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ON THE RECEPTION OF HAAVELMO'S ECONOMETRIC THOUGHT

BY
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The significance of Haavelmo's "The Probability Approach in Econometrics" (1944), the foundational document of modern econometrics, has been interpreted in widely different ways. Some regard it as a blueprint for a provocative (but ultimately unsuccessful) program dominated by the need for a priori theoretical identification of econometric models. Others focus more on statistical adequacy than on theoretical identification. They see its deepest insights as unduly neglected. The present article uses bibliometric techniques and a close reading of econometrics articles and textbooks to trace the way in which the economics profession received, interpreted, and transmitted Haavelmo's ideas. A key irony is that the first group calls for a reform of econometric thinking that goes several steps beyond Haavelmo's initial vision; the second group argues that essentially what the first group advocates was already in Haavelmo's "Probability Approach" from the beginning.

I. HAAVELMO'S CHANGING INTELLECTUAL FORTUNES

The historical importance and continuing relevance of the econometric program initiated in Trygve Haavelmo's "The Probability Approach in Econometrics" (1944) remains unsettled business among econometricians. Hendry, Spanos, and Ericsson (1989, p. 12) credit Haavelmo with having "founded modern econometrics as a separate discipline. . . ." And, while Morgan (1989, ch. 8) ends her history of econometrics with Haavelmo and the work of the Cowles Commission, she presents Haavelmo as *the* transitional figure in moving econometrics from infancy to maturity (p. 259). Hendry et al. and Morgan see Haavelmo as having bequeathed an enduring legacy to econometrics. Although not gainsaying his historical significance, Heckman sees

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Haavelmo as having led econometrics into a cul-de-sac: "Few empiricists now embrace the Cowles research program advanced by Haavelmo that remains the credo of most structural econometricians and is implicitly advocated in most econometrics textbooks" (Heckman 2000, p. 86).

Heckman himself remains committed to some sort of structural understanding, but regards "structural econometrics," as he uses it here, as a fruitless approach. Similarly, Eichenbaum rejects the continued vitality of Haavelmo's program: "The key question facing macroeconomists is: Which models should we use? The key task facing (macro) econometricians is developing tools for answering that question. Since all models are wrong along some dimension of the data the classic Haavelmo (1944) programme is not going to be useful in this context" (Eichenbaum 1995, p. 1609).

Hendry et al. agree both with Eichenbaum's assessment of the key question and the key task, but find in Haavelmo the fundamental insights from which useful tools can be, have been, and are being developed (see also Spanos 1986, ch. 1; Juselius 2006, ch. 1).

How is it that two such diametrically opposed interpretations of the nature and significance of Haavelmo's contributions to econometrics coexist side by side?[1] This is not a question that can be answered by asking what did Haavelmo *really* say, even though that is itself an interesting question (and one to which I will return at the end of this essay). Rather, it is a question of how Haavelmo's contribution was received—how it was perceived and interpreted by his contemporaries and successors. History is full of figures whose ideas, while profound, never have any actual influence on the development of the discipline. The problem is complex. Some wonderful ideas may be lost altogether: "*Full many a flower is born to blush unseen, . . .*" Others may be known only because an intellectual historian of an antiquarian persuasion rediscovers them long after their thought has any real chance of advancing a discipline. Still others—for example, Mendel's work on inheritance or Muth's rational-expectations hypothesis—are neglected for some time and, yet, ultimately contribute mightily to the course of science. Of course, Haavelmo, winner of a Nobel Prize in economics, has by no means been ignored. Yet, even some of the most vital ideas of a famous scientist may fail to take hold or to be appreciated and, in the end, "*waste their sweetness on the desert air.*" Intellectually, we remain hostage to our audiences and interpreters.

Haavelmo was not a late discovery, by any stretch of the imagination. The first econometrics textbook (Davis 1941) cites him.[2] Haavelmo had a long career, yet, by 1991, Moene and Rødseth (1991) write that "few younger economists outside of

[1]One might suspect—as a referee did—that one source of the difference in the assessment of Haavelmo stems from the fact that Heckman is primarily a microeconometrician, while Hendry is primarily a time-series econometrician. That is not, however, an adequate explanation: first, both Heckman and Hendry intend their assessments to apply broadly to econometrics in general; and, second, econometricians and historians who agree with each of them do not align so neatly on a microeconometrics versus time-series econometrics divide.

[2]A check on Google Books confirms that, even though it is not the first book to include the word "econometrics" in its title, Davis' book is, as Anderson (1991, p. 2) suspected, the first econometrics *textbook*. The content and approach of Davis' textbook is very far from what most would understand today as econometrics, but it probably does reflect fairly well the initial conception of the scope of econometrics as set out in Frisch's and the Econometrics Society's original program (Frisch 1933).

Norway have read anything he wrote…." Their conclusion is easily buttressed with numbers: Figure 1 shows eleven-year centered moving sums of the number of articles in the JSTOR journal archive that cite Haavelmo.[3] His citations peak in the early 1950s and reach their nadir in the early 1980s. While a recovery begins in the mid-1980s, it is mainly driven by citations that rise sharply after he received the Nobel Prize in 1989. The most recent data may actually overstate the recent fall, since journals frequently are available on JSTOR only with a five-year lag.

Figure 2 shows that Haavelmo's early econometric works are the most frequently cited—especially the "The Probability Approach in Econometrics" (1944) and "The Statistical Implications of a System of Simultaneous Equations" (1943).[4,5] The interest of the profession has not, however, remained consistent. Although "The Probability Approach" is Haavelmo's most-cited work, its direct influence has waxed and waned. Figure 3 shows both the moving decadal sums of citations to the "Probability Approach" and the ratio of its citations to Haavelmo's total citations. The two series track each other very well (adjusting for differences in scale), which implies that vicissitudes in Haavelmo's acknowledged influence are largely accounted for by the changing fortunes of appreciation for "The Probability Approach." ("Acknowledged" is an important qualifier here, since, as we shall see, Haavelmo had an indirect as well as a direct influence.)

Our goal is two-fold: first, to trace the history of the reception of Haavelmo's econometric thought—especially, of his "Probability Approach"—through bibliometric and other means; second, to understand the basis for the continuing dispute over Haavelmo's legacy to econometrics, exemplified in the interpretations of Heckman and Eichenbaum, on the one hand, and of Hendry, Morgan, Spanos, and Juselius, on the other.[6]

II. HAAVELMO AND THE COWLES COMMISSION

The story of Haavelmo and the Cowles Commission has been ably told by others, and there is no need to repeat it in detail here (see, e.g., Morgan 1990, ch. 8 and Conclusion). The Cowles Commission is, no doubt, the major *indirect* channel through which

[3]In reference to all automated counts using JSTOR or Google Books, *citation* refers to the appearance of a search term in an article or book. In this case, the search term is "Haavelmo." Counts record the number of distinct articles or books, and not the number of distinct times, in which the term appears, so that multiple occurrences in a single work do not increase the count. There is considerable year-to-year variation—meaningless, given the ordinary vicissitudes of publication—and the moving sums smooth this out in roughly decadal blocks: the value for, say, 1945 is the count of citations over the period 1940 to 1950.

[4]Haavelmo is cited 534 times in articles in economics journals in JSTOR. (All citation counts exclude self-citations.) The individual citations, which pick out the number of times the actual title of his books or articles are cited, collectively add up to far fewer than that. The differences are most likely due to a) mention of Haavelmo's name without a particular citation to a journal; b) citations that either do not quote the title of the article or render it incorrectly; or c) citations to articles other than those that appear in JSTOR.

[5]Full bibliographic information for the articles cited in Figure 2 are given in Table 1 following the References.

[6]As my title suggests, I am concerned here only with Haavelmo's influence on econometrics, and not on any other field or on the profession more generally (say, in Norway, where his influence was profound). I am also restricted by my own linguistic incapacities to addressing the English-language literature.

FIGURE 1. Decadal Citations to Haavelmo's Works.

Haavelmo influenced the development of econometrics, so that it is essential to say something about how his work was received there. The first thing to notice is that the Cowles Commission itself had no doubt of Haavelmo's central importance. Haavelmo was appointed to an extraordinary position of non-residential research associate at the Cowles Commission in 1943 on the basis of the "de facto cooperation between the commission and Haavelmo. The appointment did not involve any salary and was, as Marschak said, 'a purely moral tie'" (Bjerkholt 2007, p. 813).[7] Haavelmo's (1943) paper on the estimation of simultaneous-equations models is praised as a "milestone" in econometrics. Hood and Koopmans (1953, p. 118), in the Cowles Commission's second econometrics volume, credit Haavelmo (1943, 1944), "whose penetrating analysis initiated the developments in methodology reported in this volume." Anderson (1991, p. 8), who himself contributed to the Cowles Commission's 1950 volume, sees the long methodological essay by Koopmans, Rubin, and Leipnik (1950, pp. 53–237) as a direct extension of Haavelmo's earlier work, making it available to applied work. Haavelmo himself contributed a paper to the first of the Cowles Commission's volumes and two papers to the second (Koopmans 1950, pp. 258–265; Hood and Koopmans 1953, pp. 75–91 [previously published as Haavelmo (1947)] and pp. 92–111 [previously published as Girshick and Haavelmo (1947)]).

What exactly did the Cowles Commission take from Haavelmo? What was there to take? "The Probability Approach" is a methodological *tour de force*—rich in ideas, detailed in execution. The chapter headings indicate that Haavelmo addresses such

[7]By the time of the Cowles Commission's conference, 27 January to 1 February 1945, Haavelmo was regarded as a "guest" of the commission as opposed to a member of its staff (Marschak's introduction to Koopmans 1950, p. 2).

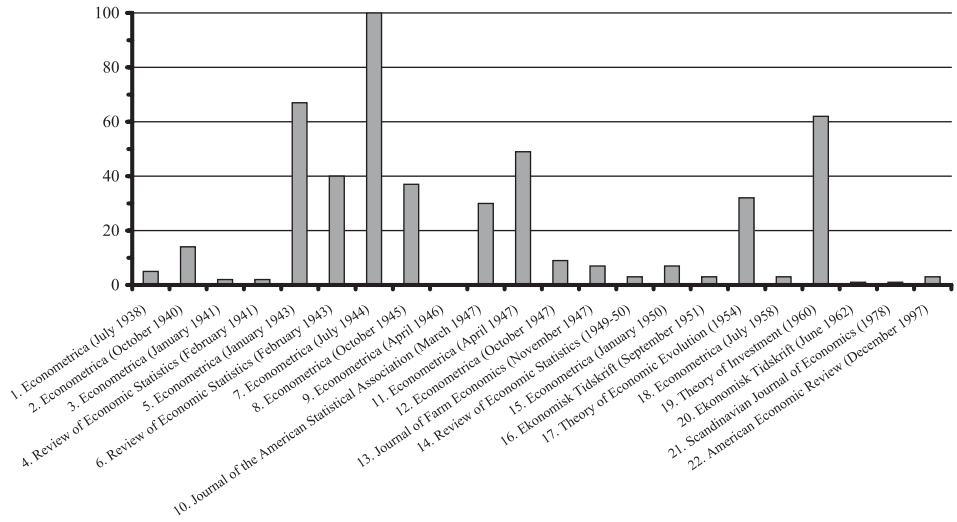

FIGURE 2. Citations to Haavelmo's Works.

fundamental questions as the relationship of economic models to reality, the stability and permanence of economic laws, the role of stochastic schemes in econometric relationships (i.e., the probability approach proper), statistical testing, estimation, and prediction. A lot of ground is covered in 115 pages.

The Cowles Commission's two econometric-theory volumes (Koopmans 1950, and Hood and Koopmans 1953) touch on most of themes introduced in "The Probability Approach." In some sense, the most pervasive connection is the general point of view—contrary to Keynes' (1939) assessment, which was shared by many economists—that economics did not present any insuperable barrier to the application of probability models. Very little is actually said on this point; rather, it is the background assumption for the entire enterprise. Anderson (1991, p. 18) suggests that the application of probability to econometrics was inevitable, and that Haavelmo's true originality lay in his analysis of simultaneous equations. As simultaneity was the dominant theme of the two econometric volumes, it is fair to say that Haavelmo's treatment of simultaneous equations was his principal legacy to the Cowles Commission.

The problem of identifying simultaneous economic relationships had been addressed by various economists during the early twentieth century (see Morgan 1990, ch. 6). Identification, as it was usually understood, is a problem of recovering the underlying behavioral economic relationships from the observable consequences of those relationships. It arises even in deterministic systems and, while it is solved by locating sources of independent variation, nothing says that such variation needs to be characterized stochastically (cf. Heckman 2000, pp. 45–48). Haavelmo provided a careful analysis of the conceptual basis for the identification problem.[8] In particular, he developed the

[8]Haavelmo's treatment of the identification problem originates with his teacher, Frisch (see Bjerkholt and Dupont 2010). According to Koopmans, Rubin, and Leipnik (1950, p. 70), "Haavelmo . . . has continued and extended Frisch's work in a very general discussion . . . of one central problem in identification. . . ."

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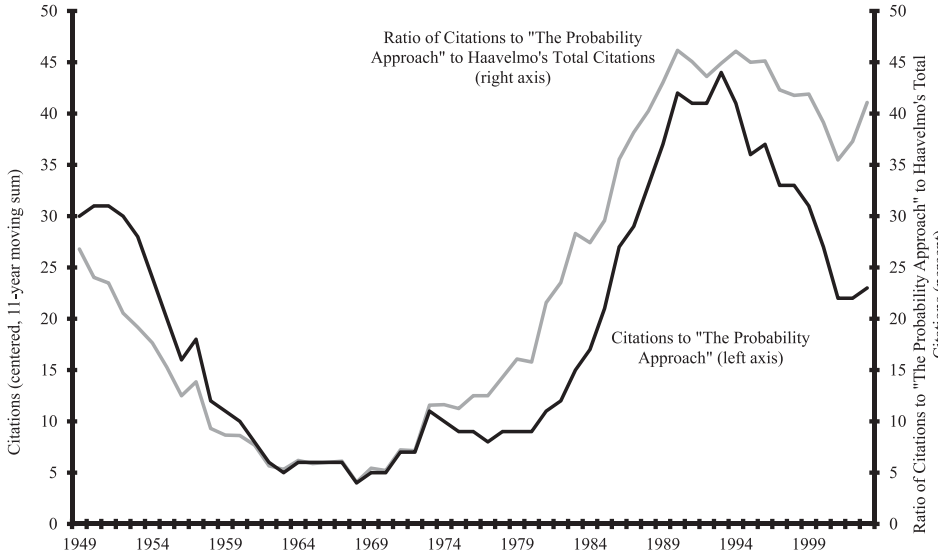

FIGURE 3. Citations to "The Probability Approach".

vocabulary of "structural" relationships; and, though he more frequently referred to Frisch's terminology of "confluent" relationships, he was aware of Mann and Wald's (1943) terminology of "reduced" form equations.[9] Contributors to the Cowles Commission's econometric volumes typically credit Haavelmo most significantly for carefully tying the deterministic identification problem to stochastic specifications. He noticed that *observational equivalence*—that is, the fact that two structurally distinct systems of stochastic equations might define the same likelihood function—is an analogous problem to traditional identification. And, crucially, he was the first to point out that the failure to account for simultaneity resulted in biased estimates of structural parameters (Haavelmo 1943).[10]

Haavelmo contributed to the detailed analysis of simultaneity. But the larger message that the Cowles Commission took away was his insistence on the systemic character of the stochastic, dynamic equations that characterized the economy. One could never forget that economics addresses interdependent systems, passively observed, and that this requires special strategies, and renders otherwise straightforward problems, such as statistical estimation, tricky. Haavelmo provided a preliminary map of the terrain of modern econometrics; the Cowles Commission used it to build a more detailed atlas, providing detailed accounts of the identification problem and structural estimation for systems of equations. They adopted some of Haavelmo's key moves.

Haavelmo (1944) had stressed the role of *a priori* economic theory in substituting for controlled experiments. Neither Haavelmo nor others at the Cowles Commission

[9]In fact, Marschak encouraged Haavelmo to collaborate with Mann and Wald on a joint monograph, though the collaboration never materialized (see Bjerkholt 2007).

[10]Despite its earlier date, and contrary to the assumption of, for example, Johnston (1963, p. 272), Haavelmo (1943) is the successor to Haavelmo (1944), which was written in 1941, and circulated widely before its publication date (Anderson 1991, p. 1).

Table 1. Key to Figure 2: Haavelmo's Works (Books and articles in JSTOR)

1. "The Method of Supplementary Confluent Relations, Illustrated by a Study of Stock Prices." *Econometrica* 6, 3 (July 1938): 203–218.
2. "The Inadequacy of Testing Dynamic Theory by Comparing Theoretical Solutions and Observed Cycles." *Econometrica* 8, 4 (October 1940): 312–321.
3. "A Note on the Variate Difference Method." *Econometrica* 9, 1 (January 1941): 74–79.
4. "The Effect of the Rate of Interest on Investment: A Note." *Review of Economic Statistics** 23, 1 (February 1941): 49–52.
5. "The Statistical Implications of a System of Simultaneous Equations." *Econometrica* 11, 1 (January 1943): 1–12.
6. "Statistical Testing of Business-Cycle Theories." *Review of Economic Statistics** 25, 1 (February 1943): 13–18.
7. "The Probability Approach in Econometrics," *Econometrica* 12, Supplement (July 1944): iii–115.
8. "Multiplier Effects of a Balanced Budget." *Econometrica* 13, 4 (October 1945): 311–318.
9. "Multiplier Effects of a Balanced Budget: Reply." *Econometrica* 14, 2 (April 1946): 156–158.
10. "Methods of Measuring the Marginal Propensity to Consume." *Journal of the American Statistical Association* (March 1947): 105–122.
11. "Statistical Analysis of the Demand for Food: Examples of Simultaneous Estimation of Structural Equations." *Econometrica* 15, 2 (April 1947): 79–110 (M.A. Girshick, co-author).
12. "Family Expenditures and the Marginal Propensity to Consume." *Econometrica* 15, 4 (October 1947): 335–341.
13. "Quantitative Research in Agricultural Economics: The Interdependence between Agriculture and the National Economy." *Journal of Farm Economics* 29, 4, Part 1 (November 1947): 910–924.
14. "A Note on the Theory of Investment." *Review of Economic Studies* 16, 2 (1949–50): 78–81.
15. "The Notion of Involuntary Economic Decisions." *Econometrica* 18, 1 (January 1950): 1–8.
16. "Begrepsapparatet i moderne inflasjonsteori. Noen refleksjoner i tilknytning til bent hansens avhandling: 'A Study in the Theory of Inflation.'" *Ekonomisk Tidskrift* 53, 3, (September 1951): 161–175.
17. A Study in the Theory of Economic Development. Amsterdam: North-Holland, 1954.
18. "The Role of the Econometrician in the Advancement of Economic Theory." *Econometrica* 26, 3 (July 1958): 351–357.
19. *A Study in the Theory of Investment.* Chicago: University of Chicago Press, 1960.
20. "Om sparing og inflasjon (On Saving and Inflation). Noen merknader til Sten Thore's doktoravhandling. 'Household Saving and the Price Level.'" Konjunkturinstitutet, Stockholm 1961. *Ekonomisk Tidskrift* 64, 2 (June 1962): 27–36.
21. "Wicksell on the Currency Theory vs. the Banking Principle." *Scandinavian Journal of Economics* 80 (2). The Arne Ryde Symposium on the Theoretical Contributions of Knut Wicksell, 1978, pp. 209–215.
22. "Econometrics and the Welfare State." *American Economic Review* 87 (6). Nobel Lectures and 1997 Survey of Members, December 1997, pp. 13–15.

*Frequently catalogued under the name adopted in 1948: *Review of Economics and Statistics*
Source*:* JSTOR journal archive, accessed October 2011

was altogether clear about the nature, scope, and evidential basis for economic theory. Haavelmo writes:

> Economic theory builds on the assumption that individuals' decisions to produce and to consume can be described by certain fundamental behavioristic relations, and that, besides, there are certain technical and institutional restrictions upon the freedom of choice (such as technical production functions, legal restrictions, etc.). (Haavelmo 1944, p. 28)

As a broad-brush description of the way that many modern economists understand economic theory, this is fine; but it is far too vague to describe exactly how identifying restrictions arise. Yet, theory is assigned that vital duty. It is easy to see, then, how later economists questioned whether modelers who claimed to follow the Cowles Commission's identification strategy were not actually imposing arbitrary restrictions with scant theoretical support (e.g., Liu 1960, Sims 1980).

I suspect that the apparent inadequacy of the theoretical foundations arises from the fact that the Cowles Commission took a rather looser view than did later economists (exemplified by Liu and Sims and the economists whom they regarded as their targets), of what theory ought to be: a theory was a sufficiently plausible mechanism that was used to provide a framework through which to view the data; it was *a priori* in the sense that it was not currently an object of question or debate, and not in the sense that it was logically compelling independently of empirical evidence and experience. Although the idea that econometrics of structural estimation is grounded in the application of *a priori* theory to the problem of identification is transmitted from Haavelmo through the Cowles Commission to form part of the folk wisdom of empirical economics, it is an underdeveloped and under-analyzed suggestion that has crystallized into firm dogma.

The methodological basis for Haavelmo's loose view of theory is found in his perspectival view of economic models (see Hoover 2012). On the one hand, Haavelmo (1944, p. 3) insists that models are creatures of our own minds and not truths to be discovered in the world. On the other hand, the test of models is their ability to describe an independent reality accurately from a particular point of view (pp. 12–13). Philosophically, Haavelmo is a kind of realist, in the sense that reality constrains what we find and how well our theories work empirically. However, theory is not a unique truth, but a flexible set of templates through which we view the world with greater or lesser success. His approach informs his discussion of *autonomy* (Haavelmo 1944, sec. 8; also see Aldrich 1989).[11] An autonomous relationship is one that remains invariant to interventions or changes in background conditions. But Haavelmo insists that autonomy is relative: some useful economic relationships are more stable or more invariant than others. Haavelmo's insightful discussion of autonomy and the failure of autonomy was picked up in Marschak's contribution to the second of the Cowles Commission's econometrics volumes; yet, by and large, Haavelmo's contribution was neglected and the topic re-emerged as a major concern only with Lucas' "policy-invariance critique" (Marschak 1953, esp. p. 25; Lucas 1976). Lucas credits Marschak, but ignores Haavelmo.

[11]The notion of *autonomy* was again one of a number of ideas that Haavelmo borrowed, extended, or elaborated from his teacher Ragnar Frisch (Frisch 1938; also see Bjerkholt and Dupont 2010).

The Cowles Commission's volumes neglect other aspects of "The Probability Approach." While they make a substantial contribution to the theory of estimation for simultaneous systems, they focus much less on statistical testing and display no special interest in the Neyman–Pearson testing framework that takes up a whole chapter in Haavelmo's monograph.

III. HAAVELMO IN THE TEXTBOOKS

Although Haavelmo's econometric writings of the 1940s have been credited with instigating a scientific revolution, it was a revolution mediated through the Cowles Commission (Morgan 1990, p. 256; Hendry 2000, p. 420). And, as Hastay (1951, pp. 388–389) noted fairly early, the Cowles Commission failed to adopt the whole of Haavelmo's methodology. What is more, after the second econometric-theory volume in 1953, there was not another one: the commission turned away from econometric theory to economic theory.[12] Anderson (1991, p. 13) attributes the waning interest in econometrics to the development of a theory for estimating simultaneous systems that had outrun the computational resources available at the time (see also Morgan 1990, p. 256; Heckman 1992, p. 883 offers a dissenting view). If influence were measured by new and direct contributions to scholarly discussion, we might mark the decline of the Cowles Commission's and, by extension, Haavelmo's influence from this point. Indeed, we have already seen (figures 1 and 3) that Haavelmo's citations by other economists collapsed later in the 1950s and 1960s, largely owing to the fall in references to "The Probability Approach." There are also indirect channels of influence. Sometimes, when ideas become particularly important and widely received, it is no longer necessary to cite them. They fade into the background as basic presuppositions that are no longer discussed because they are generally accepted. Then, we are more likely to find them, not in cutting-edge research, but in the textbooks through which the next generation of researchers is trained. In fact, the ideas of Haavelmo and the Cowles Commission were incorporated into textbooks, so that—as we have already observed—forty years later, Heckman took them to be econometric orthodoxy.

One way that I have tried to get some idea of the reception of Haavelmo's econometric thought is to use Google Books to conduct various searches. Generally, the scope of the search is limited by requiring that the word "econometrics" appear in the title. The aim of the search is to identify econometrics textbooks, but some caution is in order: the search parameters are crude, and there is no guarantee that every "econometrics" book is a textbook, as we normally understand the term. These data and all the search results using Google Books presented subsequently should be taken impressionistically rather than precisely, conveying some rough relationships rather than conceptually and quantitatively precise information.

[12]A referee observes that *econometrics* had various meanings historically, which is, of course, correct. Frisch's (1933) conception at the founding of the Econometric Society was expansive, embracing virtually all formal, mathematical, and quantitative economics. But by 1953—the date of the second of the Cowles Commission's volumes on econometric theory—the term "econometrics" had largely stabilized around its most common modern meaning as a field clearly distinct from formal mathematical economic theory that focused on statistical and other methods aimed at identifying, estimating, and testing economic relationships.

The left-hand bar for each date in Figure 4 shows econometrics books that also cite Haavelmo in the text. Haavelmo is well cited in the 1944–53 period and remains well cited through the 1980s. In contrast to his citations in scholarly articles, which rose moving into the 1990s (around the time of his Nobel Prize), his citations fall somewhat in the last three decades, though they do not display the kind of collapse evident in Figure 1. The right-hand bar for each date shows econometrics books that cite Haavelmo's "Probability Approach." Here, the story is very different. It is well cited in the decade following its publication. Its citations fall to zero after 1953. In subsequent decades, the citations climb slowly back to moderate levels. This pattern mirrors the collapse and recovery of scholarly citations to "The Probability Approach" documented in Figure 3.

To put some flesh on the bibliometric data, I conducted an unsystematic survey of econometrics textbooks based on a sample of convenience; namely, the forty-six econometrics textbooks on my own shelves. Since I am an historian and bibliophile, the start date of my collection is not the same as the date that I myself started studying economics, nor is its focus limited to my own research interests. The sample ranges from the earliest textbook (Davis 1941) right up to the present with a good representation from the 1950s and 1960s. The books include those by econometric pioneers such as Davis, Klein, and Tintner; by important representatives of the emerging class of specialized econometricians in the 1950s and 1960s, such as Christ, Johnston, Thiel, Valavanis, and Zellner; by lesser known pedagogues, such as Dutta and Stewart; by recent time-series econometricians, such as Hamilton, Harvey, Hendry, Juselius, and Spanos; and by recent microeconometricians such as Trivedi and Cameron, and Amemiya. The books are all in English, although Malinvaud is translated from French.

Of the forty-six volumes examined, twenty do not cite Haavelmo at all. The pattern of citations mirrors the pattern in scholarly journals reported in Figure 2. "The Probability Approach" is far and away the most-cited work, followed by "The Statistical Implications of a System of Simultaneous Equations" (1943), and

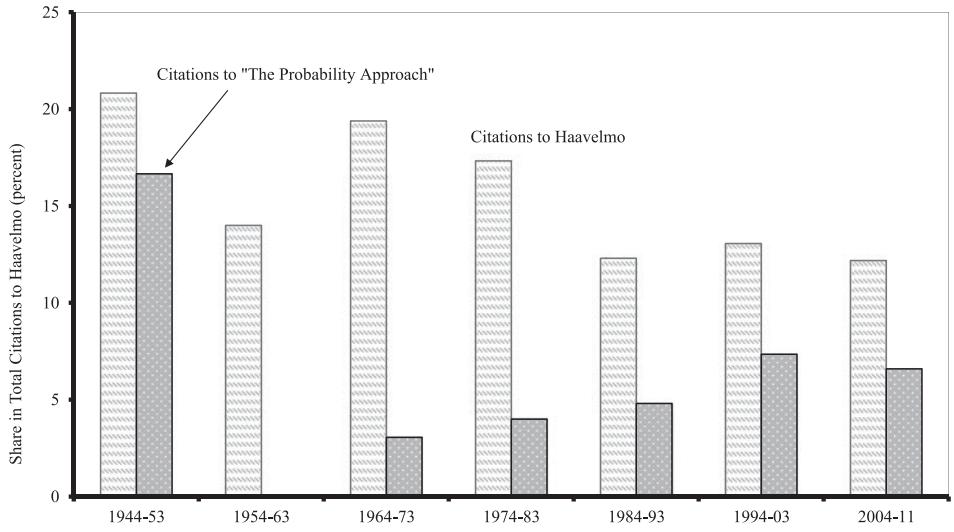

FIGURE 4. Citations to Haavelmo and "The Probability Approach" in Econometrics B.

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"Statistical Analysis of the Demand for Food" (Girshick and Haavelmo 1947). The main focus of the citations to "The Probability Approach" is on the nature of systems of simultaneous equations, their identification, and estimation. As a result, "The Probability Approach" is often cited in close proximity to Haavelmo's "Statistical Implications." Girshick and Haavelmo's "Demand for Food" is not typically cited for any positive methodological point, or for the substance of its results; rather, it is taken to be a canonical example of the estimation that takes proper account of simultaneous equations bias, and parts of it are typically reproduced in fine detail.

Aside from the top three econometrics works, none of Haavelmo's other papers appear to be highly cited in the textbooks. The vast majority of the citations occur in the 1950s, with almost none after the 1960s. This early/late division is reflected in a more complicated way with respect to the top three articles. Girshick and Haavelmo's "Demand for Food" follows the pattern exactly. But both the "Probability Approach" and "Statistical Implications," which are well cited among early textbooks, are cited far less frequently in the 1960s and 1970s.

The transformation in econometrics textbooks from giving explicit credit to Haavelmo to incorporating his ideas as common econometric knowledge is nicely illustrated by the three solo-authored editions of Jack Johnston's *Econometric Methods*. The first edition, published in 1963, cites Haavelmo's "Probability Approach" and refers to "Statistical Implications" as the "seminal article" on estimating simultaneous equations (Johnston 1963, pp. 204, 272, 288). Yet, in the 1972 and 1984 editions, with little change in the substance, Haavelmo is not cited at all.

As we have already observed, the failure to cite a scholar is not equivalent to that scholar's ideas failing to have influence: ideas frequently take on an autonomous life of their own. Figure 5 tracks key words in econometrics books using Google Books.

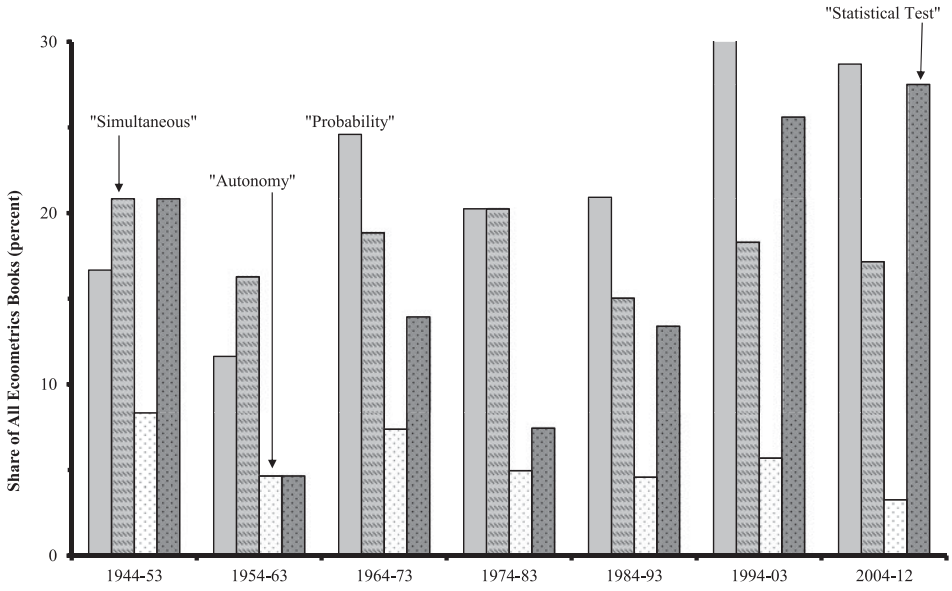

FIGURE 5. Topics in Econometric Textbooks.

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(Again, I have to insist on the crudeness of this method and to suggest an impressionistic interpretation of the data.) The key words are "simultaneous," "probability," "autonomy," and the co-occurrence of "statistical" and "test." The figure conveys several relevant impressions. First, references to simultaneity have remained moderately high over the entire period, although varying somewhat from decade to decade. Second, references to statistical tests started off strongly in the first decade after the publication of the "Probability Approach," only to fall sharply in the next three decades, but then to rise steadily, starting in the mid-1980s. Third, "probability" remains a strong theme throughout the period, though it becomes the most frequently cited of the search terms after 1994. Finally, "autonomy" is the least frequently cited search term throughout the period; its citations in every subsequent decade are lower than those in the first.

Overall, Haavelmo's key themes are well represented in later books. The low level of references to "autonomy" probably reflects a mixture of neglect of the issue in the early decades and a change in terminology in the later decades, in which the same issues are discussed under the headings of "policy noninvariance" or "the Lucas critique." In contrast, the increase in references to statistical tests and probability in the later periods probably reflects a genuine shift of emphasis, as neglected themes in the "Probability Approach" were rediscovered. The increased incidence of attention to probability and statistical testing over the past two decades coincides with a shift in the coverage in my textbooks as well. A renewed interest in Haavelmo—and, indeed, in these particular aspects of his thought—is evident in Spanos' (1986) textbook. Haavelmo is also a key figure in Hendry's (1995) *Dynamic Econometrics* and in Juselius' (2006) textbook on the cointegrated vector autoregression.

IV. HAAVELMO'S ENDURING LEGACY

The revival of citations to Haavelmo in the late 1980s and especially after 1990 may be explained in a variety of ways. One possibility is that Haavelmo was a great econometrician whose principal contributions became so well integrated into econometric practice that he himself had faded from view. His Nobel Prize brought him back into professional consciousness and, indeed, raised his status so that citations increased, although these citations were largely ornamental rather than substantive—a little history to add grace to a technical paper. The fact that citations to Haavelmo take off after his Nobel Prize suggest this hypothesis.

Another possibility, however, is that the revival of citations to Haavelmo represents the continued interest and utility of his ideas, and that Haavelmo's stock has risen as some key ideas, which were, by and large, ignored in the 1960s and 1970s, became salient and motivated the development of econometrics for the first time. On this hypothesis, history did not purvey superfluous ornaments; rather, it tended the flame and prevented the fire of Haavelmo's original ideas from burning out altogether. Haavelmo's revival was not, then, an historical grace note, but a reassessment of his revolution and a reinterpretation of the essence of the "The Probability Approach." The treatment of Haavelmo in the textbooks cited at the end of the last section, as well as the rapid take-off of discussions of probability and testing in the last decade (Figure 5), lend some credence to this hypothesis.

Impressionistic but suggestive evidence in Figure 6 provides an imperfect test of the competing hypotheses. I examined each of the papers in JSTOR that cite "The Probability Approach" and classified them by decade and by the nature of the citing paper. The papers were divided into four categories: 1) *economic and econometric methods*: papers that seek to develop specific methods to be used in the practice of economics and econometrics; 2) *methodology*: papers that provide philosophically oriented discussions of the nature and practice of economics; 3) *applied economics*: papers that seek to use economics or econometrics to analyze particular real-world economic problems; and 4) *history*: papers that recount the development of econometric ideas through time, including memoirs and interviews.

In the first decade after its publication, "The Probability Approach" was cited most by papers concerned with econometric methods and about equally in methodological and applied papers. As we have already seen, citations dropped precipitously in subsequent decades. In the decade after 1984, history citations dominate. This is hardly surprising, since the period includes Haavelmo's winning the Nobel Prize and all the attendant retrospectives and encomia, all of which are counted as history. What is more important, however, is that citations in works on method began to rise in the decade after 1974, in works on methodology after 1984, and in applied work after 1994.[13] Although Haavelmo has clearly become an historical figure, these data suggest that he is more than that and that there may be something to the notion that Haavelmo enjoyed a rebirth in substantive influence and not merely in historical fame.

What is beyond dispute is that a number of important economists have felt that they needed to re-evaluate Haavelmo's legacy. The re-evaluation was driven in part by the emergence of the history of econometrics as a special focus in the history of economics, especially with Morgan's *History of Econometric Ideas* (1990), which presented an interpretation of Haavelmo very much focused on his role in introducing probability and statistical testing into econometrics—downplaying, though not ignoring, the treatment of simultaneous equations as his fundamental contribution. Another element in the re-evaluation was the growing dissatisfaction with the Cowles Commission's conception of econometrics. If the Cowles Commission's approach is faulty and Haavelmo is the principal inspiration of that approach, then Haavelmo himself becomes a focus for criticism. Such a view is clearly articulated in Heckman's (1992) review of Morgan's history and in his millennial reassessment of the state of econometrics (Heckman 2000; see also Eichenbaum 1995).

Morgan's history grows out of—and perhaps helped to inspire—a different view. On this view, Haavelmo's ideas are transmitted through the Cowles Commission to become mainstream econometrics, but the transmission was only partial, and the Cowles Commission and mainstream econometrics failed to develop some central aspects of his thought. In particular, placing the emphasis on the identification and estimation of simultaneous equations, they read his monograph as if it were "The Simultaneous Equations Approach in Econometrics" rather than as "The Probability Approach." Such a reading ignored—or, at least, downplayed—some of Haavelmo's main themes: the methodology of modeling, the role of probability, and the nature and utility of statistical testing.

[13]The low numbers after 2004 in all categories must be treated with care because of the lag with which journals become available in JSTOR.

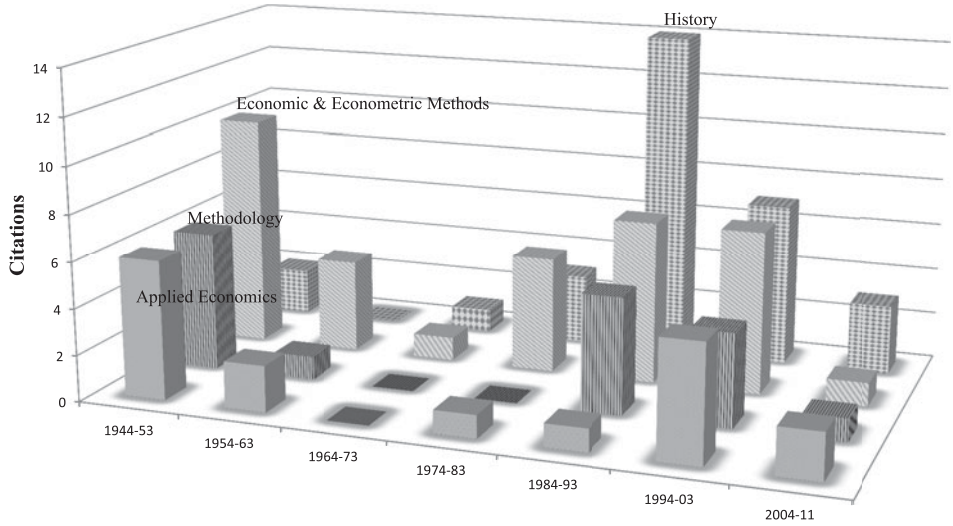

FIGURE 6. Citations to Haavelmo's "Probability Approach" by Type of Citing Article.

That Haavelmo's thought was open to opposing interpretations is not new. Morgan (1990, p. 252) notes—and Heckman (1992, p. 880) acknowledges—that both sides in the so-called measurement-without-theory debate between Koopmans for the Cowles Commission and Vining for the National Bureau of Economic Research in 1948–49 claimed the support of Haavelmo and the "The Probability Approach" (see Hendry and Morgan 1995, ch. 43 for the relevant papers). A similar difference of opinion and, in fact, some of the same issues have reappeared in debates over econometric methodology during the past two decades.

One side sees Haavelmo's contribution as having run its course and having proved over time to be inadequate to the needs of econometrics. These critics start with a particular conception of how Haavelmo imagines the construction of econometric models and their statistical testing. Malinvaud (1966, p. 1) provides a clear statement of this interpretation of Haavelmo's position:

> there exists a model, given a priori, which specifies certain general assumptions about the nature of the relationships among the x_i [i.e., the observed variables]. This model, that is to say the set of assumptions which it contains, will always be accepted without question. It will be considered to be perfectly applicable to the data on the one hand, and on the other hand to the situations in which the results of the econometric analysis will be used. In short, [the approach] deals with the problem of determining procedures which allow the set of the x_{it} to be used in order to ascertain certain elements of a model relating to the x_i, the general form of this model being given a priori.

This point of view . . . should not be considered unusual, since it is now adopted throughout mathematical statistics. However, it was not in force in the initial stages of econometrics and only appeared clearly formulated in Haavelmo [1944].

Eichenbaum (1995, p. 1619) implicitly accepts Malinvaud's characterization of Haavelmo and explicitly attacks Haavelmo's "classic programme" as

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> irrelevant to the inductive process by which theory actually evolves . . . . [It] conceives of economic theorists, unsullied by data, working in splendid isolation, and "somehow" generating hypotheses. Only when these hypotheses appear, does the econometrician enter. Armed with an array of tools he goes about his grim task—testing and rejecting models. This task complete, the econometrician returns to the laboratory in order to generate ever increasingly powerful tools for rejecting models. The theorist, no doubt stunned and disappointed to find that his model is false, returns to his office and continues his search for the "true" model.

Heckman levels a similar charge:

> The Haavelmo–Cowles way of doing business—to postulate a class of models *in advance of looking at the data* and to consider identification problems with the prescribed class—denies one commonly used process of inductive inference that leads to empirical discovery. It supposes that a wide class of models can be, or has been, enumerated in advance of looking at the data and that empirical work consists of picking one element in a fixed set. More often, empirical work suggests rich new classes of models that could not have been anticipated before the data are analyzed. (Heckman 1992, p. 883)

Heckman (2000, pp. 86–87) goes beyond Eichenbaum in specifically locating his objection in Haavelmo's use of Neyman–Pearson statistics, arguing that "classical statistics" separates the art of constructing models from verifying them. The Neyman–Pearson model on this view frames a question against a fixed space of models and answers it only with a yes or no. Neither the Neyman–Pearson framework nor Haavelmo's apriorism leaves any room for learning; it is implausible that the space of all interesting and useful models can be enumerated in advance (Heckman 1992, pp. 883–884; 2000, p. 87).

In contrast, Heckman (1992, p. 884) advocates exploratory data analysis and other strategies of learning from the data. Eichenbaum (1995, p. 1609) argues that what is needed most, for example, in recent dynamic stochastic general equilibrium models, are diagnostic tools to understand better where models fail on relevant dimensions. Eichenbaum (1995, p. 1620) notices a difference between his characterization of Haavelmo's methodological views and Haavelmo's practices in his applied work on investment (Haavelmo 1960), noting that Haavelmo does not hesitate to learn from the data.

Eichenbaum sees cognitive dissonance between Haavelmo's methodological theory and applied work. But there is another possibility: Eichenbaum and Heckman are attacking a straw man and have projected a rigidity onto Haavelmo's methodological thinking that it simply never possessed. Eichenbaum (1995, p. 1619), for instance, quotes the following passage from Haavelmo (1944) as "the key problem he chose to emphasise": "the problem of splitting on the basis of data, all a priori theories about certain variables into two groups, one containing the admissible theories, the other containing those that must be rejected" (Haavelmo 1944, p. 10).

Eichenbaum adds archly, "Indeed! I suppose that one day when theorists have run out of ideas and all *a priori* theories have been conceived of, the econometrician's job can begin in earnest. If ever there was a programme designed to minimize the interaction of theorists and econometricians this must be it" (Eichenbaum 1995, p. 1619).

Eichenbaum cited one point as "the key problem," which, in fact, was only third on the list of four problems that summarized the scope of "The Probability Approach."

What is more, the quotation is misleading, since it omits the first words of the sentence: "The problem of estimation. . . ." In other words, contrary to Eichenbaum's implication, Haavelmo's remark is not aimed at testing *per se*, but at estimation. It amounts to an interesting interpretation of estimation as a form of testing, but it in no way warrants Eichenbaum's interpretation that every possible theory must be enumerated in advance before econometrics can begin.

Heckman's interpretation of Haavelmo as committed to an extreme apriorism is similar to Eichenbaum's: "Econometricians operating within the Haavelmo paradigm too easily forget that a priori theories are often just condensations of accumulated empirical knowledge acquired using crude empirical methods" (Heckman 2000, p. 88).

It is nearly impossible to distinguish Haavelmo's description of the first problem from the point that Heckman means to deploy *against* him:

> 1. The construction of tentative models. It is almost impossible, it seems, to describe exactly how a scientist goes about constructing a model. It is a creative process, an art, operating with rationalized notions of some real phenomena and of the mechanism by which they are produced. The whole idea of such models rests upon a belief, already backed by a vast amount of experience in many fields, in the existence of certain elements of invariance in a relation between real phenomena, provided we succeed in bringing together the right ones. (Haavelmo 1944, p. 10)

So, yes, Haavelmo does not present a detailed account of the creation of theories or models—neither, indeed, do Heckman or Eichenbaum—but equally he does not conceive of *a priori* theory as the product of the armchair, but, just as Heckman conceives of it, as the product of largely informal empirical experience. Haavelmo (1944, p. 14) approvingly cites Bertrand Russell's remark that science is always an interplay of observation, hypothesis, and theory.

Haavelmo's second problem places testing ahead of estimation in his conception of econometrics: "2. The testing of theories, which is the problem of deciding, on the basis of data, whether to maintain and use a certain theory or to dismiss it in exchange for another" (Haavelmo 1944, p. 10). Far from highlighting the need for complete enumeration of all possible theories *pace* Heckman and Eichenbaum, Haavelmo treats the testing problem as a pair-wise contest between theories adjudicated by data. Nothing suggests that a new theory cannot be introduced into the competition on the basis of empirical evidence gained through investigation.

Why, then, do Heckman and Eichenbaum read Haavelmo as an extreme apriorist? The short answer is because of his advocacy of Neyman–Pearson testing, and the idea that Neyman–Pearson testing requires predesignation of the space of alternative hypotheses. Haavelmo explicitly considers and rejects extreme apriorism that rules out of court any adaptation of theories to data—reducing econometricians to the grim executioners of economic theories. Noting that many reject the idea that crafting a set of difference equations that fit business-cycle data could be an adequate test of the theory embodied in those data, Haavelmo writes:

> This argument, however, does not quite cover the real trouble point. In fact, if we could establish that the observed variables satisfied very closely a certain system of linear difference equations (say), we should have a strong and very useful restriction upon the class of a priori admissible theoretical models. In general, whenever we can establish that certain data satisfy certain relationships, we add something to our

> knowledge, namely a restriction of the class of a priori admissible hypotheses. The real difficulty lies in deciding whether or not a given relation is actually compatible with the data; and the important thing to be analyzed is the reliability of the test by which the decision is made, since we have to deal with stochastic relations and random variables, not exact relations.
>
> From this point of view there is, therefore, no justified objection against trying out various theories to find one which "fits the data." But objections may be made against certain methods of testing the fit. (Haavelmo 1944, pp. 82–83)

Haavelmo conceives of the statistical test as discriminating among the members of a set of admissible hypotheses, these hypotheses having been enumerated in advance of the test. But, 'enumerated in advance of the test' does not mean generated out of one's mind, unsullied by data: "It is clearly irrelevant *how* we happen to choose the hypothesis to be tested. . . . In particular, the hypothesis might be one that suggests itself by *inspection of the data*" (Haavelmo 1944, p. 83). All that is required, in Haavelmo's view, is that, for a given test, the set of admissible alternatives remained fixed *a priori*. By "*a priori,*" Haavelmo does not mean in advance of any knowledge of the data, but merely that, with respect to the test, the set of admissible hypotheses should not be a function of the sample point. If anything—a dream or an inspiration of the data themselves—suggests a new hypothesis. There is no objection, in Haavelmo's view, to enlarging the set of admissible hypotheses and conducting a new test. He rules out neither the role of insight and imagination nor specification search.

Haavelmo justifies his insistence on *a priori* specification of the admissible hypotheses in a statistical test by the need for a formal framework in which the notions of size and power (or type I and type II error) have precise, quantifiable counterparts. His position in this case is an extension of his general view that knowledge is perspectival: we can understand—or even properly observe—empirical reality only through a theoretical framework (cf. Hoover 2012). But the questions of the implications of that formal framework and of the degree to which it provides a fruitful window on reality are distinct: "it is one thing to build a theoretical model, it is another thing to give rules for choosing the facts to which the theoretical model is to be applied" (Haavelmo 1944, p. 4; also pp. iv, 1–4, 12–13).

With respect to a Neyman–Pearson test, Haavelmo (1944, p. 81) is quite specific that the class of *a priori* admissible hypotheses is not the class of all possible hypotheses, but only of those that we regard as fruitful because they are reasonable and tractable with respect to the statistical tools at hand. Haavelmo explicitly considers the possibility that the class of admissible hypotheses might be incomplete and that examining the power of tests against hypotheses *outside* the admissible class might be enlightening.

On the one hand, Haavelmo does insist that the statistical formulation of models is essential. He rejects the view that the tools of statistics are open to those who reject the foundations of statistics: "For *no tool developed in the theory of statistics has any meaning*—except, perhaps, for descriptive purposes—*without being referred to some stochastic scheme* (Haavelmo 1944, p. iii). In keeping with his general views on modeling, he also insists that the stochastic specification is an *integral* part of an economic model and "not merely some superficial additions 'for statistical purposes'" (Haavelmo 1944, p. 51). In discussing the isolation of autonomous relationships in economics, Haavelmo notes: "In scientific research—in the field of economics as well as in other

fields—our search for 'explanations' consists of digging down to more fundamental relations than those that appear before us when we merely 'stand and look'" (Haavelmo 1944, p. 38). His attitude here, in conjunction with his general approach to the relationship between models and reality and to his treatment of search in the space of hypotheses, suggests that Haavelmo would welcome the use of statistical testing to validate the stochastic specification ahead of statistical testing of economic hypotheses.

Time and again, Haavelmo stresses the importance of precise, formal analysis of a model; but time and again, he also stresses that the point of the model is to illuminate reality and that reality provides the ultimate test. He is, thus, not the advocate of rigid procedures of whom Malinvaud approves and whom Heckman and Eichenbaum excoriate. The question of whether the formal set-up of the theory (including its statistical specification) is correct "is, strictly speaking, always justified when we try to explain reality by a theoretical model. But if we follow this attitude to its bitter end, we shall never be able to accomplish anything in the way of explaining real phenomena" (Haavelmo 1944, p. 81).

Heckman's position, at least (I am unclear about Eichenbaum's), is that Haavelmo's methodology was a valuable contribution to econometric theory, but one that is ultimately unworkable and ineffective—a dead end. In laying out the arguments against this position, I have tipped my hand in favor of an alternative interpretation: that econometrics in the post-war period adopted only parts of Haavelmo's methodology and that the parts that they de-emphasized have recently been revived and are making a vital methodological contribution. A good example of the revised interpretation of Haavelmo is found in the opening chapter of Juselius' (2006) book on the cointegrated vector autoregression. Juselius draws three main points from Haavelmo. First, stochastic specification is an integral part of an economic model and that all the same questions of adequacy that one might direct toward the purely economic (deterministic) parts of a model must also be directed toward the stochastic specification. Second, statistical testing is not valid or empirically revealing without correct specification of the stochastic model (see also Hoover, Johansen, and Juselius 2008). Third, these points imply that the issues that Haavelmo raise with respect to simultaneous equations, including his discussion of autonomy, cannot be separated from the probabilistic aspects of "The Probability Approach." For example, Haavelmo's (1943) paper on estimation in the presence of simultaneity simply presents a concrete illustration of the more general point that misspecification (in this case, treating as a single-equation problem what is really a simultaneous-equation problem) leads to inappropriate *statistical* estimates. Statistical tests are valid only with respect to models of the phenomena that include a probability distribution that adequately characterizes the real phenomena (Haavelmo 1944, p. 66).[14]

Economists who use the LSE (London School of Economics) approach offer a complementary interpretation of Haavelmo that focuses less on simultaneous equations

[14]Stressing the explicit parallels that Haavelmo draws between adequate specification of a model in order to support passive observation and controls in active experimentation, both Spanos (1986) and Juselius (2006) relate Haavelmo's approach to the statistical methodology of R. A. Fisher. In fact, Haavelmo (1944) never cites Fisher, but takes the Neyman–Pearson approach as his statistical touchstone. In other contexts, however, the philosopher Deborah Mayo (1996, ch. 11) has argued that there is less difference between Fisher and Neyman and Pearson than is frequently thought and, in fact, that Pearson, in particular, never had the commitment to the purely decision-theoretic account of statistical inference that is usually taken to be the message of the Neyman–Pearson approach.

and more on the foundations of statistical inference in economic models (Spanos 1986; Hendry, Spanos, and Ericsson 1989; Hendry 1995). They focus on the role of the joint probability distribution of the observed variables, which Spanos (1989, p. 411) refers to as the "Haavelmo distribution." Koopmans, Rubin, and Leipnik had already noted its importance to Haavelmo's approach in the first Cowles Commission econometrics volume (Koopmans 1950, p. 55), and Hastay (1951) dubbed it the "Haavelmo proposition." Though it was sometimes recalled (e.g., by Valavanis 1959, pp. 64–66), it had, until the 1980s, by and large, dropped from active consideration. The central idea is that the Haavelmo distribution provides the general probability model within which statistical tests of all admissible models may be legitimately conducted. Hendry (1995, p. 319) cites it as providing the rationale for preferring a general-to-specific as opposed to a specific-to-general modeling strategy.

With reference to broader (not specifically stochastic) modeling issues, Haavelmo observes some of the difficulties of a specific-to-general approach:

> As ["cold-blooded" empiricists] go on collecting better and better observations, they see that their "copy" of reality needs "repair." And, successively, their schemes grow into labyrinths of "extra assumptions" and "special cases," the whole apparatus becoming more and more difficult to manage. Some clearing work is needed, and the key to such clearing is found in a priori reasoning, leading to the introduction of some very general—and often very simple—principles and relationships, from which whole classes of apparently very different things may be deduced. (Haavelmo 1944, p. 12)

More specifically, the probability distribution must, in Haavelmo's view, be more general than (i.e., must include as special cases) all admissible hypotheses or else "we have lost the control of errors, originally ascribed to the test" (Haavelmo 1944, p. 66).

Heckman (1992, p. 882) acknowledges the LSE approach, but denies that it is grounded in Haavelmo's "Probability Approach." Haavelmo, he argues, never analyzed model creation or selection. If such an analysis must be as detailed as, say, Hendry's accounts of the general-to-specific modeling strategy, then Heckman is correct. But, as we have seen, Haavelmo does provide both general methodological guidance that points to the importance of learning from models and adapting them to data, and specific guidance on the use of the tools of probability and statistics in that task.[15] While noting that "there are serious problems in using the data that suggest a theory to test that theory," Heckman (2000, p. 87) argues that strict adherence to a rigid testing framework in the manner of Malinvaud's interpretation of Haavelmo's probability approach imposes the bigger cost that we cease to learn from data. Both Heckman and Eichenbaum reject what they regard as a kind of statistical Puritanism in order to gain a better grasp on reality. Oddly, that the costs of inference outweigh the costs of search is also one of Hendry's central points (Hendry and Krolzig, 2005, p. C40).

The final irony of the debate over the meaning of Haavelmo's "Probability Approach" is this: the very practices that Heckman and Eichenbaum believe, on the

[15]Heckman (1992, p. 882) goes on to point out that the effectiveness of LSE methods "have never been rigorously established, even for analyses on large samples." And Heckman (2000, p. 87, fn. 66) claims that the tests used in general-to-specific search are sensitive to the order in which they are performed. In the meantime, we now have considerable simulation and theoretical evidence for the efficacy of general-to-specific procedures (see White 1990; Hoover and Perez 1999, 2004; Krolzig and Hendry 2001; Hendry and Krolzig 2005; Doornik 2009; Castle, Doornik, and Hendry 2011; Hendry and Johansen 2011.)

one hand, to be essential to the progress of empirical work in economics and, on the other hand, to be ruled out by Haavelmo's methodology are the practices that Spanos, Hendry, Juselius, and others believe to be implied by that methodology.

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