The Future of Public Choice

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> "Science does not rest upon solid bedrock. The bold structure of its theories rises, as it were, upon a swamp. It is like a building erected on piles. The piles are driven down into the swamp, but not down to any natural or given base; and if we stop driving the piles deeper, it is not because we have reached firm ground. We simply stop when we are satisfied that the piles are firm enough to carry the structure, at least for the time being."
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> Sir Karl Popper, *The Logic of Scientific Discovery*. New York: Harper and Row 1968, p. 111.

I. Introduction: Public Choice as a Science

The other two presentations in this plenary session have addressed the policy problems that remain to be solved in the second stage of public choice. My presentation speculates on the direction that the second stage of public choice research will likely take.

Public choice is a science. It attempts to discover universal principles of political economy. A science does not recognize "national boundaries" in the intellectual problems that need to be studied; thus, the public choice research agendas of Japan, Korea, Europe, and the United States are all fundamentally the same. Just as there is no Japanese or American physics, there is no Japanese or American public choice. This is not to say that the intellectual problems that attract attention are the same everywhere or that the solutions to specific problems are equally valuable. It is also clear that some aspects of human nature and political institutions tend to be specific to particular regions or cultures, and, therefore, the specific models that are most

appropriate for public choice analysis will vary somewhat from place to place and time to time. However, the logic of public choice is universal because public goods and coordination problems have to be solved and prisoner dilemmas avoided in all social settings. Addressing these problems require choices, and, consequently, some process of collective decision making is present in every society.

A. Building Knowledge in a Swamp

Everything that we know about the future is based on our experience in the past, and, consequently, in order to speculate about the future of public choice we must start by understanding past developments. It is, thus, necessary to spend some time understanding its recent past in order to talk about the future of public choice research.

The field of public choice is now a bit more than 50 years old. Public choice began with Duncan Black's (1948) path-breaking work on the median voter and James Buchanan's (1949) work on the theory of government finance . Research continued throughout the 1950s and 1960s with seminal books by Arrow (1951), (1960), Buchanan and Tullock (1962) Olson (1965) and many articles published in economic and political science journals. Research accelerated during the 1970s and 1980s as specialized journals emerged and as the rational choice approach to politics became more broadly accepted by orthodox political scientists and economists. Approximately 10 journals now focus exclusively on public choice research, and more than a hundred journals have published public choice research.

The research produced during its first 50 years is substantial. For example, the data base *Econ Lit* lists approximately 10,000 articles and books published in economic and major political science journals since 1969 that address the subject "public choice." There is a good deal of variety in public choice research, but a common methodology is clearly evident. Public choice research explores the implications of rational choice within stable political settings, mostly those of modern democratic states.

The breadth of public choice research is generated by varying (i) the institutional setting in which collective choices take place, (ii) the informational relationships among participants within the setting of interest (fully or partially informed, unbiased or biased signals, unbiased or biased expectations), and (iii) the policy choices under consideration. Its depth is generated by

increasingly sophisticated characterizations of self-interest, institutions, and information, and from a better understanding of the relationships among these three parameters of political decision making.

One major stand of public choice research focuses on settings where policies are indirectly determined by casting votes in elections. Voters advance their own self-interest by casting votes for the candidate closest to their own preferred policies, while candidates choose positions to maximize the votes that they receive. Voter preferences are assumed to be consistent; and the pool of voters, the slate of candidates, and the constitutional rules for counting votes are assumed to be stable during the period of analysis. Given informed voters with policy preferences that can be mapped into a single dimension, competition between two candidates leads to an electoral equilibrium in which both candidates "propose" the policies favored by the median voter.

If voters are not well informed or their policy preferences are distributed in a manner that lacks a median, electoral equilibria might not exist or other institutional features might largely determine policy. For example, an agenda controller, rather than the median voter, might secure the policy that maximizes his own interests. Alternatively, if voters can be easily persuaded about the relative merits of policy, interest groups may be able to manipulate elections by strategically subsidizing information and candidates in order to secure policies that advance their own narrow interests at the expense of the rest of the polity.

A fairly wide range of models developed by public choice scholars have similar predictions about political outcomes. In such cases, the implications of rational choice-based analysis are fairly robust across political settings, and the particular model used for public choice analysis does not substantially affect our predictions about public policy. For example, median and average voter models have quite similar comparative statics and predictions about policies if the distribution of voter preferences is very symmetric. The implications of interest group and election models are also similar under some assumptions about the distributions of voter and interest group policy preferences insofar as policy outcomes tend to be "middle of the road" outcomes in areas where the efforts of "pro-" and "anti-" interest groups largely offset each other at the margin.

In settings where the models predict different political equilibrium or policy outcomes, *empirical testing* of alternative theories becomes both possible and important. In such settings, we cannot understand or predict policy choices unless we choose "the best possible" model. In such cases, determining "the best theory" or using the "best model" becomes central to our efforts to understand and improve policy formation. It bears noting, however, that in Popper's swamp, there are no "best possible models," only models that can be continuously updated, refined, and improved. Scientific progress is generally incremental rather than definitive or revolutionary. Popper's swamp, thus, implies that the central research program of public choice can continue on into the foreseeable future as theorists analyze ever more complex settings and econometricians test these models using ever more sophisticated empirical techniques over ever more extensive data sets.

B. Scientific Progress in the Past

In the core areas of research, the future of public choice research is very likely to resemble that of the past half century. As generally true of normal science, much future public choice research will refine and extend existing concepts and increases our understanding of the strength and weaknesses of those concepts. Past experience also suggests that some extensions to the existing literature will bring new questions to the center of attention. New lines of research may emerge without overthrowing the existing framework of analysis. Not all normal science is incremental or entirely predictable.

Examples of important new branches that emerged out of the electoral research program pioneered by Black, Buchanan, and Downs include voting equilibria, institutionally induced equilibria, the theory of taxation, the pork barrel dilemma, political business cycles, campaign finance, and constitutional economics. Related, but *substantially new*, areas of public choice research were pioneered by Olson (1965), Tullock (1967), Stigler (1971), and Niskanen (1971), who focused attention on the role of interest groups, agency problems, and rent-seeking losses in policy formation. Interest group models have clearly broadened the scope of our political analyses and deepened their foundations. They have also substantially changed the way in which we think about politics. However, these new areas of public choice research were not scientific revolutions in Thomas Kuhn's (1970) sense, but, rather, new branches of the emerging

public choice paradigm. There was no radical shift in the fundamental language or of the conceptual basis of our political analysis. The research on interest groups, like that on elections, represented new applications of the rational choice models to stable political settings where public policies were determined by the self interest of politically active individuals.

Future research in public choice is likely to follow a similar pattern. A good deal of work will extend existing lines of thought, while other original developments will complement that work and perhaps change the way in which we think about politics. In most of the well-established areas of research, progress is likely to continue much as it has for the past few decades--with refinements of the institutional settings examined, better data, and increasingly sophisticated statistical techniques. Current results and methodological foundations are unlikely to be fundamentally changed by this research, although we will better understand the limits of our present models and empirical methods.

Much of the new public choice research will, consequently, remain familiar insofar as our present models are not systematically biased or wrong. In such areas of research, future research will make public choice predictions more accurate, but the new predictions will *not be systematically different* from those based on current analysis or estimates. Here we may note that the basic predictions of the first two decades of public choice research are clearly not so different from those of the last two decades. Much is new, but most is, more or less, familiar. (Gradual increases in precision are commonplace in other sciences as well; for example, gravity on earth was very well understood before Newton's and Einstein's paradigmatic revolutions.) In Popper's terms, normal research deepens our foundations and extends the edifice a little bit at a time. The foundations of public choice are reasonably secure and the edifice constructed on them during the past five decades covers a lot of ground.

II. Research at the Foundations of Public Choice: Are There Revolutions in the Wings?

A. Open Questions

However, as true of other sciences, many open research questions remain. In this respect, public choice, like other sciences, remains afloat on the "swamp." Its foundation is more

or less sufficient for the present, and for much future research, but is may not be sufficient for all of the open research questions that remain.

It is in unexplored areas along the foundation of public choice research that our understanding of political processes is most likely to change fundamentally. The research that is most likely to change our way of thinking is that which explores our fundamental assumptions about human nature or extends our analysis to extreme or unstable institutional settings. Changes in our understanding of human nature and in the range of political institutions analyzed may fundamentally change our understanding of politics and public policy formation. Such changes may well occur in the next 20 or 30 years.

B. Incorporating New Research on the Nature of Man

The psychological and biological foundations of the public choice model of individual decision making are clearly not as deep as they ultimately will be. Even if all individual behavior is rational and self-interested, it is not always obvious what is and what is not rational, nor what is and what is not in a particular person's self interest. New research on the nature and limits of self-interest and rationality may well revolutionize public choice--although it may not.

The current public choice literature includes some research that explores different assumptions about the nature of "self-interest." For example, some recent work examines settings where the typical person/voter is less "rational" or "self-interested" than is generally assumed in public choice models. (See, for example, Brennan and Lomasky, 1993, Ostrom, 1998, Caplan, 2001, or Congleton, 2002.) However, by and large, that research accepts the abstract atomistic ahistorical vision of mankind used in mainstream public choice research.

A radically different line of future research is suggested by E. O. Wilson's (1998) provocative book, *Consilience,* among others. That book suggests that social scientists integrate fully the new results from scientists studying the evolutionary foundation of human nature. In Wilson's view, human nature is fundamentally based on time-tested biological and culturally induced propensities that affect how a "typical" person acts or is inclined to act. Wilson argues that a better understanding of human nature will systematically affect our understanding of and our predictions about human behavior.

However, it is *not necessarily the case* that incorporating "evolutionary psychology" into public choice analysis *will change any of our main results.* Incorporating that research into our models may simply *reduce* the range of preferences that need to be taken into account. If preferences are biologically or culturally "hard wired," the range of opinions and preferences that need to be analyzed may simply be a *subset* of those presently allowed in our most general models. If so, incorporating evolutionary psychology into our models will generate more precise predictions than before, but not systematically different. Similarly, to the extent that "best reply functions" are hard wired, the range of feasible strategic behavior that needs to be accounted for in social settings becomes smaller, rather than larger. In these cases, *deepening* the scientific foundation of "self-interest" and "rationality" tends to *simplify* our analysis because it *narrows* the range of behavior that has to be analyzed. A better understanding of human nature can affect the way that we think about economics and politics without affecting our analytical methods or our main predictions about human behavior .

None the less, it is possible that the insights of evolutionary psychology will help us explain various public choice anomalies in a fairly straightforward fashion and lead to new predictions concerning political behavior. For example, people may be predisposed to "overreact" to new risks and overweight recent information because such responses were useful for thousands of years during early human evolution. If true, this may explain why democratic governments treat different kinds of risks differently, and why interest groups and candidates often emphasize present and future dangers rather than accomplishments. There is substantial evidence that not all economic and health risks are treated equally, nor is all news equally influential. Some of this variation may have cultural or genetic roots in the distant past.

Such overreactions may also explain the effectiveness of terrorist attacks. Terrorism might be based on a widespread tendency among individuals to "overweight" recent "big events" when predicting the future. Even in Israel more people are killed in car accidents (550 per year) than by suicide bombers (the average has been 280/year in the two years since September 2000), yet little international attention has been focused on Israeli driving habits or highway construction. A more systematic analysis of such ancient interests may also help explain why government policies look the way they do.

Cultural and biological uniformities may also help to explain why rational people vote, are able to organize interest groups and coalitions, and make contributions to interest groups that will not materially affect their own future wealth or were free riding is essentially costless. None of this behavior can be easily explained in models based on narrow self-interest. In such cases, the results of evolutionary psychology and sociology may help explain these anomalies by revising our notions of self-interest and rationality.

It is also possible that a deeper understanding of human behavior will radically affect our understanding of some fundamental features of democratic politics. For example, some of the results from evolutionary psychology suggest that people in groups behave differently than people acting alone. It seems likely that some of the "emotional" hard wiring that we refer to as human nature evolved to solve various problems of conflict and coordination in small groups--whether transmitted biologically or culturally. In this case, the same "cultural norms" that clearly affect our tastes for food, clothing, and mates, may also affect our behavior in small groups. If "group selection" has favored behavior that solves public goods problems and limits externalities, ordinary public goods and externality problems will be less common than implied in analyses based on narrow self-interest, and far easier to solve than our current models imply. The "productive state" may have less to do than most of our models imply.

On the other hand, it may be more easy to organize specialized interest groups than our current models suggest, with the consequence that "political failures" become relatively more common. The same type of group-oriented "hard wiring" implies that *government failures may be much larger than present models imply* insofar as policies favored by narrow special interest groups become more widely adopted. The existence of human predispositions for cooperation within small groups implies that both rent-seeking losses and coalition politics are more important than atomistic models of self interest imply.

Moreover, the risk of such losses may also explain neglected, but productive, features of existing constitutions that reduce such losses in well-functioning democracies.

C. Extraordinary Politics: Constitutional Dynamics

Another area of research that may affect the methodology of public choice is work on constitutional dynamics. Present research generally assumes that constitutions, once adopted,

remain in place forever. This is partly for analytical convenience, because it is far easier to model political games in settings where the rules of the game are exogenously determined. And it is partly because, much of our research is focused on the politics of modern Western democracies which have generally had very stable constitutional settings during the period in which public choice research developed.

Of course, empirically, *we know that constitutions are revised from time to time*. Constitutional reforms in most cases lead to modern democracies, and moreover, most modern democracies occasionally adopt substantial reforms of their constitutions. England is presently reforming the House of Lords, and Italy and Japan have recently reformed their electoral systems. Sweden changed from a bicameral to a unicameral legislature a few decades ago. In most of these cases, it may be argued that the constitutional reforms are evolutionary rather than revolutionary. One "minor" constitutional reform is adopted at a time, and the constitutions governing political processes remain largely intact after the reforms are adopted. However, a series of such reforms can clearly change fundamental constitutional procedures.

Several unanswered questions regarding constitutional dynamics seem obvious: Why are some constitutions more stable than others? What kinds of changes in political and economic circumstances are most likely to induce constitutional change and which are likely to increase constitutional stability? Are there tradeoffs between durability and flexibility in constitutional design? Are some constitutions more prone to civil war or overthrow than others? How does one model the politics of constitutional evolution--where some fundamental procedures and constraints remain essentially stable at the same time that others are changed, perhaps radically?

Constitutional dynamics may help explain the emergence of the modern liberal democracies that most of us cherish. The latter may require several theories, because liberal democracy emerged in different ways in different places. In Northern Europe, democracy emerged through a gradual revision of existing institutions taking place over a century or more. In other places, democracy emerged from violent revolutions as in the United States and much of South America. Moreover, at the same time that successful liberal democracies emerged in Europe, North America, and Japan, it is also clear that many others have failed, as within South America in the early twentieth century and in the post-colonial regimes in Africa. The extent to which

these very different histories is the result of systematic differences in initial circumstances or simply the effect of unsystematic internal and external political shocks are matters likely to be explored in future research.

Both the methods and the results of constitutional change vary widely. The choice of "technique" (constitutional exchange or warfare) can be modeled in conventional terms as a strategic choice conditioned on the anticipated productivity of the range of alternatives available. These processes of constitutional change have not been studied very much at this point, and future work may well produce major surprises that change the way we think about political processes in both the short and long run.

Constitutional analysis may also change our understanding of "political failure." Some constitutions appear to have been too easily transformed and others too rigid to be transformed via democratic means. In many cases, government failures are evidently caused by *dynamic rather than static features of constitutions.* Dynamic failures clearly occur when democratic governments disappear--that is, fail to replicate themselves. A first election may not be followed by a second, or political competition among ethnic and economic interests may be replaced by monopoly parties.

Research on constitutional dynamics may fundamentally change our predictions about political processes and our evaluation of them. Together these areas of research may allow us to better understand the effects of constitutional design, and also to better assess the relative merits of alternative constitutions.

D. The Overlap between Frontier and Mainstream Research.

Of course, the conventional and frontier research programs are not entirely independent of one another. Barring a paradigm shift, an extended form of the rational choice model is likely to remain the main engine of research in the new areas of work as well as in the well-established areas of research that have emerged during the past 50 years.

If voters are predisposed to particular policies or particular reactions to new circumstances, they may nonetheless behave consistently given their genetically and culturally induced preferences and best-reply functions. Interest group activities may be easier to undertake in some societies than in others, because people are more inclined to "play by the rules" or

"cooperate" in some societies than others. Nonetheless, *self interest* will tend to be important in both sorts of societies.

Cultural and genetic predispositions may also affect constitutional analysis. Not all of the procedures that characterize policy formation in a democracy are written down in a nation's formal constitutional documents and organizational charts. Informal rules and ordinary legislation often determine the balance of power within political parties, among parliamentary committees, and between levels of government. These rules clearly affect policy formation, often in a manner similar to formal features of a nation's constitution. It is possible that models of evolutionary psychology and political norms may help explain some of the informal features of modern political institutions.

Conversely, applications of existing models to historical and non-U. S. settings may induce analysis of cultural and constitutional dynamics. This tends to be true even within long-term studies in the West, but will be more important in countries where radical constitutional reforms occur every generation. Understanding constitutional dynamics will clearly require assessment of the probability of successful reform and the range of associated consequences associated with those reforms for politically active interest groups and voters. It will also require consideration of durable human reactions to risk and perceptions of self-interest. However, constitutional reform may also require us to understand changing perceptions of self-interest, as with the wave of democratic ideology that swept through Europe in the late nineteenth century. That is to say, efforts to understand politics in the long run is likely to require an improved understanding of cultural and constitutional dynamics along with a better understanding of the implications of rational choice.

III. Conclusion: There is Much to Do in the Second Stage

My discussion of the next stage of public choice research has focused on its positive research agenda, rather than its policy implications. The policy agenda of public choice has been discussed by the other two speakers. As they have argued, there are many policy issues on the table.

It may be argued that the great policy issues of public choice are substantially timeless issues on which progress has been very slow. How does one assess the relative effectiveness of alternative institutional and legal structures for advancing the "good life?" How efficient is democracy? How can the efficiency of democracy be increased? *Such questions have motivated political research for more than two thousand years.* Most of the fundamental normative questions addressed by public choice would be familiar to Aristotle and to his the many students and scholars who have read his *Politics* (1969/330 B.C.) during the past 2,500 years. None the less, public choice theory has made substantial contributions to this agenda. Public choice research has focused attention on the limits of democracy and deepened our understanding of the problems of interest group politics and of the value of durable constitutions. On the other hand, Aristotle's students would be less familiar with the positive conclusions of public choice research. Aristotle did understand about political agency problems, but did not have a median voter theorem or a model of elections, and had only a cursory theory of the rent-seeking society. Progress has clearly been made.

None the less, many questions remain unanswered and unasked. Public choice is still young as a scientific enterprise and, although "normal science" dominates our journals more than it did three decades ago, substantial research questions remain within nearly every research program. The most likely course of future research is that the new research will deepen our existing scientific and methodological foundations, while extending the reach of our models, without substantially changing our basic conclusions. To the extent that our conclusions prove robust, they are also likely to play an increasing role in future efforts to build new, more effective political institutions and to improve existing ones.

As Jim Buchanan likes to say on his way back to his typewriter, "onward and upward."

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