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The Role of the Cowles Commission in the History of Information Economics*

ABSTRACT. There is, as yet, no really comprehensive history of the role of the Cowles Commission in the development of economics in the US. There are, of course, some partial contributions concentrating mostly on the early achievements of the Cowles Commission in econometrics, which turned out to be perhaps the least important aspect of its history. The authors focus instead on the role of the Cowles Commission in introducing the models of information into American economic orthodoxy. Major contributors to the development of the economics of information include Tjalling Koopmans, Jacob Marschak, Kenneth Arrow, Herbert Simon, Stanley Reiter and Leonid Hurwicz. As economics gradually switched from being past-oriented to future-oriented, 'information' became an elementary tool which allowed to explain how the inscrutable future causes economic changes in the present. The Cowles Commissions' research agenda comprised both interpreting 'information' as a thing, and treating it as subordinate to a technology of inductive inference. Also nascent computer science played a role in the development of the economics of information. The authors also discuss the reasons why it was at the Cowles Commission where the templates for the economics of information were designed.

KEY WORDS: philosophy of economics, history of economic thought, economics of information, econometrics, Cowles Commission

1. Introduction

There is, as yet, no really comprehensive history of the role of the Cowles Commission in the development of economics in the US.[1] The

* This is a revised version of Chapter 8 of our forthcoming book, *The Knowledge we have Lost in Information*, Oxford, 2017.

[1] Some internalist memoirs include Christ, 1994; Hildreth 1986; Klein, 1991, Warsh, 1993. There is a tendency in these texts to stress the early achievements of Cowles in eco-

Cowles Commission was chartered in Colorado Springs in 1932 by the businessman Alfred Cowles, initially to serve as a sort of boutique research unit to argue against the Depression-era NRA, to pursue monetary reforms, and to explore what went wrong with the stock market. When Alfred Cowles was himself forced to move to Chicago in 1939, he worked out a deal with President Robert Hutchins and trustee Laird Bell to have the unit enjoy a semi-formal relationship with the University of Chicago [see Warsh, 1993, p. 64]. Economics faculty Oscar Lange, Jacob Mosak and Gregg Lewis were given positions as part-time staff, which began to change the tenor of the work done there. It was only at this juncture that Cowles became narrowly neoclassical, although this stance was not yet common in the US. WWII severely depleted the staff at Cowles, and by the mid-1940s, its continued existence was in doubt. A new round of hiring, beginning with Leonid Hurwicz in 1944 (after serving as an assistant to Lange and Theodore Yntema) and Jacob Marschak in 1943, began to recast the unit in its more recognizably modern guise, and in turn, staff the Commission with new Economics faculty members; that novel identity was cemented in place with the appointment of Tjalling Koopmans as research director in 1948 (having been appointed associate professor at Chicago since 1946), and the forging of direct ties with RAND and the military from thenceforth [for details, see Mirowski, 2002, pp. 216–220]. Kenneth Arrow was a research associate from 1947, and appointed a faculty member at Chicago in 1948, at which point Cowles was permitted to propose faculty hires solely from within the unit.

With hindsight, we now can appreciate that the Cowles Commission was the citadel of this political movement to forge a market socialism, at least until that organization picked up and moved to Yale in 1954. This had at least two signal implications: First, Cowles members jointly felt they

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nometrics, which turned out to be perhaps the least important aspect of its history. If we had to summarize its achievements in order of importance, they would be: (1) The introduction of the models of information into American orthodoxy; (2) genesis and promotion of the Walrasian general equilibrium as neoclassical American orthodoxy; (3) innovation of the (anti-Keynesian) rational expectations approach to macroeconomics and money; and (4) the development of full-information maximum likelihood statistical techniques for the estimation of sets of simultaneous equations.

had to respond to, and possibly refute, Hayek and the MPS representation at the Chicago economics department, because the Socialist Calculation Controversy was the greatest intellectual threat to their vision of the role of the scientific economist in society in that era. Furthermore, stoking their motivation from 1950 onwards, Hayek was physically there on the ground at Chicago, lodged at the Committee on Social Thought. Austria had come to Chicago, to the consternation of all and sundry. Marschak and Hurwicz had already clashed with Hayek and comrades well before this galling development; but now they inhabited the very same campus. As early as 1940, Hurwicz found the economists at Chicago, "very reactionary and orthodox. I met Viner, Knight and the other local celebrities… and didn't think very much of them."[2] Marschak had been one of the readers for the University of Chicago Press of the manuscript of *Road to Serfdom*; he did recommend publication, but made clear his disagreements with its contents, noting "the book is almost exclusively critical, not constructive".[3] Marschak opposed creating a Frank Knight chaired professorship at Chicago on the following grounds: "it is probably unavoidable that in filling a Frank H. Knight chair preference would have to be given – always, or at least for the next twenty years – to followers of a particular orientation in economic policy, even when candidates of higher scientific objective merit were available. Would it be possible to honor the great Martin Luther's memory by a chair and offer it to an outstanding Catholic thinker?" [4] Others at Cowles were no less stalwart in their commitment in refighting the Socialist Calculation Controversy after WWII. Tjalling Koopmans prefaced the proceedings of the Cowles conference on 'linear programming' with the following observation:

> Particular use is made of those discussions in welfare economics (opened by a challenge of L. von Mises) that dealt with the possibility of economics calculation in a socialist society. The notion of prices as constituting the information that

[2] Hurwicz to Ruth Schechter, 9/12/1940; Leonid Hurwicz Papers, Duke Archives, Box 23, File: Correspondence 1940.

[3] Jacob Marschak papers, UCLA Young Library, Box 91, Folder H.

[4] Marschak to Chancellor Lawrence Kimpton, 5/25/51; Jacob Marschak papers, UCLA Young Library, Box 92.

> should circulate between centers of decision to make consistent allocation possible emerged from the discussion by Lange, Lerner and others. [Koopmans, 1951, p. 3]

There is plenty of evidence the Cowlesmen (many of whom were European exiles) were feeling a little embattled at Chicago, especially after the war. Even Arrow, the home-grown native, has testified to his political motivations at that juncture:

> On returning from military service, I planned to write a dissertation which would redo *Value and Capital* properly, a very foolish idea. I had two motivations. One was to supply a theoretical model as a basis for econometric estimation. The other was a strong interest in planning. I would have described myself as a socialist, although one that had a strong belief in the usefulness of markets. Market socialism was a widespread view. Hotelling held it. It had been popularized especially by the works of O. Lange. [Arrow, 2009, p. 7]

Secondly, the Cowlesmen believed they would accomplish this political task *not* by following Hayek and the others into the thickets of some school or other of formal psychology, but instead through producing their own purpose-built version of the mathematical utilitarian mind, by developing a novel "decision theory" based upon *avant garde* natural science currents contemporary with their efforts. Much of what passes for modern decision theory therefore had its origins not in economics *per se*, but rather in operations research, growing out of WWII. Recalling the insight of Hunter Heyck, the military was inclined to shift the focus from the mental state of the chooser to the *choice* as a free-standing phenomenon worthy of study; operations research was the vessel that supported the reorientation [see Heyck, 2012; 2015; Bessner & Guilhot, 2016; Thomas, 2015]. Because Cowles developed a close and lasting research cooperation with the military at RAND starting in 1948, it was specifically the Cowles branch of American neoclassicism that introduced operations research into economics, and later, business schools across the nation [For documentation of this assertion, see Mirowski, 2002, chaps. 4 & 5; Augier & March, 2011, esp. pp. 68–89].

Thus we observe that it was heavily over-determined that it would be Cowles economists who were initially recruited to the front line to confront

the nature of "information" in economics, not because they were especially predisposed to be subtle specialists in epistemology, but rather, because their patrons promoted it, their politics dictated it, and everything about their own commitments as to what a scientific economics required demanded that they take a position on it. This even extended to their defense against the most withering criticisms of Neoclassical theory in the American context, most of which had emanated, not from Austrians, but from the American Institutionalists.

The Cowlesmen had decided by the late 1940s that the Institutionalists were their immediate sworn enemies, to an even greater degree than Marxists. In a catchphrase, those obstreperous Institutionalists had accused the earlier neoclassicals of tying themselves in cognitive knots. In the early 20th century, went the accusation, Neoclassical theory had come to a strange impasse.[5] By the early century, neoclassical economists wanted to renounce any dependence upon psychology, rejecting the guise of 'utility' as a mental phenomenon. But simultaneously, they also sought to address the common complaint that their theory was epistemically implausible – that it was a theory that simply assumed 'perfect knowledge' of desires, prices and possibilities, in order to place 'knowledge' out of bounds of the theory. How could there be such a 'brainless/mindless' type of knowledge, built up around non-cognitive 'preferences'? How could such passive braindead zombie agents 'know' that the market gave people what they really wanted? This was the conundrum which launched a thousand leaky vessels at Cowles.

2. Future-oriented economics and information-as-a-thing

The lead vessel in the armada was a rather simplistic notion: treating information as a 'thing' subject to market organization initially seemed to

[5] There have been a number of insightful histories commenting upon economists' encounters with psychologists, but, for reasons of concision, we shall have to make do here with a couple of bald generalizations, based on Giocoli, 2003 to get our story rolling. For further elaboration, see Mirowski, forthcoming.

offer a blissful way out of the impasse. A thing-like information was, in retrospect, only the *first* draft of a formal incorporation of information into economics. But in doing so, neoclassical theory took another vertiginous turn at Cowles, one which we can only gesture towards in this narrative. It has now become commonplace amongst historians of economics to realize that before the early 20th century, both classical and neoclassical economics were *past-oriented.* That is, the values realized today were explained as the consequences of events that had happened yesterday in the past: labor values were due to past infusions of labor, Marshallian supply prices were the consequence of prior production and investment decisions; "final degree of utility" was the consequence of prior consumption choices. From Frank Knight's *Risk, Uncertainty and Profit* [1921], price theory swivelled 180 degrees on the time axis, and economic theory rather quickly became *future-oriented* instead. One way to summarize this curious transformation is that it subsequently became commonplace to assert that *events which had not yet happened* could come to influence economic decisions in the present, in a kind of spooky action-at-a-distance. Past decisions, by contrast, were treated as irrelevant bygones, 'sunk costs' no longer meriting consideration. The introduction of inductive statistics from the 1930s forward reinforced this dramatic seachange, in the sense that current values would henceforth be said to embody an irreducible component of prospective future risk. Keynes claimed that much of the breakdowns of the 1930s were due to an irreducible uncertainty about the future. Once probability theory was married to utility theory in the 1940s, a specialty at Cowles, their 'knowledge' became knowledge about the future consequences of current decisions, and fed back directly into those decisions. If the inscrutable future was conceived to cause economic changes in the present, then 'information' became the number one causal cue through which this happened. The treatment of information as subordinate to a technology of inductive inference was the *second* version of a formalized information economics within the American profession. A *third* draft of the economics of information grew out of experience with the nascent computer at RAND and Cowles. All of these components came together at the postwar Cowles Commission.

Table 1. Cowles Members and their Informational Enthusiasms

Cowlesmen	Period at Cowles	Information Innovations
Jacob Marschak	1943–1960	Economics of Information; team theory; early experimental economics
Leonid Hurwicz	1942–1951	Incentive compatibility; Mechanism design
Kenneth Arrow	1947–1950	Moral hazard, many papers on information
Stanley Reiter	1948–1950	Mechanism design, computability
Herbert Simon	1947–1949	Artificial intelligence, bounded rationality, computation

The advent of information processing at Cowles was the confluence of a far more complex set of events than the spare vignettes recounted here, and cannot be done justice here in a few short paragraphs.[6] Nevertheless, a crude gloss would point to the fact that Jacob Marschak, Kenneth Arrow, Herbert Simon, Stanley Reiter and Leonid Hurwicz were all heavily influenced by contemporary developments within cybernetics, but that they nevertheless all started out (under the influence of Claude Shannon and RAND) by treating information as a fungible commodity – "Uncertainty usually creates a still more subtle problem in resource allocation; information becomes a commodity" [Arrow, 1962, p. 614] – but only to rather rapidly back off from this option (although never entirely denouncing it), only then to subsequently transfer allegiance to conflating information processing with statistical induction. This move was something they had learned by watching the operations researchers; but was also closely related to their retreat from a full-blown econometric empiricism (for which they had originally gained recognition) in favor of models of the economic agent as himself portrayed as a miniature econometrician.[7] At that point,

[6] For a detailed summary, see Mirowski, 2002, pp. 370–389. Even there, many important Cowles initiatives are left unexplored.

[7] This abrupt about-face was first described in Mirowski, 2002.

the original Cowles team spun off into various semi-unrelated research programs into the economics of information, as the allure of purely epistemic econometrics palled.

3. Tjalling Koopmans

One should not glean a mistaken impression of fleeting attention from the limited span of time that some of the figures in Table 1 spent at Cowles: their sojourn in Chicago was decisive for each and every one of them when it comes to information economics. Let us very briefly explore this landscape as it developed in the critical period 1948–1954. We shall begin with Tjalling Koopmans, who does not really make our league table, but helped set the stage, nonetheless.

Some members of Cowles started out believing that the existing Walrasian model, as formalized by Arrow and Debreu, was sufficient in and of itself for the refutation of Hayek's proposed revision of the marketplace of ideas. Tjalling Koopmans adopted the position that the Walrasian model then being reformulated at Cowles by Arrow and others actually showed that agent cognition was effectively unnecessary, since the individual agent only needed to know his own preferences and parametric prices in order for equilibrium to be obtained:

> [O]ne can in particular interpret the proposition as a statement of conditions under which the simplicity of incentive structure and the economies of information handling characteristic of a competitive market organization can be secured without loss of efficiency of allocation… The price system carries to each producer, resource holder, or consumer a summary of information about the production possibilities, resource availabilities and preferences of all other decision makers. Under the conditions postulated, this summary is all that is needed to keep all decision makers reconciled with a Pareto optimal state once it has been established. [Koopmans, 1957, p. 53]

Perhaps the most incongruous aspect of this assertion was its utter lack of connection to the Cowles stated political orientation of market socialism, or its justification. Somehow arriving at this amazing state of coordi-

nation dispelled any need for communication or thought. Koopmans' endorsement of the amazing powers of general equilibrium came a little too close to sounding like Friedrich Hayek, which may explain why the other Cowlesmen so noticeably distanced themselves from this construal of 'information'. Other members of Cowles were not quite so publicly confident that their sanctioned heritage of Walrasian models adequately addressed this supposed exquisite effortless economy of information: Marschak and Hurwicz in particular were more inclined towards doubt, whereas Arrow (as usual) began to cast about for various glitches that nominally frustrated the welfare theorems. Much of this discussion within Cowles initially sought to bundle together their various worries and qualms onto the Procrustean Bed of "uncertainty", and one can observe by the mid-1950s that Koopmans first floated the trial balloon of blaming this insufficiency on "missing markets" (a doctrine later popularized by Arrow):

> Here, perhaps the most crucial kind of uncertainty…arises from the lack of information on the part of any one decision-maker as to what other decision-makers are doing or deciding to do. It is a puzzling question why there are not more markets for future delivery through which the relevant information about concurrent decisions could circulate in an anonymous manner.[8]

4. Jacob Marschak

Koopmans was nowhere quite so daunted by these problems as Jacob Marschak, about whom "on many occasions during the 1950s and 1960s we heard economists question whether Marschak had not actually left economics for other disciplines, such as psychology [or] information science" [see McGuire & Radner, 1986, p. viii]. Yet, far from being a flighty dilettante or fickle fellow traveller, Jacob Marschak was situated at the very

[8] "Comments in Thursday afternoon session" of the Conference on Expectations, Uncertainty and Business Behavior, Pittsburgh, Oct. 27–29, 1955, Box 5 folder 81, Tjalling Koopmans Papers, Sterling Library, Yale University. Note that, even though Koopmans was close to John von Neumann in this era, he did not then entertain the notion that game theory was a better formalism for addressing these questions.

core of the Cowles project in the 1950s. For someone whose allegiance to the Walrasian orthodoxy was never in doubt, even to the point of rejecting Keynesian economics [see Mirowski, 2012], Marschak seemed painfully sensitive to the ways in which 'information' might disrupt economic equilibrium. Because he had participated in some early decision theory experiments at RAND and elsewhere, perhaps to a greater degree than his comrades, he appreciated the latent empirical failures of decision theory and its offshoot, the theory of expected utility. He early on adopted a stance which has later become second nature in the rise of so-called 'behavioural economics': "If we know what makes people more or less logical or mathematically inept or poor decision makers, we may also find out how best to enable them to learn the 'recommended' type of behavior… The normative and descriptive analyses complete each other" [(Marschak, 1974, vol. 1, p. 93]. This convenient fusion of the normative and descriptive into a tautology might be instituted by means of elaborations upon the notion of 'information,' or so Marschak hoped.

Marschak tentatively tried out various paths towards his own grail of an economics of information (and he was one of the earliest American economists to use the term[9]), but none of them seemed to pan out: first he struggled with subjecting Shannon information to a supply/demand framework, and then subsequently entertained the Blackwell formalism, which suggested an instrument reading was more informative if it could distinguish observations over a finer partition of the state space of possibilities, only later to reject it; later he dallied with the idea of transactions costs as capturing informational issues; he also pioneered a computer/organization metaphor in the format of what he called "team theory". He taught one of the very first courses anywhere on "The Economic Theory of Information and Organization" at Yale in the late 1950s, even using Ross Ashby's *Introduction to Cybernetics* as a required text. He corresponded with AI progenitor John McCarthy concerning his paper "Measure of the Value of

[9] But certainly *not* "the first to develop a systematic theory of the economic value of information", as asserted in his biography in the *New Palgrave* by Roy Radner. See Mirowski, 2002, pp. 372–375.

Information" in 1957.[10] He was among the first to participate with professional psychologists in experiments designed to test the limits of decision theory, back when that was still an anathema within the economics profession.

As late as 1966, Marschak was weighing the relative merits of the Shannon information versus Blackwell's inductive approaches for economics:

> Currently, my primary pet project is 'Economics of Information' and in particular the question why communication engineers like to use the entropy formula (presumably needed for purposes of mass producers, not the individual users, of communication equipment) while statisticians and other users must either specify the individual loss function or content themselves with the partial ordering of information systems (also called 'experiments') that is induced by Blackwell-Girshick's 'greater informativeness' relation.[11]

Marschak's conundrum was that his experimental work had induced him to become rather skeptical when it came to game theory, so he decided he had to concoct a special new field of mathematical economics in order to explore the fine points of information economics; he called it "team theory". As early as 1954 he revealed the centrality of 'information' to his concept:

> In a team of executives, each member has to decide something different. These decisions determine an expected joint reward (payoff) received by the team, and depend on the distribution of information (Who learns what?) among the several partners, and on the decision rules… that determine the response of each partner to the information content he receives. The distribution of information depends on the team's communications network and the coding rules used in operating it, and therefore a cost is attached to every form of the distribution of information. The team problem consists in choosing simultaneously a distribution of information and a set of decision rules that yield, in conjunction, the highest expected reward to the team, net of communications costs.[12]

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[10] Marschak to John McCarthy, 11/27/1957; Marschak papers UCLA Young Library, Box 94, Folder: M.

[11] Marschak to Howard Raiffa, 7/7/1966; in Box 111, Folder: R , Marschak papers, UCLA Young Library.

[12] Marschak, "Elements for a Theory of Teams" October 1954, Box 90, Jacob Marschak papers, UCLA Young Library. Another motivation for team theory was to produce models of

Marschak's 'team theory' never really caught on in economics (or anywhere else, for that matter) even though he devoted substantial efforts to its elaboration; but in retrospect, we might entertain it as a somewhat deformed and distended version of what came to be known much later as 'mechanism design'. But having glimpsed the glistening shore, Marschak was never ushered into the Promised Land.

5. Leonid Hurwicz

In modern parlance, the role of Moses in information economics is often bestowed in retrospect upon Leonid Hurwicz. This honorific is dealt with in detail in our forthcoming book [see Mirowski, Nik-Khah, 2016]. However, we shall engage in some preliminaries concerning the importance of Cowles by now by demonstrating that Hurwicz was already deep into 'information processing' at the crucial juncture of the early 1950s, and was frequently discussing it with Marschak, a fact illustrated by reproducing an unpublished research outline provided to Marschak entitled, "Economic Decision-making Processes and their Organizational Structure of Uncertainty":

> The research outlined in the present note is focused on decision-making under uncertainty. The emphasis is, however, not so much on the criteria of optimality among alternative choices as on the technology of the processes by whereby decisions are reached and choices are made. Under the customary conditions of 'rationality', the final decision is preceded by certain operations which may, in general, be characterized as information processing… when the information processing aspects of the problem are taken explicitly into account it is found that the concept of 'rational action' is modified. This is true even when applied to the action of a single individual, but it comes particularly interesting when considered in situations involving many persons…The uncertainty need not be generated by external factors like weather prospects: it may be man-made.[13]

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command and control for the military, something Marschak acknowledged in print. See Marschak, 1974, vol. 2, pp. 64–66).

[13] Leonid Hurwicz, "Economic Decision-making Processes…" [no date, possibly 1951] Box 91, File: Hurwicz, Jacob Marschak Papers, UCLA Young Library.

The politics of this nascent move seemed somewhat more promising; Hurwicz in the early 1950s was feeling his way towards an account where it was not so much explicit cognitive issues, but rather the accessory *technologies* which hindered the grand optimum promised by the Walrasian general equilibrium. 'Market socialism' might then take on a much less threatening colouring of the provision of the 'technological augmentation' of existing markets to achieve full Pareto Optima, all in the name of capacities for information processing. Compared to Marschak, it was not so much the 'team' that was at fault, as it was the accompanying hardware used to convey the price and data messages.

The intention at Cowles back then was still to make use of the Walrasian general equilibrium theory in an effort to rebut the political claims of Hayek; yet some bad news on the theoretical front began to queer the market socialist pitch. As a result of the mathematical counterexamples of Herbert Scarf and David Gale in the 1960s, it became clear that the Cowlesmen could give no general dynamic account of global convergence of Walrasian dynamics to a general equilibrium, especially since Hurwicz had been collaborating with Arrow on precisely this question. The legitimacy of the full Walrasian equilibrium as the benchmark of ultimate market success would therefore seem to have been put at risk. Rather than give up on the Walrasian project, Hurwicz sought a way to "build in" sufficient stability to encourage convergence to an optimum, by conceiving of an economic system loosely as a "convergent computational system." It was therefore Hurwicz who first appropriated the Cowles fascination with information and parlayed it into a major subfield of economics, now called "mechanism design".

We cannot cover the history of mechanism design in this short paper, although elsewhere we have argued this was the primary apotheosis of the attempt to bring informational considerations into economics [see Mirowski & Nik-Khah, 2017]. Yet there is one aspect of Hurwicz's turn that is so eminently idiosyncratic, and yet bearing the unmistakable Cowles stamp, that we must raise it here. Rather than explore a concertedly computational approach to information in the economy, Hurwicz's "mechanism design" studied communication within a tâtonnement-type market system –

— only now, with the auctioneer augmented with the ability to communicate further information other than prices. For Hurwicz, the problem with The Market was that its convergence to a Pareto Optimum would be thwarted by pesky indivisibilities and nonconvexities, and since these conditions are reputedly not very rare, it should be possible to find another "adjustment process" that could do better. Market socialism (broadly speaking) might offer a way to improve matters, but one could object to such proposals on informational grounds. The point of "mechanism design" was initially to find other adjustment processes that could improve upon the performance of The Market, without imposing too burdensome a communication requirement. These studies quickly became wrapped up in considerations of "decentralization," which had concerned Hurwicz since his earlier work on activity analysis (in 1950).[14] Once market socialist proposals underwent redescription as informationally decentralized mechanisms, it became an accepted creed of market socialists that they, like Hayek, rejected a centralized solution. Socialism (inadvertently?) thus began to shed its more conventional connotations.

But what was the appropriate principle by which to demarcate "centralization" from "decentralization?" Establishing something like this was of fundamental importance to a project that hoped to generate alternative "market-like" non-market allocation mechanisms, where "market-like" was understood to be "decentralized." Hurwicz proposed various definitions of this decentralization, none of which turned out to be especially persuasive, and he was eventually brought to admit, "it is more interesting to see what questions can be asked given a (not *the*) concept of information decentralization." [see Hurwicz, 1969, p. 517]. Presumably it had something to do with the "costs" of transmission, which in practice was conceived as the size or dimensionality of the message space. Eventually, the coterie of mechanism designers around Hurwicz settled on conceiving decentraliza-

[14] "I was writing a more or less expository paper on dealing with activity analysis....when I used the word, 'decentralization' I thought I should explain what it meant….But then it struck me that I did not in fact know what we meant by decentralization. That was the beginning of many years of work trying to clarify the concept" [Hurwicz in Feiwel 1987, pp. 271–272].

tion as implying a limitation on "channel capacity," and then attempted to ascertain how such limitations restrict the performance of mechanisms. By drawing attention to the importance of restrictions in channel capacity, which was viewed "analogously to a limitation of the (cross-sectional) diameter of a pipe restricting the flow of a fluid through that pipe," [Reiter 1977, p. 230] the image before these mechanism designers was, essentially, that of Shannon's information theory. The doctrine that emerged in this tradition was the view that there was a tradeoff between performance and information costs, which was deemed the real message of the economics of information. But this did not exhaust the various mutations of 'information' at Cowles.

6. Herbert Simon

The figure of Herbert Simon represents an even more dramatic reprocessing of 'information' into whole new disciplinary imperatives, certainly by contrast with Marschak and Hurwicz. Simon shows us how the various influences and problem situations ricocheting around Cowles could lead in an entirely orthogonal direction, as long as you gave up the severely inhibiting commitment to the Walrasian model.

Simon's trajectory is wonderfully covered in his own autobiography [see Simon, 1991 and also Sent, 2001], so we provide only a brief summary here. Simon was an odd duck in the Cowles sord: a political scientist specializing in organization theory, but really a polymath. He was invited to sit in at Cowles by William Cooper, while teaching at the Illinois Institute of Technology in Chicago. It would be a challenge to enumerate all the ways he was an outsider to that conclave, but perhaps the most salient was his abiding skepticism towards basic neoclassical microeconomics. In an extremely roundabout manner, this eventually led him to become one of the most important figures in the history of 20th century information processing, as one of the three or four founders of the field of artificial intelligence.

Simon did credit his time at Cowles with reorienting his research towards information processing, both in his autobiography, but also in response to a questionnaire from Clifford Hildreth:

> Perhaps the greatest impact of the Cowles exposure on me was to encourage me to try to mathematize my previous research in organization theory and decision making – especially the theory developed in *Administrative Behavior*. I think this project was on the agenda anyway, but the Cowles contact certainly egged me on and gave it higher priority… During the period 1950-56, I was doing at least as much economics research as research in management and organizations… The stint as an almost full-time economist was certainly brought about by my involvement with the Cowles Commission, and later, through the Commission, my association with the RAND corporation. The final unanticipated consequence of these events was to turn me away from economics toward psychology, as my interest in decision making led me to see the need for empirically based theories of human problem solving, and as my RAND consulting brought about my association with Allen Newell and computers.[15]

While most scholarly attention has been devoted to his achievements after his conversion experience with the computer at RAND, our interest here is the underdeveloped narrative of Simon's runup to thisat watershed at Cowles, and its relationship to 'information'. While always believing that the rational choice model was a terrible representation of human thought, one way that Simon fit in was that he, too, was at heart a market socialist. However, he approached his socialism from the side of organization theory, and decision making in organizations. Cowles was at the center of all kinds of ferment in the mathematical modelling of these issues, and this is what attracted Simon to them in the later 1940s. Furthermore, the young Simon harboured ambitions to model human rationality as it functioned in social situations, without much in the way of previous guidance. One focus of Cowles attention in the late 1940s was game theory, and various mathematical ideas of John von Neumann. Simon was one of the phalanx of Cowlesmen to review *Theory of Games and Economic Behavior* (1944) soon after its appearance; but he was very disappointed in the book as a theory of *organization*, something concerning which he had very

[15] "Inquiry on Cowles Commission", memo from Herbert Simon to Clifford Hildreth, 8/2/1982; Folder: correspondence, Hildreth Papers, Duke University.

strong opinions early on. The axiomatization of expected utility theory found therein also did not appeal. But one thing that did capture his imagination was von Neumann's proselytizing for the new-fangled electronic computer, something well represented in Cowles records. Yet, he was further stymied when von Neumann concurrently rejected the computer as a model of the brain, or of psychology in general. The solution to his own personal conundrum came when military work at RAND exposed him to the machine psychology of the man/machine interface at the Systems Research Lab, and consequently he arrived at the position that the scientific objective was to *simulate* human thought, while ignoring basic questions about the mind and psychology. This is what it meant to be an information processor: "we began more and more to see decision-making processes as essentially the same as problem solving processes" [Simon 1991, p. 163]. By his own admission, the simulation of heuristics of actual reasoning was good enough to constitute a 'theory' of intelligence – hence the designation 'artificial intelligence'. Once he arrived at that epiphany, he built a school around him at Carnegie Mellon that explored man machine heuristics in greater detail, but also used the template to develop a theory of the firm patterned on computer metaphors of hierarchy and information processing.

It will be significant for what follows that, although the computer was indispensable to virtually everything that Simon accomplished in social theory after 1955, it was always as a *platform for simulation*, rather than as a full-blooded application of formal theories of computation to mental phenomena. Simon was intermittently dismissive of those who sought to apply Turing computability to mental processes: "It gradually dawned on computer scientists that the decidability question was not usually the right question to ask about an algorithm or a problem domain." [Simon, 1978, p. 500] It is noteworthy that it was renegade Cowles affiliates (such as Alain Lewis, Roy Radner and Gerald Kramer), and *not* members of the other 20th century schools of neoclassical economics, who came to the early realization that tinkering with the utility framework was just too timid a response to the challenge of information, and struck out to construct a more full-blooded cognitive model. Simon, for instance, kept insisting that rationality

was 'bounded'. Yet it also plain that the closer the figure was to the inner circles of Cowles orthodoxy, the more loathe they were to apply explicit computational analysis to the logic of the constrained optimization of utility. One might, like Simon, suggest that maximizing utility was empirically implausible; but the figures such as Lewis and Kramer who insisted it was mathematically computationally *impossible* were rapidly consigned to utter obscurity.[16] This massive blind spot is also distinctively constitutive of what 'information processing' meant at Cowles in the later 20th century.

7. Kenneth Arrow

In our Cowles roster, it was not always the person who innovated the most profound contributions who came to wear the laurels when it came to retrospective honours in the history of the economics of information. In any event, for the average orthodox economist it was Kenneth Arrow who eventually became the Cowles poster boy for an economics of information, and indeed, many of the themes covered here would be found in his work at one time or another: information as a thinglike commodity, information as a public good, knowledge flaws due to missing markets, cognition as intuitive statistics, tacit knowledge in the guise of learning-by-doing, decision theory as ersatz psychology, the Blackwell formalism, asymmetric information and moral hazard, bounded rationality, complexity theory, and even (a brief flirtation, quickly repudiated) cognition as computation. If one does not look too comprehensively at his *oeuvre*, one can find some modicum of support for just about any subsequent orthodox approach to the economics of information one might care to promote; and this may account for some of Arrow's popularity within the profession. The irony of the profession's praise of this eclecticism is that at one juncture or another, he has also repudiated each and every one of them.[17] The pattern seemed to

[16] For the story of Kramer and Lewis, see Mirowski, 2002, pp. 422–432.

[17] For the explicit repudiation of the Shannon concept, see Arrow in McGuire & Radner, 1986. For the admission that his models had little to do with cognitive information processing, see Arrow, 1984, p. 200. "There is no general way of defining units of informa-

be that whenever a particular research line concerning information threatened to invalidate some critical foundational aspect or other of the Walrasian program or other, Arrow would belatedly repudiate the research line and retreat. The one thing he never ever countenanced, however, was the primary notion that neoclassical models were an awkward, galumphing, inappropriate vehicle with which to express the primacy of the marketplace of ideas in the first place. This may explain some of his recent crotchety statements, such as:

> The idea that people have difficulty computing the system has a long history; you can see it in Veblen, for example. But nothing followed from this insight. Herb Simon was a great apostle of this view. He's a great figure, and his work did lead to a research program, but in my view, it fizzled out… As I think more about complexity theory, I become more convinced that there is some sense we will never know how the economy operates. [Kenneth Arrow, in: Colander et al, 2004, p. 293, 298]

The mordant fact is, of course, in his old age Arrow never sounded more like anyone else than Friedrich Hayek.

Roy Radner was a minor Cowles figure who sought to ponder even more seriously the implications of cognitive science for the Walrasian program, exploring the observation that no agent should be presumed to engage in a trade that depends upon information not available to him at that juncture, and insisted that a Pareto optimum could only be defined relative to a given structure of information. Contradicting Arrow, he insisted that the separation between informational and computational considerations was entirely artificial, and wrote, "The Arrow-Debreu world is strained to the limit by the problem of choice of information. It breaks down completely in the face of limits on the ability of agents to compute optimal strategies" [1968, p. 35]. Radner's insights have been subsequently ignored for the most part, for reasons already broached.

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tion" [Arrow, 1996, p. 120]. For Arrow's role in suppressing the work of Alain Lewis, see Mirowski, 2002, pp. 427–436.

8. Stanley Reiter

The final figure in our cast of mid-century Cowles Information Argonauts is Stanley Reiter (1925–2014). Now utterly ignored in the history of modern economics,[18] we think he deserves to be resurrected as a significant transitional figure in the Cowles landscape: from mechanism design to experimentalism in economics, from the Walrasian tradition to a particular conception of computationalism, and from the orthodox position that 'all markets are alike' to something hinting at a diversity of market forms. In residence as a Research Associate at Cowles for only the two years 1948-1950, he ended up acting as the most devoted acolyte of Leo Hurwicz, turning much of the Purdue Economics Department into a Cowles outpost in the period 1954–1967. Purdue was a powerhouse in this period, producing such students as Hugo Sonnenschein, John Ledyard, Nancy Schwartz and Mort Kamien. More to the point, Purdue was a hotbed of Cowles-style Walrasianism when it was still of dubious general popularity: it included the Hurwicz students James Quirk and Rubin Saposnik. Because Hurwicz maintained close ties with the Purdue faculty, Ed Ames called him "an honorary member of the department." [Ames, 1981, p. 358]. But most significant for our narrative, Reiter in the period 1954–1967 overlapped with Vernon Smith, precisely when he was innovating his distinctive version of experimental economics. Reiter was the conduit through which Hurwicz's nascent mechanism design (and its Cowles flavour) came into conversation with the simplistic Marshallianism of the early Vernon Smith. In his memoirs, Smith calls Reiter "our leading economist" at Purdue [Smith, 2008, p. 230]; but the intellectual connections can only be fleshed out by briefly considering Reiter's work.

Reiter started off at Cowles as a student of Koopmans' linear activity analysis, but that soon palled, and afterwards he lent his mathematical to talents to statistics, but also to problems of the dynamics of price formation. The latter issue brought him close to Hurwicz, even though his thesis advisor at Chicago had originally been Milton Friedman. He also demon-

[18] The exception is Lee, 2015, to which we owe our appreciation of his importance.

strated a fascination with algorithms for optimization under uncertainty, for instance in the 'job shop problem' in the early 1960s. By the 1970s, he announced himself as one of the first popularizers of the Hurwicz research program in mechanism design, retailing it as the $(New)^2$ Welfare Economics, an awkward moniker that never really caught on. This paper stressed that informational considerations were central to the theory of economic mechanisms, positing that "An initial distribution of knowledge about the economy is assumed. Each agent knows something, but generally not everything… No agent by himself knows enough to figure out the feasible allocations." Even compared to Hurwicz, he was much more insistent upon portraying the economy as "a kind of machine which accepts as inputs the basic data of the economy and produces as an output an allocation of commodities among the participants" [Reiter, 1977, p. 227]. The computer as information processor overtly structured Reiter's account of mechanism design, to a much greater extent than many of other theorists of that era. This was exemplified by his work with Kenneth Mount on the "Informational Size of Message Spaces" and the restrictions that they imposed on Hurwicz-style mechanisms [Reiter, Mount, 1974].

Probably at Purdue, Reiter began to try and bring Cowles-style concerns to Vernon Smith's stress on market formats in his early experimental work. As acknowledged by Smith himself, he later attempted in his famous 1982 paper to undertake "the bridge-building… between experimental economics and the Reiter-Hurwicz – sometimes called the Northwestern – view of economic theory" [Smith, 1991, p. 162]. The computer, and the concern over information processing, was the unlikely common denominator of the two streams of Cowles-inspired Walrasian market socialism and Smith's Marshallian-style Hayakianism. Thus we would argue that Stan Reiter turned out to be one of the key obligatory passage points between the earliest Walrasian mechanism design and the more modern subsequent market constructivism, often associated with experimental economics [see Mirowski, Nik-Khah, 2017].

Reiter was not only a vector of ideas, but also played a major role in the reorganization of the social sciences in the early 1980s. In a narrative

first recounted by Kyu Sang Lee, there had been an attack on funding for the social sciences during the early Reagan Administration, which provoked a response organized by the National Research Council and the National Science Foundation; they created a "Committee on Basic Research in the Behavioral and Social Sciences", launched in 1980. In an attempt to reassert the legitimacy of the state funding of the social sciences, a number of reports were commissioned, the details of which will not occupy us here. However, the working group on "Markets and Organizations" was chaired by Stanley Reiter; and they produced a report which was extremely revealing about the contemporary state of the economics of information and its relationship to mechanism design when it appeared in 1989 [Reiter, 1989; see also Lee, 2015)]. This was the first document we have uncovered that argued that "information" would change the entire orientation of orthodox economics.

Our impression is that Reiter moved further and further away from the Walrasian organon as he grew older, which may account for some of his neglect by historians. For instance, in 2001 he made the extremely revealing admission that there were no real markets in the Walrasian model, something which would tend to disrupt the allegiance of a true follower of Hurwicz [see Reiter, 2001, p. 271]. Even later, he conceded that there was still no plausible dynamics for the Walrasian general equilibrium, and thus dabbled with simulation in agent-based models to explore other formats of convergence to equilibrium. In this late paper, he reveals he still seeks to make connections to the Vernon Smith school of experimentation:

> [T]he well-known informational efficiency of the competitive allocation mechanism is limited to the static model… One line of response to this challenge has its roots in computer science. Developments in distributed computation have inspired research in which computational algorithms… compute market equilibria or optima… There is also a substantial literature reporting laboratory experiments in trading… Experiments tend to show that the behavior of experimental subjects is roughly consistent with what economic theory assumes. It is not clear how much of the behavior observed in small scale experimental settings survives in a large economy…. Vernon Smith and Charles Plott have pioneered this approach to studying trading. [Reiter, Maroulis, 2008, pp. 1399–1400]

9. Conclusions

Now we have introduced the cast of characters at Cowles, we have to briefly return to the larger question: Why Cowles, and not, say, MIT? Why was it Cowles that set the templates for developments in the postwar economics of information?

There are a number of things to keep in mind about Cowles when observing various American economists foraging about for inspiration concerning an economics of knowledge. First, because of their intimate connections with RAND, they were in much closer physical proximity to key natural scientists engaged with innovating new approaches to information than were any other schools of economics, neoclassical or otherwise. For instance, John von Neumann had made a number of overtures to Cowles economists in the late 1940s, which explains why they were the first to entertain formal game theory. Kenneth Arrow in particular was a close colleague of David Blackwell; Leonid Hurwicz and Stanley Reiter enjoyed close collaborations with various computer scientists. David Blackwell turns out to be an extremely important protagonist in this story: he was a mathematician who started out as an advocate of Bayesian statistical inference, but while working for the military at RAND, came up with a novel formalization of information as measures over partitions of discernable states of the world.[19]

Secondly, as we have repeatedly stressed, many of the Cowlesmen explicitly admitted that their motivation in the 1940s–1960s in discussing information was to refute Friedrich Hayek, and thus to show that information economics need not have neoliberal implications. MIT was so intellectually isolated in the 1950s, it didn't even realize that Hayek posed a threat of some sort to economists' self-confident science. Whatever the postwar lay of the landscape, in the longer view, it seems apparent in retrospect that the hunter got captured by the game, in that the seductive frame tale of the omniscient neoliberal marketplace of ideas came to dominate much of their

[19] See Blackwell in: DeGroot, 1986, p. 47: "My work on the comparison of experiments was stimulated by some work by Bohnenblust, Sherman and Shapley."

own work in mechanism design, asymmetric information, 'failures' of expected utility theory, 'incomplete markets' and a host of other innovations. Although Cowles as an institution decamped from Chicago in 1954 for Yale, the program it pioneered was continued at RAND, Stanford, Israel, Louvain, and wherever else operations researchers gathered together under military auspices.

Third, in the modern orthodoxy, the primary visible heritage of Cowles for the median economist circa 1990 came with their latching on to the 'state space' formalism as purportedly plug-compatible with their general equilibrium orientation; first pioneered at RAND by David Blackwell (1951; 1953). In effect, information processing became confused with an image of the neoclassical agent as a little econometrician; one can observe this very starkly in some subfields as rational expectations economics, or in Bayes-Nash game theory. This is still currently treated in some retrograde quarters as the 'standard model' of information in economics [Samuelson, 2004].

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