Rationality, institutions, and explanation

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In Chapter 1, I tried to introduce the reader to some of the themes he or she would encounter in this volume. These included: (1) the critique of maximizing rationality; (2) an emphasis on processes and sequences of events; and (3) a concern with the nature and role of social institutions. In this final chapter, I want to explore these themes more deeply. What I say here will, of course, have a good deal of bearing on a program for a New Institutional Economics. Indeed, I will even offer explicitly my own proposal for a research program. But, ultimately, this chapter sets for itself an earlier and more basic task – to sort out some of the methodological issues that surround these three themes.

10.1 The critique of rationality

The trail begins with the crucial but somewhat slippery notion of rationality in economics. One of the best-known and most influential modern discussions of economic rationality is that by Herbert Simon, who put forth the notion of bounded rationality (1955, 1957, 1959) and more recently argued for a distinction between substantive rationality and procedural rationality (1976, 1978a, 1978b). Simon's analysis involves the recognition that individuals often face very complicated decision-problems that they cannot be expected to solve instantly and optimally; such individuals are thus afflicted with bounded rationality, and they must "*satisfice* because they have not the wits to maximize" (Simon 1957, p. xxviii, emphasis in the original). As a consequence, Simon recommends a reduced emphasis on the optimality of particular courses of action (substantive rationality) and greater emphasis on the effectiveness of the procedures used in the choosing (procedural rationality). Simon's critique of maximizing rationality is well taken. None-

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theless, I will argue that his idea of procedural rationality, if taken literally, is misplaced. The problem with maximizing rationality (if I may put the matter somewhat mysteriously) is not that it is substantive, but precisely the opposite – that it is *too procedural*.

However much economists may wish to distance their ideas of economic rationality from the philosophical doctrines of rationalism, the two must ultimately remain related. Maximizing rationality is closely connected with the rationalist tradition, usually traced from Plato and Descartes, that sees reason as conscious, logical deduction from explicit premises. The criterion for economic rationality is thus the logical consistency of the agent's actions with his or her (explicit) knowledge and preferences. And since, even under conditions of subjective uncertainty, that knowledge and those preferences logically imply a best course of action, the agent is rational only when he or she selects that particular best course.

By this criterion, an economic agent is rational when successful in maximizing some explicit objective (such as utility or profit) within the constraints of well-defined alternatives.[1] Simon's critique of substantive rationality rests on what we might call the argument from complexity. In the real world, this argument goes, problems of economic decision making are frequently extremely complicated: The agent's difficulty in processing masses of information and computing the optimal solution, coupled with natural biases and assorted human frailties, will inevitably prevent him or her from taking the correct rational action.

This understanding of bounded rationality has a good deal of intuitive appeal. But it is important to recognize how little one has to stray from maximizing rationality in order to accept the argument from complexity. The argument in most forms implicitly accepts the Cartesian definition of reason, finding a need for procedural rationality only in the relative difficulty of carrying out the required logical deduction. And this is ultimately quite significant: for if all that's at stake is some constraint on information-processing and computational capacity, then one's satisficing alternative quickly collapses into substantive rationality – satisficing is actually the optimal course of action in view of costly computational resources (Baumol and Quandt 1964). Simon's own conception of bounded rationality is closely tied up with his fascina-

[1] In modern economics, this is normally construed as a subjective criterion – the agent is rational when he or she maximizes according to subjectively defined preferences and perceived alternatives. In practice, however, most models assume that the agent is also objectively rational, in the sense that perceived alternatives are in fact the "true" alternatives or (what amounts to the same thing) that all agents perceive the alternatives – and sometimes even the probability distributions over relevant states of the world – identically.

tion with the computer. He often writes as if there really does exist a well-defined optimization problem out there, and the solution to that problem is ultimately the benchmark of rationality; the only difficulty is computational complexity. This is why analogies to chess games or the solution of complex differential equations (e.g., Simon and Stedry 1968) appear so frequently in his discussions. In the end, Simon's theory of knowledge is the Cartesian one, a fact brought out most clearly in his work on so-called artificial intelligence (Dreyfus 1979).

There are, however, a couple of other bounded-rationality arguments in which forms of satisficing behavior and procedural rationality emerge that are *not* logically reducible to substantive rationality. The first, and less interesting, is what Jon Elster (1983, p. 74) calls the special argument for satisficing: We can construct problem situations in which there is simply no substantively rational solution. (For an example, see Frydman, O'Driscoll, and Schotter 1982.) In such cases, the agent is necessarily satisficing, since he or she simply can't be rational according to substantive criteria. The more interesting line of reasoning is what Elster (1983, p. 75) calls the general argument for satisficing.[2] This approach is indeed general in that it effectively extends to *all* choice situations the dilemma of the rational problem without solution. If rationality consists only in the optimal adjustment of means to ends, then rationality must presuppose some framework of means and ends within which the optimization is to take place. But where do these frameworks come from? As a logical matter, they cannot themselves be explained as the result of maximizing choice. For if the choice of frameworks *were* the result of maximizing within some higher framework, the choice of that higher framework would remain unexplained – and so on ad infinitum (Winter 1964, pp. 262–4; Kirzner 1982, pp. 143–5). If we insist that the economic agent is rational only insofar as he or she makes consistent logical choices, then we must consign an important aspect of his or her behavior – the perception of new alternatives and possibilities – to the realm of the nonrational.[3]

[2] By "satisficing," I should note, I simply mean any otherwise reasonable behavior that can't be characterized as substantively rational. This is to be distinguished from the narrower sense of satisficing as thermostat-like behavior.

[3] Here we need to make an important distinction between what is nonrational (in that it is not based on fully specified and complete evidence, and thus cannot be placed in the form of a deduction from explicit premises) and what is irrational (in that it is contrary to logical argument). One necessarily misses this distinction if one clings to the neoclassical definition of rationality, since one assumes, in effect, that evidence is always complete. The distinction becomes important when we look at its flip side: It can be not irrational (i.e., not contrary to logical argument) to act on the basis of incomplete evidence – and, therefore, the agent can be rational (in a nonneoclassical sense) even in an open-ended world. (I am indebted to Brian Loasby for suggesting this distinction to me.)

As Littlechild (in this volume) makes clear, this argument does not have to do with the uncertainty faced by an economic agent – at least not with uncertainty as it is construed in conventional modeling. Consider Kenneth Arrow's definition of uncertainty, which I am perhaps unduly fond of quoting. "Uncertainty," he says, "means that we do not have a complete description of the world which we fully believe to be true. Instead, we consider the world to be in one or another of a range of states. Each state of the world is a description which is complete for all relevant purposes. Our uncertainty consists in not knowing which state is the true one" (Arrow 1974, p. 33). The Winter-Kirzner argument is effectively a denial that the economic agent does or could know a collectively exhaustive set of states that are "complete for all relevant purposes."[4] If the argument has to do with uncertainty, it is to the extent that it suggests a broader conception of uncertainty: One can be uncertain not merely about which pregiven state will obtain, but also about which states are possible. This is a view that G. L. S. Shackle has long advocated, and his writings have been perhaps as seminal as those of Simon in the contemporary discussion of economic rationality. Many others – often influenced by Shackle – have also argued for a broader view of uncertainty. Some have talked about "radical" uncertainty or simply ignorance (Loasby 1976); others have suggested the terms "genuine" uncertainty (O'Driscoll and Rizzo 1985) and "extended" uncertainty (Bookstaber and Langsam 1983). My own taxonomic preference is "structural" (as distinguished from "parametric") uncertainty, since that way of putting things highlights the qualitative nature of the distinction between this wider kind of uncertainty and the uncertainty of Arrow's definition[5] (Langlois 1984). This wider conception of uncertainty has lately been creeping into mainstream discussions through a

[4] If the accent is on the word "all," then the Simonian argument from complexity is perhaps more appropriate. But if the accent is on the word "relevant," then the Winter-Kirzner argument clearly applies – the agent cannot decide on (neoclassically) rational grounds which states are relevant. The same infinite-regress is at work (see Langlois 1984, n. 14).

[5] The long-familiar way of discussing uncertainty, of course, is to invoke Frank Knight's distinction between risk and uncertainty (Knight [1921] 1971). This has led to much confusion, though, since Knight's uncertainty is subject to several different interpretations. His conception of risk is clear enough: It is what we would now call insurable risk. But his category of uncertainty blurs the distinction between situations of structural uncertainty and situations in which there are well-defined states of the world but simply no objective probabilities to assign those states. Since the so-called Bayesian revolution, we have learned that the absence of objective probabilities is no bar to conceiving of uncertainty along the lines of Arrow's definition. Whether Knight was a radical subjectivist or just an incipient Bayesian is a doctrinal issue into which I have no desire to enter. I would prefer to avoid confusion by banishing the term "Knightian uncertainty" from the discussion.

somewhat different rhetorical channel: the assertion that the agent may be laboring under an optimization problem that is "misspecified" (Cohen and Axelrod 1984).

If one accepts this critique of maximizing rationality in any of its forms, one is left with two choices: either to consign some behavior to the nonrational (and thus to place its consequences beyond the range of economic explanation) or to find a conception of rationality different from that implied by the maximizing criterion.

At the highest level, the latter alternative entails questioning the very idea that reason must involve, and therefore that economic rationality must be defined as, conscious, logical deduction from explicit premises. The implications of this alternative have been most clearly stated by F. A. Hayek (1967, 1973), who identifies a long and well-developed philosophical tradition in which "reason had meant the capacity to recognize truth, especially moral truth, when [one meets] it, rather than a capacity of deductive reasoning from explicit premisses"[6] (Hayek 1967, p. 84). Lawrence Boland makes a somewhat similar distinction. He objects to the maximizing conception of rationality not merely because it limits reason to the process of logical deduction from explicit premises but because of the very fact that it conceives of rationality as a psychological process at all.[7]

> The view that rationality is a psychological process is a relic of the late eighteenth century. Even today it is still commonplace to distinguish humans from other animals on the basis that humans can be rational. Thus any criticism of a psychologistic view of rationality might be considered dangerous. Nevertheless, the psychologistic view is based on a simple mistake. It confuses one's *argument* in favor of an individual's decision with the *process* of making the decision. It also confuses being rational with being reasonable – the latter only implies the willingness to provide reasons for one's actions. The reasons may not always be adequate.
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> The case against psychologistic rationality is rather straightforward. Simply stated, humans cannot be rational – only arguments can be rational. An argument is rational only if it is not logically inconsistent. . . . But, most important, whether an argument is rational can be decided independently of the process of its creation or the psychological state of its creator. (Boland 1982, p. 38)

[6] Hayek (1973, pp. 5, 29) distinguishes the two views of reason as "Cartesian constructivist" versus "evolutionary" rationalism or, in Karl Popper's phrase, as "naive" versus "critical" rationalism. In Hayek's view, it was evolutionary rationalism that characterized the thought of eighteenth-century Scottish moral philosophers such as Adam Smith and David Hume.

[7] Compare Elster's (1983, p. 70) distinction between acting *with* a reason and acting *for* a reason: "Acting with a reason means that the actor has reasons for doing what he does, acting for a reason implies in addition that he did what he did because of those reasons." As Roger Koppl pointed out to me, this is similar to the distinction Alfred Schutz made between "in-order-to motives" and "because motives."

Thus, Boland's alternative is neither substantive *nor* procedural rationality – but rather a situational or institutional conception. (More on this shortly.)

To Hayek, this criterion of rationality consists in nothing so much as the ability to learn from experience. Although this is certainly one aspect of it, I don't think learning fully circumscribes the alternate conception of rationality I'm after. More broadly, I would argue, the criterion of ratonality is the ability to act reasonably, to act appropriately to one's circumstances, to adapt. We will need a name for this, so I will refer to behavior that meets the alternate criterion of rationality as "adaptive" behavior.

10.2 Arbitrariness, problem-situations, and exits

The general argument for satisficing (which ought more properly be called the general argument against maximizing rationality) rests on the arbitrariness of maximization as a criterion of the rational. The power of this criterion is that it supplies a unique outcome as rational: There is only one rational exit. But this apparent uniqueness is in fact illusory. It is bought at the expense of an arbitrary specification of the means-ends framework in which the optimization is to take place. What we are left with is a kind of conservation-of-arbitrariness doctrine. If we want non-arbitrary (rational) behavior, we must specify (arbitrarily) the agent's means-ends framework. If we eliminate this arbitrariness by leaving the framework unspecified – so that the agent is free to choose his or her own framework – then we must impose some (arbitrary) behavioral assumption on the agent in order to arrive at a determinate (unique) result. We can't eliminate arbitrariness without also eliminating determinateness.

The practice of restricting the agent's means-ends framework in order to produce a determinate outcome is what Spiro Latsis (1972, 1976a, 1976b) refers to as single-exit modeling, an approach he finds at the base of the neoclassical research program. As first introduced to economics by Ludwig von Mises, single-exit modeling was an attempt to reconcile a desire for determinate models with a belief in the free will of the economic agent. In a single-exit model, the agent's behavior is not formally preprogrammed. Yet the model has determinate results, because we place the agent in a situation with only one reasonable exit. The agent is free to do as he or she likes; but, by analyzing the logic of the situation, we can determine the unique course of action a reasonable person would take. This is to a large extent an antipsychological method. It doesn't require that we delve too deeply into the motivations of the agent. The constraints imposed by the agent's situation reduce his or her options sufficiently, that a light postulate of reasonable conduct is adequate to secure a determinate outcome.

It was Karl Popper (1957, 1966, 1967) who articulated this idea that theory in the social sciences consists in analyzing the "logic of the situation," although the basic technique goes back at least to Max Weber. What is interesting is that Boland, also citing Popper, offers this situational-logic method as his *alternative* to the "psychologistic" rationalism of neoclassical economics. We thus have situational determinism (a phrase Latsis uses synonymously with single-exit modeling) held up both as the method of neoclassical economics and as the alternative to the method of neoclassical economics. How can this be?

As is normally the case, the paradox turns on the meaning of words. In particular, Latsis and Boland seem to mean different things when they describe a method as antipsychological. Moreover, they have somewhat different notions of what analyzing the logic of the situation entails. Influenced by Simon, Latsis views situational determinism as antipsychological not because it eliminates the psychological conception of rationality, but because it eliminates the psychological *details* of the individual's motivation. Thus, for Latsis, situational determinism uses the problem-situation as a way to avoid considering the agent's internal psychological landscape. "For instance," he says, "in certain situations the *suspension of a vehicle* and *a parachutist's legs* both behave as *shock absorbers*. Yet the internal environments that generate this activity are of very diverse structure and complexity" (Latsis 1976b, p. 18, emphasis in the original). This is nothing other than the ideal-typical method as developed out of the Weberian tradition by Alfred Schutz and Fritz Machlup (Langlois and Koppl 1984). A quick translation of Latsis's physical analogy illustrates the point: We can, in some situations, represent the concrete types "vehicle suspension" and "parachutist's legs" as the ideal type "shock absorber." But this is situational determinism only in a specific and limited sense. The constraints of the situation give one license to replace a diverse and complex set of concrete types with a single ideal type: Because of the nature of the situation, the psychological details just don't matter.

From Boland's point of view, this is psychologism and not analyzing the "logic of the situation" at all. The principle of maximizing behavior programmed into the neoclassical ideal type is not innocent of the psychological conception of rationality merely because the psychology it embodies is an abstract and simplified one. The problem with maximizing rationality is not that it is substantive rationality, but that it is in fact *too procedural*. From Latsis's point of view, by contrast, the act of collapsing the complex psychological process of a concrete agent into the abstract rationality of the ideal type is in fact a transformation to situational logic. The actual decision processes of the agent are replaced by an optimization problem defined solely by the agent's goals and the situation's constraints. Thus, the neoclassical program is psychologism for Boland because it attempts to explain economic phenomena in terms of the psychological states of the agent; and it is antipsychological for Latsis because it makes no reference to the inner environment of the agent.

This is, of course, a little too pat. In fact, the neoclassical program is not as psychologistic as Boland thinks nor as antipsychological as Latsis believes. Psychologism for Boland (1982, p. 30, following Popper) "is the methodological prescription that psychological states are the *only* exogenous variables permitted beyond natural givens (e.g., weather, contents of the Universe, etc.)" (emphasis in the original). Now, it is arguably correct that psychologism of this sort is a long-term goal of the neoclassical program. This is what I described in Chapter 1 as the attempt to construct an institution-free theory. And general-equilibrium theory in the manner of Arrow, Debreu, and Hahn (Debreu, 1959; Arrow and Hahn, 1971) may indeed have accomplished this goal. But as far as the day-to-day positive heuristic of the neoclassical program is concerned, it is clearly wrong to say that psychological states and natural givens are the only exogenous variables: Latsis is quite right that neoclassical modeling takes problem-situations – which are not usually natural givens – as exogenous. This is clearly true of the Marshallian marginalism that Machlup defended in the so-called marginalist controversy (Langlois and Koppl 1984). It is even true of the Stigler-Becker models (Stigler and Becker 1977) that Boland describes as "simple" psychologism. In these models all agents have identical preferences but face different constraints. The constraints are *not* natural givens, however: they are unreduced economic artifacts like budgets and human-capital endowments. Indeed, we can identify a contrapuntal theme within the same neoclassical circles – the attempt to show that economic phenomena can be explained without *any* reference to preferences (Alchian 1950; Becker 1962). I will have more to say about this latter program in a moment.

On the other hand, Latsis is wrong to think that the neoclassical program is antipsychological in any strong sense. If we are considering textbook Marshallian economics, it may be easy to persuade ourselves that not much psychology is at work, that we are studying the reasonable response of agents to a problem at the margin. But if we are looking at models that represent the agent as solving an optimal-control problem with a foot-long objective function, are we really seeing an appeal to the logic of the situation? We might just as well view this kind of maximizing behavior as a form of behavioralism – with a perhaps implausible psychology behind it.

In the end, the neoclassical program is a convolution of psychologism and situational determinism. In most of its manifestations, it takes both psychological states and problem-situations as exogenous. Of course, neither Boland nor Latsis is a fan of this program. Boland wants to eliminate the psychologism; Latsis wants to eliminate the situational determinism in favor of *more* psychology. What is ironic is that the programmatic results of these divergent prescriptions turn out ultimately to be rather similar.

The reason for the similarity has to do with the ambiguity of what it means to add "psychology" to economic theory. Boland is disdainful of psychology. What he wants to add to economics is a more sophisticated philosophy of knowledge. Knowing, he argues, ought to be seen not as a psychological process but as a logical and scientific process. Thus, we should represent actors not as possessing psychologies but as possessing views or philosophies of knowledge; moreover, we should not limit these views of knowledge to inductive philosophies, according to which we build up knowledge by accumulating data, but should consider the Popperian theory of knowledge, according to which we can learn from counterinstances that refute our conjectures[8] (Boland 1982, pp. 178–82). The distinction might be encapsulated as learning-from-success versus learning-from-failure.

I think we should follow Boland in this rejection of psychologism. At the same time, however, we should recognize that the distinction be-

[8] Ironically – and contrary to what Boland seems to think – Popper himself repudiated his own larger notion of critical rationalism in discussing the rationality principle in economics. "It is necessary," he writes, "to distinguish between rationality as a personal attitude (of which, normally, all individuals with a healthy mind are capable) and the rationality principle. Rationality as a personal attitude consists in the disposition to correct our ideas. Intellectually, in its most developed form, it is a disposition to examine our ideas in a critical spirit and to revise them in the light of critical discussion with others. The 'rationality principle,' on the other hand, has nothing to do with the hypothesis that men are rational in this sense and that they always adopt a rational attitude" (Popper 1967, p. 149, translation mine). This may seem odd, but it connects with what Wade Hands (1984) describes as the division between Popper-sub-n and Popper-sub-s – that is, between Popper's views on the methodology of the natural sciences and his views on the methodology of the social sciences. In discussing the social sciences, Hands argues, Popper abandoned much of his own falsificationist position and came to argue for the rationality principle as a kind of irrefutable "hard core." Whether we should follow Popper in this is another matter. As I've already suggested, Popper's friend Hayek is very willing to associate the conception of rationality in economics with the ability to learn from experience. Hayek, it seems, is more Popperian than Popper (see Caldwell 1984).

tween psychology and the philosophy of knowledge is not at all a sharp one. Boland's recommendation is essentially the man-as-scientist approach that Loasby (in this volume) discusses so well. What is interesting is that Loasby finds this view in the work of a *psychologist*, George Kelly. This begins to suggest, I think, why those who have tried to take up the Simon-Latsis program, who have tried to incorporate more of the agent's internal environment into their models, have often ended up doing precisely what Boland recommends: studying the views of knowledge agents possess and specifying the theories they take with them into their problem-situations (see, for example, Nelson and Winter 1982, chaps. 4, 5). Discussing an agent's psychology and discussing his or her theory of knowledge may be less distinct activities in practice than in principle. In order for the knowledge possessed by the agent to have a role in an economic theory, that knowledge must be *causally adequate.* Boland is right that the agent's knowledge need not be adequate in the sense that it reflects a true and complete theory of his or her situation; but it must be causally adequate in the sense that the knowledge the agent possesses – whatever it may be – provides us with an explanation of his or her behavior (so that we can say it was the "cause" of that behavior). And "explaining behavior" is very often what one means by psychology. Again, this needn't commit us to a simple neoclassical psychology in which preferences are given and unchanging. It may be that Boland would object to some of the theories of decision making that come out of Simonian psychologizing.[9] But that doesn't mean he and the Simonians aren't engaged in activities that are similar for purposes of economic modeling.

From one point of view, this might seem like an untenable claim. It often appears as though there are two conflicting themes in Simon's treatment. On the one hand, his famous notion of satisficing reflects the opinion – consistent with Boland's – that there is in effect too much psychology in economics. "A comparative examination of the models of adaptive behavior employed in psychology (e.g., learning theories), and

[9] His main complaint, of course, would be that most theories attributed to the agents are inductivist in his sense: They are theories in which agents acquire knowledge by gathering data. But, even though one may want to reject inductivism as an approach to the philosophy of science, one may still wish to represent an *economic agent* as possessing an inductivist theory. Indeed, there are grounds to argue that inductive learning is precisely the theory held by most people who aren't trained in philosophy (as well as a good many who are). Nor, I think, would Boland deny this. Nonetheless, the idea of modeling economic agents as Popperian falsificationists is an approach worth trying. The picture of man-as-scientist that Loasby paints in his chapter has elements of both inductivism and falsificationism. And, as I will suggest in the next section, one can have a model in which the economic system as a whole is falsificationist – i.e., rejects false conjectures – even when the agents within the system all hold inductive learning theories.

of the models of rational behavior employed in economics, shows," he writes, "that in almost all respects the latter postulate a much greater complexity in the choice mechanisms, and a much larger capacity in the organism for obtaining information and performing computations, than do the former" (Simon [1956, p. 129] 1982, p. 259). In this sense, Simon is calling for economics to *simplify* its treatment of the agent's inner environment. But we can also find Simon seemingly arguing the opposite case – that the psychology of neoclassical models is not complex enough. Consider his famous "molasses" example, which Latsis cites approvingly. Suppose we were pouring molasses into a bowl of irregular shape. How much, he asks, would we have to know about the properties of molasses in order to predict its behavior?

> If the bowl were held motionless, and if we wanted only to predict behavior in equilibrium, we would have to know little, indeed, about molasses. The single essential assumption would be that molasses, under the force of gravity, would minimize the height of its center of gravity. With this assumption, which would apply as well to any other liquid, and a complete knowledge of the environment – in this case, the shape of the bowl – the equilibrium is completely determined. Just so, the equilibrium behavior of a perfectly adapting organism depends only on its goal and its environment; it is otherwise completely independent of the internal properties of the organism. (Simon [1959, p. 255] 1982, p. 289)

By contrast, says Simon, we would need more detailed information about the properties of molasses in order to predict its behavior in disequilibrium. "Likewise, to predict the short-run behavior of an adaptive organism, or its behavior in a complex or rapidly changing environment, it is not enough to know its goals. We must know also a great deal about its internal structure and particularly its methods of adaptation" (Simon [1959, p. 255] 1982, p. 289). Clearly, the need to know the agent's methods of adaptation is fully consistent with the Popperian program. But what about the need to know "a great deal about its internal structure"? Again, a certain amount of "procedural rationality" is consistent with the Popper-Boland program: Appropriate behavior may require different decision rules in different situations, and analyzing such behavior may require attention to "the effectiveness, in light of human cognitive powers and limitations, of the procedures used to choose actions" (Simon [1978a, p. 9] 1982, p. 452, emphasis deleted).

10.3 Rationality and system constraints

Having minimized the differences between the Popper-Boland view and the Simon-Latsis view, however, let me now suggest that there is at least one distinction with a difference. Simon's belief that we need to know "a great deal" about the inner workings of the agent is based on a misconception about the nature of social science explanation.

There are several important issues here, and they need to be carefully sorted. The first thing to notice is a certain ambiguity in Simon about the explanandum of economic theory (see Machlup [1967, p. 9] 1978, p. 399). Is economic theory designed, as Simon clearly seems to suggest in the passages above, to predict (or explain) the *behavior of the agent?* Causal adequacy requires that the agent's knowledge explain his or her behavior. But explaining that behavior is not the goal of theory. Rather, as Hayek (1979, pp. 146–7) argues, the social sciences "are concerned with man's actions, *and their aim is to explain the unintended or undesigned results of the actions of many men*" (emphasis added). Similarly, Popper (1965, p. 342) holds that the main task of the theoretical social sciences "*is to trace the unintended social repercussions of intentional human actions*" (emphasis in the original). If this and not individual behavior is the explanandum of theory, then it need not follow that we have to know more about the psychology of the agent – even under disequilibrium conditions – than is implied in the notion of adapting to the logic of one's situation.

Another way to approach the issue is to ask what Simon means by "the environment." It seems that, in both the equilibrium and disequilibrium cases, Simon takes the environment to mean exactly what we have called the agent's situation. Situational determinism, he seems to be saying, is possible in the equilibrium case but not in the disequilibrium one. But if we adopt the view of theory advocated by Hayek and Popper, then the relevant environment must somehow extend beyond the agent's situation to encompass the effects of his or her interactions with other agents. At first glance, this observation would tend to cement Simon's case: the more complicated the environment, the more we need to know about the agent's internal workings. To the contrary, I would argue, a knowledge of the agent's environment serves in some cases – including many of those most relevant to economic theory – as a *substitute* for a knowledge of the details of his or her internal psychology.

First of all, the very idea of taking the agent's situation as exogenous is a way of limiting the demands on his cognitive powers. In general-equilibrium theory, which seeks to reduce economic phenomena to psychological states (utility functions) and natural givens[10] (endowments and

[10] These are "natural" givens, of course, only in the sense that the problem-situation they embody encompasses the entire economic universe. This is also presumably what Boland means, though he never puts it this way.

technological possibilities), the cognitive demands on the agent are phenomenal. If instead we permit the agent to respond not to the entire economic universe (or an unreasonably large part of it), but to a manageable subset, then the demands are much attenuated, and what we need to know about his internal landscape is reduced. This may seem an arbitrary procedure: Does the agent not in fact face a problem-situation ultimately identical to the entire economic universe? But this procedure is *not* entirely arbitrary. This is so not merely because, as a subjective matter, agents do not typically conceive of their problem-situations as taking in the whole economic universe. It is so, more interestingly, because the method of situational analysis permits one to take as exogenous the existence of various *social institutions*. As I suggested in Chapter 1, institutions have an informational-support function. They are, in effect, interpersonal stores of coordinative knowledge; as such, they serve to restrict at once the dimensions of the agent's problem-situation and the extent of the cognitive demands placed upon the agent. This is why Joseph Agassi refers to the method of situational analysis as "institutional individualism." The problem-situations that we take as exogenous are not fully arbitrary – they have, as it were, an objective correlative in various "distinct social yet not psychological entities (called institutions, customs, traditions, societies, etc.)" (Agassi 1974, p. 145). These entities – which I will continue to refer to as institutions – are ultimately the result of individual action, but they cannot be reduced to psychological states.

There is also a somewhat different sense in which a knowledge of the agent's environment can act as a substitute for a knowledge of the details of his psychology. If we take seriously the Popper-Hayek program – that the explanandum of theory is the unintended social consequences of individual action – then we can appeal not only to the environment of the agent's problem-situation, but also to the larger environment that his or her actions help form. We can appeal, in effect, to the system constraint (Langlois and Koppl 1984).

As I've already hinted, this is not a new theme in economics. The idea that constraints can substitute – indeed, substitute perfectly – for rational behavior has been put forward by some of the very writers Boland finds most guilty of psychologism. The seminal paper is by Alchian (1950), which I will turn to shortly; but the critical locus of this argument for present purposes is an exchange between Gary Becker (1962, 1963) and Israel Kirzner (1962, 1963) that occurred more than two decades ago in the *Journal of Political Economy*. This exchange has, I feel, been both neglected and misunderstood.

Becker (1962, p. 1) set out to show that "the important theorems of modern economics" can be derived exclusively from considering the "opportunity set" faced by a population of agents – in a manner independent of what one assumes about the behavior of those agents. In particular, he wanted to show that the so-called law of demand – the notion that demand curves slope downward – can be derived even if behavior is capricious or habit-bound rather than rational in the manner of conventional models. Consider, he says, a world of two goods in which agents choose capriciously in the sense that they select their consumption bundles at random. Suppose now that the relative price of the two goods changes, incomes remaining constant. This price-shift changes the opportunity set of the population in that some previously feasible randomly chosen bundles are now ruled out by the constraint of a finite income. Those choosing the infeasible bundles will not be able to pay for them, and will go home disappointed. This means that aggregate consumption of the good whose price rose will necessarily decline. Voilà the law of demand. (The same story applies for habit-bound behavior.)[11]

Kirzner's article set out to show that, despite this incontrovertible argument, Becker still had not demonstrated that all forms of rationality could be eliminated in deriving the important theorems of economics. Becker's story depends crucially on the assumption that price is set on the supply side of the market and that the consumers are just price-takers. The reason that one side of the market can be irrational, Kirzner argues, is that, in effect, the other side of the market is doing all the rational work in adjusting prices to market conditions: "The crux of the matter is that for the market process to work, even within the market for a single commodity, *it cannot be assumed that all market participants are price-takers*" (Kirzner 1962, p. 382, emphasis in the original). For prices to move from one equilibrium level to another requires that

[11] Random behavior arguably violates the Popperian criterion of situational appropriateness: A choice of that sort is inappropriate to the situation in the sense that it makes no reference at all to the situation. Incorrigibly habit-bound behavior would probably also violate the criterion, since it is implicit in the situation of this model that a reasonable person would know of the price change and would also realize that he could make himself better off by changing the composition of his bundle. (After the price change, some consumers will find they don't have enough income to continue buying their habitual bundles; but others will find that they have income left over, which is strictly a waste in a two-good world for anyone who prefers more to less.) In a wider context, of course, habitual behavior can certainly be appropriate to the agent's situation. Let me also note in passing that Becker's attempt to derive the law of demand without assuming rationality is really an aggregate version of Paul Samuelson's (1938) frankly behaviorist attempt to construct a demand theory at the individual level that makes no reference to subjective or non-observable theoretical terms (Samuelson 1938). On the failure of revealed-preference theory in this regard, see the excellent discussions by Majumdar (1958) and Wong (1978).

someone somewhere recognize the change in economic conditions and actively adapt to the new conditions.[12]

Becker's reply to this challenge is somewhat confused and ultimately unsatisfactory. He tries to tell a story in which a population of irrational agents (in this case firms) can in fact change the market price. Suppose, he says, that market price is too high in the sense that some sellers could not sell all they wanted at that price. This, he says, would be reflected in the "production opportunity set" that constrains the price and output levels of the firms. This opportunity set "shifts to the left," causing a reduction in the average price offered regardless of the rationality of the firms. As Kirzner quickly pointed out in a rejoinder, this is nonsense. In the short run, the firms can set any prices and quantities they want. And, if the firms are irrational, there is no reason why average price should move toward the equilibrium level. In the short run, then, rationality of some sort is indispensable if markets are to have a tendency toward equilibrium.

In the somewhat longer run, of course, firms cannot go on setting any prices and quantities they like. If there are underlying changes in demand or the scarcity of resources, some previously profitable price and output combinations can no longer be sustained. Some firms – especially those charging higher-than-average prices – may well go bankrupt; and their departure will lower the average price in the direction of equilibrium. But not only do arguments of this sort apply only in the longer run, they are in fact quite complicated and subtle.[13]

What should we learn from this exchange? On the one hand, the assumption of rationality – of some kind – is essential in the short run and difficult (or maybe impossible) to eliminate even in the long run. On the other hand, the system constraint does ultimately remove much of the burden that rationality is often thought – by friend and foe alike – to carry in theory. Rationality, in the limited sense of the method of

[12] The best way to understand Kirzner's argument is in the light of his own work (especially Kirzner 1973), which can be understood in part as an attempt to solve precisely this problem: How can we explain the disequilibrium process through which markets move from one equilibrium to another? Although he was certainly defending the necessity of a postulate of rationality, he was not defending the conception of maximizing rationality.

[13] For example, if the behavior of the firms is truly random, there will be no systematic tendency for the pricier firms to go bankrupt first. Moreover, even if the firms are habit-followers – an assumption much more consistent with selection stories – the argument still requires a careful specification of the dynamic selection process supposed to be operating (Winter 1964, p. 240 and passim). Notice, furthermore, that talking about bankruptcy does not by itself eliminate all conscious adjustment even in the long run – it just pushes that rationality back one stage into the process of bankruptcy, which arguably consists in creditors, stockholders, etc., reacting consciously to the actions of the firm.

situational analysis, is necessary for a coherent story; but it is also sufficient for deriving the important theorems of economics.

The role of rationality in basic economic theory (with an emphasis on the theory of the firm) received an extensive airing in an earlier and probably more famous exchange: the marginalist controversy between Richard Lester (1946, 1947) and Fritz Machlup (1946, 1947). It was largely in reaction to this controversy that Armen Alchian produced his 1950 article "Uncertainty, Evolution, and Economic Theory." "It's very embarrassing," he said in a recent published discussion (Zerbe 1982, p. 149); "you write an article in response to two misplaced articles, one by a fellow named Lester and one by a guy named Machlup. Lester was arguing that businessmen do not think in terms of Marshall's cost calculations and [marginalist theory] therefore cannot be right; and Machlup says oh, yes they do and therefore it is right. Both of them irrelevant positions and so you simply apply the well-known evolutionary theory, put it on paper, and it becomes a classic." The article is indeed a classic, and the issues it raises are crucial to understanding the relationship of rationality and system constraints in theory.

As his remarks about the Lester-Machlup debate suggest, Alchian set out in large part to show – as did Becker – that the conclusions of traditional marginalist theory do not depend on the performance of Marshallian cost calculations or other assumptions of rationality. But, in applying evolutionary theory (or, more correctly, a selection argument) to economic explanation, Alchian ultimately does something more interesting. (Sometimes even theorizing about the unintended consequences of human action can have its unintended consequences.)

Before considering selection arguments in detail, though, it might be useful to say a word about the Lester-Machlup debate. For it is a small irony that Machlup's argument was in fact very much in the spirit of Alchian's argument, albeit at a slightly different level of discourse. As a strong proponent of the method of ideal types, Machlup certainly did affirm the necessity of the rationality postulate – in essentially the situational-analysis form I have cast it here. But he was also concerned with the relationship between this rationality postulate and the larger system constraint. Since the basic results of price theory are derived from considering the behavior of large numbers of agents, he argued, the appropriate ideal type is a very general and anonymous one. It is the system constraint, in effect, that allows us to use an ideal type that is only "boundedly rational." The tighter the constraint, the less we have to worry about the informational demands placed on the agent and about the internal details of his psychology. Thus, like Alchian, Machlup is saying that the supposed failure of the businessman to perform explicit Marshallian cost calculations does not invalidate the results of the theory because (in some sense) the system as a whole obviates such conscious rationality.[14]

The real issue then is whether the relevant system constraint is tight or loose.[15] When the constraints are loose, which often means that social outcomes depend crucially on the behavior of one or a few pivotal individuals (the Schumpeterian entrepreneur, perhaps, or the chairman of the Federal Reserve Board), then we need to know a lot more about the agent's situation and how he perceives it. But very often the interesting explananda involve large-numbers situations with more or less tight constraints. And in those situations we can use a more simplified ideal type. But this does not automatically mean we are limited to Marshallian theories in which we draw conclusions about aggregate outcomes simply by scaling up the behavior of any single representative individual.[16] *This,* I think, is the real message of Alchian's article.[17] It is not so much that we can or should eliminate marginalism in the sense of eliminating any particular motivational assumption: Acting "on the margin," after all, often means nothing more than acting in a boundedly rational way – acting appropriately to the situation one faces on the margin rather than reacting to the total picture. The aspect of marginalism that Alchian calls most seriously into question is its compositional structure – its assumption that aggregate outcomes are just individual outcomes writ large.

10.4 Invisible-hand explanations

The idea that we should pay attention to compositional principles is implicit in the statement of the Popper-Hayek program: We want to explain the unintended consequences of individual action. This is not to say that Marshallian theory does not in fact have this as its goal or that it

[14] Of course, Machlup does at times appear to argue that Lester's businessmen really did perform Marshallian cost calculations. But this is a reflection of the multileveled attack Machlup pursued against Lester. His fundamental methodological position is a form of conventionalism coupled with the ideal-typical method of Weber and Schutz. See Langlois and Koppl (1984).

[15] As we will see, Simon was not far off the mark in distinguishing between situations of equilibrium and situations of disequilibrium – but only because disequilibrium conditions may imply looser system constraints. This is the case, for example, in Becker's failed argument about the opportunity sets of firms in his reply to Kirzner.

[16] More correctly, the representative firm in Marshall is gotten by scaling down aggregate behavior. In any event, I call the resulting compositional principle Marshallian only because the idea of the representative firm comes from Marshall. Perhaps this terminology is unfair to Marshall, for he was always aware of the dangers of using the representative compositional principle for analytic tasks to which it is ill-adapted (see Loasby 1976, chap. 11).

[17] It is this aspect that Nelson and Winter (1982) have seized upon and built into a full-fledged theory.

partakes of a fallacy-of-composition error. Marshallian explanations, based on constructs like the representative firm, do in fact yield the relationships of price and quantity as the unintended consequences of individual action. No competitor consciously intends the particular price or aggregate quantity that obtains in equilibrium; and certainly none intends the condition of zero profit that obtains there. The point is that the Marshallian compositional principle does not exhaust the compositional principles we might reasonably use in explaining aggregate outcomes as the result of individual action.

The best way to understand the issues here is in terms of what the philosophers have taken to calling invisible-hand explanations. The name comes from Adam Smith ([1776, IV. ii. 9] 1976, p. 456), of course, and his description of how the businessman is "led by an *invisible hand* to promote an end which is no part of his intention" (emphasis in the original). It was popularized by Robert Nozick in his discussion of the hypothetical emergence of a minimal state (Nozick 1974, pp. 18–22). And the idea has been developed by Edna Ullmann-Margalit (1978) and by Elster (1979, 1983) in treatments I will draw on here.

At base, to provide an invisible-hand explanation is to do nothing other than to follow the dictates of the Popper-Hayek program:[18] to explain organized social phenomena as the unintended result of individual action. Such explanations are to be distinguished primarily from intentional explanations, which attribute social phenomena to the conscious design of an individual or group.

Ullmann-Margalit distinguishes two "molds" of invisible-hand explanations: aggregate-mold explanations and functional-evolutionary explanations. The first mold speaks primarily to the causal-genetic process by which individual action brings about the aggregate pattern to be explained. The latter addresses the somewhat more complex issue of the pattern's maintenance once established, along with the question of how mechanisms that maintain the pattern relate to those that brought it about in the first place.[19]

These two molds overlap almost completely when, as in the Becker-Kirzner exchange, the phenomena to be explained are the basic con-

[18] I have so far used this term in a way narrower than Boland (1982, p. 178), though I will eventually want to endorse something very like his larger meaning.

[19] Elster (1979, p. 30) appears to make this distinction in a somewhat different way. Unfortunately, his terminology is idiosyncratic and rather confusing, in that he refers to causal-genetic explanations that do not speak to the issue of maintenance as invisible-hand explanations. I prefer to follow Ullmann-Margalit (and Nozick) in using this term as the catch-all category. Any explanation that casts its explananda as the undesigned results of individual action is, for my purposes, an invisible-hand explanation. This would include explanations in which some (or even all) the agents know, suspect, or guess at the overall outcome their decentralized actions would lead to, so long as the pursuit of that outcome is not the principal motivation for their actions.

cerns of price theory. The focus here is not on the formation and maintenance of organized social structures but on the response of observed prices and quantities to changes in such things as tastes, resource availability, expectations, government policies, and so on. As a consequence, the accent is on the causal-genetic aspects of the process and not on functions or mechanisms of maintenance.

As I've already suggested, Becker was not the first to argue that these questions might be tackled without assuming maximizing behavior by a representative agent. Alchian's 1950 article is an attempt to show that there is "an alternative method which treats the decisions and criteria dictated by the economic *system* as more important than those made by the individuals in it" (Alchian [1950] 1977, p. 19, emphasis in the original). He begins by arguing, very much in the spirit of our earlier discussion, that, in a world of uncertainty, profit maximization is not a well-defined notion and thus not a guide to action.[20] Profit, he says, is not something that one maximizes ex ante. Positive – not maximum – profit is something that is awarded ex post by the economic system; it is often as much a result of good luck as of good planning. What's more, he says, it's possible to get results in the absence of foresight by the agents that look very much as if such foresight had been present:

> Assume that thousands of travelers set out from Chicago, selecting their roads completely at random and without foresight. Only our "economist" knows that on but one road are there any gasoline stations. He can state categorically that travelers will *continue* to travel only on that road; those on other roads will soon run out of gas. Even though each one selected his route at random, we might have called those travelers who were so fortunate as to have picked the right road wise, efficient, foresighted, etc. . . . If gasoline supplies were now moved to a new road, some formerly luckless travelers again would be able to move; and a new pattern of travel would be observed, although none of the travelers had changed his particular path. The really possible paths have changed with the changing environment. (Alchian [1950] 1977, p. 22, emphasis in the original.)

This is clearly an invisible-hand explanation.[21] Translated into the eco-

[20] Unfortunately, his argument here is not a very good one and is based on what most present-day students of decision making under uncertainty would regard as a misunderstanding. But substituting the special or general arguments against maximizing rationality (discussed earlier) saves his conclusions entirely.

[21] Alchian (1977, p. 22) asserts immediately that his approach "embodies the principles of biological evolution and natural selection." Despite the resonance of this association and its continued presence in the literature, I will try to refrain from dragging in biological evolution unless absolutely necessary. My reason for this reluctance is less the confusion and misunderstandings that invariably attend the comparison with biology than it is a desire to stress the notion of an invisible-hand explanation. Such explanations are not limited to mechanisms fully (or even partly) indebted to the biological notion of evolution, but rather comprise a wide range of selection and filtering principles.

how strong the claim is. Friedman is not merely asserting that, for purposes of basic price and allocation theory, the conclusions one arrives at with selection arguments are often the same as those reached using marginalist reasoning.[24] Rather, he is arguing that the two approaches are fully isomorphic in their conclusions – marginalism is in effect a sufficient statistic for the selection approach under all circumstances. (And, therefore, parsimony dictates that marginalism be the approach of choice, with selection relegated to the role of backup defense and heuristic device.)

The key to understanding what's going on in this strong form of the identity argument – and why it's wrong – is to notice that Friedman has effected a subtle shift in the explanandum of the selection explanation.

I have been careful so far to limit my discussion of invisible-hand explanations to cases in which the phenomena to be explained are concerns of basic price theory such as changes in prices and quantities. But there is another class of explananda in the social sciences with which we might be concerned (and with which the explananda of price theory are often confused). That is, we could see our invisible-hand explanation as seeking to explain *behavior patterns* and the organized social structures – institutions, in the broadest sense – that they form. This is an altogether different matter; and it moves us into a world in which the two molds of invisible-hand explanations do not overlap completely and in which we have to concern ourselves with the issues of functionalism.

We can see this clearly in Elster (1983, p. 57; see also 1979, p. 31), who suggests that the only successful functional model in the social sciences[25] is "the attempt by the Chicago school of economists to explain profit-maximizing behavior as the result of the 'natural selection' of firms by the market." It's not explicit whom he has in mind here, but he presumably means the discussions by Alchian or Friedman that we've just examined. But is it obvious that *profit-maximizing behavior* is the phenomenon to be explained? Or is it – once again – the basic phenomena and concerns of price theory that are the explananda? If the latter, it is confusing for Elster to cast these questions in the functionalist mode at all. The sort of Marshallian partial-equilibrium comparative-static questions that (under this view) Friedman and Alchian are interested in are questions of change from one stable position (equilibrium) to another. There is thus no question of a *change* being maintained by a feedback mechanism of some kind. What are maintained, of course, are the equilibrium positions before and after the change. There is certainly a

[24] A conclusion, by the way, that I think is quite supportable as a first approximation and that, in my reading, is borne out in the models of Nelson and Winter (1982, p. 175).

[25] By which he presumably means that it meets all the criteria for a cogent functional explanation, not necessarily that it is unassailable.

question of what maintains these equilibria, but the mechanism involved enters only indirectly into the explanation of the changes in prices and quantities. Indeed, as Machlup (1963) points out, the equilibria are only conceptual devices used to make sure that all *cetera* are kept *paria* during the transition.[26] (Of course, whether economists really treat equilibrium this way is another matter.)

But the point is that, in arguing so strongly for the isomorphism between selection and marginalism, Friedman is necessarily moving into the realm in which functionalist considerations are important. That is, he is implicitly doing just what Elster suggests – using selection as a way of explaining the pattern called profit-maximizing behavior. He then associates this behavior pattern with the profit-maximizing behavior that is an intermediate term in the marginalist explanation of price and quantity changes. The selection explanation, he says in effect, thereby adds credence to our use of profit maximization as a basis for marginalist theory and reinforces withal a commitment to marginalism.

The problem with this argument is that the selection model does not in fact provide an assurance that the profit-maximizing behavior pattern will always maintain itself in the economy or, relatedly, that the behavior that does result from the selection process can be meaningfully identified with the profit-maximizing behavior in marginalist theory. The counterarguments are mostly due to Winter (1964), although some are already adumbrated in Alchian. Even if we can assert that we can define some behavior as profit maximizing in a way that's meaningfully related to the concept of the same name in marginalist theory, we are by no means assured that such behavior will always result from the selection process. A change in the environment implies a disequilibrium situation; and, in disequilibrium, we cannot speak unambiguously about a firm's relative deviation from profit maximization, since to do so "presumes a particular state of the environment, but the environment is changed by the dynamic process itself" (Winter 1964, p. 240). To put it another way, what is profit maximizing in disequilibrium may not be profit maximizing in equilibrium. This raises the issue of definition: What is profit maximizing? If we assert that we mean profit maximization to be whatever behavior is best adapted in equilibrium, this immediately raises a second issue: If those firms who would be profit maximizing in the eventual equilibrium are badly adapted to disequilibrium, they may be selected out before the equilibrium is reached, leaving in equilibrium only firms that by definition *aren't* profit maximizers.[27]

[26] For a discussion see Langlois and Koppl (1984).

[27] These two arguments are what I have elsewhere christened the disequilibrium problem and the path-dependency problem (Langlois 1984). At a broader level, of course, one might well question on methodological grounds whether it is meaningful in any

If the two approaches – selection and maximizing marginalism – are not thus isomorphic in the manner Friedman suggests, the two must go their own way as separate programs. Selection arguments are not justifications of marginalism but hints at an alternative. It is this view that has animated the work of Nelson and Winter (1982), who cite Alchian's article as inspiration. Their approach clearly reflects invisible-hand explanation, and a careful analysis of it would illustrate many of the issues with which I'm concerned. Nonetheless, I will pass up the temptation to treat this work in detail. Instead, I will look to a different (though related) area: the explanation of social institutions.

10.5 Explaining institutions

In Chapter 1, I described the dual role that social institutions play in economic theory. In their first role, institutions form part of the situation in which the actor finds himself or herself; they serve as behavioral guides that reduce the knowledge and cognitive skills necessary for successful action. In this sense, then, institutions enter theory as exogenous residues of the past.[28] But these institutions are themselves the result of past human actions, of previous situations in which actors found themselves. Such institutions are thus fair game for theoretical explanation; and this – the Menger program – is the second role of social institutions: to play the explananda of invisible-hand explanations.

In attempting explanation of this sort, however, we must confront a number of issues that we had previously been able to skirt. Institutions, as we have seen, are orderly and more or less persistent behavior patterns. At a more abstract level, they are the rules or sets of rules that constrain or govern organized patterns of behavior. In either case, institutions are structures. And explaining them requires attention both to their origins and to their maintenance.[29]

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sense to identify the theoretical term "survivorship" from the selection approach with the term "profit-maximizing behavior" from the marginalist theory; but this I won't pursue here. I should note that the attempt to identify survival with maximization also raises a question of tautology, which I will address below.

[28] Whether models that incorporate institutions in this way exhibit "hysteresis" in the sense of Georgescu-Roegen (1971, p. 123ff.) is an issue into which I don't want to enter. My suspicion, for what it's worth, is that the answer would be "no," since situational analysis may be understood as a way of "substitut[ing] for past causes the traces left in the present by the operation of those causes" (Elster 1983, p. 33; cf. Elster 1976).

[29] To put it another way, we have to switch from what Roger Koppl (in an unpublished manuscript) calls level-one invisible-hand explanations to what he calls level-two explanations.

As we have seen, explanations that focus on the origin of an institution are what Ullmann-Margalit calls aggregate-mold explanations. These are causal-genetic stories about how individual actions unintentionally led (or might have led) to the development of some social or economic structure. The paradigm of this has long been Carl Menger's theory of the origin of money (Menger 1963, pp. 152ff.; 1981, p. 257), in which the self-interested actions of traders lead from a barter economy to one in which a single commodity has become the universal medium of exchange. The other approach, which Ullmann-Margalit calls the functional-evolutionary mold, focuses not on the process through which the structure emerged but on the processes that maintain the structure once established.

It is important to distinguish this functional mold from the more naive doctrines called functionalism, which assert, more or less baldly, that we can explain a structure simply by finding out what function it serves. Consider Elster's schema for a valid invisible-hand explanation in the functional mold.[30] An institution or behavior pattern X, he says (1979, p. 28; 1983, p. 57), is explained by its function Y for group Z if and only if

1. Y is an effect of X;
2. Y is beneficial for Z;
3. Y is unintended by the actors producing X;
4. Y (or at least the causal relationship between X and Y) is unrecognized[31] by the actors in Z; and
5. Y maintains X by a causal feedback loop passing through Z.

As both Elster and Ullmann-Margalit point out, the problem with much that passes under the name functionalism is that it operates as if criterion 5 – the existence of a feedback mechanism – is either unnecessary to successful explanation or can be inferred from some combination of criteria 1–4.

This is certainly correct. But what exactly have we explained by

[30] Again, this is not his terminology.

[31] A word about criterion 4 is in order. Elster holds it to be necessary to a successful functionalist explanation. An explanation that meets criteria 1–3 and 5 but not 4 is what he calls a filter explanation. These are explanations in which (A) the agents eventually become aware of the nature of the institution that is evolving and of the function it serves for them, and (B) this awareness then leads the agents consciously to maintain the institution. In other words, the institution is created organically but maintained pragmatically (to use Menger's terms). Although a filter explanation of this sort may not be a successful functionalist explanation in Elster's sense, it should still certainly count as an invisible-hand explanation in the wide sense in which I'm using that term.

showing that some institution has fulfilled all five criteria? Ullmann-Margalit (1978, p. 284) puts the matter well: "The basic question of the functionalist-evolutionary mold is this: given that a certain social pattern or institution exists, *why is it in existence?*" (emphasis in the original). Thus the functionalist mode differs from the aggregate mold in that the latter provides "a chronicle of (a particular mode) of emergence," whereas the former is concerned with "establishing *raisons d'être*" (emphasis altered).

The concern with functions is, of course, a legacy of the biological analogy that inspired much of functionalism. The biologist can be more or less safely concerned with function alone, since he or she can rely on a generally agreed-upon process – natural selection – that both provides the feedback mechanism of criterion 5 and deflects attention away from causes: for in evolutionary biology, the source of a structure is random variation or mutation; and what is random is inexplicable. This attitude carries over to functionalist explanations in the social sciences.

It should be noted and emphasized that an explanation of this type involves no commitment as to how the scrutinized pattern actually originated. For all that it tells us the pattern in question could have come into being as a result of intentional design and careful execution,[32] or, for that matter, it may have originated (somehow) through people's "stumbling upon establishments, which are indeed the result of human action, but not the execution of any human design." (Ullmann-Margalit 1978, p. 284. The internal quotation is from Adam Ferguson 1767.)

This also helps us to see, I think, the connection between functionalism and the optimization-and-equilibrium approach in economics. To say that a structure has a function is to say that it solves some kind of problem for the group in question – a problem usually cast in terms of the selection mechanism thought to be operating. The structure is functional because it solves a problem linked to the group's relative success or survival; the structure is efficient in some sense. There are a number of intricate problems here that I won't delve into. My point is merely that this functionalist problem is easily recast in the form of an operations-research problem.[33] That is to say, we can easily transform

[32] That the structure originated through intentional design is, of course, ruled out in Elster's formulation of a functionalist explanation – but not in most formulations.

[33] The alert reader will notice a connection here to my earlier discussion of situational analysis. This replacement of the functional problem by a mathematical optimization problem sounds a bit like situational analysis. But there is an important difference. The idea of situational analysis in the manner of Popper is to analyze the actual situation faced by an individual agent as that agent perceives the situation. This may or may not imply optimization – merely rationality in a broad sense. The substitution of an optimization problem for the functionalist problem, on the other hand, is an "as if"

the functionalist explanation into an intentional explanation: It is as if the agents possessed certain information and consciously brought about certain outcomes in view of that information. This procedure is closely related to the Friedman arguments discussed above, and it is at the root of most of the discussions – and confusions – about perfect information and its absence.

It is, of course, a quick jump from saying that something is functional to saying that it is optimal, especially when we get to choose (within limits) the superimposed operations-research problem that we think best captures the functional problem. This is not an incidental matter, since there is often enough leeway that we can find, for any given structure, a corresponding operations-research problem to which the structure is an optimal solution. This is a problem even in biology, where functionalism is governed by a natural selection argument that is relatively strict and clear (by social science standards, at any rate). Biologists Stephen Jay Gould and R. C. Lewontin (1979) have identified what they call the "Panglossian paradigm"–after Voltaire's Dr. Pangloss – according to which every observed biological structure is presumed to reflect an optimal adaptation to its environment. They have their arguments for why this "best of all possible worlds" view should be banished from biology – reasons that involve attacking the presumption that selection is the exclusive mechanism for evolution and that it always operates tightly when it *is* the mechanism. The arguments against Panglossianism in the social sciences are similar and necessarily stronger. Here there is no presumption that natural selection or something similar is the *sole* mechanism operating. Social institutions may be susceptible to invisible-hand explanations employing any number of selection or filtering processes, and may also involve elements of intention. This makes it a much trickier business to specify the problem that the structure solves and, not incidentally, makes it easier to find a problem to which the structure is an optimal solution.[34] Moreover, even when we can specify a well-defined process that operates somewhat like natural selection, the conclusions of our operations-research model are

exercise: it is as if the agents were attempting to solve a certain (global) problem (whose optimal solution becomes a normative standard). The agents may in turn be represented as if they were solving their own little pieces of the global problem (with any inability to solve this superimposed global problem labeled a "market failure"), but, as I argued earlier, this has no more claim to being situational analysis than it does to being behavioralism with an implausible behavioral assumption. Moreover, my suspicion is that it is this superimposition of the operations-research problem – and not situational analysis properly understood – that really lies behind the complaints of Latsis.

[34] Alchian provides a humorous example of this in Zerbe (1982, p. 178).

still subject to the disequilibrium and path-dependency problems outlined in the Section 10.4 (see also Langlois 1984).

My point here is not that we ought to do away with functionalist explanations or analyses of the "problem" a structure solves. For one thing, the notion of a function or problem-solution is actually necessary, within the evolutionary mold, to save the selection mechanism from tautology; the problem provides an independent criterion of survival value, so that survival does not become its own explanation (Gould 1977, p. 42). But even from the somewhat different perspective of the aggregate mold, the notion of a function serves a purpose: It draws our attention to the mechanisms that maintain a structure. And, since the mechanisms that maintain the structure can often be quite different from those that brought it into existence in the first place, an explanation that excludes the maintenance function is incomplete.[35]

At the risk of leaving out the punch line, I will not try to apply these considerations to the explanations of social institutions discussed in this volume (or elsewhere). In the case of the game-theoretical models offered by Schotter, one would want to ponder the extent to which these models are part of a causal-genetic explanation or of a functionalist one. Do the game-situations represent a true intentional explanation or are they "as if" explanations of what in fact arose through unintentional processes? To what extent do these models (qua functionalist explanations) satisfy Elster's five criteria? Schotter (in this volume) is clearly sensitive to many of these issues; but there remains room for a thorough methodological study. We can ask similar questions about Williamson's transaction-cost paradigm, as I have in fact attempted in part elsewhere.[36]

[35] In Menger's theory of money, the mechanisms that bring about the universal money-commodity are the same that maintain it as the monetary unit – the individual efforts to increase liquidity and reduce transaction costs. But consider Edelman's theory of government regulatory commissions (Edelman 1964). Voters, he asserts, are plagued with vague fears about and a sense of powerlessness over certain phenomena they can't control. The fear of monopoly, he says, is one of these. In order to gain votes, politicians make symbolic gestures to placate these fears – in this case, the formation of regulatory commissions. Voilà the *origin* of such commissions. But, once in place, the commissions, usually facing no real monopoly problem they have not themselves created, are quickly captured in the familiar way by those they were supposed to regulate. Thus a quite different mechanism *maintains* them once created; they serve the functon of cartelizing the industry and are kept in business by the political action of that industry. The full invisible-hand explanation – and the full impact of what Kenneth Boulding (1978, p. 195) calls "the law of political irony"—requires both types of explanation.

[36] My (1984) paper is in some measure an attempt to appraise in methodological terms similar to these the transaction-cost paradigm as applied to explaining the internal organization of firms.

4. Institutions II: Use this program to explain not only the basic phenomena of price theory but also the nature and origin of social institutions. This is the dual role in its second guise.

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