather, much of the latter would have to be developed and settled based on reasoning that is largely independent of the law’s content.

Notably, while Moreau’s account is pluralistic in the sense that it appeals to the three different principles mentioned above, it does not accommodate this wide range of equality-related normative views compatible with IDN. At least, the book does not specify why its three suggested principles better align with existing law, relative to many other equality-related principles. Specifically, it is not clear why the former should play a more central role in a normative account of discrimination, as compared to principles such as equality of opportunity and merit-based distribution, which figure more prominently in existing jurisprudence (Griggs, 401 U.S. at 431–433; Ricci, 557 U.S. at 20, 25; Ricci, 557 U.S. at 13, 19, 28 [Ginsburg, J., dissenting]; Moreau 2020, 5).

III. ANTI-DISCRIMINATION NORMS REFLECT EVIDENTIARY CONSIDERATIONS

Even assuming that anti-discrimination norms were underpinned by one or several clearly identifiable equality-related principles, however, that would not necessarily mean that the set of cases they would typically designate as illegally discriminatory could accurately guide normative

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5 Moreau employs the book’s general methodology with regard to indirect discrimination norms in particular (13–14). See also the discussion in footnote 4 above.

6 It is not clear whether Moreau maintains that equality-related normative principles beyond those she specifically mentions in her own account can account for the moral objection to discrimination. She does, however, seem to suggest that such principles would not be central to a normative account, and specifically rejects accounts appealing to equality of opportunity based on the methodological point discussed here (25).
discussions of discrimination. This is because the content of anti-discrimination norms has been designed, and is consistently interpreted and implemented in the adjudication of particular cases, based on evidentiary considerations. These relate to the fact-finding process in legal proceedings, and not to the normative principles explaining why cases coming before courts are wrong.

To see this, let us again closely examine how IDN are interpreted and applied in employment discrimination lawsuits. As previously noted, some legal authorities and scholars maintain that IDN should be interpreted solely as an evidentiary tool, designed to uncover instances where discriminators have operated on inegalitarian attitudes. This ('subjective' or 'motivation-oriented') interpretive approach is often taken to reflect the normative view that what generates the wrongness of discriminatory actions is such attitudes. On this approach, then, the norms’ function in legal proceedings involves assisting plaintiffs’ efforts of supporting the factual aspects of their claim that wrongful discrimination has taken place in a given case: for instance, showing that an employer in a given employment discrimination lawsuit has in fact been motivated by discriminatory attitudes in designing their candidate screening policy. Applying the second condition of IDN’s formulation to adjudicate this factual question serves, in a range of typical lawsuits, the role of showing that the employer had no other reason for adopting the policy, hence providing evidence that she operated on discriminatory attitudes.

In other words, the norms’ role under this interpretation is not that of designating as unlawful all cases that would be objectionable under such an attitude-oriented normative view (while assuming that the relevant facts obtained). Rather, their content focuses on identifying those cases where the facts deemed relevant under this normative view actually obtain, bearing in mind evidentiary difficulties that are characteristic of typical discrimination lawsuits. This evidentiary role is, of course, important in legal proceedings, where both plaintiffs and defendants are interested in concealing facts that would assist the other side in supporting their legal claims; but no similar fact-finding problem exists in purely normative debates.

This indicates that using the content of IDN to guide ethical discussions, or requiring that normative accounts accord with their content—for instance, by examining instances of differential treatment that typically fall within their purview, and specifying the reasons why discrimination is wrong based on their characteristics—is likely to be misleading.
This is because the norms would not reliably identify the class of cases that are relevant for purely normative debates. Rather, their scope of application is likely to be skewed towards those cases where facts deemed relevant under a certain normative view are likely to obtain (in typical legal settings). This does not necessarily align with the class of cases that would be considered wrongfully discriminatory under the same normative approach, assuming the relevant facts obtained. For instance, applying IDN’s rule would fail to designate policies that are underlain by discriminatory attitudes as unlawful, where operating on such attitudes also serves a legitimate business aim (for instance, if this attracts customers). This is even though such policies would, presumably, be considered objectionable under an attitude-oriented normative view of discrimination.

The injection of evidentiary considerations into the content of anti-discrimination norms in the manner just described does not seem to be restricted to this attitude-oriented interpretation of IDN. For instance, in employment discrimination lawsuits that are adjudicated based on an outcome-oriented interpretation, similar evidentiary considerations seem to be intertwined with more substantive considerations—that is, background normative convictions about which instances of differential treatment are, in principle, wrongfully discriminatory—in guiding the norms’ application. In general, this can be learned from the usage of the term ‘indirect discrimination’ in such contexts. There, the norms’ formulation is not understood as laying out the nature of a distinct normative phenomenon that the proceeding is concerned with, but rather as detailing what plaintiffs and defendants need to show to prove or refute a claim of (legal liability for) discrimination in such proceedings. The latter, presumably, includes both principled normative claims, and factual claims about the particular case being examined (Ricci, 557 U.S. at 18–19).

For instance, in a scenario where a standardized written test is used to screen out candidates for a job where the relevant skills are primarily physical or interpersonal, a legal claim that such a policy wrongfully discriminates against members of a certain group (based on an outcome-oriented approach) would involve a normative component (for instance, the claim that screening job candidates based on a test measuring irrelevant skills is wrongfully discriminatory, because it violates equality of opportunity or merit-based distribution) and a factual component (this particular written test does not measure skills relevant for the job). A 'bottom line' determination that the policy under consideration is unlawfully discriminatory rests on both these elements, and, importantly, the
formulation of IDN is suitable for adjudicating both elements in typical employment discrimination proceedings (*Ricci*, 557 U.S. at 4–11, 27–32 [Ginsburg, J., dissenting]). On this outcome-oriented interpretation too, then, IDN would not reliably identify cases that are representative of the phenomenon of wrongful discrimination, and requiring that normative accounts match their content is likely to mislead us in developing accurate claims about when and why instances of group-based differential treatment constitute wrongful discrimination.

**IV. CONCLUSION**

The discussion conducted throughout Moreau’s book sometimes seems to suggest that there is much to learn from legal materials about how wrongful discrimination instantiates in real-life situations, especially considering the abstract character of much of the egalitarian-philosophical literature (29–30). This seems plausible: the variety of ways in which different policies and practices—with their everyday subtleties, peculiarities, and technical complexities—could fail to treat people belonging to certain groups as equals is not easily predicted or characterized from the philosophical ‘armchair’. Discussions of discrimination would thus benefit from incorporating detailed and realistic descriptions of the institutional, societal, and interpersonal mechanisms and contexts in which the phenomenon is often embedded, and some of these are indeed documented in legal materials. Moreau’s book makes an important contribution to highlighting and pursuing this non-ideal-theoretical approach that normative discussions of discrimination should arguably adopt.

This is different, however, from maintaining that *normative claims* about discrimination—that is, claims specifying when and why such everyday practices are wrongful, by appealing to normative principles—should be gleaned from or accord with existing anti-discrimination law. As I hope to have shown by closely examining one prominent, influential anti-discrimination norm, existing jurisprudence can serve this particular guiding role only to a very limited degree, and using its content to guide normative discussions of discrimination may be misleading.

Beyond this methodological point, the discussion here can be of assistance in beginning to clarify some contemporary political and moral debates about discrimination. Specifically, while interlocutors often appeal to the formulation of indirect discrimination norms in such debates—for instance, in claiming that a certain policy is wrongfully discriminatory because of its negative disparate or disproportionate impact
on minority groups—the discussion here has shown that such positions are compatible with a large variety of substantive normative commitments, and do not obviously conflict with what is often perceived as opposing positions, such as those maintaining that disparate impact is no indication of wrongful discrimination.

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In a world of greedy jobs, gender equity is expensive within families. This is the main takeaway of Claudia Goldin’s book *Career and Family*. Goldin explains how workplace success often requires burning the midnight oil, investing in higher education, and devoting spare time to developing one’s career. However, even when women are allowed to work in these ‘greedy jobs’, they are often incompatible with the care burdens they are expected to carry, which often leads to pay inequities. Goldin artfully describes how these tensions have evolved in the past century for one specific group: women with higher education in the United States.

Goldin begins her book by introducing her definition of ‘greedy work’, that is, careers that require one be on call at home, produce constant cerebral output, and never tune out of work. Because of family obligation and childcare, couples are often faced with a choice between a marriage of equals or a marriage with more money, where one takes on greedy work and the other flexible work.

Goldin then outlines the five cohorts of college-graduate women and discusses each of their experiences with work and family in the subsequent chapters, often using unique historical data. The first cohort (women who graduated college in 1890s–1910s) are those who Goldin describes as having to choose between family or career: very few were able to have both. The second group, women graduating in the next two decades, are marked as having a job and *then* a family. Women graduating college in the 1950s are a group which commonly had a family and *then* a job. In these chapters, Goldin indicates differences between job and career, noting that a career offers advancement and intellectual fulfillment while a job is simply a way to earn money. Goldin then turns to the fourth cohort: women graduating from college in the 1970s, which she classifies as having a career and *then* a family. It is not until the fifth group, women graduating in the 1980s and 1990s, that college-educated women are able to have a career *and* a family in Goldin’s eyes.
Throughout these chapters, Goldin shows several tables comparing data points across cohorts: namely, the fraction of each group never married, with no births, labor force participation among married women, and overall college graduation rates. Goldin’s description of the evolution of work and family from the early 1900s to today, with the corresponding contextualization of improvements in fertility planning, laws around gender discrimination in the labor force, and gender norms more broadly, was nicely written and clearly conceptualized.

When describing each of the five cohorts in their own respective chapters, Goldin sprinkles in some commonly recognizable figures. These range from economists like Hazel Kyrzk, to feminists like Betty Friedan, to more recent celebrities like Tina Fey, which makes the book digestible and accessible to audiences from several generations and backgrounds. In fact, one of the best aspects of the book is Goldin’s infusion of her personal connections to most of the five groups she studies. From her discussion of her fleeting connection to Margaret Reid, who Goldin situates with Hazel Kyrk in ‘Group One’, to her forward-looking perspective on her ambitious women students at Harvard, Goldin is sure to keep a personal touch throughout the book.

In addition to making the text more relatable, it helps readers better understand Goldin’s background, and thus, her interest in focusing on college-educated women in the United States. Understanding a researcher's background and overlapping social identities is important from the perspective of feminist standpoint theory (Harding 2004). Standpoint theory is often used by feminist researchers to understand and reflect on how knowledge production is embedded in social, political, and historical contexts. In general, a standpoint epistemology argues “that knowledge is constructed from specific positions and that what a knower can see is shaped by the location from which the knower's inquiry begins” (Sprague 2016, 47). More simply, “ideas cannot be divorced from the individuals who create and share them” (Collins 2015, 252). It seems that Goldin herself is engaging in standpoint theory by sharing with the reader her own background as a highly educated woman in the United States and as a University of Chicago trained economist, both of which have certainly influenced her interest in inequities faced by college-educated women and the way she approaches her book’s research questions. I found that including her background in the book, rather than remaining a faceless narrator, was refreshing and invaluable.
After wrapping up her discussion of the five cohorts’ balance between career and family, Goldin then shifts to explaining the recent persistence of the gender wage gap among college-educated workers. She ultimately continues to point to ‘greedy work’, but explains the wage gap, its measurement, and its contributors very carefully. In fact, I could see myself assigning several sections to my undergraduate students studying gender in the economy, particularly chapter 8, which breaks down the nuances of the gender wage gap in a very clear way.

For fans of Goldin’s earlier work (namely Goldin 2006, 2014, and Goldin and Katz 2016), the book is a very welcome supplement. In fact, Goldin previews the first half of the book’s structure in her 2006 work, where she outlines four phases of women’s economic roles over the 1900s. In chapter 6, “Career and Family”, she specifically covers what she calls the ‘quiet revolution’ of the 1970s, a label she also used in her 2006 work. The second half of the book, where Goldin focuses on recent wage gaps between college educated men and women, closely resembles her 2014 work, with its special focus on greedy work in corporate, finance, and legal occupations.

Ultimately, I found Goldin’s book thought provoking in several ways. For instance, as Goldin was tracing the career and family trajectories across the five cohorts of women, I found myself asking both about the evolving role of a college degree and the evolving role of marriage. Goldin answered many of my questions about the evolving role of a college degree: for instance, she writes about how college was often used to get a job before marriage or to acquire backup skills if a marriage were to fail for women in the 1950s; but for women in the subsequent cohort, it was more about establishing an enriching career. However, I remained hungry for more context on shifts in the role of marriage. Like Lundberg and Pollak (2014) point out, the retreat from marriage since the 1950s has to do with shifts in the gains from marriage. They argue that as the ratio of men’s to women’s wage rates fell the traditional structures of gendered division of labor within the household weakened. Because of this, the primary gains from marriage shifted from the production of household services to financial investments in children and financial security, which Lundberg and Pollak (2014) find is particularly common among college-educated parents. Goldin’s work would have been well supplemented with more discussion on the evolution of marriage market dynamics, spending on children, and women’s intra-household bargaining positions over the 1900s. Each of these would have helped readers better understand the
constraints under which college-educated women have made choices about career and family.

Goldin makes it clear how women’s ability to have a career and family are really a very recent phenomenon. Still, enjoying both a career and family remain precarious, and Goldin leaves readers of *Career and Family* wondering what a hypothetical ‘Group Six’ will be faced with, especially in a world reshaped by COVID-19. I wonder how recent attacks on reproductive rights may also shape their careers and families. Further, Goldin’s focus on college-educated women in the United States excludes examination of many women whose abilities to juggle career and family are often even more challenging.

Goldin’s book includes very little discussion about low-income women, immigrant women, and women of color, despite the fact that many such groups are overrepresented in the market care infrastructure which have allowed so many college-educated White women to balance their career and family (Ferguson 2017). Ultimately, because the book discusses many of the other mechanisms which allowed college-educated women to hold both careers and families, I think the book would have benefitted from a more nuanced discussion of care infrastructures and their evolution across Goldin’s five cohorts, perhaps pulling from some of Rose’s (1999) work or discussion of global care chains (Ferguson 2017). In many ways, Goldin’s book felt like it was targeted as a guidebook to women like me: White women who are beginning their careers after completing several years of higher education in the United States and are starting to ask questions about family formation. Indeed, one well-written and concise source for many of our questions about the policies and cultural shifts which allowed college-educated women to reach today’s point is useful. Still, additional acknowledgement and a deeper consideration of how college-educated women have attained a balance of work and family off the backs of less-privileged women would have made the book even more useful.

Many higher-income, college-educated women benefit from keeping the price of outsourced domestic services low and, in turn, keeping the wages of domestic workers low and their positions precarious. This dynamic can lead to discrimination toward domestic workers and to ambivalence about the rights of women commonly employed in domestic service and care work. Goldin provides a limited analysis of this fact, which I found concerning. As women of my generation and education status begin to balance career and family, we cannot in good conscious continue
to focus solely on our own successes at the expense of other women. When readers consider the future of career and family that Goldin asks us to consider, I hope they keep the exploitation of women without college degrees at the forefront of their minds, even if not prompted by the text.

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It’s difficult to overstate the significance of uncertainty within the history of human thought. From Pyrrhonian skepticism to Cartesian anxiety, quantum mechanics to predictive processing, uncertainty is the spark that ignites doubt and innovation. It is in attempt to cope with uncertainty that philosophers and scientists share a common goal. Nowhere is this better demonstrated than in the study of rational decision-making.

*Taming Uncertainty* (TU) represents the forefront of decision science under uncertainty. Authored by Ralph Hertwig, Timothy Pleskac, and Thorsten Pachur, along with various members1 of the Center for Adaptive Rationality (Max Planck Institute of Human Development, Berlin), the 18-chapter volume is an interdisciplinary work of staggering achievement, integrating cutting-edge research from cognitive and social psychology, behavioral economics, and evolutionary biology. Those familiar with the Center (which is home to the Adaptive Behavior and Cognition [ABC] group, directed by renowned psychologist, Gerd Gigerenzer) will have a good idea of what TU is all about. For those uninitiated, this review will provide a crash course in the movement known as ‘ecological rationality’ and its newest developments.

Unlike formal investigations of uncertainty in economics and decision theory (Knight [1921] 2002; Savage 1972; Gilboa 2009), TU is deeply embedded in the tradition of Herbert Simon’s theory of *bounded rationality* (1955, 1982). Among other things, Simon emphasized that: (i) humans are cognitively, and hence computationally, limited; but (ii) despite these limitations, ordinary persons are still capable of (sometimes remarkable) feats of judgment and reasoning; So, (iii) the science of decision-making ought to be grounded in the empirical (psychological) investigation of these limitations and how persons overcome them. To this end, Simon

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1 For information about the members of the Center for Adaptive Rationality, visit: https://www.mpib-berlin.mpg.de/research/research-centers/adaptive-rationality/people.
posed that successful decision-making often involves the use of simple heuristics—i.e., problem-solving behaviors that reduce the burden of computing optimal outcomes. Decision heuristics are thus frequently described as ‘short-cuts’ and ‘rules of thumb’.

Following Simon, the formal (empirical) study of decision heuristics and their manifold practical applications has transformed how we understand human rationality—this is largely thanks to Gigerenzer and ABC researchers (see, e.g., Gigerenzer and Todd 1999; Todd and Gigerenzer 2007; Hertwig and Hoffrage 2013). In addition to refining and expanding the list of simple (‘fast and frugal’) heuristics, Gigerenzer has been instrumental in advancing the theory that humans are, in general, ecologically rational, that is, rational with respect to the repertoire of strategies they can deploy to meet choice-task demands. Central to the theory of ecological rationality is the belief that humans possess an ‘adaptive toolbox’—i.e., an assortment of cognitive and behavioral tools for navigating uncertainty and making ‘smart’ decisions. The toolbox metaphor is important because it illustrates that different tasks call for different tools (heuristics) to make appropriate judgments and decisions. It is this positive spin on our cognitive limitations that sets ecological rationality apart from other mainstream theories of rationality which treat humans as inherently irrational and unsuited to complex reasoning (cf. Gilovich, Griffin, and Kahneman 2002; Kahneman 2003).

How does ecological rationality relate to (taming) uncertainty? If Gigerenzer’s adaptive toolbox represents the first systematic approach to theorizing about how humans cope with uncertainty by utilizing fast and frugal heuristics, then TU represents the next stage of scientific development: not only does it expand the contents of the adaptive toolbox to include new environmental and social heuristics, but it offers compelling new insights about the socio-material basis and evolutionary origins of ecological rationality.

TU is loosely organized around four themes: the heuristic mind, the exploring mind, the social mind, and the unfinished mind. Chapters 2–6 (the heuristic mind) lay the groundwork for the book by revisiting the idea that simple heuristics are often as accurate as traditional maximization procedures for reasoning about choice when objective information, e.g., risk probabilities or statistics about world events, isn’t available. Chapter 2 uses simulations to demonstrate how simple heuristics, such as the
‘equiprobable heuristic’ and ‘natural-mean heuristic’, outperform expected utility theory when estimating the value of choice outcomes. Chapters 3 and 4 reflect on how these sorts of heuristics exploit environmental structures to overcome complexity. Chapter 3 presents the ‘risk-reward heuristic’ as a prime example of ecological inference-making in the absence of objective information. Chapter 4 extends this idea to non-decision-theoretic contexts, where uncertainty is amplified by real world contingencies and messiness. Both chapters appeal to the view that one's personal network and implicit understanding of social structures can provide a useful basis for making estimations and reasoning about issues of public relevance.

Chapters 7–11 (the exploring mind) constitute a major contribution of the volume. While it is known that simple heuristics have immense inferential value, it is still not well understood what grounds or explains their accuracy or appropriateness for different tasks. This is where the significance of search and learning procedures come into play for expanding the adaptive toolbox. Chapters 7 and 8 investigate what has come to be known as the ‘description-experience gap’. The gap refers to the observed difference in choice behavior among subjects who are given full information, e.g., probabilities of choice outcomes, versus those who have no prior information and must learn from experience alone. The key insight of these chapters is that traditional decision-theory can't account for search procedures that enable persons to make accurate inferences and decisions in situations of incomplete information. Chapter 9 applies the insights of the description-experience gap to the study of intertemporal choice, and it makes the case that irrational time preferences are, in fact, rational when viewed as adaptive responses to uncertain futures. Chapter 10 makes a similar case for understanding impulsive choice phenomena. Chapter 11 closes the section by reflecting on the nature of strategy selection, drawing a distinction between ‘rule-based’ and ‘exemplar-based’ heuristics for navigating new environments.

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2 The equiprobable heuristic calculates the arithmetic mean of all outcomes within each option and chooses the option with the highest mean. The natural-mean heuristic calculates the average of all outcomes sampled per option and divides the average by the number of sampled outcomes. It chooses the payoff distribution with the highest average outcome. For more information on these heuristics and other useful heuristics, see (30).

3 The risk-reward heuristic recommends that, when the probability of winning is unknown, infer that it is equal to the costs of the gamble (G) divided by the value of the prize (P). See Pleskac & Hertwig (2014) for more information.
Chapters 12–14 (the social mind), demonstrate the value of search and learning heuristics for reasoning in social contexts and, in particular, how they can be harnessed for strategizing in competitive situations. Chapter 12 looks at how ‘environmental uncertainty’ interacts with ‘strategic uncertainty’ when there is competition between individuals. The chapter explores several game-theoretic scenarios to argue that an ecological approach to uncertain decision-making will need to be sensitive to different forms of competition. These will be contingent upon factors like prior experience, familiarity with competitors and conspecifics, and availability of information. Chapter 13 considers the vices and virtues of aggregation heuristics, i.e., heuristics for managing large groups of agents. It defends two strategies, or ‘crowd rules’, that improve standard readings of the ‘wisdom of the crowd’: (rule 1) when in doubt, aggregate more rather than fewer judgments; (rule 2) instead of trying to understand the environment, use experience to adapt to it—and become more selective if the environment calls for it. Chapter 14 continues the focus on aggregation heuristics, venturing several ‘pedestrian heuristics’ to help manage problems that emerge in dense populations, such as car accidents and bottlenecks. Complex systems analysis reveals that monitoring for, e.g., ‘frequency of body contacts’ and ‘stop-and-go waves’ can be used to maintain crowd safety and create more efficient trafficking systems in a less computationally demanding way.

Chapters 15–17 (the unfinished mind) close out the book by reflecting on the evolutionary and developmental trajectory of the adaptive toolbox. Chapter 15 uses computational evolution—the simulated modeling of evolutionary processes with artificial (virtual) organisms—to investigate how human evolution (might) have favored the selection of adaptive decision-making traits. Among other things, the authors found that the virtual agents of their models went from producing random behaviors to executing strategies that enhanced their fitness to survive. Chapters 16 and 17 return to the present to investigate how cognitive development shapes adaptive decision-making through maturity. Chapter 16 investigates how adolescents deal with uncertainty in different age groups and promotes an ecological framework for interpreting and intervening in impulsive and imprudent teen behavior. Chapter 17 continues this line of inquiry to understand which mechanisms cause changes in risk-taking behaviors as humans age. It argues that because risk preferences are an expression of one’s uncertainty about future prospects, such preferences are better un-
understood as forms of heuristic reasoning. This has the following implication: while popular social psychological research tends to favor the generalization that risk-taking behavior declines with age, risk preferences are variant across changing socio-environmental landscapes.

This brings to a close my summary of the book’s content. Yet, for all its promise, there are aspects of TU I found puzzling. The first issue concerns how the authors depict the ecological basis of cognition. Hertwig, Pleskac, and Pachur begin the volume by claiming that “our vision of the mind is not that of an optimizing prediction machine” (23), a reference to Andy Clark’s *Surfing Uncertainty* (2015). In contrast to Clark, they argue that the mind is like a ‘good mechanic’ in that it “learns to select a tool that can address the problem at hand, repurposes existing tools when necessary, and designs new ones using the available evolved capacities of the human mind, such as recognition, emotions, and perspective taking” (23). While the book takes great pains to demonstrate the importance of the body and socio-material environment for supporting reasoning and decision-making strategies, there is virtually no engagement with the philosophical or cognitive scientific literature on the embodiment or extension of human cognition. To put this into perspective, *embodied cognition* posits that intelligent behavior is not merely causally dependent on a physical body but is constituted by skilled bodily actions—from perception and motor control to higher thinking and reasoning, cognition is fundamentally integrated with an organism’s body (Shapiro 2010; Varela, Thompson and Rosch 2017). *Extended cognition* takes this perspective a step further by positing that many forms of thinking and reasoning are not only enhanced by bodily skills but are refined and transformed through the use of social and technological artifacts outside the human body (Clark and Chalmers 1998; Clark 2008; Menary 2010).

TU’s failure to engage with the philosophical literature on embodied and extended cognition is unfortunate, not just because there are deep theoretical ties between ecological rationality and adaptive decision-making (Arnau et al. 2014; Mastrogiorgio and Petracca 2016)—exploring these ties would have provided useful explanatory support and nuance in explicating the value of heuristics for overcoming uncertainty—but also because decision researchers still don’t fully comprehend the role of the human body and the environment in modelling and explaining adaptive decision-making. On the one hand, the ‘cognitive’ science of ecological rationality is still relatively young and so is not to be blamed for not hav-
ing all the answers. Yet, it is also evident from reading TU that the ‘cognitive’ science of ecological rationality remains ill-defined with respect to its ontological and metaphysical commitments about the human mind vis-à-vis the body and the environment. As such, it seems to me the editors have missed a pivotal opportunity to better define the cognitive foundations of ecological rationality with respect to these commitments.⁴

The second issue is less philosophically disconcerting, though it builds on the previous inquiry. One of the principle aims of TU is to understand how individuals subjectively assess and respond to the environmental information they are given (this applies to both implicit and explicit information). Yet, for all its focus on how persons utilize search and learning heuristics to overcome uncertainty, there is surprisingly little discussion of the psychological concept of framing with respect to how individuals subjectively interpret information. This is odd. Afterall, supporters of ecological rationality readily admit that the tools of the adaptive toolbox are both the causes of and solutions to cognitive limitations. It would have been interesting to see how researchers draw a distinction between adaptive response and subjective interpretation of the same information. I suspect the topic was avoided in part because the concept of framing is typically cast negatively, being associated with perceptual biases and suboptimal decision-making (Kahneman and Frederick 2007). This obviously runs contrary to the theme of TU, which is that the human mind appears quite rational once we adopt an ecological perspective. It’s a shame this topic wasn’t discussed more explicitly as there is clearly a need to investigate the positive aspects of framing in the study of rational decision-making (Bermúdez 2020).

In sum, there’s no doubt that TU will inspire readers across disciplines, especially those who have interests in the behavioral foundations of human rationality. Although one would benefit from having exposure to economic and/or decision-theoretic research and debates about bounded rationality, such a background is not necessary to appreciate the book’s many merits. Hertwig, Pleskac, and Pachur have done a remarkable job not only in promoting ecological rationality as a worthy alternative to the heuristics and biases tradition (cf. Kahneman 2003, Kahneman and Frederick 2007), but they have presented a clear and comprehensive overview of the adaptive toolbox and its many practical applications.

⁴ Since the publication of Taming Uncertainty, there have been a few attempts to determine in how far ecological rationality is, or should be seen as, committed to embodied and extended cognition (Gallese et al. 2020; Petracca 2021). These represent exciting developments in the cross-disciplinary study of adaptive decision-making.
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José Luis Bermúdez’s *Frame It Again* provides an important antidote to the story that we are fundamentally irrational by challenging one of its central tenets, the principle of *extensionality* (i.e., that “valuations [should not] depend upon how we describe different courses of action or their alternative possible outcomes” (67)). This principle implies that rational individuals should not be susceptible to so-called framing effects. However, by arguing that taking on different perspectives is a requirement of rationality, Bermúdez builds the case that framing is an essential ingredient of good decision-making. Different values are salient in different frames. Yet, these values can, for instance, be incommensurable. Thus, we *should* end up ranking the same outcome differently under different frames. Bermúdez’s provoking argument is laid out with impressive knowledge and provides entertaining and accessible introductions to rational choice theory, economics, and cognitive science.

He starts the book with several well-illustrated examples of framing effects. While these illustrations initially seem conceptually disconnected, Bermúdez argues that they can be unified by a minimal definition of frames as a “schemata of interpretation” (14), where a wide range of phenomena may count as such. With the concept itself clarified, Bermúdez highlights that experimental researchers from different disciplines have contributed to telling a story he calls the “litany of irrationality” (10), i.e., the view that “that human beings are fundamentally flawed reasoners, regularly contravening the basic principles of rationality” (8). For those who subscribe to this narrative, susceptibility to a framing effect is considered a hallmark of irrationality.

Consequently, Bermúdez highlights next why framing effects are considered irrational by those intoning the litany of irrationality. In a nutshell,
Bermúdez presents two reasons. The first is that the set-theoretic formulation of decision theory snuck the principle of extensionality into our thinking about rationality, as extensionality is also the most basic axiom of set theory, i.e., “there cannot be two different sets with the same members” (78). The second reason is that it is usually assumed that we need extensionality for the standard decision-theoretic axioms like transitivity to exert their normative force.

With the basics clarified, Bermúdez lays out the concepts that will serve his main argument. He argues that framing can serve the vital task of making a decision maker take another perspective. Yet, the exercise of framing can lead to quasi-cyclical preferences. You have quasi-cyclical preferences if you prefer A to B and B to C, even though you know that A and C are the same outcome yet differently framed. These preferences can nonetheless be rational if we are in what Bermúdez calls an ultra-intensional context where the extensionality principle does not apply.

Based on these foundations, Bermúdez argues for the key component of his theory of rationality: the due diligence requirement. One way of putting this requirement is that an agent is not just required to choose optimally with respect to an already well-defined decision problem. Instead, an agent is also required to be “appropriately sensitive to as many potential consequences of different courses of action available to them as possible” (121) when setting up a decision problem. For Bermúdez, preferences are grounded in valuations, and valuations, in turn, depend (at least partly) on our emotional responses. Framing can highlight different aspects of an outcome towards which it is rationally permissible to have different emotional responses. These various reactions, however, can result in quasi-cyclical preferences. Yet, as we are rationally required to explore different aspects of an outcome, we can end up in a situation where it is rational to have quasi-cyclical preferences. Bermúdez adds the constraint that having quasi-cyclical preferences should not force us to adopt contradictory beliefs. Yet, Bermúdez is convinced that such preferences can often facilitate good decision-making. The remaining part of the book is aimed at several illustrations of how this is possible.

One illustration of the power of framing is team reasoning, i.e., the idea that group identification allows agents to reach intuitively rational solutions, which conventional game theory would preclude. While Bermúdez thinks that work on team reasoning already does much to illustrate the power of framing by contrasting the ‘I’-frame with the ‘We’-frame, he argues that there are substantial shortcomings in the literature.
First, he holds that we lack an adequate explanation for why it is rational to switch from the 'I'-frame to the 'We'-frame. Second, Bermúdez argues that the literature focuses too much on pareto-optimality when considering which option should be preferred under the 'We'-frame. An outcome is pareto-optimal if it is impossible to make some individuals better off without making some other individuals worse off (Mas-Colell, Whinston, and Green 1995, 307). Bermúdez proposes that we can solve both problems via the concept of frame-neutral values. The rough idea is that there are reasons behind our preferences that apply to how we rank outcomes in the 'I'- and the 'We'-frame. For instance, one reason we prefer an outcome in which both we and our neighbor dig snow on our street could be that it seems unfair if we do all the digging alone. Yet, once we focus on the value of fairness, we may realize that it is better served by the 'We'-frame as team reasoning allows us to reach the solution under which the burden is shared, while the 'I'-frame precludes this outcome. As a result of having both the 'I'- and the 'We'-frame available, we may end up with quasi-cyclical preferences. Nevertheless, selecting between these preferences, the frame-neutral value of fairness allows us to choose the option only available under the 'We'-frame. Hence, frame-neutral values and (rationally permissible) quasi-cyclical preferences can explain how we can reason ourselves into the 'We'-frame. Moreover, Bermúdez thinks that sticking with our quasi-cyclical preferences (instead of completely dropping the 'I'-frame preference) allows us to reason ourselves back out of the 'We'-frame if the situation calls for it (e.g., if we would otherwise be exploited). Finally, frame-neutral values can help to rank multiple pareto-optimal solutions under the 'We'-frame and, therefore, allow us to overcome the second problem Bermúdez identified.

Another illustration concerns discursive deadlocks. Bermúdez attributes such deadlocks in public debates (e.g., gun control in the USA) to a clash of values associated with different frames. The claim is that certain issues are highly complex and cannot adequately be captured by merely one description (i.e., frame). On top of that, different framings will imply different actions. Multiple descriptions, however, are not necessarily inconsistent but rather complement each other to the complete picture. As overcoming these deadlocks requires us to reason across frames, Bermúdez proposes requirements of rationality that are sensitive to the importance of framing in good decision-making. One crucial distinction that he proposes is between factual and non-factual beliefs. Some beliefs should not change with frames (factual beliefs), and some connected to
value judgment may change with different frames (non-factual beliefs). “A factual proposition is one whose truth or falsity can be determined by standard techniques” (229), with a standard technique being context-dependent (233). Thus, non-factual beliefs may be contradictory across frames. Yet, according to Bermúdez, we are rationally required not to believe implicitly or explicitly any false factual propositions.

To explore how we can reason across frames to resolve value conflicts, Bermúdez connects the discussion about requirements of rationality with research in cognitive science. In this regard, he points out how cognitive mechanisms like reflection, reason construction, imaginative simulation, and others can figure into a productive discourse of highly complex issues. The argument here is that a reasoner who has well-developed cognitive capacities and follows processes of reasoning flawlessly might still fail to satisfy the coherence requirements laid out by orthodox requirements of rationality, i.e., they might develop quasi-cyclical preferences.

What we have outlined here is, of course, only a rough sketch of far more detailed arguments. Nevertheless, in the spirit of further engagement with the book’s topic, we wish to offer three critical remarks. The first remark concerns the chapters on game theory and team reasoning. While Bermúdez is highly careful in his outline of various research programs, we think his engagement with team reasoning provides an exception. For instance, as mentioned, Bermúdez criticizes Bacharach’s (2006) alleged focus on pareto-optimality as it is only sufficient in a subset of games (e.g., Hi-Lo) but will fail in others (e.g., Game of Chicken), where there are multiple pareto-optimal outcomes. However, the problem with Bermúdez’s critique is that, for Bacharach, pareto-optimality is only a minimal requirement on the team function. While it is already enough to identify a clear outcome in coordination games (i.e., games like Hi-Lo where agents merely benefit from coordination), we need additional assumptions to solve mixed-motive games (i.e., games like the Prisoner’s Dilemma, or the Game of Chicken where there is also some conflict of interests). In this regard, Bacharach (2006, 88) hypothesizes that often “principles of fairness such as those of Nash’s axiomatic bargaining theory will be embedded” in the team’s utility function. In light of this, one may wonder how precisely Bermúdez’s appeal to fairness as a frame-neutral value is meant to go beyond the existing literature.

Our second remark concerns the treatment of rationality. When Bermúdez talks about rationality, he is “talking about a process—the pro-
cess of reasoning” (14). He holds that certain standards govern this process, and rational thinkers should “aspire to abide by those laws and principles” (15). Therefore, we take it that the requirements Bermúdez proposes throughout the book are meant to explicate these standards of reasoning. Yet, we wonder whether all of the requirements he proposes can be understood this way. For instance, Bermúdez argues that rationality requires a model frame-sensitive reasoner not to implicitly or explicitly believe any false factual propositions. While having only true factual beliefs is certainly highly desirable, it appears to be an unreasonable standard if we construe it as a law that should govern reasoning processes. After all, also the most law-abiding reasoner who processes all their evidence correctly may end up with false factual beliefs if we only provide them with inaccurate information. We, therefore, think that Bermúdez could have spent more time delineating different notions of rationality. Rationality may well be an umbrella-term encompassing distinct principles (in different contexts). For example, one could understand agents as rational if they use rules that are, in expectation, beneficial for them, e.g., heuristic in the ecological rationality tradition (Gigerenzer and Todd 1999). Alternatively, one may understand agents as being rational if their attitudes relate to the world in a certain way (e.g., only having correct factual beliefs) or if they respond correctly to their reasons (Kiesewetter 2017). Moreover, in line with traditional economics, one can look at how attitudes cohere with each other (Broome 2013). It would, therefore, be helpful if Bermúdez were more explicit concerning his stance on these various conceptions of rationality. Especially because rationality is a thick and politically charged term, his process perspective may otherwise fail to convince those who think about rationality differently.

This brings us to our third remark relating to the due diligence requirement. This requirement reminds us of well-known problems in decision theory, such as the multi-armed bandit problem. This problem is characterized by situations where the consequences of each choice are only partially known, and an agent needs to decide how much of their resources to allocate to get more information on the consequences of a potential choice. Researchers occupying themselves with this problem typically hold that there is a uniquely rational response to how much an agent should explore. We, thus, get something that sounds like a due diligence requirement for utility-maximizing agents. Yet, Bermúdez’s concept of rationality is process oriented and not one of utility maximization. Hence, we would like to see how Bermúdez would determine precisely
what due diligence requires of us in specific contexts. This would be especially interesting as his requirement partly defines rationality and is not implied by rationality, as researchers in the literature around multi-armed bandits assume. For example, you and I may disagree on what is implied by being appropriately sensitive to the possible consequences of flying to Spain, e.g., finding out your carbon footprint. Bermúdez’s theory of rationality will not necessarily help us to settle this disagreement as the due diligence requirement is not derived from any other principle of rationality but defines part of what it means to be rational. Yet, it seems intuitive to expect disagreement concerning such issues, eventually leading to different numbers of frames considered and diverging decisions. Thus, if our theory of rationality cannot provide us with a notion of appropriateness, we need to reach beyond rationality to settle such conflicts. Our question then is for which standards we should reach.

These three remarks should not distract from the fact that Bermúdez has written a remarkable book. We highly recommend it to everyone seeking a fresh perspective on framing and rational decision-making. Even though we expect that Bermúdez’s arguments will not convince everyone, due diligence requires any scholar of rational decision-making to engage with this work, which has the potential to spark an exciting research agenda on the power of changing perspectives.

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I. INTRODUCTION AND SUMMARY
A central notion of decision theory is that of credence: the idea that beliefs come in degrees. I could be 80% confident that there is a sheep standing in the field before me and 20% confident that it is, instead, a dog disguised as a sheep. Not all credences make sense, however. My credence that, in this field, there is a sheep hidden behind a disguised dog cannot be greater than my credence that there is at least one disguised dog. For the former cannot be true unless the latter is as well. To understand how credences relate, a rich academic literature has proposed several laws of credence: these are laws that rational persons should abide by when forming credences. More recently, some have sought to establish such laws with stronger formal foundations. Richard Pettigrew’s *Dutch Book Arguments* constitutes a remarkable attempt to define and refine laws of credence using Dutch Books—that is, betting situations in which the gambler loses money for sure. By analysing these situations, this book skilfully progresses towards the formal establishment of some laws of credence. It thereby contributes in many ways to a larger effort aimed at ascertaining the cogency of formal arguments for decision theory (see Vineberg 2022).

Dutch Books can be useful formal tools in the following manner. A Dutch Book refers to (a series of) bets that, once accepted, ensure that a gambler loses money. Still, a gambler’s credences can be so that she must or might nevertheless accept the Dutch Book. She is thus guaranteed to lose money. Since her loss is due to the state of her credences, they appear faulty. As such, Dutch Books can reveal different ways in which one’s credences are irrational, from a formal perspective. Several laws governing these credences can then be defined so that, if violated, the gambler will incur a loss.

Establishing the intuitiveness of these laws is the first challenge that the book takes up. Following a brief overview, the second section discusses five laws of credence. For each, the reader follows a protagonist
who accepts losing bets. Take Norma, a climate scientist, who has to estimate the likelihood of three possible worlds: the average temperature increase in one hundred years will either be Low (at most 0°C), Medium (between 0°C and 1°C), or High (1°C and above). Surprisingly, the sum of her credences in either Low, Medium, or High does not equal 100%, but only 90%. Since all possible worlds are considered, why is Norma not fully confident that one of them will obtain? Such credences seem irrational. With clarity and simplicity, Pettigrew shows that they are indeed irrational: there exists a Dutch Book against Norma that she must accept even though it results in her losing money. As such, he introduces the first law of credence, Normalisation, according to which one’s credence in a necessarily true proposition should be maximal (that is, equal to 100%). Had Norma abided by Normalisation, she would not have been susceptible to a Dutch Book loss.

Similarly, Pettigrew adds protagonists to propose four other laws of credence. Finella breaks Finite Additivity: her credences in Low and Medium are 30% and 40% respectively, so her credence in both should be the sum of her credences in each (i.e., 70%), but they are not. Constanze breaks Countable Additivity: her mistake is akin to Finella’s, except that her credences concern infinitely many propositions. Reg fails to observe Regularity: he cannot rule out the possibility that the average temperature increase will be High, so his credence in this possible world should be positive, yet it equals zero. Finally, Pritpal breaks Principal Principle: he has a positive credence that the objective chance that a storm reaches the United Kingdom by midnight is 50%, yet his credence in this proposition conditional on its objective chance is not equal to 50%, but it should be.

While these laws are first intuited, the third section refines them until a more rigorous formulation of each is reached. Only one (Countable Additivity) cannot be worked out, and so it is abandoned. Moreover, Normalisation and Finite Additivity are considered together as Probabilism. Thus, the section culminates in three final formulations of Dutch Book arguments (33–34). While the arguments are elaborate, their common structure can be summarised in this way:

(DB1) Suppose that a gambler’s credences require or permit her to accept certain bets; then,

(DB2) If and only if her credences violate one of the three laws, they can be exploited to make her lose money;
(DB3) If her credences are exploitable (and there exists no alternative, unexploitable credences), then her credences are irrational.

The fourth section offers a pleasant *caesura* during which the reader is introduced to diachronic laws of credence. What happens when a gambler updates her credences after she acquires new evidence? Are there Dutch Books in such situations too? Pettigrew suggests that anyone who updates their credences in light of new evidence could be subject to a Dutch Book. Nevertheless, he refrains from concluding that updating is irrational. Rather, he recommends focusing not on actual updating behaviour but on the updating *rule*, as this move enables to make a more fine-grained distinction between rational and irrational updating.

The fifth section resumes the discussion where the third section left it. It addresses two challenges raised against the first premise (DB1) of the final Dutch Book arguments. The first one is due to Hedden (2013) who argues that, when credences are non-probabilistic, the betting behaviour prescribed under DB1 is no longer aligned with that of expected utility theory. In such cases, DB1 may arguably provide a flawed account of rationality. Pettigrew denies that expected utility should be the yardstick against which other accounts, such as DB1, are evaluated. DB1 could offer a plausible account of decision theory as well. Moreover, with non-probabilistic credences, expected utility might in fact lead to making suboptimal decisions, unlike DB1. Interestingly, Pettigrew goes on to elaborate a disjunctive norm combining DB1 and expected utility theory which, he argues, provides a better picture of rationality.\footnote{The disjunctive norm relies almost exclusively on expected utility theory. However, when considering a bet on two propositions, one with a 0% credence and another with a positive credence, DB1 should be used. This is so because in such cases expected utility theory, unlike DB1, could lead a gambler to select a dominated act.} The second challenge pertains to risk-weighted expected utility. Within such a framework, a gambler can remain unexploitable although she breaks some of the laws formulated above. Here, however, the discussion is not pursued.

Likewise, the sixth section addresses an objection to the final Dutch Book arguments which, this time, concerns their third premise (DB3). It thereby offers an insightful reflection on the connection between exploitability, consistency, and rationality. After reviewing and refuting several arguments defending the claim that inconsistent preferences are irrational, Pettigrew develops a pragmatic argument for *Probabilism* (i.e., *Normalisation* and *Finite Additivity*) before extending it to the other laws of credence. In a nutshell, he shows that any non-probabilistic credence
function can be replaced by a probabilistic credence function providing greater utility. Hence, Probabilism better guides our gamblers.

The seventh section conducts a series of ‘robustness checks’ to evaluate Dutch Book arguments’ potential for generalisation, and finally, the eighth section compiles the mathematical proofs used throughout the book.

In brief, this concise book guides the reader nicely through the literature surrounding Dutch Book Arguments and presents interesting solutions to several issues. With a compelling overall view, some smaller points may appear less convincing. I highlight three below.

II. The Relevance of Dutch Book Arguments

If one subscribes to the view that a book must discuss its own relevance, the conciseness of Dutch Book Arguments might be gained at the expense of presenting its usefulness: why care about Dutch Book arguments in the first place? The book is mostly elusive about this question, even though Dutch Books have been used in diverse ways in the academic literature. One illustration of this can be found in probability theory. In this field, several established theories of probability adopt an objective interpretation of probabilities. Roughly put, this means that, when a flipped coin has a 50% probability of landing tails-up, this probability measures a feature of the world—this feature obtains independently of my beliefs about what the outcome will be. Nevertheless, objective theories of probability encounter difficulties that so far have not been resolved satisfyingly (see Resnik 1987 for a discussion).

Alternatively, probabilities can be interpreted subjectively. That is, a 50% probability that my coin will land tails-up reflects my confidence—i.e., my credence—in this statement. If, for example, I have reasons to doubt the fairness of the coin, the probability could vary. In short, credence shapes probability. Against this backdrop, Dutch Book arguments can help clarify how credence and probability (should) relate, thereby laying the ground for a deeper understanding of probability. More generally, Dutch Book arguments can be applied across decision theory for a variety of purposes (see Vineberg 2022 for an overview).

III. Epistemically Possible Worlds

Another point concerns possible worlds. As noted, Norma defines what credences she has given three possible worlds: Low, Medium, or High. But what does a ‘possible world’ amount to? In subsection 3.5, Pettigrew
defends the idea that the modality of possible worlds should be understood epistemically or logically, but not metaphysically. He reasons with the following example:

Southey is 10% confident in the proposition $I$, which says that Charlotte and Currer are the same person. Now, in fact, $I$ is true. And so it is metaphysically necessary. But it is not logically necessary. And it is not epistemically necessary either, providing Southey does not have definitive evidence that Charlotte and Currer are the same person. [...] I propose the following: $W$ is the set of all and only the worlds that are epistemically possible for you at the time in question. Thus, Southey is not dutchbookable in [this example], for while it is not metaphysically possible for Charlotte and Currer to be different people, it is epistemically possible for Southey, and thus a world in which $I$ is false is an epistemically possible world for him. [...] In order to establish that someone is irrational, we have to show that their credences demand that they accept a series of bets that loses them money at every world that for all they know is the actual world. (21, 23)

But this reasoning is too quick. Consider again Norma. She, too, has to set her credences given three possible worlds, only one of which is metaphysically possible. However, she could argue that, for all she knows, the evidence she has does not justify raising her credences so much so that their sum equals 100%. Indeed, it remains open in her case to determine the benchmark against which a piece of evidence is compelling enough to raise her credences. She could for instance argue that, so far, her evidence only justifies a credence of 30% in each possible world, amounting to 90% in total. More would be epistemically unwarranted. On this argument, her credences appear epistemically rational although they do not obey normalisation. Norma seems rational after all.

Still then, it could be retorted that this is a matter of logic: the proposition that global average temperature will either be Low, Medium, or High is simply a logical necessity. It does not matter whether Norma knows this: betting against a logical necessity is always irrational. In other words, the modality of possible worlds should be understood logically; if none is available (as is the case for Southey), then it should be understood epistemically.
But bringing in logical necessities might blur rather than clarify the issue, for logical and epistemic modalities may clash.\textsuperscript{2} Consider the following situation: imagine that Norma invites all her colleagues to a restaurant on the first of June, the date of her birthday. Twenty-three people come (including herself). During a conversation with Reg, she learns that he, too, was born on a first of June. Surprised by this fact, they discuss how unlikely it must be, given this small gathering. Each of them takes a guess, but they disagree: Norma believes that the chances are below 30\%, while Reg thinks they are higher. Eventually, he proposes to bet on the probability that, in this gathering, two of them share the same birth date. Norma quickly accepts (she bets that it is below 30\%) before drawing her phone’s calculator. The result is known as the Birthday Paradox: for only twenty-three people, the probability that two of them share the same birth date is in fact over 50\%.

Now, was Norma irrational in accepting this bet? After all, she bet against a logical necessity, and so from a logical perspective she seems irrational. At the same time, she could not mentally calculate the probability quickly enough before accepting the bet. In other words, she did not know what the logical necessity was (nor did Reg). From an epistemic perspective, accepting the bet is rational. In sum, epistemic and logical modalities may clash. Their relation must be specified further.

\textbf{IV. PREVIOUS BETS}

My third and last point concerns previous bets (subsection 3.4). The claim Pettigrew defends is that previous bets matter: they can make it reasonable to accept a Dutch Book. He illustrates this point with Finella, a colleague of Norma’s. Finella has a 30\% credence in Low and a 40\% credence in Medium. As a result, she sells two £100 bets, one for £35 on Low and one for £45 on Medium. Moreover, her credence in the disjunction Low ∨ Medium is 90\%. This means that she fails to abide by Finite Additivity (given that 30 + 40 = 70). Based on this latter credence, she considers buying a third £100 bet on Low ∨ Medium for £85. Here are her net gains given each possible world, dependent on whether she refuses or accepts the third bet:

\begin{itemize}
  \item \textbf{Refuses the third bet:}
    \begin{itemize}
      \item<()>\textbf{needs to pay £85}
      \item<()>\textbf{gains £100}
      \item{}\textbf{Net gain: £15}
    \end{itemize}
  \item \textbf{Accepts the third bet:}
    \begin{itemize}
      \item {}\textbf{needs to pay £85}
      \item {}\textbf{gains £100}
      \item {}\textbf{Net gain: £15}
    \end{itemize}
\end{itemize}

\textsuperscript{2} This issue arises because logical necessities are not always obvious; some necessitate a fair amount of cogitation. Such a critique is akin to that repeatedly raised against expected utility theory, as it assumes that individual agents can readily calculate and compare the utility of all prospects under consideration, even when this process requires complex computational power (Simon 1955).
Dutch Book Arguments / Book Review

Table 1: Finella’s Net Gains.

<table>
<thead>
<tr>
<th></th>
<th>Refuse</th>
<th>Accept</th>
</tr>
</thead>
<tbody>
<tr>
<td>LOW</td>
<td>(\£(35 - 100 + 45) = -\£20)</td>
<td>(\£(35 - 100 + 45 - 85 + 100) = -\£5)</td>
</tr>
<tr>
<td>MEDIUM</td>
<td>(\£(35 + 45 - 100) = -\£20)</td>
<td>(\£(35 - 100 + 45 - 85 + 100) = -\£5)</td>
</tr>
<tr>
<td>HIGH</td>
<td>(\£(35 + 45) = \£80)</td>
<td>(\£(35 + 45 - 85) = -\£5)</td>
</tr>
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Intuitively, given Finella’s credences, there is a Dutch Book against her: by accepting the new bet (which Finella is required to do given her credence in \(\text{Low} \lor \text{Medium}\)), she incurs a sure loss. So, it seems, she should refuse it. But Pettigrew argues that this is mistaken. First, he observes, Finella is in fact choosing between, on the one hand, a sure loss of \(\£5\) and, on the other, a bet on either a greater loss (\(-\£20\)) or a large gain (\(\£80\)). Finella's attitude towards risk is however not discussed here. Rather, Pettigrew suggests that her expected net gains are higher if she accepts the third bet (as it amounts to \(-\£5\)) than if she refuses it (\([0.9 \times -\£20] + [0.1 \times \£80] = -\£10\)). She should then accept it. Therefore, he concludes, previous bets can make it rational to accept a Dutch Book.

To reach this conclusion, Pettigrew uses Finella’s 0.9 (or 90%) credence in \(\text{Low} \lor \text{Medium}\) and infers that in \(\text{High}\) as equal to 1 - 0.9 = 0.1. but since Finella breaks \textit{Finite Additivity}, there exists another approach through which we can, assuming completeness, determine her ‘full’ credences. We could instead use her 0.3 credence in \(\text{Low}\), her 0.4 credence in \(\text{Medium}\), and infer that in \(\text{High}\) as equal to 1 - 0.3 - 0.4 = 0.3. With these in mind, Finella's expected net gains are higher if she refuses the third bet (\([0.3 \times -\£20] + [0.4 \times -\£20] + [0.3 \times \£80] = \£10\) than if she accepts it (\(-\£5\)), and so, clearly, she should refuse it. Given these two approaches, it may seem arbitrary to calculate the expected net gains of several bets using only a portion of the relevant credences.\(^3\)

Pettigrew does not discuss this second approach because, in his view, Finella considers whether or not to accept the third bet; therefore, only her credence in \(\text{Low} \lor \text{Medium}\) should matter in calculating the expected gains of this bet (21). But Finella is not considering whether or not to accept the third bet \textit{tout court}, she is considering whether or not to accept

\[^3\text{This seems even more counterintuitive when we calculate Finella’s expected gains in case she refuses the third bet, for in this case the expected gains amalgamate two bets (on \(\text{Low}\) and \(\text{Medium}\)) and a credence that initially played no role in accepting these bets.}\]
it \textit{given the previous two she has already accepted}. This is why we calculate here the expected gains not only of the last bet, but of all three. Yet the claim that Finella must use her credence in \textit{Low v Medium} to determine whether the third bet is worth accepting does not imply that she must also use this credence to calculate the expected gains of all three bets. As such, Pettigrew seems to rely on an unstated assumption: that the credence(s) relevant for \textit{present} bet(s) must also be used to calculate the expected gains of \textit{both past and present} bets. Thus, no argument is provided for this assumption, on which the claim that 'previous bets matter' hinges.

In spite of this, \textit{Dutch Book Arguments} makes numerous convincing cases, spanning a large array of related topics. If the book is of interest to academics across disciplines, it may also serve well those who enjoy leisurely gambling, and possibly future bookies.

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Inequalities between old and young seem different from inequalities between other social groups. This is because the old used to be the young, and the young will mostly live to become old. This contrasts with other types of groups (race, gender, social class, etc.), where a change of membership tends to be difficult or costly, if not impossible. The inevitability of aging suggests that inequalities between age groups are offset as people move from one age group to another: if the young lack access to benefits enjoyed by the old, then they may gain these benefits once they are older themselves. And if certain opportunities are harder to access at an older age, it may be that older people were given these opportunities earlier in life. Generalising, inequalities between age groups might be tolerable so long as there is “complete life” equality (37), this being the absence of inequality given people’s lives as a whole, even if there is some inequality between age groups.

Here, two questions can be posed that play a central role in Juliana Bidadanure’s excellent book. These are (1) how the idea of complete lives equality should be analysed, and (2) whether complete lives inequality is, under examination, enough to render age group inequalities morally unproblematic. In short, Bidadanure’s view is that complete lives equality is both plausible and yet not exhaustive as a principle of justice across ages. While complete lives equality represents a plausible distributive conception of equality, some age group inequalities have a relational character whose significance endures even when complete lives equality obtains. The first part of the book (chapters 1 to 4) develops a general account of age justice, engaging with foundational questions around complete lives equality and age group equality. The second part (chapters 5 to 7) applies this general approach to some specific examples of age group inequality.

Chapter 1 reviews the way in which age is unlike gender and race (etc.) and establishes some important conceptual points, such as the difference between birth cohorts and age groups. Chapter 2 clarifies and defends
the idea of complete lives inequality. Importantly, complete lives equality leaves much open as to how to distribute resources within a person’s life. Building on some influential work by Norman Daniels, Bidadanure seeks to defend principles of “lifespan” prudence (51). Here there are two principles that work together. First, the Lifespan Sufficiency Principle, on which “institutions must ensure that all age groups have enough to enjoy a normal range of opportunities at each and every stage of their lives” (56, 59–60) and the Lifespan Efficiency Principle: “Institutions should allocate resources earlier rather than later in the lifespan when doing so would increase diachronic returns significantly (hence maximising lifespan utility)” (64). Bidadanure works through some examples, such as healthcare and education spending, to illustrate how these principles work in practice. The chapter concludes by defending the more basic idea of complete lives equality from some objections not met in Daniels’ work.

Chapter 3 then makes the case for why we might still care about age-group inequalities. Bidadanure relies partly on hypotheticals, like the married couple who alternate between positions of master and slave in a way that balances out over the duration of their relationship (87). But she also draws on significant real-world cases. Principal among these are cases of “infantilization by age” (105), both of elderly persons and of young adults. Bidadanure concedes that some age group inequalities might be addressed by proper implementation of the lifespan prudence principles. For example, much treatment of the elderly, particularly in residential care facilities, may violate the lifespan sufficiency principle. Nevertheless, she argues, failure of young and old to interact as equals remains a core feature of some age group inequalities.

Chapter 4 concludes the book’s more foundational section by summarising points made in the preceding chapters and expands on some points regarding ways in which complete lives equality may not in fact obtain. Here the focus is on ways in which today’s young adults, the millennial birth cohort, are worse off than today’s post-retirement birth cohort—the baby boomers. Age group inequalities like these shape the agenda of the book’s second, more applied part. In chapter 5, Bidadanure defends “the youth job guarantee” (154), whereby young adults receive a degree of prioritisation with respect to policies aimed at assisting the unemployed. This is accompanied by a discussion of mandatory retirement. Chapter 6 compares basic income proposals with the alternative of basic capital (or stakeholder grant). The riskier nature of basic capital likely means that UBI is preferred by the lifespan sufficiency principle. On the other hand,
basic capital does more to address inequalities in longevity, by ensuring that those who die young do not miss out as much as they do under UBI, which favours those already fortunate enough to live longer (196–197). In the end, Bidadanure considers some hybrid proposals, including one where young adults have the option to ‘mortgage’ some of their future UBI payments so as to access a lump sum earlier in life (206–208). Chapter 7, the last, defends the case for youth quotas in parliaments. While much legislation stands to impact disproportionately on the young, the composition of legislative bodies is subject to an overrepresentation of older people (212). According to Bidadanure, a requirement that some members of parliament come from younger age groups would do much to enhance cognitive diversity in legislative decision making, and might increase youth participation in elections in jurisdictions where voting is not compulsory.

Upon finishing this book, one is struck by how much ground Bidadanure manages to cover, both with respect to the foundational issues discussed and their various applications. Despite the huge amount of writing in recent decades on the value of equality and how it should fit into a theory of justice, Bidadanure finds room to develop new ideas and avoids sacrificing excessive space to mapping the copious internal disagreements of contemporary egalitarian political philosophy. A further strength is the thoroughness with which Bidadanure anticipates interesting objections to the positions she endorses, and her ability to find often ingenious ways to address them. Bidadanure has provided an impressive analysis of justice across age groups, one that is comprehensive in scope while maintaining a high standard of argument and attention to detail at each stage. The book’s moderate length (under 250 pages) makes this a remarkable achievement.

Nonetheless, I want to make some critical points on relational equality between age groups, and particularly Bidadanure’s concerns about what she calls “prolonged parental dependency” (136). Here, I think at least two dimensions of dependency risk being insufficiently distinguished. One is financial dependency, and the other is involuntary cohabitation, as when young adults unable to leave the parental home are denied “spatial independence” (137). It is not obvious that these are equally problematic, or problematic in the same way, from the perspective of relational equality. Bidadanure speaks quite generally of the idea that young adults might “live under the authority and control of their parents” (140). This can occur when parents retain an objectionable degree of oversight concerning
an adult offspring’s lifestyle, and retain authority over the way in which
the physical living space is managed. I agree with Bidadanure that invol-
untary cohabitation will tend to delay important milestones in life, such
having one’s own children (137). But the dependency associated with co-
habitation might contrast, normatively, with narrower forms of financial
dependency, as when parents help with the down payment on an off-
spring’s home purchase, or higher education costs. These may still count
as forms of dependency, at least if there will be a considerable cost to the
offspring if such support were to be withdrawn, and perhaps especially
where parental support comes with stipulations, say, about what (not) to
study at university. But in other cases, parental support might be pro-
foundly enabling of an adult offspring’s pursuit of their own life plans. I
should stress that I do not take Bidadanure to be committed to an im-
plausibly one-dimensional view of parental dependency. But greater at-
tention to the differences here might be instructive as to exactly when
relational equality between parents and adult offspring is undermined.

It may also be that the proper understanding of relational equality can
be illuminated by Bidadanure’s focus on cases involving how adult chil-
dren relate to their parents. As Bidadanure notes, the concern with
strongly distributive conceptions of equality has largely been with their
(alleged) inability to take oppression and social hierarchy sufficiently se-
riously (96). It bears emphasising here that standard accounts of social
hierarchy typically place considerable emphasis on the idea of group dif-
ference rather than on isolated relationships between individuals. A core
feature of hierarchies of race and gender is the tendency for the treatment
of individuals to track perceived group membership. Stereotyping, for ex-
ample, is largely understood in terms of apparent group membership be-
ing used to explain and/or predict an individual’s behaviour. I am not
sure that we should think of inequalities between age groups as really
between entire groups. An adult offspring who remains housed by con-
trolling parents is arguably stuck in a problematic relationship with their
parents. But even when this sort of relationship is replicated in many fam-
ilies, we might still see it as a recurring case of a relationship between
similarly situated individuals, rather than a genuinely inter-group rela-
tionship. One reason we might say this is that the mechanisms by which
group-based oppression can emerge may not be present: I find it hard to
see how an increased tendency for young adults to endure involuntary
parental cohabitation is something that can lead them to be negatively
stereotyped by members of the wider population. Again, no substantive
criticism of Bidadanure here, who I think does not take an explicit position on whether we should think or relational inequalities between age groups as irreducibly or paradigmatically group-based. The tendency for relational egalitarians to think in terms of inter-group rather than (isolated) inter-personal relationships may be due to age having got less attention than gender or race. But whatever one’s view of the significance of racial and gender inequality, it is not obvious why age-based inequalities should be seen as conceptually subordinate to analyses of other relational inequalities, even if one thinks that inequalities of race and gender are often more morally urgent or troubling.

I have not said anything about the later, more practical chapters. But I will make a few observations about the role of young people in politics. In her closing chapter, Bidadanure’s focus is on elected members of parliament. There is no discussion of youth quotas in other branches of government. BIDADANURE is right, of course, to note that legislation often dramatically impacts the young. But recent acts by other branches of government should remind us that legislating is not everything when it comes to government’s use of power. Bidadanure’s book was published shortly before the US Supreme Court repealed Roe v. Wade and shortly before many central banks moved to dramatically raise interest rates in a bid to combat inflation. Both decisions create costs that fall largely on the young. (A rise in interest rates will also benefit older people, who tend to be net savers and with their mortgages paid off, while younger people will now further struggle to access enough credit to buy homes.) It is not obvious whether the answer is greater youth representation: non-legislative roles in the judicial and technocratic institutions of government are ones that arguably should be held by people who have accumulated years of expertise, rather than simply life experience as such. But these examples reinforce Bidadanure’s important observation that “our political communities are [...] very age-unequal in terms of access to, and exercise of, political power, and the young are systematically worse off in that respect” (210).

Once again, Bidadanure’s book is excellent, and probably the best piece of philosophy on its subject matter. Anyone remotely interested in how we should understand justice between the old and the young will benefit from a careful reading of this book.
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Jeff E. Biddle has produced a remarkable book on the history of regression studies on the Cobb-Douglas production function covering the period from the 1920s through the 1960s. He used published literature rather than archival or audio (e.g., recordings of interviews) materials to depict the rich research context surrounding the Cobb-Douglas production function. He begins with Paul Douglas’s (1892–1976) initial attempt to produce a tool for analysing the statistical relationship between production, labour, and capital, and discusses various appraisals, criticisms, replies, and new applications of this analytical tool. Biddle then describes how through these developments the production function eventually gained acceptance as a general-purpose tool in the community of economists.

The Cobb-Douglas function relates output ($P$) to the product of a coefficient ($b$), labour ($L$) to the power of $k$, and capital ($C$) to the power of $j$, namely $P = bL^kC$. It was first used by the economist Douglas and mathematician Charles W. Cobb as the basis of their statistical research estimating the parameters of the equation in the manufacturing sector. According to Biddle, it was a bold statistical attempt to find the shape of the production function, which was studied abstractly in economic theory, but that few had attempted to specify (45). Nonetheless, the two made their parameters a ‘good-fit’ start in regression estimations which could relate to neoclassical theory.

The book has two main parts: Part I, “Paul Douglas and His Regression, 1927–1948” and Part II, “The Diffusion of the Cobb-Douglas Regression”. Chapter 1 is titled “The Origins of Douglas’s Production Function Research Program and his Initial Time Series Studies”. From the late 1910s, Douglas was interested in labour legislation and the living standards of the working-class. While lecturing at Amherst, Douglas came up with the idea of using the function to relate production to the total fixed capital
and total number of wage earners employed in manufacturing. Douglas presented the research results at the 1927 meetings of the American Economic Association (AEA) and Cobb and Douglas (1928) was published in the *American Economic Review*. Douglas (1934) linked the function with marginal productivity theory and firmly connected his estimations with abstract theory.

Chapter 2 is titled “The Douglas-Mendershausen Debate and the Cross Section Studies”. Horst Mendershausen, who was trained by Ragner Frisch, rightly argued that production function estimation should be based on the measures of capital and labour *actually used*, whereas Douglas used *available capital* as a representation. Douglas admitted that available capital was a lesser approximation of the capital actually used especially for 1908 and 1921, which were years of depression. Against Douglas’s claim that labour-saving technology might increase capital, Mendershausen also attacked Douglas’s assumptions that neither technical progress nor improvements in the efficiency of labour had occurred over the sample period. Mendershausen was also concerned about multicollinearity in the time series data and the choice of the regression direction. Douglas and his associates moved on to use cross-sectional data and corresponded the estimated production function to plant-level production theory to bridge economic theory and their regression estimations.

Chapter 3 is titled “Theoretical and Econometric Challenges of the Early 1940s, and Douglas’s Final Word”. First, Biddle explained why Jan Tinbergen was concerned about the importance of technical change and the collinearity between time and the capital-labour ratio. In fact, Spurgeon Bell showed that the data indicated output in 1937 was slightly greater than output in 1929 despite smaller quantities of both capital and labour being employed in 1937 (110). Jacob Marschak and William Andrews (of the Cowles Commission) made the most comprehensive and critical assessment and considered the limitations in the available published data. They appreciated Douglas’s method as a way avoiding the criticism of “Measurement without Theory” (Koopmans 1947) and put Douglas’s production function in a system of stochastic simultaneous equations by considering Trygve M. Haavelmo’s probability approach to econometrics. Victor Smith was interested in the value of $k + j$, which he called the ‘degree’ of the production function (for the automobile industry). According to Biddle, Smith demonstrated that the numerical results were sensitive to the “peculiarities of statistical techniques” (126).
After being seriously injured while serving in the U.S. Marine Corps in Okinawa, Japan in May 1945 and spending one year of recuperation in the United States, Douglas was elected as AEA president and gave his final word on his production function estimates as a presidential address to member economists in 1947. He successfully campaigned to be elected a United States senator in 1948 and switched his career as an economist to the political arena. However, the Cobb-Douglas regression research programme continued, and the production function (regression) became a widely accepted tool for empirical research in economics. This is the theme of the second part “The Diffusion of the Cobb-Douglas Regression”.

Chapter 4 is titled “Three Important Developments in the Life of the Cobb-Douglas Regression, 1952–1961”. First, Biddle refers to two econometrics textbooks for graduate students, Gerhard Tintner (1952) and Lawrence Klein (1953), which covered both production function regressions and the probabilistic approach promoted by the Cowles Commission. Biddle states:

通过这些书籍，几代经济学研究生被具体例子接触到了估计跨截面和时间序列的Cobb-Douglas回归。 (148)

Notably, the two authors mentioned little about the criticism of empirical production function studies that had been raised before.

Second, tracing E. H. Phelps Brown’s “The Meaning of the Fitted Cobb-Douglas Function” (1957), Biddle states:

Phelps Brown was rejecting a priori Douglas’s implicit assumption that the production of value product from capital and labour in all of the heterogeneous industries represented in his cross-section data sets was governed by the same Cobb-Douglas function. (152)

Biddle argues that Phelps Brown’s criticism was less jargon-laden and was probably more clearly understood by readers than Mendershausen’s of the 1930s.

Third, a constant elasticity of substitution, or CES, production function was introduced by the collaboration of Kenneth Arrow, Hollis
Chenery, Bagicha Minhas, and Robert Solow (1961), which was immediately adopted as the first generalization of the Cobb-Douglas production function used by many empirical researchers. Biddle underscores the importance of the ‘elasticity of substitution’ between labour and capital and its generalization in economic theory. Biddle trails their collaboration and the massive reactions until the early 1970s.

Chapter 5 is titled “The Cobb-Douglas Regression in Agricultural Economics, 1944–1965”. During the interwar period, the U.S. Department of Agriculture and the federal government supported land grant colleges to employ economists that could conduct research useful for farmers and agricultural policymakers. Relatively rich agricultural data collected by the government and funding for the collection and analysis of experimental and survey data on the activities of individual farmers were made available. Gerhard Tintner (1944) was the first to apply the Cobb-Douglas regression using data from (Iowa) farms.

Chapter 6, entitled “The Cobb-Douglas Regression as a Tool for Measuring and Explaining Economic Growth”, conveys a broader story of the economists’ efforts to provide a proper understanding of how and why the Cobb-Douglas regression became a common tool in the 1950s. Interestingly, the U.S. government was considering public policy measures to increase the economic growth rate and that many economists became involved in the measurement of economic growth and the quantification of its causes. Biddle’s story begins in the 1920s and covers Simon Kuznets, his direction of the National Bureau of Economic Research, and other economists’ contributions to the measurement of national income and its components, including physical capital. Morris Copeland and E. M. Martin managed to produce new time series for the real value of the capital stock for sectors and for the entire economy, which were the first such capital measures developed since 1928. Thanks to the creation and refinement of capital stock measures during the 1940s and 1950s, it became possible to implement the Copeland-Martin procedure to construct what would become called indexes of ‘total factor productivity’.

The next part pivots to Robert Solow’s “Technical Change and the Aggregate Production Function” (1957). Solow first used an Aggregate Production Function that related output to the two inputs of capital and labour measured in physical units and time. He defined technical change as ‘any kind of shift’ in the production function. Then, he decomposed it and inserted a coefficient (dependent on time) and multiplied it to another production function. This coefficient measured the cumulative shifts in
the production function over time and was regarded as the technological change factor. Citing the neoclassical assumption that factors are paid their marginal products, Biddle stated:

This assumption along with some calculus and algebra allowed Solow to conclude that the percentage change in output would be equal to the percentage growth in the technological change factor plus the percentage growth in capital time capital’s share in output, plus the percentage growth in the labour input times labour's share in output.

(248)

Constructing empirical analogues for the variables covering the period between 1909 and 1949, Solow estimated that seven-eighths of the economic growth per capita were due to technical progress and demonstrated that capital accumulation did not account for economic growth. Solow and other economists' research led to the statement that ‘the measure of ignorance’ in economic growth was large. Evsey Domar (1961) named it ‘the Residual’. The high level of interest in economic growth led researchers to work on technical changes being embodied and not embodied in capital, the vintage of capital, education, research, public health, expertise, and know-how, and they opened up a new field called ‘growth accounting’.

The Cobb-Douglas production function was born out of the regression studies and polished through friendly criticisms. It is not just related with neoclassical theory, whose image is static. Interestingly, based on the neoclassical assumption of distribution, it has contributed to measuring most of dynamic technical progress under the name of total factor productivity, and has become a general-purpose tool to make economists continually work with both theory and statistical estimation. Yet, Biddle’s book is written mainly for an American readership.

On a final note, Biddle suggests that Martin Bronfenbrenner made a graduate study of statistical techniques (114). However, Bronfenbrenner completed his dissertation thesis, “Monetary Theory and General Equilibrium”, in 1938 under the strong influence of his supervisor Henry Schultz (1893–1938) and received his doctorate from the University of Chicago in 1939. After the sudden death of Schultz, Bronfenbrenner was fortunately hired by Douglas as one of his assistants in statistical studies and gained necessary skills through on-the-job-training rather than through graduate studies. In September 1945, he was sent to Japan as a Japanese language
officer for the U.S. Occupation Force. His mission included communication with Japanese economists and he was pleased to find Yukichi Kurimura (1899–1983) writing something like a Cobb-Douglas production function on the blackboard in a class at Kyushu University. Kurimura was authoring books on economic theory and statistical estimations while he was reading related articles carried in economics journals (Ikeo 2014).

REFERENCES

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PHD THESIS SUMMARY:
The Making and Unmaking of Ordoliberal Language:
A Digital Conceptual History of European Competition Law

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PhD in History, June 2022
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The historical existence of different schools of competition analysis is well established, ranging from the Freiburg School’s ordoliberalism to the various versions of the Chicago school. However, what is less clear is these schools’ actual relevance to influence on European policy over time. My dissertation addresses this gap in the historiography of EU competition law by reviewing the negotiations and initial conceptualisations of the critical rules, by analysing relevant case law and a large set of primary sources—ranging from academic journals, oral history interviews, and Festschriften to private papers by Franz Böhm, Walter Eucken, and Heinrich Kronstein—and by applying novel text mining methods (published as Küsters, forthcoming). While the negotiations on the Founding Treaties were still dominated by linguistic misunderstandings and different normative conceptions of what competition was and what role it should play in future Europe, several scholars and advisors close to ordoliberalism soon started to popularise the Freiburg School’s distinctive conception of competition when the new competition rules needed to be applied in the 1970s and 1980s. It was not until the reforms under the More Economic Approach, implemented by the Commission in the early 2000s, that this

1 Thinking in terms of ‘orders’ has a long tradition in German economic thought but reached a distinctive manifestation in the 1920s and 1930s when a group of economists and lawyers at the University of Freiburg called for increased competition, monetary stability, and the rule of law. Their ideas later became known as ordoliberalism and were associated with Ludwig Erhard’s social market economy (see Biebricher, Bonefeld, and Nedergaard 2022 for an overview). Despite many similarities, the Chicago School, founded around the same time, adopted a much looser competition law stance, particularly after the Second World War, as it focused on maximising economic efficiency to the detriment of aspects like the individual freedom of small businesses and the reduction of concentrated power, which had been essential to the early ordoliberals.
ordoliberal language was replaced by other concepts and semantics borrowed from the classical Chicago school and the new Industrial Organisation literature, ushering in a neoliberal period.

In the Introduction, I point out that a fundamental problem in this literature is that ordoliberalism itself is a heavily contested concept. To remedy this situation, in chapter 1 I provide a historical account of the early Freiburg School, which is interpreted as a paradigm change concerning the prevailing competition thinking and language. The competition understanding espoused by first-generation ordoliberals went beyond classic *allocative efficiency* considerations, encompassing *political* and *social efficiencies*.

I discuss in chapter 2 how the first two decades of West Germany’s social market economy provided the quickly consolidating school with an institutional backdrop and the possibility to influence policy, such as the country’s first strict competition law enacted in 1957. Ordoliberals contributed through various draft laws and consultations, ensuring that their competition model and key concepts like *Leistungswettbewerb* (competition on the merits), *vollständiger Wettbewerb* (complete competition), and *Wirtschaftsverfassung* (economic constitution) were ingrained into the political and academic discourse of the day.

In chapter 3, I describe how the ordoliberal school subsequently experienced a Hayekian moment from the late 1960s onwards that shifted attention away from the problem of accumulated economic power toward competition distortions brought about by the state. Chapter 4 provides a comparison with the Chicago school, which experienced such a ‘neoliberalisation’ much earlier, despite similar initial interests. From the 1950s onwards, it developed a ‘consumer welfare standard’ and an *economic efficiency*-based understanding of competition that differed from the ordoliberal theory.

On this basis, my dissertation turns to these two schools’ influence on the emergence and application of European competition law. I describe in chapter 5 how, contrary to the standard account, the early Europeans who convened in Paris, Rome, and other negotiation venues did not form an epistemic community that applied a commonly shared ordoliberal value system. The different expressions and functions that contemporaries associated with competition echoed the *Begriffswettbewerb* (competition of concepts) seen in other fields of early European integration (Mangold 2011, 444–448) and reflected diverging traditions of economic
thought, whose tense coexistence was further complicated by partly incompatible translations. Given this ambivalence, one cannot emphasise enough that the European competition discourse was soon shaped by ordoliberal semantics introduced by Hans von der Groeben, Alfred Müller-Armack, Walter Hallstein, and the *Wissenschaftlicher Beirat* (Scientific Advisory Board at the German Ministry of Economics).

In chapter 6 I trace the legal effects of this rhetorical strategy by analysing the emerging case law. The Commission initially focused on targeting vertical restraints via Art. 85 EEC Treaty (known today as Art. 101 TFEU), in line with post-war ordoliberals’ priorities. In its case law under Art. 86 EEC Treaty (Art. 102 TFEU), the Court followed ordoliberal recommendations by defining dominance as the position of an undertaking that can hinder effective competition through its distortion of the market structure and its resulting ability to compete through measures that do not classify as *Leistungswettbewerb*. In the 1970s and 1980s, ordoliberals watched with delight how the Court implemented their holistic understanding of a European ‘economic constitution’. Afterwards, the Commission’s attention shifted to state aid and, in the 1990s, also to merger control, but this was still in line with ordoliberal priorities. In this sense, the ‘neoliberal shift’ often linked to this period did not differ from contemporary ordoliberalism, which experienced its own ‘neoliberalisation’ at the time.

As I explain in chapter 7, the unmaking of this ordoliberal regime only occurred in the 2000s, when the Commission proposed a set of reforms known as the More Economic Approach (MEA). Ordoliberals framed competition as a rule-based legal system that focuses on legal forms, accepts possible false positives, and is amenable to basic democratic decision-making regarding the framework rules, with the ultimate goals being the protection of consumer choice, political and competitive efficiency, and human freedom. By contrast, the MEA approach, being based on both Chicago and Post-Chicago elements, understood competition as an economic trade-off exercise that focuses on a conduct’s predicted or testable economic effects, relies heavily on technocratic economic expertise, and aims to avert false negatives so that consumer welfare and economic efficiency can be maximised.

From a semantic perspective, as I show in chapter 8, this neoliberal period was marked by an increasing divergence from ordoliberal language, as evidenced by the development of crucial competition expressions, a rise in Chicago-style vocabulary, and quantified merger control
techniques, as well as a more positive language reflecting the ‘effects’ analysis typical for welfare economics. Tellingly, this development also impacted the usage of the ordoliberal concept of an ‘economic constitution’, since I find in chapter 9 that shifting the ordoliberal economic constitution from the level of the Prussian Gewerbeordnung (trade act) to a European and later even a global level eventually went hand in hand with a specific ‘conceptual overreach’.

Overall, the qualitative and corpus-linguistic evidence presented in my dissertation provides nuance to the ordoliberal ‘constitutionalisation’ argument initially put forward by Gerber (1998), according to which an ordoliberal understanding of competition was reflected in the European Treaties and the Court’s jurisprudence from the very beginning. Instead, I argue that the ordoliberalisation of European competition language was not a given but had to be made; it was a conscious process that only acquired normative power from the 1960s onwards. I also propose that the subsequent ordoliberal period of competition policy ended abruptly in the early 2000s when a neoliberal vocabulary was consciously introduced to unmake the previous regime. Both content and timing of this neoliberalisation, which this study links for the first time to quantifiable semantic trends, differ from the neoliberal turn depicted in the literature, which is typically located between the 1973 oil crisis and the 1986 Single Act and connected with de-regulation and actions against state aids (Warlouzet 2017, 225; Baccaro and Howell 2017). The results also differ from the accounts of legal scholars who assume that European case law is highly resilient and argue that economic thinking was primarily driven by the Court (Ibáñez Colomo 2018, 329).

By tracing the making and unmaking of ordoliberal language at the European level both qualitatively and quantitatively, my dissertation illustrates the extent to which traditional economic and legal research can be enriched with the interdisciplinary toolbox of the Digital Humanities. To do so, I manually created several large-scale corpora via web scraping and OCR that captured, respectively, all articles published in ORDO between 1948–2014, all Journal of Law and Economics articles between 1955–2015, all Common Market Law Review articles between 1963–2000, all speeches on EU competition policy held between 1995–2020, and, most importantly, the roughly 11,000 decisions and judgments given in the field of EU competition law between 1961–2021. By exploring these digital corpora with different text mining methods, ranging from word frequency analysis to theoretically more demanding methods like Topic Modelling.
or sentiment analysis (see Grimmer and Stewart 2013 for an overview), I detect significant semantic correlations that support the overall argument. In particular, the ordoliberal period between 1960–1990 was characterised by the dominance of the ‘effective competition’ collocate over other expressions that would have been semantically closer to the Treaty text, by the quantitative role of Arts. 101 and 102 TFEU case law, by a harsh language legitimising 'by object' restrictions, and by the presence of merit-based imagery.

In this way, my dissertation illuminates a vital channel for how ordoliberal ideas formulated during the interwar or early post-war years could still influence law and politics later—namely by shaping the competition discourse with concepts like Leistungswettbewerb, vollständiger Wettbewerb, and Wirtschaftsverfassung. Due to its ahistorical self-perception and inclination to keep existing doctrines alive, emerging European law provided a fertile ground for preserving this ordoliberal language. The implicit vision of an atomistic market characterised by many suppliers, akin to the ordoliberal ‘complete competition’ model, influenced the Court’s language since Geitling. Similarly, merit-based rhetoric conveying the ordoliberal conviction that businesses must adhere to the same ‘rules of the game’ and an ethical form of Leistungswettbewerb on a ‘level playing field’ guided Art. 102 TFEU cases. Another example concerns the ordoliberal reasoning about an ‘economic constitution’, which was not only echoed in ordoliberal discussions of Germany’s Basic Law and Competition Act but also in the interpretation of the Treaties by the European Courts. As a result, the ordoliberal school of competition thought can be understood as a distinct linguistic community whose conceptual and semantic influence went beyond Germany and eventually shaped the European legal order.

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KÜSTERS / PHD THESIS SUMMARY

York: Cambridge University Press.

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Although microeconomics and macroeconomics differ in their object of study—microeconomics studies how economic individual agents make decisions and how those decisions interact, while macroeconomics studies the fluctuations of the economy as a whole—there is a significant tradition in economics that argues that macroeconomic models to be seen as epistemically compelling need to be given microfoundations. That is, 'macro' aggregates need to be shown to be derivable from the choice patterns of individual economic agents.

This dissertation focuses on arguments that take macroeconomic and microeconomic entities' respective ontological natures as the reason why macroeconomics cannot be fully microfounded (Epstein 2014, 2015; Hoover 2001, 2009, 2010, 2015). Since these arguments depend on the metaphysical relationship between macroeconomics and microeconomics and metaphysical arguments tend to be controversial, their conclusions cannot directly be verified empirically. This distracts us from an important question: “how should macroeconomists build models?” The core questions addressed in this dissertation are: (1) To what extent do macroeconomic models require microfoundations? And (2) what grounds the need (or absence) for microfoundations?

Chapter two (published as Ruiz 2021) assesses Brian Epstein’s (2014, 2015) arguments. Epstein argues that the microfoundationalist approach is unconvincing because economists overlook the influence ontological commitments have on scientific practices, specifically ontological individualism (OI). Since OI states that there is nothing more to social phenomena above and beyond facts about individual people, macroeconomic models, in turn, must represent individuals’ choice patterns. For Epstein, wrong ontology entails poor modeling, thus economists must adopt a
form of social ontology\textsuperscript{1} to improve the foundations of their methodology. Furthermore, Epstein states that OI does not capture macroeconomics’ nature and suggests a social ontology in which grounding/anchoring (GA) metaphysical relations will uncover macro-phenomena’s nature.\textsuperscript{2} Thus, for a model to accurately represent macroeconomic phenomena, according to Epstein, economists must first establish the GA relations of the modeled object(s).

I argue, instead, that fixing social ontology prior to the process of model construction is optional rather than necessary. I draw attention to Weisberg’s (2013) target-directed model account to show that in the process of model-building scientists choose/design target-systems for many different reasons. Representational accuracy—i.e., how well the structure of the model maps the structure of the world—is just one among them. Complete knowledge of all the GA facts of a phenomenon—fixed ontology—is not only what scientists consider when designing target-systems’ features to conform with a mathematical structure. Take, for example, Jevons’ (1871) development of ‘economic man’: Jevons reduced Bentham’s utility account from seven dimensions to two dimensions to accommodate the demands of the mathematical structures he was working with, thus representing the dimensions of pleasure/pain in a two-dimensional space (Morgan 2006). This shows that what is mathematically tractable is as important for good modeling as accurate representations.

This process of targeted-model construction shows that addressing ontological questions first does not necessarily help modelers in the model-building process. Epstein’s account, therefore, cannot answer the question about microfoundations because whether macroeconomic models are built based on individual agents’ choices (or not) is not solely an ontological question—it is also a methodological one. Macroeconomists have different intentions when designing target-systems; whether or not they are committed to OI is not all that matters to their model-building

\textsuperscript{1} Social ontology studies whether macro(economic) phenomena are constituted of exclusively individual agents or something beyond these, i.e., a distinctly social entity.

\textsuperscript{2} Grounding is a relation in which lower-level facts—facts about economic agents—are the metaphysical reason for why a set of higher-level facts—facts about macroeconomics—are the case. Anchoring is a frame principle in which collective acceptance of a constitutive rule (society’s individuals accepting a rule) sets the metaphysical reason why for a set of grounding conditions of a social macro-phenomena are the case (Epstein 2015). For example, the reason why a greenish piece of paper is a United States Dollar (USD) is because of society’s collective acceptance of a constitutive rule “being printed by the Bureau of Engraving and Printing (BEP) grounds what is being a USD” anchors the existence of what grounds (being printed by BEP) being a USD.
practices. A better question is: when do microfoundations lead to good modeling practices?

Chapter 3 focuses on one argument found in Kevin Hoover's book *Causality in Macroeconomics*. Hoover argues that macroeconomics is a suitable science for a structural causal analysis (SCA), which studies variables as part of “a network of counterfactual relations that maps out the underlying mechanisms through which one thing is used to control or manipulate another” (Hoover 2001, 24). Interestingly, I point out, this is only plausible given Hoover's realist commitments to causality—causes are real properties of a variable in a structure—and to an ontological dichotomy between natural and synthetic macroeconomic aggregates.

Natural aggregates are simple sums or averages. Synthetic aggregates are fabricated out of components whose structure is altered—an independent macro-premise, such as *Indexing*, has been added (Hoover 2001). Synthetic aggregates, therefore, are not fully composed of individual economic agents. If synthetic aggregates cannot legitimately be derivable from the micro-configuration of the economy, microfoundations cannot fully eliminate macroeconomics and macrovariables can be studied in SCA (Hoover 2001). I argue, instead, that Hoover's ontological dichotomy is a measurement problem. To measure unemployment, depending on the purpose at hand, it might either be enough to add the number of working-age people failing to get a job or it might be more appealing to use the measurement procedure like the one used in *Natural Rate of Unemployment*, which considers facts about the economy’s expected future, changes in labor force, technological progress, changes in labor market institutions, actual wage settings, and changes in government policies (Krugman and Wells 2009). Unemployment rates could be treated as either a natural or a synthetic aggregate. Hoover's methodological approach, therefore, does not get off the ground.

Finally, I shed light on a different approach to macroeconomic modeling. Dani Rodrik’s (2015) modeling account makes explicit that to assess/chose a model to work with economists must consider the purpose the model is going to be used for—for instance, predictive accuracy, empirical relevance, explanation, data fitness, internal consistency, policy

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3 Hoover has several arguments pointing that macroeconomic models cannot exhaustively derive microfoundations. I focus on this to best assess the import of ontological arguments to methodological practices.
goals, etc. Although it does not completely solve the debate about microfoundations, it illustrates that the question can be evaluated from a practice-based perspective, instead of for ontological-based reasons.

Chapter 4 further explores the issue of ‘model diversity’ (a term used by Rodrick 2015). In contrast to the accounts of Aydinonat (2018) and Veit (2020, 2021), I argue that economic model diversity doesn’t entail model pluralism. Using a microfoundationalist example, I illustrate one of the methodological virtues of model diversity not found in model pluralism.

For Aydinonat (2018), models are tools for how-possibly reasoning about economic phenomena that allow economists to think of possible answers to an ‘explanation-seeking question’—that is, different if-then ways to think of y’s or z’s relevance in phenomenon X. Since there is no single model that can capture all the causal factors of one phenomenon, having many models about phenomenon X expands the menu of possible what-if questions/answers. Model diversity, therefore, secures better economic explanations (Aydinonat 2018). Veit (2020, 2021) dubs model pluralism the practice that for any scientific goal z scientists require multiple models of aspect x of phenomenon y. Contrary to these, I take model diversity to not entail epistemic completeness (for instance, read Morrison 2011), at least not necessarily. Note that in some cases—climate science—multiple models for forecasting purposes are used, but forecasting (and explanation) are not the only scientific goals of a model. Model diversity gives economists the ability to adapt/build on existing models—adjust critical assumptions, parameters, idealizations, de-idealizations, etc.—to best serve the purpose of the model. Following Rodrik, I propose that model diversity’s epistemic virtue is the ability to choose a model that best fits a chosen purpose: “we have a menu to choose from and need an empirical method for making that choice” (Rodrik 2015, 44).

Consider two models of economic growth, one with and one without microfoundations. Model 1—a version of the Solow model (see, e.g., Jones 2002)—treats technological progress as an exogenous variable that grows at a fixed rate g. Model 2—a version of Romer’s endogenous growth model—treats technological progress as the result of intentional investment decisions made by profit-maximizing agents (Romer 1990). For cer-

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4 Veit identifies four forms of model pluralism—weak, weakly moderate, moderate, and strong. He identifies Rodrik’s and Aydinonat’s account as weak moderate model pluralism (Veit 2020, 96). Since I take this version to concur with pluralistic accounts of scientific practices, I refer to all four forms as model pluralism.
tain epistemic and practical purposes, Model 1 may be preferrable in virtue that it might be easier to understand by the targeted audience (policy makers and voters). That is to say, economists can more easily justify policy interventions that increase the rate of technological progress—increase funding to public universities—to policy makers and the wider public because of Model 1’s explanatory virtues as compared to Model 2. In another context, Model 2 may be preferrable because its predictive accuracy allows economists to justify how incentives to private investors have an input in general economic growth. When it comes to model choice (afforded by model diversity), there need not be one right answer: it depends on the context in question.

In sum, the contribution of this dissertation can be stated as follows: by treating the debate over microfoundations as a purely methodological issue, rather than an ontological issue, it is possible to view macroeconomic phenomena as a product of individual economic agents’ decisions, but other times the former must be treated as independent from its microeconomic parts. That is, the debate is in an either/or question.5

REFERENCES


5 Read also Ruiz & Schulz, forthcoming.

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PHD THESIS SUMMARY:
Why We Need to Talk About Preferences: A Federalist Proposal

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Among the social sciences, economics has often been portrayed as the one that has its house in order. Whereas the other social sciences are engaged in never-ending squabbles over words and definitions, their dismal monarch purports to have no interest in such profanities. Economics is content with having built its house on solid theoretical foundations by using a precise and formal toolkit, i.e., (rational) choice-theory. The long-winded debates of its wordy relatives concerning ‘what some x really is’ provoke nothing but bewilderment and sometimes even disdain in economics.1 While becoming more sentimental in recent years, economics tried to stay away from its relatives as best as it could for most of its life (see Hausman 1992).2 Some of economics’ disciples have even gone so far as claiming that if you train to become a social scientist, you either learn to use the precise toolkit of choice-theory or nothing at all (e.g., Gintis 2016, 138).

Nevertheless, there is one issue that economics would like to hide in the basement. Ever since the initial development of its choice-theoretic toolkit in the 1870s, questions have been raised about what ‘utility’ and ‘preferences’ really are (see Moscati 2018). ‘Preference’ is arguably the most central concept in economics. Yet, there is no explicit definition of the concept in economic textbooks (see, for example, Mas-Colell, Whinston, and Green 1995; Rubinstein 2012; Varian 2014). Thus, the last few years have seen the emergence of several views concerning the nature of preferences in economics. There are two main camps: mentalists and behaviorists. Behaviorists (e.g., Ross 2005; Gül and Pesendorfer 2008; Clarke 2016) hold that preferences just refer to patterns in choice-behavior. On

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1 For instance, consider debates about definitions of (i) class in sociology (Kincaid 2016), (ii) interstate cooperation in political science (Graefrath and Jahn 2021), and (iii) attention in psychology (Watzl 2011).

2 That economics has become more open to outside influences is, for example, evidenced by the rise of behavioral economics (see Angner 2019).
the other hand, mentalists (e.g., Hausman 2012) argue that preferences are some kind of mental state (for a more detailed outline of these two positions, see Beck and Grayot 2021).

What is the point of this debate? One may assume that debating what preferences in economics really are—i.e., mental phenomena or only choice patterns—can inform us about what kind of evidence is relevant for economics. However, the problem with this view is that the evidential base of a theory can be both narrower and broader than its content. For example, even if economic models were only about patterns in choice, this would not entail that the evidential base of economics is only choice data. It is, therefore, unclear if and how debating the nature of preferences can inform the practice of economists.

So, is a debate about the concept of preferences in economics worth having? In my doctoral thesis, I make the case that, contrary to the appearances of unity, economists are highly disunified in their understanding of preferences. Pace the dominant conceptions of preferences (i.e., mentalism and behaviorism), which are meant to apply across all of economics, I hold that defending a single, comprehensive story is a mistake. Instead, I propose that we should acknowledge and take seriously the conceptual disunity within economics regarding central concepts like 'preference'. I hold that explicating this disunity not only helps us account for important controversies at the forefront of economic research, but also points us towards potential resolutions of these controversies.

As a first step in this argument, I demonstrate that the grand narratives about preferences philosophers have explored so far (i.e., mentalism and behaviorism) fail to account for substantial contributions and practices in economics. For instance, mentalism has trouble accounting for choice-theoretic models that aim at describing entities we usually do not ascribe mental states to. Moreover, behaviorism has difficulties accounting for the importance of beliefs for inferring preferences from choices and the role of preferences in causal explanations (chapters 2 and 3). I then argue that these shortcomings of the two dominant narratives about preferences are to be expected as only a minimal conception of preferences holds the federation of economic research programs together. Following Guala (2019), I argue that if our aim were merely to reconstruct economic practice, it would be best served by dispositionalism.
Dispositionalism. An agent prefers X over Y if she is disposed (in a way that depends on her informational state) to choose X in some circumstances, where Y is also an available option.

Nevertheless, while dispositionalism can account for the practices that behaviorism and mentalism have trouble capturing, it is too barebones to guide economic research (chapter 4).

However, in the thesis, I then propose that dispositionalism—qua being merely a minimal conception—is usually enriched with further local assumptions that differ across various research programs and are tailored to their specific agendas. My key claim is that explicating and appraising these local and often implicit assumptions will—in contrast to how the debate currently proceeds—allow us to contribute substantially to the progress of economics. The thesis supports this claim by looking in detail at (i) the disagreements concerning what kind of experiments microeconomics needs and (ii) the recent controversy about preference purification in behavioral welfare economics.

Concerning the first debate, I argue that the disagreement between the two most prominent experimental paradigms in economics can be understood in terms of diverging assumptions about the causal base of preferences (chapter 5). In particular, I argue that proponents of the heuristics-and-bias program usually put internalist restrictions on the causal base of preferences, while proponents of experimental economics in the tradition of Vernon Smith (2008) permit agents' external environments to play a crucial role in the constitution of their preferences. I point out that the burden of proof in this debate lies with the heuristics-and-bias tradition and those who want to uphold the more demanding internalist restrictions on the constituents of preferences (published as Beck 2022a).

Regarding the second debate, I introduce the idea of preference purification as one of the aims of behavioral welfare economics and show that scholars fiercely disagree about its plausibility (chapter 6). I then argue that disentangling two different substantial notions of rationality, i.e., procedural and structural rationality, which go beyond its technical meaning in economics, can help us account for the vastly different assessments

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3 To illustrate, I argue that dispositionalism can account for the role of beliefs in capturing choice-behavior by making preferences information-dependent choice dispositions.
of the plausibility of preference purification by offering two different visions of what purified preferences are. In particular, I explicate a structural alternative to the predominant proceduralist understanding of purified preferences and defend it against several obstacles to highlight how one can conceive of preference purification as a feasible enterprise (published as Beck 2022b).

All in all, my doctoral thesis makes the case that economics has more to gain from an explication of the implicit assumptions about choice-theoretic concepts made by different research programs than from overly broad narratives about ‘what preferences really are’.

REFERENCES


4 Procedural rationality ascribes rationality to agents that have formed their attitudes by relying on a particular process, while structural rationality concerns the relations between an agent’s different attitudes.
Lukas Beck obtained his Ph.D. at the University of Cambridge. He is currently a member of the Scientific Assessments, Ethics, and Public Policy working group at the Mercator Research Institute on Global Commons and Climate Change (MCC) in Berlin, where he works on the FORMAS-funded Rivet project (with Lund University) on ‘Risk, values, and decision-making in the economics of climate change.’ His research focuses on economic methodology, the intersection between economics and cognitive science, and the normativity of the sciences. What unifies these interests is a desire to understand better what it means to make justified decisions in complex scenarios and what this implies for how we should live together.

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PHD THESIS SUMMARY:
Attitudes First: Rationality Attributions and the Normativity of Rationality

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Rationality seems obviously normative—after all, it features heavily in our practices of evaluating agents, their thought processes, and actions. We criticise our interlocutors for being irrational and praise students for displaying rationality.

This practice motivates one of two main themes of this thesis: getting rationality attributions right. Because we use rationality in this important way, we better have a reliable procedure for deciding when a rationality attribution is warranted, and when not. Ultimately, this has the further advantage of shedding some light on the property of rationality, and what it takes for agents to be rational.

The other main theme of this thesis is the normativity of rationality. The two main themes obviously interact. The normativity of rationality can provide an explanation for our practice of using rationality attributions in criticism or praise—the reason why someone is criticisable if they are irrational is because rationality is normative. So being irrational would amount to not living up to the demands of a normative notion. But it is notoriously difficult to provide an argument for the normativity of rationality that is not subject to a number of immediate counterexamples. Developing such an argument is the aim connected to this second main theme of the thesis.

Chapter 1 provides an overview of standard ways of making rationality attributions, such as simply checking for compliance with rational requirements (for instance, intending the means to our ends). It also introduces some issues surrounding arguments for the normativity of rationality, like the bootstrapping problem: the problem of how coherence requirements may seem to create reasons out of thin air (see, for example, Kolodny 2005). Moving forward, chapters 2–5 are concerned with the first aim of this thesis: better rationality attributions.
Chapter 2 explores the extent to which rational requirements can be used as guides for rationality attribution—is it enough to simply check whether an agent satisfies rational requirements, and to call them rational on that basis? It includes an extensive survey of various choice points in debates about the correct formulation of rationality requirements, such as whether these requirements are synchronic or diachronic. The chapter concludes that such requirements alone cannot provide sufficient guidance for rationality attributions.

Chapter 3 then explores the different strategy of bypassing rational requirements and directly focussing on rationality attributions. It lays out three desiderata for an adequate account of such attributions: flexibility (with regard to the amount of attitudes we can evaluate), informativeness (allowing for comparisons of attributions), and delimitation (providing a procedure for deciding on the relevant set of attitudes).

Chapter 4 puts forward a novel account of rationality attributions. This account focusses on explicitly mentioning sets of an agent’s attitudes, and also includes a measure of the attribution’s robustness. For example, an agent would be rational with regard to subset \( a_2 \) of their attitudes, which may include beliefs \( b_1 \) and \( b_2 \), and an intention \( i \). The robustness of this attribution is measured in terms of the distance between \( a_2 \) and the largest subset at which the agent does not violate any rational requirements. Thanks to these features, the account meets the desiderata stated in the previous chapter, and also allows for progress on persisting disagreements in the debate, such as the question whether our rationality attributions should take into account all or only some of an agent’s attitudes.

Concluding this first part of the thesis, chapter 5 further illustrates the account by contrasting it with an alternative understanding of rationality attributions that understands them contextually (that is, the idea that whether someone counts as rational varies with context).

Chapters 6–8 are dedicated to the second aim of this thesis, that is, to defend the normativity of rationality. In chapter 6, I consider the problems for a reasons-based understanding of the normativity of rationality which arise from so-called transmission principles: principles which maintain that reasons are transmitted from ends to means (see, for example, Way 2010). I question the plausibility of such transmission principles in general, and point out various additional strategies to defend rationality’s normativity.
Chapter 7 (forming the basis of Bastian 2020) provides further support for one of these strategies: to understand rationality’s normativity in terms of (potentially weak) reasons. It shows that existing criticisms of coherence-rationality often implicitly assume an evidentialist conception of epistemic rationality. Once we make this explicit and open up the possibility of endorsing a different, coherentist conception of epistemic rationality instead, these criticisms lose force—we can then allow that some pragmatic reasons are indeed reasons of the right kind for belief.

Finally, chapter 8 presents my positive argument. I propose to understand rationality’s normativity in terms of commitment—if you are rationally required to x, you are committed to x. Commitment can avoid the counterexamples of alternative understandings in terms of reasons or ought by combining features of both notions: just like reasons, it can be outweighed by stronger competing considerations, and just like ought it still exerts strong normative force. This makes commitment a promising normative notion in its own right.

Next to this defence of the normativity of rationality, there is an underlying methodological current, or lesson, to this thesis. It highlights how greater complexity and specificity in our debates is helpful and might even be required. Specifically, my treatment of the matter shows that, often, taking a closer and more fine-grained look at rationality can help to solve persisting problems.

REFERENCES

Lisa Bastian is an Assistant Professor at Vrije Universiteit Amsterdam. Her research focusses on the notion of rationality, normativity, and on metaethics and epistemology in general. She received a PhD in Philosophy from the University of St Andrews in 2020.
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