

INTRODUCTION

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When we originally envisaged this series of conversations—asking Nobelists to talk about what they thought they had contributed to political science, and political scientists to talk about what they thought those influences were—we hardly expected that we would without much intervention produce what has proven to be a remarkably well integrated and consistent set of contributions. Of course, our contributors are not all saying the same thing by any means, but there are clear commonalities in their thoughts, and it is easy to draw out linkages among them. One theme, often unspoken but present nevertheless, pervades their remarks: a chafing dissatisfaction with the standard neoclassical paradigm of economic analysis, particularly as a foundation for positive analysis. Although dissatisfaction with neoclassical analysis is not new (see, for example, Furubotn and Richter 1994, 11), it surprised us a little to hear it expressed with such regularity by so many who had received economics' highest award for their contributions to the field!

We have to be careful not to overstate this. There is no dissatisfaction evident with the fundamental postulate of neoclassical economics—that the unit of analysis on which all else is built is the individual choice. Indeed, it was our own shared interest in the questions of how institutions shape and yet are shaped by individual choices, beliefs, and strategies that led us to organize this series of conversations on taking economics seriously. Not only is the neoclassical paradigm worth thinking about because it has worked well in many contexts, but any one of the Nobelists might view his contributions through a lens different from the one that gives unity to our purposes here. Nevertheless, what we are looking at in these conversations are the ways in which the neoclassical paradigm, already familiar forty or fifty years ago, is being not just picked at but massively reworked by those dissatisfied with its assumptions as a basis for analyzing human behavior.

There are various ways to characterize the assumptions of the neoclassical model (see, for example, Eggertsson 1990, 3–25). Individuals make welfare-maximizing choices. Four key assumptions underlie all individual choices: no externalities, no scale economies, no decision costs (that is, full information and costless information and exchange), and preferences that are fixed and not interdependent among individuals. Each part of this structure comes under scrutiny in one or another of the essays that follow. We offer a sketch of where the contribution of each Nobelists starts and how they fit together; deliberately limited and kept simple (though we hope the outcome of our chopping and squeezing intellectual careers under headings is not too Procrustean!), the sketches foreshadow the directions we discuss further in the conclusion.

One disclaimer is in order. This volume is not intended to be a full review of the impact of economics on political science. For a superb recent example of such a

review, the interested reader should see Miller (1997). If it were such a review, we would certainly have commissioned papers by Anthony Downs and the late Mancur Olson. What follows, consequently, has less to say about the economic analysis of party competition, voting, and collective action. Nor does this collection pretend to be an exhaustive list of Nobel economist who have given something to the study of politics. Coase (1988) is already available as an example of a paper similar in spirit to the essay in this volume, representing what might have been included had resources and time been more plentiful.

Let us turn to the many contributions that we do include. In his opening remarks at one of the panels in Washington, D.C., Bob Goodin playfully commented that

one of the amusing payoffs of being roped into this project is discovering what it takes to win a Nobel. The first thing it seems to take is to have an idea—usually one is quite sufficient—that has really wide ramifications and can be summarized in two, at most three, words. . . . The second thing . . . is a CV that would choke a horse.

Clearly, in pulling together the commentaries, comments on the commentators, and discussions, the editors discovered right away that all the Nobelists have more than one idea. Nevertheless, if we had to choose a summary phrase for the origins of the political science legacy of each Nobelist, we would assign Arrow “majority cycles,” Becker “interest groups,” Buchanan “externalities,” North “transaction costs,” Selten “backward induction,” and Simon “bounded rationality.” We have to qualify these assignments immediately: Simon was not an economist at all; Selten regards himself as a dualist with a behavioral contribution as large or larger than the one we mention; Buchanan also studied constitutions and ethics; and every one of the Nobelists made other contributions that have resonated in political science. But these labels gives us a place to start, a sample of the sort of contributions more fully discussed in this volume.

Several of these critiques of neoclassicism focus on the assumption that there are no decision costs. Arrow’s “majority cycles,” for example—under which individually transitive preferences cannot be guaranteed to aggregate up to a transitive ordering under majority rule—is at its narrowest a demonstration of the logical inconsistency of several desirable properties of a method of aggregating individual preferences. Within economics, this renders indeterminate any “social welfare function” used to evaluate the output of the economy. Within political science, however, it produced two research programs, both theoretical and empirical: one conceptualizes, describes, and measures the instability of majoritarian collective choices; the other shows how and to what extent different institutional arrangements constrain majoritarian instability and has produced a whole literature of ways to think about how cycling and other problems can be overcome in order to get to an efficient frontier when production requires social aggregation of preferences. With the possibility of endless cycling, of course, decision costs are not only positive but possibly infinite—in which case, no production at all might take place!

Buchanan’s contribution is grounded in the consequences of “externalities” and decision costs. In a nutshell, he transferred the concept of gains derived from mutual exchange between individuals from the exclusive realm of economic markets to the realm of political decisionmaking. Thus, although only individuals choose, econom-

ics cannot be studied properly outside of politics. Partly this is because economics is about a game played within rules, and the choices between different rules of the game cannot be ignored. But even within a given set of rules, there are decisions about (minimally) taxes and public goods that need to be taken collectively. These decisions are made by coalitions and impose externalities on those outside the coalitions in ways that depend on the costs of decision and the size of the coalitions that form. An enduring legacy of Buchanan's work is the question of whether the rent-seeking and externalities that arise under supermajority procedures are actually better for us than the ones that arise under majoritarian procedures. Again, however, the central point is to see costly political decisions as central even to economic processes.

North's work, for our purposes here, delivers a closely related message but deals more with interpersonal interactions. His vision of the "cost of transacting" covered the difficulties and costs buyers face in observing product quality (and the costs to sellers of assuring buyers about quality), in monitoring and enforcing agreements that involve subsequent performance, and even in discerning and preparing for opportunistic behavior when contracting. From there it was a short step to beginning a theoretical program of reasoning about how institutions evolve to try to solve these costly transaction problems so that the aggregate value or volume of transactions and output can nevertheless grow. As an economic historian concerned with long-term secular change, North's research program puts transaction costs, the new economic institutionalism, and the dimension of time in the service of understanding economic performance and change. Political science applications of transaction cost analysis and the new institutionalism are growing more common, especially in the study of the design and organization of institutions like legislatures and bureaucracy and of arrangements that support political as well as economic development.

The program Herbert Simon recommends was the study of "bounded rationality" or the impact of cognitive capacity limits on rational individual choices. In his original formulation of "satisficing" (Simon 1957), actors are content with a certain level of achievement and indifferent to gains beyond that. That is, they do not attempt to optimize beyond a personal level of satisfaction. Simon's view is that we can't do economics until we have a full-blown theory of cognition to augment it, as well as enough empirical work to ground the definition of rationality in actual behavior. A minimalist response (the position of a skeptical scientist or an economist less patient than Simon) is to suppose that information is costly rather than free and universally available. Many economic and political models (including some very interesting work of Arrow's on the role and limits of organization) do just this. However, Simon's argument also depends on the characteristics of human beings as decisionmakers (our hard wiring) and of the processes under which we make our decisions.

Reinhard Selten notes that his own experimental, behavioral program is closely aligned to Simon's. But his "backward induction"—a method of analyzing how individuals form multiperiod strategies in iterated games that leads directly to the equilibrium concept of subgame perfection—was instrumental in moving formal analysis in politics from cooperative to noncooperative game theory and in bringing to the forefront the analysis of games involving reputation and incomplete information. An early application in economics was a model of predatory pricing (Kreps and Wilson 1982) subsequently extended to phenomena in international relations (Alt, Calvert, and Humes 1988). Subgame perfection, interestingly, requires players to consider counterfactuals but ignore those that are inconsistent with rational

behavior by other players. The introduction of reasoning by players uncertain of each other's rationality, in contrast, has been beneficial in explaining "excessively" cooperative behavior in contribution games. Thus, the broader literature itself reflects the contrasts in Selten's own work between the abstract standard of hyper-rationality represented by subgame perfection and the limited cognitive capacity evident in his behavioral models.

Even Gary Becker's work on competition between interest groups evinces the unease about the neoclassical paradigm we heard in all the Nobelists' contributions. Long interested in the study of political competition, Becker describes how the government is a major factor, one that has enormous influence on what happens in the economy, and indeed in social life. He models firms as interest groups that compete politically to create favorable market structures and conditions like patterns of ownership, regulation, and taxes. The economic basis of interest-group (firm) competition is the elasticity of demand for its product and the deadweight loss caused by the political benefits secured. Although his work says little explicitly about the political supply side, Becker raises provocative questions about how much actually is explained by differences among institutions across polities. Like many political scientists, Becker finds that interest-group competition explains a lot in areas of economic organization and public policy, including the selection of industries in which to impose tariffs, securities and banking regulation and deregulation, national airlines, and the environmental movement. Simply absent in the neoclassical paradigm, his analysis is important in giving government its due as an influence on outcomes.

There are several common elements in even this brief description of the Nobelists' legacies in political science. First, the neoclassical paradigm is too narrow to be a satisfactory model for behavior, whatever its status as a normative standard. This is obvious in the work of Simon and equally clear in Selten's (1994, 42–43) comment that although "people do have evaluative feelings about hedonic experiences . . . social norms, moral constraints, and unreflected routines are of similar importance." In conversations and discussions, Buchanan refers to the importance of the work ethic, Becker discusses endogenous preferences, North argues for the importance of appreciating cognitive complexity, and Arrow is concerned with whether we can explain how markets aggregate information into equilibria.

Second, the restrictiveness of the neoclassical paradigm produces widespread recognition of the importance of institutions. Institutions help individuals with fundamental problems of exchange, collective choice, and collective action. If nothing were ever chosen by vote, there would be no problem of cyclical instability. If there were no social dilemmas, we would have less need to deal with problems of communication, cooperation, and coordination. If information were freely available, specialization and delegation would not produce agency costs. If there were no nonsimultaneous exchange, *ex post* opportunism would not be a concern. However, all these problems exist, and institutions ubiquitously deal with the trade-offs they create, providing opportunities for beneficial transactions that would not take place in the absence of the institutions.

Third, many of the research agendas begun by the Nobelists call out for extensive empirical work. Consider Buchanan's externalities. There's clearly a need for an empirical program to determine whether supermajorities actually increase or decrease externalities and rent-seeking. Is it deadweight loss that leads to lobbying, fear of opportunistic behavior by others, or both? Does the structure of the political

supply side really not create incentives for strategic behavior by interest groups, as Becker alleges? North has measured transaction costs (Wallis and North 1995), but in the field at large there's scarcely agreement about what constitutes a transaction cost, the size of the aggregate volume of transaction costs, and the number of forgone contracts. There could be an empirical program on the ability of institutions of different sorts (party systems, legislative organizations) to overcome cycling problems. There is no shortage of good projects to be carried out!

Finally, we are struck by the importance of cognitive science in these contributions. With the exception only of Buchanan, all the Nobelists are questioning some aspect of the rationality assumption. They are in fact carrying out the research program for which Simon won the Nobel, as is evident in North's investigation of "mental models" as a means to get at a theory of ideology and culture; Becker's new work on social capital as a means of bringing norms, trust, and the like into the neoclassical model; Arrow's long-term interest in incomplete and asymmetric information; and Selten's experimental research on the limits of rationality. This orientation may be partly attributable to the fact that the empirical program that has proceeded from Simon's work is much more advanced than anything from, say, Buchanan's, or indeed from North's work. There is nothing, for instance, that comes close to the kind of clear and convincing evidence that Kahneman (1994) presents from his experiments that people frequently use what Bryan Jones calls nonproportionate rules in processing information. Although there are many reasons for this move toward cognitive science, we take some satisfaction in seeing economists return to the agenda of a political scientist. Once joined at the hip as political economy, then separated by huge differences in subject matter and methodology, this volume suggests a new convergence of theme and approach. It demonstrates that our field, political science, is not just a learner or importer of the ideas of others; its practitioners are actively contributing to the modification of the neoclassical framework and to a new political economy.

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