
01 SCIENCE PRESS

002 Economic Review—0333-002-9 TERY 04-30-79 & CIND 5-2-79
003 EREV002A01.2S- GALLEY 1

06 Rational Decision Making in Business 07 Organizations

By HERBERT A. SIMON*

* 008 In the opening words of his *Principles*,
009 Alfred Marshall proclaimed economics to be
010 a psychological science:

012 Political Economy or Economics is a
013 study of mankind in the ordinary busi-
014 ness of life; it examines that part of
015 individual and social action which is
016 most closely connected with the attain-
017 ment and with the use of the material
018 requisites of wellbeing.

019 Thus it is on the one side a study of
020 wealth; and on the other, and more
021 important side, a part of the study of
022 man. For man's character has been
023 moulded by his every-day work, and the
024 material resources which he thereby
025 procures, more than by any other
026 influence unless it be that of his religious
027 ideals.

028 In its actual development, however, eco-
029 nomic science has focused on just one aspect
030 of man's character, his reason, and particu-
031 larly on the application of that reason to
032 problems of allocation in the face of scarcity.
033 Still, modern definitions of the economic
034 sciences, whether phrased in terms of allocat-
035 ing scarce resources or in terms of rational
036 decision making, mark out a vast domain for
037 conquest and settlement. In recent years there
038 has been considerable exploration by econo-
039 mists even of parts of this domain that were
040 thought traditionally to belong to the disci-
041 plines of political science, sociology, and
042 psychology.

043 I. Decision Theory as Economic Science

044 The density of settlement of economists
045 over the whole empire of economic science is
046 very uneven, with a few areas of modest size
047 holding the bulk of the population. The
048 economic Heartland is the normative study of
049 the international and national economies and
050 their markets, with its triple main concerns of
051 full employment of resources, the efficient
052 allocation of resources, and equity in distribu-
053 tion of the economic product. Instead of the
054 ambiguous and over-general term "econom-
055 ics," I will use "political economy" to desig-
056 nate this Heartland, and "economic sciences"
057 to denote the whole empire, including its most
058 remote colonies. Our principal concern in this
059 paper will be with the important colonial
060 territory known as decision theory. I will have
061 something to say about its normative and
062 descriptive aspects, and particularly about its

067 applications to the theory of the firm. It is
068 through the latter topic that the discussion
069 will be linked back to the Heartland of
070 political economy.

071 Underpinning the corpus of policy-oriented
072 normative economics, there is, of course, an
073 impressive body of descriptive or "positive"
074 theory which rivals in its mathematical
075 beauty and elegance some of the finest theo-
076 ries in the physical sciences. As examples I
077 need only remind you of Walrasian general
078 equilibrium theories and their modern
079 descendants in the works of Henry Schultz,
080 Samuelson, Hicks, and others; or the subtle
081 and impressive body of theory created by
082 Arrow, Hurwicz, Debreu, Malinvaud, and
083 their colleagues showing the equivalence,
084 under certain conditions, of competitive equi-
085 librium with Pareto optimality.

086 The relevance of some of the more refined
087 parts of this work to the real world can be,
088 and has been, questioned. Perhaps some of
089 these intellectual mountains have been
090 climbed simply because they were there—
091 because of the sheer challenge and joy of
092 scaling them. That is as it should be in any
093 human scientific or artistic effort. But regard-
094 less of the motives of the climbers, regardless
095 of real-world veridicality, there is no question
096 but that positive political economy has been
097 strongly shaped by the demands of economic
098 policy for advice on basic public issues.

099 This too is as it should be. It is a vulgar
100 fallacy to suppose that scientific inquiry
101 cannot be fundamental if it threatens to
102 become useful, or if it arises in response to
103 problems posed by the everyday world. The
104 real world, in fact, is perhaps the most fertile
105 of all sources of good research questions
106 calling for basic scientific inquiry.

107 A. *Decision Theory in the Service of*
108 *Political Economy*

109 There is, however, a converse fallacy that
110 deserves equal condemnation: the fallacy of
111 supposing that fundamental inquiry is worth
112 pursuing only if its relevance to questions of
113 policy is immediate and obvious. In the
114 contemporary world, this fallacy is perhaps
115 not widely accepted, at least as the natural
116 sciences are concerned. We have now lived
117 through three centuries or more of vigorous
118 and highly successful inquiry into the laws of
119 nature. Much of that inquiry has been driven
120 by the simple urge to understand, to find the
121 beauty of order hidden in complexity. Time
122 and again, we have found the "idle" truths
123 arrived at through the process of inquiry to be
124 of the greatest moment for practical human
125 affairs. I need not take time here to argue the
126 point. Scientists know it, engineers and physi-
127 cians know it, congressmen and members of
128 parliaments know it, the man on the street
129 knows it.

130 But I am not sure that this truth is as

See: How, how?

far as

135 widely known in economics as it ought to be. I
136 cannot otherwise explain the rather weak and
137 backward development of the descriptive
138 theory of decision making including the
139 theory of the firm, the sparse and scattered
140 settlement of its terrain, and the fact that
141 many, if not most, of its investigators are
142 drawn from outside economics—from sociology,
143 from psychology, and from political
144 science. Respected and distinguished figures
145 in economics—Edward Mason, Fritz Mach-
146 lup, and Milton Friedman, for example—
147 have placed it outside the Pale (more accu-
148 rately, have placed economics outside *its*
149 Pale), and have offered it full autonomy
150 provided that it did not claim close kinship
151 with genuine economic inquiry.

152 Thus, Mason, commenting on Papan-
153 dreou's 1952 survey of research on the behav-
154 ioral theory of the firm, mused aloud:

155 ... has the contribution of this litera-
156 ture to economic analysis really been a
157 large one? ... The writer of this critique
158 must confess a lack of confidence in the
159 marked superiority, *for purposes of*
160 *economic analysis* of this newer concept
161 of the firm, over the older conception of
162 the entrepreneur. [pp. 221-22]

163 And, in a similar vein, Friedman sums up
164 his celebrated polemic against realism in
165 theory:

166 Complete "realism" is clearly unat-
167 tainable, and the question whether a
168 theory is realistic "enough" can be
169 settled only by seeing whether it yields
170 predictions that are good enough *for the*
171 *purpose in hand* or that are better than
172 predictions from alternative theories. [p.
173 41, emphasis added]

174 The "purpose in hand" that is implicit in
175 both of these quotations is providing decision-
176 theoretic foundations for positive, and then
177 for normative political economy. In the views
178 of Mason and Friedman, fundamental inquiry
179 into rational human behavior in the context of
180 business organizations is simply not (by defi-
181 nition) economics—that is to say, political
182 economy—unless it contributes in a major
183 way to that purpose. This is sometimes even
184 interpreted to mean that economic theories of
185 decision making are not falsified in any inter-
186 esting or relevant sense when their empirical
187 predictions of *microphenomena* are found to
188 be grossly incompatible with the observed
189 data. Such theories, we are told, are still
190 realistic "enough" provided that they do not
191 contradict aggregate observations of concern
192 to political economy. Thus economists who
193 are zealous in insisting that economic actors
194 maximize turn around and become satisficers
195 when the evaluation of their own theories is
196 concerned. They believe that businessmen
197 maximize, but they know that economic theo-
198 rists satisfy.

199 The application of the principle of satisfac-

204 ing to theories is sometimes defended as an
205 application of Occam's Razor: accept the
206 simplest theory that works.¹ But Occam's
207 Razor has a double edge. Succinctness of
208 statement is not the only measure of a theo-
209 ry's simplicity. Occam understood his rule as
210 recommending theories that make no more
211 assumptions than necessary to account for the
212 phenomena (*Essentia non sunt multiplicanda*
213 *praeter necessitatem*). A theory of profit or
214 utility maximization can be stated more
215 briefly than a satisficing theory of the sort I
216 shall discuss later. But the former makes
217 much stronger assumptions than the latter
218 about the human cognitive system. Hence, in
219 the case before us, the two edges of the razor
220 cut in opposite directions.

221 In whichever way we interpret Occam's
222 principle, parsimony can be only a secondary
223 consideration in choosing between theories,
224 unless those theories make identical predic-
225 tions. Hence, we must come back to a consid-
226 eration of the phenomena that positive deci-
227 sion theory is supposed to handle. These may
228 include both phenomena at the microscopic
229 level of the decision-making agents, or aggre-
230 gative phenomena of concern to political
231 economy.

232 B. *Decision Theory Pursued for its*
233 *Intrinsic Interest*

234 Of course the definition of the word "eco-
235 nomics" is not important. Like Humpty
236 Dumpty, we can make words mean anything
237 we want them to mean. But the professional
238 training and range of concern of economists
239 does have importance. Acceptance of the
240 narrow view that economics is concerned only
241 with the aggregative phenomena of political
242 economy defines away a whole rich domain of
243 rational human behavior as inappropriate for
244 economic research.

245 I do not wish to appear to be admitting that
246 the behavioral theory of the firm *has been*
247 irrelevant to the construction of political
248 economy. I will have more to say about its
249 relevance in a moment. My present argument
250 is counterfactual in form: *even if* there were
251 no present evidence of such relevance, human
252 behavior in business firms constitutes a highly
253 interesting body of empirical phenomena that
254 calls out for explanation as do all bodies of
255 phenomena. And if we may extrapolate from
256 the history of the other sciences, there is every
257 reason to expect that as explanations emerge,
258 relevance for important areas of practical
259 application will not be long delayed.

260 It has sometimes been implied (Friedman,
261 p. 14) that the correctness of the assumptions
262 of rational behavior underlying the classical
263 theory of the firm is not merely irrelevant, but
264 is not even empirically testable in any direct
265 way, the only valid test being whether these
266 assumptions lead to tolerably correct predic-
267 tions at the macroscopic level. That would be

272 true, of course, if we had no microscopes, so
273 that the micro-level behavior was not directly
274 observable. But we do have microscopes.
275 There are many techniques for observing
276 decision-making behavior, even at second-
277 by-second intervals if that is wanted. In test-
278 ing our economic theories, we do not have to
279 depend on the rough aggregate time-series
280 that are the main grist for the econometric
281 mill, or even upon company financial state-
282 ments.

283 The classical theories of economic decision
284 making and of the business firm make very
285 specific testable predictions about the con-
286 crete behavior of decision-making agents.
287 Behavioral theories make quite different
288 predictions. Since these predictions can be
289 tested directly by observation, either theory
290 (or both) may be falsified as readily when
291 such predictions fail as when predictions
292 about aggregate phenomena are in error.

293 C. *Aggregative Tests of Decision Theory:*
294 *Marginalism*

295 If some economists have erroneously
296 supposed that micro-economic theory can
297 only be tested by its predictions of aggregate
298 phenomena, we should avoid the converse
299 error of supposing that aggregate phenomena
300 are irrelevant to testing decision theory. In
301 particular, are there important, *empirically*
302 *verified*, aggregate predictions that follow
303 from the theory of perfect rationality but that
304 do not follow from behavioral theories of
305 rationality?

306 The classical theory of omniscient rational-
307 ity is strikingly simple and beautiful. More-
308 over, it allows us to predict (correctly or not)
309 human behavior without stirring out of our
310 armchairs to observe what such behavior is
311 like. All the predictive power comes from
312 characterizing the shape of the environment
313 in which the behavior takes place. The envi-
314 ronment, combined with the assumptions of
315 perfect rationality, fully determines the
316 behavior. Behavioral theories of rational
317 choice—theories of bounded rationality—do
318 not have this kind of simplicity. But, by way
319 of compensation, their assumptions about
320 human capabilities are far weaker than those
321 of the classical theory. Thus, they make
322 modest and realistic demands on the knowl-
323 edge and computational abilities of the
324 human agents, but they also fail to predict
325 that those agents will equate costs and returns
326 at the margin.

327 D. *Have the Marginalist Predictions Been*
328 *Tested?*

329 A number of empirical phenomena have
330 been cited as providing more or less conclu-
331 sive support for the classical theory of the
332 firm as against its behavioral competitors (see
333 Dale Jorgensen and Calvin Siebert). But

23

to + recte.
be.

338 there are no direct observations that individu-
339 als or firms do actually equate marginal costs
340 and revenues. The empirically verified conse-
341 quences of the classical theory are almost
342 always weaker than this. Let us look at four of
343 the most important of them: the fact that
344 demand curves generally have negative
345 slopes; the fact that fitted Cobb-Douglas
346 functions are approximately homogeneous of
347 the first degree; the fact of decreasing returns
348 to scale; and the fact that executive salaries
349 vary with the logarithm of company size. Are
350 these indeed facts? And does the evidence
351 support a maximizing theory against a satis-
352 fying theory?

353 *Negatively Sloping Demand*
354 *Curves.* Evidence that consumers actually
355 distribute their purchases in such a way as to
356 maximizing their utilities, and hence to
357 equate marginal utilities, is nonexistent.
358 What the empirical data do confirm is that
359 demand curves generally have negative
360 slopes. (Even this "obvious" fact is tricky to
361 verify, as Henry Schultz showed long years
362 ago.) But negatively sloping demand curves
363 could result from a wide range of behaviors
364 satisfying the assumptions of bounded ration-
365 ality rather than those of utility maximiza-
366 tion. Gary Becker, who can scarcely be
367 regarded as a hostile witness for the classical
368 theory, states the case very well:

369 Economists have long been aware that
370 some changes in the feasible or opportu-
371 nity sets of households would lead to the
372 same response *regardless of the decision*
373 *rule used.* For example, a decrease in
374 real income necessarily decreases the
375 amount spent on at least one commodi-
376 ty. . . It has seldom been realized, howev-
377 er, that the change in opportunities
378 resulting from a change in relative prices
379 also tends to produce a systematic
380 response, regardless of the decision rule.
381 In particular, the fundamental theorem of
382 traditional theory—that demand curves
383 are negatively inclined—largely results
384 from the change in opportunities alone
385 and is largely independent of the decision
386 rule. [p. 4]

387 Later, Becker is even more explicit, saying,
388 "Not only utility maximization but also many
389 other decision rules, incorporating a wide
390 variety of irrational behavior, lead to nega-
391 tively inclined demand curves because of the
392 effect of a change in prices on opportunities"
393 (p. 5).²

394 *First-Degree Homogeneity of Prod-*
395 *uction Functions.* Another example of an
396 observed phenomenon for which the classical
397 assumptions provide sufficient, but not neces-
398 sary, conditions is the equality between labor's
399 share of product and the exponent of the labor
400 factor in fitted Cobb-Douglas production
401 functions (see Simon and Ferdinand Levy).
402 Fitted Cobb-Douglas functions are homogen-
403 eous, generally of degree close to unity and

g

- loose

e

tr

= sec /

- neg / corr

tr

f

= sec /

g

g / tr

408 with a labor exponent of about the right
409 magnitude. These findings, however, cannot
410 be taken as strong evidence for the classical
411 theory, for the identical results can readily be
412 produced by mistakenly fitting a Cobb-Douglas
413 function to data that (we) in fact generated
414 by a linear accounting identity (value of goods
415 equals labor cost plus capital cost), (see E. H.
416 Phelps-Brown). The same comment applies to
417 the SMAC production function (see Richard
418 Cyert and Simon). Hence, the empirical find-
419 ings do not allow us to draw any particular
420 conclusions about the relative plausibility of
421 classical and behavioral theories, both of
422 which are equally compatible with the data.

423 *The Long-Run Cost Curve.* Somewhat
424 different is the case of the firm's long-run cost
425 curve, which classical theory requires to be U
426 shaped if competitive equilibrium is to be
427 stable. Theories of bounded rationality do not
428 predict this—fortunately, for the observed
429 data make it exceedingly doubtful that the
430 cost curves are in fact generally U shaped.
431 The evidence for many industries shows costs
432 at the high-scale ends of the curves to be
433 essentially constant or even declining (see
434 Alan Walters). This finding is compatible
435 with stochastic models of business firm
436 growth and size (see Y. Ijiri and Simon), but
437 not with the static equilibrium model of
438 classical theory.

439 *Executive Salaries.* Average salaries of
440 top corporate executives grow with the loga-
441 rithm of corporate size (see David Roberts).
442 This finding has been derived from the
443 assumptions of the classical theory of profit
444 maximization only with the help of very
445 particular *ad hoc* assumptions about the
446 distribution of managerial ability (see Robert
447 Lucas, 1978). The observed relation is
448 implied by a simple behavioral theory that
449 assumes only that there is a single, culturally
450 determined, parameter which fixes the average
451 ratio of the salaries of managers to the sala-
452 ries of their immediate subordinates (see
453 Simon, 1957). In the case of the executive
454 salary data, the behavioral model that
455 explains the observations is substantially
456 more parsimonious (in terms of assumptions
457 about exogenous variables) than the classical
458 model that explains the same observations.

459 *Summary: Phenomena That Fail to*
460 *Discriminate.* It would take a much more
461 extensive review than is provided here to
462 establish the point conclusively, but I believe
463 it is the case that specific phenomena requir-
464 ing a theory of utility or profit maximization
465 for their explanation rather than a theory of
466 bounded rationality simply have not been
467 observed in aggregate data. In fact, as my last
468 two examples indicate, it is the classical
469 rather than the behavioral form of the theory
470 that faces real difficulties in handling some of
471 the empirical observations.

472 *Failures of Classical Theory.* It may
473 well be that classical theory can be patched

free = wref

= p/tr

which

or

478 up sufficiently to handle a wide range of
479 situations where uncertainty and outguessing
480 phenomena do not play a central role—that
481 is, to handle the behavior of economies that
482 are relatively stable and not too distant from a
483 competitive equilibrium. However, a strong
484 positive case for replacing the classical theory
485 by a model of bounded rationality begins to
486 emerge when we examine situations involving
487 decision making under uncertainty and
488 imperfect competition. These situations the
489 classical theory was never designed to handle,
490 and has never handled satisfactorily. Statistical
491 decision theory employing the idea of
492 subjective expected utility, on the one hand,
493 and game theory, on the other, have contributed
494 enormous conceptual clarification to
495 these kinds of situations without providing
496 satisfactory descriptions of actual human
497 behavior, or even, for most cases, normative
498 theories that are actually usable in the face of
499 the limited computational powers of men and
500 computers.

501 I shall have more to say later about the
502 positive case for a descriptive theory of
503 bounded rationality, but I would like to turn
504 first to another territory within economic
505 science that has gained rapidly in population
506 since World War II, the domain of normative
507 decision theory.

508 E. Normative Decision Theory

509 Decision theory can be pursued not only for
510 the purposes of building foundations for political
511 economy, or of understanding and
512 explaining phenomena that are in themselves
513 intrinsically interesting, but also for the
514 purpose of offering direct advice to business
515 and governmental decision makers. For
516 reasons not clear to me, this territory was very
517 sparsely settled prior to World War II. Such
518 inhabitants as it had were mainly industrial
519 engineers, students of public administration,
520 and specialists in business functions, none of
521 whom especially identified themselves with
522 the economic sciences. Prominent pioneers
523 included the mathematician, Charles Babbage,
524 inventor of the digital computer, the
525 engineer, Frederick Taylor, and the administrator,
526 Henri Fayol.

527 During World War II, this territory,
528 almost abandoned, was rediscovered by scientists,
529 mathematicians, and statisticians concerned
530 with military management and logistics,
531 and was renamed "operations research" or
532 "operations analysis." So remote were the
533 operations researchers from the social science
534 community that economists wishing to enter
535 the territory had to establish their own colony,
536 which they called "management science."
537 The two professional organizations thus
538 engendered still retain their separate identities,
539 though they are not amicably federated
540 in a number of common endeavors.

541 Optimization techniques were transported

into management science from economics, and new optimization techniques, notably linear programming, were invented and developed, the names of Dantzig, Kantorovich, and Koopmans being prominent in the early development of that tool.

Now the salient characteristic of the decision tools employed in management science is that they have to be capable of actually making or recommending decisions, taking as their inputs the kinds of empirical data that are available in the real world, and performing only such computations as can reasonably be performed by existing desk calculators or, a little later, electronic computers. For these domains, idealized models of optimizing entrepreneurs, equipped with complete certainty about the world—~~or~~, at worst, having full probability distributions for uncertainty events—are of little use. Models have to be fashioned with an eye to practical computability, no matter how severe the approximations and simplifications that are thereby imposed on them.

Model construction under these stringent conditions has taken two directions. The first is to retain optimization, but to simplify sufficiently so that the optimum (in the simplified world!) is computable. The second is to construct satisficing models that provide good enough decisions with reasonable costs of computation. By giving up optimization, a richer set of properties of the real world can be retained in the models. Stated otherwise, decision makers can satisfice either by finding optimum solutions for a simplified world, or by finding satisfactory solutions for a more realistic world. Neither approach, in general, dominates the other, and both have continued to co-exist in the world of management science.

Thus, the body of theory that has developed in management science shares with the body of theory ~~that has developed in management science shares with the body of theory~~ in descriptive decision theory a central concern with the ways in which decisions are made, and not just with the decision outcomes. As I have suggested elsewhere (1978b), these are theories of *how* to decide rather than theories of *what* to decide.

Let me cite one example, from work in which I participated, of how model building in normative economics is shaped by computational considerations (see Charles Holt, Franco Modigliani, John Muth and Simon). In fact of uncertain and fluctuating production demands, a company can smooth and stabilize its production and employment levels at the cost of holding buffer inventories. What kind of decision rule will secure a reasonable balance of costs? Formally, we are faced with a dynamic programming problem, and these generally pose formidable and often intolerable computational burdens for their solution.

One way out of this difficulty is to seek a

= a =

= p =
s Harry

lc
e/

lc
e/tr

tr

616 special case of the problem that will be
617 computational, tractable. If we assume the *clg*
618 cost functions facing the company all to be
619 quadratic in form, the optimal decision rule
620 will then be a linear function of the decision
621 variables, which can readily be computed in
622 terms of the cost parameters. Equally impor-
623 tant, under uncertainty about future sales,
624 only the expected values, and not the higher
625 moments, of the probability distributions
626 enter into the decision rule (Simon, 1956b).
627 Hence the assumption of quadratic costs
628 reduces the original problem to one that is
629 readily solved. Of course the solution, though
630 it provides optimal decisions for the simplified
631 world of our assumptions, provides, at best,
632 satisfactory solutions for the real-world deci-
633 sion problem that the quadratic function
634 approximates. In-principle, unattainable op-
635 timization is sacrificed for in-practice, attain-
636 able satisfaction.

637 If human decision makers are as rational as
638 their limited computational capabilities and
639 their incomplete information permit them to
640 be, then there will be a close relation between
641 normative and descriptive decision theory.
642 Both areas of inquiry are concerned primarily
643 with procedural rather than substantive
644 rationality (Simon, 1978a). As new mathe-
645 matical tools for computing optimal and satis-
646 factory decisions are discovered, and as
647 computers become more and more powerful,
648 the recommendations of normative decision
649 theory will change. But as the new recommen-
650 dations are diffused, the actual, observed,
651 practice of decision making in business firms
652 will change also. And these changes may have
653 macro-economic consequences. For example,
654 there is some agreement that average inven-
655 tory holdings of American firms have been
656 reduced significantly by the introduction of
657 formal procedures for calculating reorder
658 points and quantities.

659 II. Characterizing Bounded Rationality

660 The principal forerunner of a behavioral
661 theory of the firm is the tradition usually
662 called Institutionalism. It is not clear that all
663 of the writings, European and American,
664 usually lumped under this rubric have much
665 in common, or that their authors would agree
666 with each other's views. At best, they share a
667 conviction that economic theory must be
668 reformulated to take account of the social and
669 legal structures amidst which market transac-
670 tions are carried out. Today, we even find a
671 vigorous development within economics that
672 seeks to achieve institutionalist goals within
673 the context of neoclassical price theory. I will
674 have more to say about that a little later.

675 The name of John R. Commons is promi-
676 nent—perhaps the most prominent—among
677 American Institutionalists. Commons' diffi-
678 cult writings (for example, ~~International~~ Institutional
679 Economics) borrow their language heavily

684 from the law, and seek to use the *transaction*
685 as their basic unit of behavior. I will not
686 undertake to review Commons' ideas here,
687 but simply remark that they provided me with
688 many insights in my initial studies of organi-
689 zational decision making (see my *Adminis-*
690 *trative Behavior*, p. 136).

691 Commons also had a substantial influence
692 on the thinking of Chester I. Barnard, an
693 intellectually curious business executive who
694 distilled from his experience as president of
695 the New Jersey Bell Telephone Company,
696 and as executive of other business, govern- *try*
697 mental, and nonprofit organizations, a pro-
698 found book on decision making titled *The*
699 *Functions of the Executive*. Barnard pro-
700 posed original theories, which have stood up
701 well under empirical scrutiny, of the nature of
702 the authority mechanism in organizations,
703 and of the motivational bases for employee
704 acceptance of organizational goals (the so-
705 called "inducements-contributions" theory);
706 and he provided a realistic description of
707 organizational decision making, which he
708 characterized as "opportunistic." The numer- *lc*
709 ous references to Barnard's work in *Adminis-*
710 *trative Behavior* attest, though inadequately,
711 to the impact he had on my own thinking
712 about organizations.

713 A. In Search of a Descriptive Theory

714 In 1934-35, in the course of a field study of
715 the administration of public recreational
716 facilities in Milwaukee, which were managed
717 jointly by the school board and the city public
718 works department, I encountered a puzzling
719 phenomenon. Although the heads of the two
720 agencies appeared to agree as to the objec-
721 tives of the recreation program, and did not
722 appear to be competing for empire, yet there
723 was continual disagreement and tension
724 between them with respect to the allocation of
725 funds between physical maintenance, on the
726 one hand, and play supervision on the other.
727 Why did they not, as my economics books
728 suggested, simply balance off the marginal
729 return of the one activity against that of the
730 other?

731 Further exploration made it apparent that
732 they didn't equate expenditures at the margin
733 because, intellectually, they couldn't. There
734 was no measurable production function from
735 which quantitative inferences about marginal
736 productivities could be drawn; and such qual-
737 itative notions of a production function as the
738 two managers possessed were mutually
739 incompatible. To the public works adminis-
740 trator, a playground was a physical facility,
741 serving as a green oasis in the crowded gray
742 city. To the recreation administrator, a play-
743 ground was a social facility, where children
744 could play together with adult help and guid-
745 ance.

746 How can human beings make rational deci-
747 sions in circumstances like these? How are

822 very imperfectly known both because of
823 limited computational power and because of
824 uncertainty in the external world, and the
825 decision maker did not possess a general and
826 consistent utility function for comparing heterogeneous alternatives.

827
828 Several procedures of rather general applicability and wide use have been discovered
829 that transform intractable decision problems
830 into tractable ones. One procedure already
831 mentioned is to look for satisfactory choices
832 instead of optimal ones. Another is to replace
833 abstract, global goals with tangible subgoals,
834 whose achievement can be observed and
835 measured. A third is to divide up the decision-making task among many specialists, coordinating their work by means of a structure of communications and authority relations. All of these, and others, fit the general rubric of "bounded rationality," and it is now clear that the elaborate organizations that human beings have constructed in the modern world to carry out the work of production and government can only be understood as machinery for coping with the limits of man's abilities to comprehend and compute in the face of complexity and uncertainty.

849 This rather vague and general initial
850 formulation of the idea of bounded rationality
851 called for elaboration in two directions:
852 greater formalization of the theory, and
853 empirical verification of its main claims.
854 During the decade that followed the publication of *Administrative Behavior*, substantial
855 progress was made in both directions, some of
856 it through the efforts of my colleagues and
857 myself, much of it by other research groups
858 that shared the same Zeitgeist.

roms/cas

860 **B. Empirical Studies**

861 The principal source of empirical data
862 about organizational decision making has
863 been straightforward "anthropological" field
864 study, eliciting descriptions of decision making procedures and observing the course of specific decision-making episodes. Examples are a study, with Guetzkow, Kozmetsky, and Tyndall (1954), of the ways in which accounting data were used in decision making in large corporations; and a series of studies, with Cyert, James March, and others, of specific nonprogrammed policy decisions in a number of different companies (see Cyert, Simon and Donald Trow). The latter line of work was greatly developed and expanded by Cyert and March and its theoretical implications for economics explored in their important work, *A Behavioral Theory of the Firm*.

J

cas

880 At about the same time, the fortuitous
881 availability of some data on businessmen's
882 perceptions of a problem situation described
883 in a business policy casebook enabled DeWitt
884 Dearborn and me to demonstrate empirically
885 the cognitive basis for identification with

752 they to apply the marginal calculus? Or, if it
753 does not apply, what do they substitute for
754 it?

755 The phenomenon observed in Milwaukee is
756 ubiquitous in human decision making. In
757 organization theory it is usually referred to as
758 *subgoal identification*. When the goals of an
759 organization cannot be connected operation-
760 ally with actions (when the production func-
761 tion can't be formulated in concrete terms),
762 then decisions will be judged against subordi-
763 nate goals that can be so connected. There is
764 no unique determination of these subordinate
765 goals. Their formulation will depend on the
766 knowledge, experience, and organizational
767 environment of the decision maker. In the
768 face of this ambiguity, the formulation can
769 also be influenced in subtle, and not so subtle,
770 ways by his self-interest and power drives.

771 The phenomenon arises as frequently in
772 individual as in social decision making and
773 problem solving. Today, under the rubric of
774 *problem representation*, it is a central
775 research interest of cognitive psychology.
776 Given a particular environment of stimuli,
777 and a particular background of previous
778 knowledge, how will a person organize this
779 complex mass of information into a problem
780 formulation that will facilitate his solution
781 efforts? How did Newton's experience of the
782 apple, if he had one, get represented as an
783 instance of attraction of apple by Earth?

784 Phenomena like these provided the central
785 theme for *Administrative Behavior*. That
786 study represented "an attempt to construct
787 tools useful in my own research in the field of
788 public administration." The product was
789 actually not so much a theory as a prolegom-
790 ena to a theory, stemming from the conviction
791 "that decision making is the heart of adminis-
792 tration, and that the vocabulary of adminis-
793 trative theory must be derived from the logic
794 and psychology of human choice." It was, if
795 you please, an exercise in problem representa-
796 tion.

797 On examination, the phenomenon of
798 subgoal identification proved to be the visible
799 tip of a very large iceberg. The shape of the
800 iceberg is best appreciated by contrasting it
801 with classical models of rational choice. The
802 classical model calls for knowledge of all the
803 alternatives that are open to choice. It calls
804 for complete knowledge of, or ability to
805 compute, the consequences that will follow on
806 each of the alternatives. It calls for certainty
807 in the decision maker's present and future
808 evaluation of these consequences. It calls for
809 the ability to compare consequences, no
810 matter how diverse and heterogeneous, in
811 terms of some consistent measure of utility.
812 The task, then, was to replace the classical
813 model with one that would describe how
814 decisions could be (and probably actually
815 were) made when the alternatives of search
816 had to be sought out, the consequences of
817 choosing particular alternatives were only

ccc

890 subgoals, the phenomenon that had so
891 impressed me in the Milwaukee recreation
892 study. The businessmen's perceptions of the
893 principal problems facing the company
894 described in the case were mostly determined
895 by their own business experiences—sales and
896 accounting executives identified a sales prob-
897 lem, manufacturing executives, a problem of
898 internal organization. *ce*

899 Of course there is vastly more to be learned
900 and tested about organization decision mak- *cal*
901 ing than can be dealt with in a handful of
902 studies. Although many subsequent studies
903 have been carried out in Europe and the
904 United States, this domain is still grossly
905 undercultivated (for references, see March,
906 1965; E. Johnsen, 1968; G. Eliasson, 1976).
907 Among the reasons for the relative neglect of
908 such studies, as contrasted, say, with labora-
909 tory experiments in social psychology, is that
910 they are extremely costly and time consum-
911 ing, with a high grist-to-grain ratio, the meth-
912 odology for carrying them out is primitive,
913 and satisfactory access to decision-making
914 behavior is hard to secure. This part of
915 economics has not yet acquired the habits of *ce/*
916 patience and persistence in the pursuit of facts
917 that is exemplified in other domains by the
918 work, say, of Simon Kuznets or of the archi-
919 tects of the MIT-SSRC-Penn econometric
920 models.

C. Theoretical Inquiries

922 On the theoretical side, three questions
923 seemed especially to call for clarification:
924 what are the circumstances under which an
925 employment relation will be preferred to some
926 other form of contract as the arrangement for
927 securing the performance of work; what is the
928 relation between the classical theory of the
929 firm and theories of organizational equilib-
930 rium first proposed by Chester Barnard; and
931 what are the main characteristics of human
932 rational choice in situations where complexity
933 precludes omniscience?

934 *The Employment Relation.* A funda-
935 mental characteristic of modern industrial
936 society is that most work is performed, not by
937 individuals who produce products for sale, nor
938 by individual contractors, but by persons who
939 have accepted employment in a business firm
940 and the authority relation with the employer
941 that employment entails. Acceptance of
942 authority means willingness to permit one's
943 behavior to be determined by the employer, at
944 least within some zone of indifference or
945 acceptance. What is the advantage of this
946 arrangement over a contract for specified
947 goods ~~to~~ services? Why is so much of the
948 world's work performed in large, hierarchic
949 organizations? *or*

950 Analysis showed (Simon, 1951) that a
951 combination of two factors could account for
952 preference for the employment contract over
953 other forms of contracts: uncertainty as to

958 which future behaviors would be advanta-
959 geous to the employer, and a greater indiffer-
960 ence of the employee as compared with the
961 employer (within the former's area of accep-
962 tance) as to which of these behaviors he
963 carried out. When the secretary is hired, the
964 employer does not know what letters he will
965 want her to type, and the secretary has no
966 great preference for typing one letter rather
967 than another. The employment contract
968 permits the choice to be postponed until the
969 uncertainty is resolved, with little cost to the
970 employee and great advantage to the
971 employer. The explanation is closely analo-
972 gous to one Jacob Marschak had proposed for
973 liquidity preference. Under conditions of
974 uncertainty it is advantageous to hold
975 resources in liquid, flexible form.

976 *Organizational Equilibrium.* Barnard
977 had described the survival of organizations in
978 terms of the motivations that make their
979 participants (employees, investors, customers,
980 suppliers) willing to remain in the system. In
981 *Administrative Behavior*, I had developed
982 this notion further into a motivational theory
983 of the balance between the inducements that
984 were provided by organizations to their parti-
985 cipants, and the contributions those partici-
986 pants made to the organizations' resources.

987 A formalization of this theory (Simon,
988 1952; 1953) showed its close affinity to the
989 classical theory of the firm, but with an
990 important and instructive difference. In
991 comparing the two theories, each inducement-
992 contribution relation became a supply sched-
993 ule for the firm. The survival conditions
994 became the conditions for positive profit. But
995 while the classical theory of the firm assumes
996 that all profits accrue to a particular set of
997 participants, the owners, the organization
998 theory treats the surplus more symmetrically,
999 and does not predict how it will be distributed.
:00 Hence the latter theory leaves room, under
B002 conditions of monopoly and imperfect compe-
001 tition, for bargaining among the participants
002 (for example, between labor and owners) for
003 the surplus. The survival conditions—positive
004 profits rather than maximum profits—also
005 permit a departure from the assumptions of
006 perfect rationality.

007 *Mechanisms of Bounded Rationality.* In
008 *Administrative Behavior*, bounded rationality
009 is largely characterized as a residual catego-
010 ry—rationality is bounded when it falls short
011 of omniscience. And the failures of omnis-
012 cience are largely failures of knowing all the
013 alternatives, uncertainty about relevant ex-
014ogenous events, and inability to calculate
015 consequences. There was needed a more posi-
016 tive and formal characterization of the mech-
017 anisms of choice under conditions of bounded
018 rationality. Two paper (Simon, 1955; 1956a)
019 undertook first steps in that direction.

020 Two concepts are central to the characteri-
021 zation: *search* and *satisficing*. If the alterna-
022 tives for choice are not given initially to the

tr
tr
tr

ca

027 decision maker, then he must search for them.
028 Hence, a theory of bounded rationality must
029 incorporate a theory of search. This idea was
030 later developed independently by George
031 Stigler in a very influential paper that took as
032 its example of a decision situation the
033 purchase of a second-hand automobile.
034 Stigler poured the search theory back into the
035 old bottle of classical utility maximization,
036 the cost of search being equated with its
037 marginal return. In my 1956 paper, I had
038 demonstrated the same formal equivalence,
039 using as my example a dynamic programming
040 formulation of the process of selling a
041 house.

042 But utility maximization, as I showed, was
043 not essential to the search scheme—fortu-
044 nately, for it would have required the decision
045 maker to be able to estimate the marginal
046 costs and returns of search in a decision
047 situation that was already too complex for the
048 exercise of global rationality. As an alterna-
049 tive, one could postulate that the decision
050 maker had formed some *aspiration* as to how
051 good an alternative he should find. As soon as
052 he discovered an alternative for choice meet-
053 ing his level of aspiration, he would terminate
054 the search and choose that alternative. I
055 called this mode of selection *satisficing*. It
056 had its roots in the empirically based psycho-
057 logical theories, due to Lewin and others, of
058 aspiration levels. As psychological inquiry
059 had shown, aspiration levels are not static, but
060 tend to rise and fall in consonance with
061 changing experiences. In a benign environ-
062 ment that provides many good alternatives,
063 aspirations rise; in a harsher environment,
064 they fall.

065 In long-run equilibrium it might even be
066 the case that choice with dynamically adapt-
067 ing aspiration levels would be equivalent to
068 optimal choice, taking the costs of search into
069 account. But the important thing about the
070 search and satisficing theory is that it showed
071 how choice could actually be made with
072 reasonable amounts of calculation, and using
073 very incomplete information, without the
074 need of performing the impossible—of carry-
075 ing out this optimizing procedure.

076 *C. Summary*

077 Thus, by the middle 1950's, a theory of
078 bounded rationality had been proposed as an
079 alternative to classical omniscient rationality,
080 a significant number of empirical studies had
081 been carried out that showed actual business
082 decision making to conform reasonably well
083 with the assumptions of bounded rationality
084 but not with the assumptions of perfect
085 rationality, and key components of the theo-
086 ry—the nature of the authority and employ-
087 ment relations, organizational equilibrium,
088 and the mechanisms of search and satisfic-
089 ing—had been elucidated formally. In the
090 remaining parts of this paper, I should like to

095 trace subsequent developments of decision-
096 making theory, including developments com-
097 petitive with the theory of bounded rationali-
098] ty, and then to comment on the implications
099 (and potential implications) of the new
100 descriptive theory of decision for political
101 economy.

III. The Neoclassical Revival

103 Peering forward from the late 1950's, it
104 would not have been unreasonable to predict
105 that theories of bounded rationality would
106 soon find a large place in the mainstream of
107 economic thought. Substantial progress had
108 been made in providing the theories with
109 some formal structure, and an increasing
110 body of empirical evidence showed them to
111 provide a far more veridical picture of deci-
112 sion making in business organizations than
113 did the classical concepts of perfect rationali-
114] ty.

115 History has not followed any such simple
116 course, even though many aspects of the
117 Zeitgeist were favorable to movement in this
118 direction. During and after World War II, a
119 large number of academic economists were
120 exposed directly to business life, and had
121 more or less extensive opportunities to observe
122 how decisions were actually made in business
123 organizations. Moreover, those who became
124 active in the development of the new manage-
125 ment science were faced with the necessity of
126 developing decision-making procedures that
127 could actually be applied in practical situa-
128 tions. Surely these trends would be conducive
129 to moving the basic assumptions of economic
130 rationality in the direction of greater real-
131 ism.

132 But these were not the only things that
133 were happening in economics in the postwar
134 period. First, there was a vigorous reaction
135 that sought to defend classical theory from
136 behavioralism on methodological grounds. I
137 have already commented on these methodo-
138 logical arguments in the first part of my talk.
139 However deeply one may disagree with them,
140 they were stated persuasively and are still
141 influential among academic economists.

142 Second, the rapid spread of mathematical
143 knowledge and competence in the economics
144 profession permitted the classical theory,
145 especially when combined with statistical
146 decision theory and the theory of games due
147 to von Neuman and Morgenstern, to develop
148 to new heights of sophistication and elegance,
149 and to expand to embrace, albeit in highly
150 stylized form, some of the phenomena of
151 uncertainty and imperfect information. The
152 flowering of mathematical economics and
153 econometrics has provided two generations of
154 economic theorists with a vast garden of
155 formal and technical problems that have
156 absorbed their energies and postponed
157 encounters with the inelegancies of the real
158 world.

163 If I sound mildly critical of these develop-
164 ments, I should confess that I have also been a
165 part of them, admire them, and would be
166 decidedly unhappy to return to the pre-mathe-
167 matical world they have replaced. My
168 concern is that the economics profession has
169 exhibited some of the serial one-thing-at-
170 a-time character of human rationality, and
171 has seemed sometimes to be unable to distri-
172 bute its attention in a balanced fashion
173 among neoclassical theory, macroeconomic
174 tricks, and descriptive decision theory. As a
175 result, not as much professional effort has
176 been devoted to the latter two, and especially
177 the third, as one might have hoped and
178 expected. The Heartland is more overpopu-
179 lated than ever, while rich lands in other parts
180 of the empire go untended.

P
tr

untitled/

181 A. Search and Information Transfer

182 Let me allude to just three of the ways in
183 which classical theory has sought to cope with
184 some of its traditional limitations, and has
185 even sought to make the development of a
186 behavioral theory, incorporating psychologi-
187 cal assumptions, unnecessary. The first was to
188 introduce search and information transfer
189 explicitly as economic activities, with asso-
190 ciated costs and outputs, that could be
191 inserted into the classical production func-
192 tion. I have already referred to Stigler's 1961
193 paper on the economics of information, and
194 my own venture in the same direction in the
195 1956 essay cited earlier.

196 In theory of this genre, the decision maker
197 is still an individual. A very important new
198 direction, in which decisions are made by
199 groups of individuals, in teams or organiza-
200 tions, is the economic theory of teams devel-
201 oped by Jacob Marschak and Roy Radner.
202 Here we see genuine organizational phenom-
203 ena—specialization of decision making as a
204 consequence of the costs of transmitting infor-
205 mation—emerge from the rational calculus.
206 Because the mathematical difficulties are
207 formidable, the theory remains largely illus-
208 trative and limited to very simple situations in
209 miniature organizations. Nevertheless, it has
210 greatly broadened our understanding of the
211 economics of information.

212 In none of these theories—any more than
213 in statistical decision theory or the theory of
214 games—is the assumption of perfect maxim-
215 ization abandoned. Limits and costs of infor-
216 mation are introduced, not as psychological
217 characteristics of the decision maker, but as
218 part of his technological environment. Hence,
219 the new theories do nothing to alleviate the
220 computational complexities facing the deci-
221 sion maker—do not see him coping with them
222 by heroic approximation, simplifying and
223 satisficing, but simply magnify and multiply
224 them. Now he needs to compute not merely
225 the shapes of his supply and demand curves,
226 but, in addition, the costs and benefits of

c o c

c e c

c o c

231 computing those shaped to greater accuracy
232 as well. Hence, to some extent, the impression
233 that these new theories deal with the hitherto
234 ignored phenomena of uncertainty and infor-
235 mation transmission is illusory. For many
236 economists, however, the illusion has been
237 persuasive.

238 B. *Rational Expectations Theory*

239 A second development in neoclassical
240 theory on which I wish to comment is the
241 so-called "rational expectations" theory.
242 There is a bit of historical irony surrounding
243 its origins. I have already described the
244 management science inquiry of Holt, Modi-
245 gliani, Muth, and myself that developed a
246 dynamic programming algorithm for the
247 special (and easily computed) case of
248 quadratic cost functions. In this case, the
249 decision rules are linear, and the probability
250 distributions of future events can be replaced
251 by their expected values, which serve as
252 certainty equivalents (Simon, 1956; Henri
253 Theil, 1957).

254 Muth imaginatively saw in this special case
255 a paradigm for rational behavior under uncer-
256 tainty. What to some of us in the HMMS
257 research team was an approximating, satisfic-
258 ing simplification, served for him as a major
259 line of defence for perfect rationality. He said
260 in his seminal 1961 *Econometrica*, "It is
261 sometimes argued that the assumption of
262 rationality in economics leads to theories
263 inconsistent with, or inadequate to explain,
264 observed phenomena, especially changes over
265 time. . . Our hypothesis is based on exactly
266 the opposite point of view: that dynamic
267 economic models do not assume enough
268 rationality" (p. 316).

269 The new increment of rationality that
270 Muth proposed was that "expectations, since
271 they are informed predictions of future
272 events, are essentially the same as the predic-
273 tions of the relevant economic theory" (p.
274 316). He would cut the Gordian knot. Instead
275 of dealing with uncertainty by elaborating the
276 model of the decision process, he would once
277 and for all—if his hypothesis were correct—
278 make process irrelevant. The subsequent
279 vigorous development of rational expectations
280 theory, in the hands of Sargent, Lucas,
281 Prescott, and others, is well known to most
282 readers (see, for example, Lucas, 1975).

283 It is too early to render a final verdict on
284 the rational expectations theory. The issue
285 will ultimately be decided, as all scientific
286 debates should be, by a gradual winnowing of
287 the empirical evidence, and that winnowing
288 process has just begun. Meanwhile, certain
289 grave theoretical difficulties have already
290 been noticed. As Muth himself has pointed
291 out it is rational (i.e., profit maximizing) to
292 use the "rational expectations" decision rule
293 if the relevant cost equations are in fact
294 quadratic. I have suggested elsewhere

#article, #

cec/tr

299 (1978a) that it might therefore be less
300 misleading to call the rule a "consistent
301 expectations" rule.

302 Perhaps even more important, Albert Ando
303 and Benjamin Friedman (1978, 1979) have
304 shown that the policy implications of the
305 rational expectations rule are quite different
306 under conditions where new information
307 continually becomes available to the system,
308 structural changes occur, and the decision
309 maker learns, than they are under steady-
310 state conditions. For example, under the more
311 dynamic conditions, monetary neutrality—
312 which in general holds for the static consistent
313 expectations models—is no longer guaranteed
314 for any finite time horizon.

315 In the recent "revisionist" versions of
316 consistent expectations theory, moreover,
317 where account is taken of a changing environ-
318 ment of information, various behavioral
319 assumptions reappear to explain how expecta-
320 tions are formed—what information decision
321 makers will consider, and what they will
322 ignore. But unless these assumptions are to be
323 made on a wholly *ad hoc* and arbitrary basis,
324 they create again the need for an explicit and
325 valid theory of the decision-making process
326 (see Simon, 1958a; B. Friedman, 1979).

327 C. Statistical Decision Theory and Game
328 Theory *to be checked*

329 Statistical decision theory and game theory
330 are two other important components of the
331 neoclassical revival. The former addresses
332 itself to the question of incorporating uncer-
333 tainty (or more properly, risk) into the deci-
334 sion-making models. It requires heuristic
335 assumptions about the information the deci-
336 sion maker has concerning the probability
337 distributions of the relevant variables, and
338 simply increases by orders of magnitude the
339 computational problems he faces. *to*

340 Game theory addresses itself to the "out-
341 guessing" problem that arises whenever an
342 economic actor takes into account the possible
343 reactions to his own decisions of the other
344 actors. To my mind, the main product of the
345 very elegant apparatus of game theory has
346 been to demonstrate quite clearly that it is
347 virtually impossible to define an unambiguous
348 criterion of rationality for this class of situa-
349 tions (or, what amounts to the same thing, a
350 definitive definition of the "solution" of a
351 game). Hence, game theory has not brought
352 to the theories of oligopoly and imperfect
353 competition the relief from their contradic-
354 tions and complexities that was originally
355 hoped for it. Rather, it has shown that these
356 difficulties are ineradicable. We may be able
357 to reach consensus that a certain criterion of
358 rationality is appropriate to a particular
359 game, but if someone challenges the consen-
360 sus, preferring a different criterion, we will
361 have no logical basis for persuading him that
362 he is wrong. *#*
(S)

367 D. Conclusion . 22

368 Perhaps I have said enough about the
369 neoclassical revival to suggest why it has been
370 a highly attractive commodity in competition
371 with the behavioral theories. To some econo-
372 mists at least, it has held open the possibility
373 and hope that important questions that had
374 been troublesome for classical economics
375 could now be addressed without sacrifice of
376 the central assumption of perfect rationality,
377 and hence also with a maximum of a priori
378 inference and a minimum of tiresome grub-
379 bing with empirical data. I have perhaps said
380 enough also with respect to the limitations of
381 these new constructs to indicate why I do not
382 believe that they solve the problems that
383 motivated their development.

384 IV. Advances in the Behavioral Theory

385 Although they have played a muted role in
386 the total economic research activity during
387 the past two decades, theories of bounded
388 rationality and the behavioral theory of the
389 business firm have undergone steady develop-
390 ment during that period. Since surveying the
391 whole body of work would be a major under-
392 taking, I shall have to be satisfied here with
393 suggesting the flavor of the whole by citing a
394 few samples of different kinds of important
395 research falling in this domain. Where
396 surveys on particular topics have been
397 published, I will limit myself to references to
398 them.

399 First, there has been work in the psycholog-
400 ical laboratory and the field to test whether
401 people in relatively simple choice situations
402 behave as statistical decision theory (maximi-
403 zation of expected utilities) say they do.
404 Second, there has been extensive psycholog-
405 ical research, in which Allen Newell and I
406 have been heavily involved, to discover the
407 actual microprocesses of human decision
408 making and problem solving. Third, there
409 have been numerous empirical observations—
410 most of them in the form of “case studies”—
411 of the actual processes of decision making in
412 organizational and business contexts. Fourth,
413 there have been reformulations and exten-
414 sions of the theory of the firm replacing
415 classical maximization with behavioral deci-
416 sion postulates.

417 A. Utility Theory and Human Choice *ccc*

418 The axiomatization of utility and probabil-
419 ity after World War II and the revival of
420 Bayesian statistics opened the way to testing
421 empirically whether people behaved in choice
422 situations so as to maximize subjective
423 expected utility (*SEU*). In early studies, using
424 extremely simple choice situations, it appeared
425 that perhaps they did. When even small
426 complications were introduced into the situa-
427 tions, wide departures of behavior from the *ccc*

432 predictions of *SEU* theory soon became
433 evident. Some of the most dramatic and
434 convincing empirical refutations of the theory
435 have been reported by D. Kahneman and A.
436 Tversky, who showed that under one set of
437 circumstances, decision makers gave far too
438 little weight to prior knowledge and based
439 their choices almost entirely on new evidence,
440 while in other circumstances new evidence
441 had little influence on opinions already
442 formed. Equally large and striking departures
443 from the behavior predicted by the *SEU*
444 knowledge were found by Howard Kun-
445 reuther and his colleagues in their studies of
446 individual decisions to purchase or not to
447 purchase flood insurance. On the basis of
448 these and other pieces of evidence, the conclu-
449 sion seems unavoidable that the *SEU* theory
450 does not provide a good prediction—not even
451 a good approximation—of actual behavior.
452 Notice that the refutation of the theory has
453 to do with the *substance* of the decisions, and
454 not just the process by which they are
455 reached. It is not that people do not go
456 through the calculations that would be
457 required to reach the *SEU* decision—neoclas-
458 sical thought has never claimed that they did.
459 What has been shown is that they do not even
460 behave *as if* they had carried out those
461 calculations, and that result is a direct refuta-
462 tion of the neoclassical assumptions.

463 B. Psychology of Problem Solving

464 The evidence on rational decision making is
465 largely negative evidence, evidence of what
466 people do *not* do. In the past twenty years a
467 large body of positive evidence has also accu-
468 mulated about the processes that people use
469 to make difficult decisions and solve complex
470 problems. The body of theory that has been
471 built up around this evidence is called infor-
472 mation processing psychology, and is usually
473 expressed formally in computer programming
474 languages. Newell and I have summed up our
475 own version of this theory in our book,
476 *Human Problem Solving*, which is part of a
477 large and rapidly growing literature that
478 assumes an information processing frame-
479 work and makes use of computer simulation
480 as a central tool for expressing and testing
481 theories.

482 Information processing theories envisage
483 problem solving as involving very selective
484 search through problem spaces that are often
485 immense. Selectivity, based on rules of thumb
486 or “heuristics,” tends to guide the search into
487 promising regions, so that solutions will
488 generally be found after search of only a tiny
489 part of the total space. Satisficing criteria
490 terminate search when satisfactory problem
491 solutions have been found. Thus, these theo-
492 ries of problem solving clearly fit within the
493 framework of bounded rationality that I have
494 been expounding here.

495 By now the empirical evidence for this

500 general picture of the problem solving process
501 is extensive. Most of the evidence pertains to
502 relatively simple, puzzle-like situations of the
503 sort that can be brought into the psychological
504 laboratory for controlled study, but a
505 great deal has been learned, also, about
506 professional level human tasks like making
507 medical diagnoses, investing in portfolios of
508 stocks and bonds, and playing chess. In tasks
509 of these kinds, the general search mechanisms
510 operate in a rich context of information stored
511 in human long-term memory, but the general
512 organization of the process is substantially the
513 same as for the simpler, more specific tasks.

514 At the present time, research in information-
515 processing psychology is proceeding in
516 several directions. Exploration of professional-
517 level skills continues. A good deal of effort is
518 now being devoted also to determining how
519 initial representations for new problems are
520 acquired. Even in simple problem domains,
521 the problem solver has much latitude in the
522 way he formulates the problem space in which
523 he will search, a finding that underlines again
524 how far the actual process is from a search for
525 a uniquely determined optimum (see J. R.
526 Hayes and Simon).

527 The main import for economic theory of
528 the research in information processing psychology
529 is to provide rather conclusive empirical
530 evidence that the decision-making process
531 in problem situations conforms closely to the
532 models of bounded rationality described
533 earlier. This finding implies, in turn, that
534 choice is not determined uniquely by the
535 objective characteristics of the problem situation,
536 but depends also on the particular
537 heuristic process that is used to reach the
538 decision. It would appear, therefore, that a
539 model of process is an essential component in
540 any positive theory of decision making that
541 purports to describe the real world, and that
542 the neoclassical ambition of avoiding the
543 necessity for such a model is unrealizable
544 (Simon, 1978a).

545 C. Organizational Decision Making

546 It would be desirable to have, in addition to
547 the evidence from the psychological research
548 just described, empirical studies of the
549 process of decision making in organizational
550 contexts. The studies of individual problem
551 solving and decision making do not touch on
552 the many social-psychological factors that
553 enter into the decision process in organizations.
554 A substantial number of investigations
555 have been carried out in the past twenty years
556 of the decision-making process in organizations,
557 but they are not easily summarized.
558 The difficulty is that most of these investigations
559 have taken the form of case studies of
560 specific decisions or particular classes of decisions
561 in individual organizations. To the best
562 of my knowledge, no good review of this
563 literature has been published, so that it is

3
568 difficult even to locate and identify the studies
569 that have been carried out.³ Nor have any
570 systematic methods been developed and
571 tested for distilling out from these individual
572 case studies their implications for the general
573 theory of the decision-making process.

574 The case studies of organizational decision
575 making, therefore, represent the natural
576 history stage of scientific inquiry. They
577 provide us with a multitude of facts about the
578 decision-making process—facts that are al-
579 most uniformly consistent with the kind of
580 behavioral model that has been proposed
581 here. But we do not yet know how to use these
582 facts to test the model in any formal way. Nor
583 do we quite know what to do with the obser-
584 vation that the specific decision-making
585 procedures used by organizations differ from
586 one organization to another, and within each
587 organization, even from one situation to
588 another. We must not expect from these data
589 generalizations as neat and precise as those
590 incorporated in neoclassical theory.

591 Perhaps the closest approach to a method
592 for extracting theoretically relevant informa-
593 tion from case studies is computer simulation.
594 By converting empirical evidence about a
595 decision-making process into a computer
596 program, a path is opened both for testing the
597 adequacy of the program mechanisms for
598 explaining the data, and for discovering the
599 key features of the program that account,
600 qualitatively, for the interesting and impor-
601 tant characteristics of its behavior. Examples
602 of the use of this technique are Clarkson's
603 (1963) simulation of the decision making of
604 an investment trust officer, Cyert, E. A.
605 Feigenbaum, and March's simulation of the
606 history of a duopoly, and C. P. Bonini's model
607 of the effects of accounting information and
608 supervisory pressures in altering employee
609 motivations in a business firm. The simulation
610 methodology is discussed from a variety of
611 viewpoints in Dutton and Starbuck.⁴

4
612 D. *Theories of the Business Firm*

613 The general features of bounded rationali- *tr*
614 ty—selective search, satisficing, and so on—
615 have been taken as the starting points for a
616 number of attempts to build theories of the
617 business firm incorporating behavioral as-
618 sumptions. Examples of such theories would
619 include the theory of Cyert and March,
620 already mentioned; William Baumol's theory
621 of sales maximization subject to minimum
622 profit constraints; Robin Marris's models of
623 firms whose goals are stated in terms of rates
624 of growth; Harvey Leibenstein's theory of
625 "X-inefficiency" that depresses production
626 below the theoretically attainable; Janos
627 Kornai's dichotomy between supply-driven
628 and demand-driven management; Oliver Wil-
629 liamson's theory of transactional costs; the
630 evolutionary models of Richard Nelson and
631 Sidney Winter (1973); Cyert and Morris

636 DeGroft's (1974) models incorporating
637 adaptive learning; Radner's (1975a,b) expli- *o*
638 cit satisficing models; and others.

639 Characterized in this way, there seems to
640 be little commonality among all of these
641 theories and models, except that they depart
642 in one way or another from the classical
643 assumption of perfect rationality in firm deci-
644 sion making. A closer look, however, and a
645 more abstract description of their assump-
646 tions, shows that they share several basic
647 characteristics. Most of them depart from the
648 assumption of profit maximization in the
649 short run, and replace it with an assumption
650 of goals defined in terms of targets—that is,
651 they are to greater or lesser degree satisficing
652 theories. If they do retain maximizing
653 assumptions, they contain some kind of mech-
654 anism that prevents the maximum from being
655 attained, at least in the short run. In the
656 Cyert-March theory, and that of Leibenstein,
657 this mechanism can be viewed as producing
658 "organizational slack," the magnitude of
659 which may itself be a function of motivational
660 and environmental variables.

661 Finally, a number of these theories assume
662 that organizational learning takes place, so
663 that if the environment were stationary for a
664 sufficient length of time, the system equilib-
665 rium would approach closer and closer to the
666 classical profit-maximizing equilibrium. Of
667 course they generally also assume that the
668 environmental disturbances will generally be
669 large enough to prevent the classical solution
670 from being an adequate approximation to the
671 actual behavior. *cc = 2/*

672 The presence of something like organiza-
673 tional slack in a model of the business firm
674 introduces complexity in the firm's behavior
675 in the short run. Since the firm may operate
676 very far from any optimum, the slack serves
677 as a buffer between the environment and the
678 firm's decisions. Responses to environmental
679 events can no longer be predicted simply by
680 analyzing the "requirements of the situation,"
681 but depend on the specific decision processes
682 that the firm employs. However well this
683 characteristic of a business firm model corre-
684 sponds to reality, it reduces the attractiveness
685 of the model for many economists, who are
686 reluctant to give up the process-independent
687 predictions of classical theory, and who do not
688 feel at home with the kind of empirical
689 investigation that is required for disclosing
690 actual real-world decision processes.

691 But there is another side to the matter. If,
692 in the face of identical environmental condi-
693 tions, different decision mechanisms can
694 produce different firm behaviors, this sensi-
695 tivity of outcomes to process can have impor-
696 tant consequences for analysis at the level of
697 markets and the economy. Political economy,
698 whether descriptive or normative, cannot
699 remain indifferent to this source of variability
700 in response. At the very least it demands
701 that—before we draw policy conclusions from

cc = 2/

See definition? (see no 37)

Cm

706 our theories, and particularly before we act on
707 those policy conclusions—we carry out sensi-
708 tivity analyses to test how far our conclusions
709 would be changed if we made different
710 assumptions about the decision mechanisms
711 at the micro-level.

712 If our conclusions are robust—if they are
713 not changed materially by substituting one or
714 another variant of the behavioral model for
715 the classical model—we will gain confidence
716 in our predictions and recommendations; if
717 the conclusions are sensitive to such substitu-
718 tions, we will use them warily until we can
719 determine which micro theory is the correct
720 one.

721 As reference to the literature cited earlier
722 in this section will verify, our predictions of
723 the operations of markets and of the economy
724 are sensitive to our assumptions about mech-
725 anisms at the level of decision processes.
726 Moreover, the assumptions of the behavioral
727 theories are almost certainly closer to reality
728 than those of the classical theory. These two
729 facts, in combination, constitute a direct refu-
730 tation of the argument that the unrealism of
731 the assumptions of the classical theory is
732 harmless. We cannot use the *in vacua* version
733 of the law of falling bodies to predict the
734 sinking of a heavy body in molasses. The
735 predictions of the classical and neoclassical
736 theories and the policy recommendations
737 derived from them must be treated with the
738 greatest caution.

tr

739 V. Conclusion

740 There is a saying in politics that “you can’t
741 beat something with nothing.” You can’t
742 defeat a measure or a candidate simply by
743 pointing to defects and inadequacies. You
744 must offer an alternative.

745 The same principle applies to scientific
746 theory. Once a theory is well entrenched, it
747 will survive many assaults of empirical
748 evidence that purports to refute it unless an
749 alternative theory, consistent with the evi-
750 dence, stands ready to replace it. Such conser-
751 vative protectiveness of established beliefs is,
752 indeed, not unreasonable. In the first place, in
753 empirical science we aspire only to approxi-
754 mate truths; we are under no illusion that we
755 can find a single formula, or even a moder-
756 ately complex one, that captures the whole
757 truth and nothing else. We are committed to a
758 strategy of successive approximations, and
759 when we find discrepancies between theory
760 and data, our first impulse is to patch rather
761 than to rebuild from the foundations.

762 In the second place, when discrepancies
763 appear, it is seldom immediately obvious
764 where the trouble lies. It may be located in
765 the fundamental assumptions of the theory,
766 but it may as well be merely a defect in the
767 auxiliary hypotheses and measurement postu-
768 lates we have had to assume in order to
769 connect theory with observations. Revisions in

774 these latter parts of the structure may be
775 sufficient to save the remainder.

776 What then is the present status of the
777 classical theory of the firm? There can no
778 longer be any doubt that the micro assump-
779 tions of the theory—the assumptions of
780 perfect rationality—are contrary to fact. It is
781 not a question of approximation; they do not
782 even remotely describe the processes that
783 human beings use for making decisions in
784 complex situations.

785 Moreover, there is an alternative. If
786 anything, there is an embarrassing richness of
787 alternatives. Today, we have a large mass of
788 descriptive data, from both laboratory and
789 field, that show how human problem solving
790 and decision making actually take place in a
791 wide variety of situations. A number of theo-
792 ries have been constructed to account for
793 these data, and while these theories certainly
794 do not yet constitute a single coherent whole,
795 there is much in common among them. In one
796 way or another, they incorporate the notions
797 of bounded rationality: the need to search for
798 decision alternatives, the replacement of
799 optimization by targets and satisficing goals,
800 and mechanisms of learning and adaptation.
801 If our interest lies in descriptive decision
802 theory (or even normative decision theory), it
803 is now entirely clear that the classical and
804 neoclassical theories have been replaced by a
805 superior alternative that provides us with a
806 much closer approximation to what is actually
807 going on.

808 But what if our interest lies primarily in
809 normative political economy rather than in
810 the more remote regions of the economic
811 sciences? Is there then any reason why we
812 should give up the familiar theories? Have the
813 newer concepts of decision making and the
814 firm shown their superiority “for purposes of
815 economic analysis”?

816 If the classical and neoclassical theories
817 were, as is sometimes argued, simply powerful
818 tools for deriving aggregative consequences
819 that held alike for both perfect and bounded
820 rationality, we would have every reason to
821 retain them for this purpose. But we have
822 seen, on the contrary, that neoclassical theory
823 does not always lead to the same conclusions
824 at the level of aggregate phenomena and
825 policy as are implied by the postulate of
826 bounded rationality, in any of its variants.
827 Hence, we cannot defend an uncritical use of
828 these contrary-to-fact assumptions by the
829 argument that their veridicality is unimpor-
830 tant. In many cases, in fact, this veridicality
831 may be crucial to reaching correct conclu-
832 sions about the central questions of political
833 economy. Only a comparison of predictions
834 can tell us whether a case before us is one of
835 these.

836 The social sciences have been accustomed
837 to look for models in the most spectacular
838 successes of the natural sciences. There is no
839 harm in that, provided that it is not done in a

844 spirit of slavish imitation. In economics, it has
845 been common enough to admire Newtonian
846 mechanics (or, as we have seen, the Law of
847 Falling Bodies), and to search for the
848 economic equivalent of the laws of motion.
849 But this is not the only model for a science,
850 and it seems, indeed, not to be the right one
851 for our purposes.

852 Human behavior, even rational human
853 behavior, is not to be accounted for by a
854 handful of invariants. It is certainly not to be
855 accounted for by assuming perfect adaptation
856 to the environment. Its basic mechanisms
857 may be relatively simple, and I believe they
858 are, but that simplicity operates in interaction
859 with extremely complex boundary conditions
860 imposed by the environment and by the very
861 facts of human long-term memory and of the
862 capacity of human beings, individually and
863 collectively, to learn.

864 If we wish to be guided by a natural science
865 metaphor, I suggest one drawn from biology
866 rather than physics (see Newell and Simon,
867 1976). Obvious lessons are to be learned from
868 evolutionary biology, and rather less obvious
869 ones from molecular biology. From molecular
870 biology, in particular, we can glimpse a
871 picture of how a few basic mechanisms—the
872 DNA of the Double Helix, for example, or the
873 energy transfer mechanisms elucidated so
874 elegantly by Peter Mitchell—can account for
875 a wide range of complex phenomena. We can
876 see the role in science of laws of qualitative
877 structure, and the power of qualitative, as
878 well as quantitative explanation.

879 I am always reluctant to end a talk about
880 the sciences of man in the future tense. It
881 conveys too much the impression that these
882 are potential sciences which may some day be
883 actualized, but that do not really exist at the
884 present time. Of course that is not the case at
885 all. However, much our knowledge of human
886 behavior falls short of our need for such
887 knowledge, still it is enormous. Sometimes we
888 tend to discount it, because so many of the
889 phenomena are accessible to us in the very
890 activity of living as human beings among
891 human beings that it seems commonplace to
892 us. Moreover, it does not always answer the
893 questions for which we need answers. We
894 cannot predict very well the course of the
895 business cycle nor manage the employment
896 rate. (We cannot, it might be added, predict
897 very well the time of the next thunderstorm in
898 Stockholm, or manage the earth's climates.)

899 With all these qualifications and reserva-
900 tions, we do understand today many of the
901 mechanisms of human rational choice. We do
902 know how the information processing system
903 called Man, faced with complexity beyond his
904 ken, uses his information processing capaci-
905 ties to seek out alternatives, to calculate
906 consequences, to resolve uncertainties, and
907 thereby—sometimes, not always—to find
908 ways of action that are sufficient unto the
909 day, that satisfy.

3
n

Cr

911 Economic Review—0333-002-9 TERY 04-30-79 & CIND 5-2-79
912 EREV002A01.2S- GALLEY 29

914 REFERENCES - 22

- 915 A. A. Alchian, "Uncertainty, Evolution, and
916 Economic Theory," *J. Polit. Econ.*, June
917 1950, 58:211-21.
918 A. Ando, "On a Theoretical and Empirical
919 Basis of Macroeconometric Models," paper
920 presented to the NSF-NBER Conference
921 on Macroeconomic Modeling, Ann Arbor,
922 Oct. 1978.
923 Chester I. Barnard, *The Functions of the Exec-*
924 *utive*, Cambridge, Mass. 1938.
925 William Baumol, *Business Behavior, Value and*
926 *Growth*, New York 1959.
927 G. S. Becker, "Irrational Behavior and
928 Economic Theory," *J. Polit. Econ.*, Feb.
929 1962, 70, 1-13.
930 Charles P. Bonini, *Simulation of Information*
931 *and Decision Systems in the Firm*, Engle-
932 wood Cliffs 1963.
933 Alfred Chandler, *Strategy and Structure*, Cam-
934 bridge, Mass. 1962.
935 N. C. Churchill, W. W. Cooper, and T. Sainsbury,
936 "Laboratory and Field Studies of the
937 Behavioral Effects of Audits," In C. P.
938 Bonini et al., eds., *Management Controls*,
939 New York 1964.
940 John R. Commons, *Institutional Economics*,
941 Madison 1934.
942 R. M. Cyert, E. A. Feigenbaum, and J. G. March,
943 "Models in a Behavioral Theory of the
944 Firm," *Behav. Sci.*, 1959, 4, 81-95.
945 ——— and M. H. DeGroot, "Rational Expecta-
946 tions and Bayesian Analysis," *J. Polit.*
947 *Econ.*, May/June 1974, 82, 521-36.
948 ——— and ——— "Adaptive Utility," *Adapt-*
949 *ive Economic Models*, New York 1975,
950 233-46.
951 ——— and (John) G. March, *A Behavioral*
952 *Theory of the Firm*, Englewood Cliffs
953 1963.
954 ——— and H. A. Simon, "Theory of the Firm:
955 Behavioralism and Marginalism," unpub-
956 lished work. paper, Carnegie-Mellon Univ.
957 1971.
958 ———, ———, and D. B. Trow, "Observation
959 of a Business Decision," *J. Bus. Univ.*
960 *Chicago*, Oct. 1956, 29, 237-48.
961 D. C. Dearborn and H. A. Simon, "Selective
962 Perception: The Identifications of Execu-
963 tives," *Sociometry*, 1958 21, 140-144;
964 reprinted in *Administrative Behavior*, ch.
965 15, 3d ed., New York 1976.
966 J. M. Dutton, and W. H. Starbuck, *Computer*
967 *Simulation of Human Behavior*, New
968 York 1971.
969 G. Eliasson, *Business Economic Planning*,
970 New York 1976.
971 ——— *A Micro-to-Micro Model of the*
972 *Swedish Economy*, Stockholm 1978.
973 B. M. Friedman, "Optimal Expectations and
974 the Extreme Information Assumptions of
975 'Rational Expectations' Macromodels," *J.*
976 *Monet. Econ.*, 1979, 3, 1-11.
977 ———, "A Discussion of the Methodological
978 Premises of Professors Lucas and Sargent,"

22

lf.

lc { CLARKSON, G. P. E., "A model of the Trust
Investment Process," in E. A. Feigenbaum &
J. Feldman, eds., *Computers and Thought*,
New York 1963.

22/ April/
Henry/ o

22 in R. H. DAY & T. GROVES, eds.,

James/

a

JAN. / 5, 23-41.

Start
Ch. 10
Ch. 11
Ch. 12

Ch. 10
Ch. 11
Ch. 12

Ch. 10
Ch. 11
Ch. 12

Cm

- 983 in *After the Phillips Curve: The Persistence of High Inflation and High Unemployment*, Boston 1978.
- 984
- 985
- 986 Milton Friedman, *Essays in Positive Economics*. Chicago 1953.
- 987
- 988 J. R. Hayes and H. A. Simon, "Understanding Written Problem Instructions," in W. Gregg, ed., *Knowledge and Cognition*, Potomac 1974, 167-200.
- 989
- 990
- 991 A. O. Hirschman, *Exit, Voice and Loyalty*, Cambridge, Mass. 1970.
- 992
- 993 Charles C. Holt, Franco Modigliani, John F. Muth, and Herbert A. Simon, *Planning Production, Inventories and Work Force*, Englewood Cliffs 1960.
- 994
- 995 Y. Ijiri and H. A. Simon, *Skew Distributions and the Sizes of Business Firms*, Amsterdam 1977.
- 996
- 997
- 998
- 999
- :00
- B003 E. Jonsen, *Studies in Multiobjective Decision Models*, Lund 1968.
- 001
- 002 D. W. Jorgenson and C. D. Siebert, "A Comparison of Alternative Theories of Corporate Investment Behavior," *Amer. Econ. Rev.* Sept. 1968, 58, 681-712.
- 003
- 004
- 005
- 006 D. Kahneman and A. Tversky, "On the Psychology of Prediction," *Psychol. Rev.*, 1973, 80, 237-51.
- 007
- 008
- 009 Janos Kornai, *Anti-Equilibrium*, Amsterdam 1971.
- 010
- 011 Howard Kunreuther, et al., *Disaster Insurance Protection: Public Policy Lessons*, New York 1978.
- 012
- 013
- 014 Harvey Leibenstein, *Beyond Economic Man*, Cambridge, Mass. 1976.
- 015
- 016 J. Lesourne, *A Theory of the Individual for Economic Analysis*, Vol. 1, Amsterdam 1977.
- 017
- 018
- 019 R. E. Lucas, Jr., "An Equilibrium Model of the Business Cycle," *J. Polit. Econ.*, Dec. 1975, 83, 1113-44.
- 020
- 021
- 022 ———, "On the Size Distribution of Business Firms," *Bell J. Econ.*, Autumn 1978, 9, 508-23.
- 023
- 024
- 025 James G. March, *Handbook of Organizations*, Chicago 1965.
- 026
- 027 ——— and H. A. Simon, *Organizations*, New York 1958.
- 028
- 029 Robin Marris, *The Economic Theory of "Managerial" Capitalism*, London 1964.
- 030
- 031 Jacob Marschak, "Role of Liquidity under Complete and Incomplete Information," *Amer. Econ. Rev. Proc.*, May 1949, 39, 182-95.
- 032
- 033
- 034
- 035 ——— and Roy Radner, *Economic Theory of Teams*, New Haven 1972.
- 036
- 037 Alfred Marshal, *Principles of Economics*, 8th ed. New York 1920.
- 038
- 039 E. S. Mason, "Comment," in Bernard T. Haley, ed., *A Survey of Contemporary Economics*, Vol. II, Homewood 1952, 221-22.
- 040
- 041
- 042
- 043 J. M. Montias, *The Structure of Economic Systems*, New Haven 1976.
- 044
- 045 J. F. Muth, "Rational Expectations and the Theory of Price Movements," *Econometrica*, July 1961, 29, 15-53.
- 046
- 047

nom

ch

July/

cc

cc/

30

- 052 ———, "Optimal Properties of Exponentially Weighted Forecasts," *J. Amer. Statist. Assoc.*, 1960, 55, 299-306.
- 054 R. R. Nelson, and S. Winter, "Toward an Evolutionary Theory of Economic Capabilities," *Amer. Econ. Rev. Proc.*, May 1973, 63, 440-49.
- 055 ——— and ———, "Neoclassical vs. Evolutionary Theories of Economic Growth," *Econ. J.*, Dec. 1974, 84, 886-905.
- 056 Allen Newell and Herbert A. Simon, *Human Problem Solving*, Englewood Cliffs, 1972.
- 057 ——— and ———, "Computer Science as Empirical Inquiry: Symbols and Search," *Communications of the ACM*, 1976, 19:113-26.
- 058 G. Orcutt, and R. Caldwell-Wertheimer II, *Policy Exploration through Microanalytic Simulation*, Washington 1976.
- 059 A. Papandreou, "Some Basic Problems in the Theory of the Firm," in Bernard F. Haley, ed., *A Survey of Contemporary Economics*, Vol. II, Homewood 1952.
- 060 E. H. Phelps-Brown, "The meaning of the Fitted Cobb-Douglas Function," *Quart. J. Econ.*, Nov. 1957, 71, 546-60.
- 061 R. Radner, "A Behavioral Model of Cost Reduction," *Bell J. Econ.*, Spring 1975, 6, 196-215.
- 062 ———, (1975b) "Satisficing," *J. Math. Econ.*, 1975, 2, 253-62.
- 063 David R. Roberts, *Executive Compensation*, Glencoe 1959.
- 064 P. A. Samuelson, "Discussion: Problems of Methodology," *Amer. Econ. Rev. Proc.*, May 1963, 53, 231-36.
- 065 Henry Schultz, *The Theory and Measurement of Demand*, Chicago 1938.
- 066 Herbert A. Simon, *Administrative Behavior*, New York 1947; 3d ed. 1976.
- 067 ———, "A Formal Theory of the Employment Relation," *Econometrica*, July 1951, 19, 293-305.
- 068 ———, "A Comparison of Organization Theories," *Rev. Econ. Stud.* No. 1, 1952, 20, 40-48.
- 069 ———, "Theories of Decision Making in Economics and Behavioral Science," *Amer. Econ. Rev.*, June 1959, 49, 223-83.
- 070 ———, "A Behavioral Model of Rational Choice," *Quart. J. Econ.*, Feb. 1955, 69, 99-118.
- 071 ———, "Rational choice and the structure of the environment," *Psychol. Rev.*, 1956, 63.
- 072 ———, "Dynamic Programming under Uncertainty with a Quadratic Criterion Function," *Econometrica*, Jan. 1956, 24, 74-81.
- 073 ———, *Models of Man*, New York 1957.
- 074 ———, "The Compensation of Executives," *Sociometry*, 1957, 20, 32-35.
- 075 ———, "Discussion: Problems of Methodology," *Amer. Econ. Rev. Proc.*, May 1963, 53, 229-31.
- 076 ———, "From Substantive to Procedural Rationality," in Spiro J. Latsis, ed., *Method-*

rom. June/

march/

(1975a)
22

June-Sept./

)lf.

transpare order/

B
=

4-31

March/

tr

22/

Civ. Mon. 1977?

Civ. Mon. 1977?

Civ. Mon. 1977?

Crr!

119 Economic Review—0333-002-9 TERY 04-30-79 & CIND 5-2-79
 120 EREV002A01.2S- GALLEY 32

- 122 *ological Appraisal in Economics*, Cambridge 1976.
 123
 124 ———, (1978a) "Rationality as Process and
 125 as Product of Thought," *Amer. Econ. Rev.*
 126 *Proc.*, May 1978, 68, 1-16.
 127 ———, (1978b) "On How to Decide What to
 128 Decide," *Bell J. Econ.*, Autumn 1978, 9,
 129 494-507.
 130 ———, G. Kozmetsky, H. Guetzkow, and G.
 131 Tyndall, *Centralization vs. Decentralization*
 132 *in Organizing the Controller's Department*,
 133 New York 1954; reprinted Houston (1978)
 134 Texas.
 135 ——— and F. K. Levy, "A Note on the Pobb-
 136 Douglas Function," *Rev. Econ. Stud.*, June
 137 1963, 30, 93-94.
 138 G. J. Stigler, "The Economics of Information,"
 139 *J. Polit. Econ.*, June 1961, 69, 213-15.
 140 H. Theil, "A Note on Certainty Equivalence in
 141 Dynamic Planning," *Econometrica*, Apr.
 142 1957, 25, 346-49.
 143 John Von Neuman and Oscar Morgenstern,
 144 *Theory of Games and Economic Behavior*,
 145 Princeton 1944.
 146 A. A. Walters, "Production and Cost Func-
 147 tions: An Econometric Survey," *Economet-*
 148 *rica*, Jan.-Apr. 1963, 31, 1-66.
 149 Oliver Williamson, *Markets and Hierarchies*,
 150 *Analysis and Antitrust Implications*, New
 151 York 1975.
 152 S. Winter, "Satisficing, Selection, and the
 153 Innovating Remnant," *Quart. J. Econ.*
 154 May 1971, 85, 237-61.

Do/

tr/C 22/

66/

tr
~~some~~ stat/

157 *Carnegie-Mellon University. This article is the
 158 lecture Herbert Simon delivered in Stockholm, Sweden,
 159 December 8, 1978, when he received the Nobel Prize in
 160 Economic Science. The article is copyright © the Nobel
 161 Foundation 1978. It is published here with the permission
 162 of the Nobel Foundation.

163 The author is indebted to Albert Ando, Otto A. Davis,
 164 and Benjamin M. Friedman for valuable comments on an
 165 earlier draft of this paper.

166 The phrase "that works" refutes, out of hand, Fried-
 167 man's celebrated paean of praise for lack of realism in
 168 assumptions. Consider his example of falling bodies (pp.
 169 16-19). His valid point is that it is advantageous to use
 170 the simple law, ignoring air resistance, when it gives a
 171 "good enough" approximation. But of course the condi-
 172 tions under which it gives a good approximation are not
 173 at all the conditions under which it is unrealistic or a
 174 "wildly inaccurate descriptive representation of reality."
 175 We can use it to predict the path of a body falling in a
 176 vacuum, but not the path of one falling through the
 177 Earth's atmosphere. I cannot in this brief space
 178 mention, much less discuss, all of the numerous logical
 179 fallacies that can be found in Friedman's 40-page essay.
 180 For additional criticism, see Simon (1962) and Samu-
 181 elson (1962).

182 In a footnote, Becker indicates that he denotes as
 183 irrational "[A]ny deviation from utility maximization."
 184 Thus, what I have called "bounded rationality" is irra-
 185 tionality" in Becker's terminology.

186 For leads into the literature, see March and Simon;
 187 March; Jensen; J. M. Dutton and W. H. Starbuck.
 188 However, there are large numbers of specific case studies,
 189 some of them carried out as thesis projects, some
 190 concerned with particular fields of business application,
 191 which have never been recorded in these reference
 192 sources (for example, Eliasson, 1976).

193 In addition to simulations of the firm, there are very
 194 interesting and potentially important efforts to use simu-

u
c o c

tr

22/

2/

3/

c h =

95 SCIENCE PRESS *cm*

196 Economic Review—0333-002-9 TERY 04-30-79 & CIND 5-2-79
197 EREV002A01.2S- GALLEY **33**

199 lation to build bridges directly from decision theory to
200 political economy. See G. Orcutt and R. Caldwell-
201 Wertheimer, and Eliasson (1978).

203 0 88351 3948 EREV002A01.2S-