Purpose, Method and Theory in the New Political Economy: A Response to H.W. Arndt

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Prof. H.W. Arndt has written an engaged and wide-ranging, if brief, critique of the field of political economy. His concerns are legitimate ones. Inquiry into the purposes, methods, and theoretical underpinnings of new approaches in the social sciences constitutes a healthy antidote against faddism, and is useful in focusing our attention on fundamentals.

Yet Prof. Arndt’s account is flawed on a number of accounts. He misunderstands the theoretical underpinnings of contemporary political economy and his methodological and epistemological biases are limiting. His contention that political economists have a hidden normative agenda should be treated with particular scepticism; Prof. Arndt’s article provides ample evidence that economists are quite capable of blurring the distinction between the positive and the normative. Prof. Arndt’s admission that “orthodox economics has not been very successful in its efforts to establish a rigorous corpus of welfare economics” (p.116) also understates the problem. Rather, the relative inattention of the core the economics profession to questions of distribution is itself revealing of interesting and important ideological biases that are worthy of explication.

Some Observations on Theory

To begin, let me outline some priors. Positive political economy is united by the interest in explaining economic outcomes through reference to political variables. In some cases, political variables might operate on economic ones directly. An example would be the hypothesis that political instability lowers investor confidence, and thus reduces aggregate levels of investment. A second well-known example is the effect of different sharecropping contracts – themselves dependent on power relations – on incentives for tenants to invest.

Much contemporary political economy, however, views the relationship between politics and economic outcomes as a mediated one. Politics affects economic policy which in turn affects the efficiency of resource allocation as well as the distribution of income and assets. Contemporary political economy can be viewed as the theory of public policy. This definition may seem trivial and obvious, but in fact it marks a major advance over traditional economics, which generally treated policy choice as wholly exogenous. Economists have long been aware of the theoretical and econometric problems associated with endogeneity. In general, political economy is doing nothing more than posing a question typically asked of the models of aspiring economics graduate students: “isn’t that variable endogenous?”

The next step is to isolate the nature of the relevant political variables that might operate. There are three clusters of contenders. I find it much more useful to think in terms of these analytic categories than to get stuck in ideological debates between Marxist and non-Marxist, neoclassical and non-neoclassical political economy. Closer inspection reveals that many of these distinctions collapse. For example, what is known in North America as neoclassical political economy shares substantial similarities to Marxism, including the emphasis on structural position within the economy as a determinant of preferences and the portrayal of policy, and of the state more generally, as constrained by the interests of social groups (for an exposition along these lines, see Frieden 1991).

The first set of variables that are of use in understanding policy are the interest and power (to include organization) of social groups. In this view, the political realm is viewed as an arena for the play of underlying social forces, whose interests are derived from their position in the economy. How the economy should be disaggregated for analytic purposes is a complicated issue that will depend on what is to be explained; class, sectoral, or more discrete interest group approaches are possible and might have utility under different circumstances. Moreover, it is important to be alert to the fact that non-economic identifications – race, ethnicity, religion, region – may play an important role in understanding policy choice.
Yet it is possible to use the insights of neoclassical economics to derive the preferences of different groups within a multidimensional issue space. For example, we would expect creditors and borrowers to have different inflation preferences, tradable and non-tradable goods producers to have different preferences over the exchange rate, import-substituting and export-oriented industries to have different views of trade policy, and capital and labor to have conflicting interests vis-a-vis wage policy.

There is now an extensive theoretical literature on rent-seeking (Collander 1984), as well as a highly developed empirical literature on trade policy (Nelson 1988) that has begun with the assumption that trade policy choices can be derived from the interests of groups. In contrast to Prof. Arndt's claims, this literature has microeconomic foundations, is testable, and has been tested extensively econometrically. There is a growing literature on the effects of partisanship on inflation that begins with roughly similar fundamental assumptions, and is also marked by similar theoretical and empirical sophistication. (Alesina 1988).

It is beyond the scope of this comment to explore the limitations on this sort of analysis; I have done so elsewhere (Haggard and Kaufman 1992a). It is worth noting, however, that there are typically problems of aggregating preferences into policy choices. At the crudest level, one could model policy as the vector of individual preferences, but we know that how preferences are weighted and actually aggregated is a more complex function of lobbying, the nature of electoral rules, and the reactions, both real and anticipated, of groups. These facts are an important justification for the analysis of a second cluster of variables: institutional ones.

The central theoretical claim of the literature on political institutions is that they provide incentives to actors just as prices and markets do (Bates 1990). This fact is gradually being understood by economists, who are developing more nuanced theoretical approaches to the study of non-market institutions, including the firm itself (Williamson 1985). It should be underlined that this literature is rooted in microeconomics, and increasingly in game theory, and can hardly be accused of lack of theoretical rigor.

Institutional analysis tries to show how the structure of political organizations—bureaucracies, party systems, different types of regimes—affect the nature of policy choices. An example might be the effect of the fragmentation of the party system on macroeconomic stability in democratic countries (Alesina 1987; Sachs and Roubini 1988; Haggard and Kaufman 1992b). One of the most extensive debates along these lines has centered on the differences between the performance of democratic and authoritarian regimes. These are empirically testable propositions and they thus meet Prof. Arndt's demand that science be predictive. Substantial bodies of literature exist that have tested these claims both cross-nationally and through case study work, though with admittedly mixed results (for a review, see Haggard and Kaufman 1989).

Ideas and ideology constitute the final cluster of variables that might be used to explain policy outcomes. This form of analysis does depart, to some extent, from the choice-theoretic underpinnings that are common to both economics and contemporary political economy. The choices individuals make are seen not as a function of their structural economic position or institutional incentives, but the ideas they hold about cause-effect relations, in short, by ideologies. Proponents of this view argue that it is impossible, or misleading, to impute preferences to actors, that preferences themselves must be investigated, and that while actors may maximize, they do so subject to uncertainty and limits on both information and on their models of the world. The research program here might involve interviewing or surveying officials for evidence concerning reigning economic theories, or correlating trends in public opinion or ideological orientation against policy choices.

The foregoing suggests that Prof. Arndt repeatedly creates straw men. One extended example should suffice to make the point. Citing MacIntyre's work on Indonesia disapprovingly, Prof. Arndt asks whether it is useful to analyze the motives behind credit policy if the answer is merely that they are adopted to purchase social stability and political support. Given that staying in power is a ubiquitous motive, he argues, this would always seem to produce policies that "everyone wants." Moreover, he continues, politicians act from a mixture of motives, staying in power and maximizing some social welfare function, which he takes as disconfirming MacIntyre's approach.

But the purpose of MacIntyre's excellent work on Indonesian finance is not to make the general proposition that Arndt cites, but to show how the political and social structure of Indonesia poses particular constraints on the maintenance of political power, and that these
constraints are reflected in composition and allocation of preferential credit. Change the political and social structure and you get different patterns of credit allocation. A cursory comparison of Indonesia, with its greater emphasis on credit to the rural sector, and Korea, with its bias towards big business, suggests that this is a highly plausible claim (Haggard and Maxfield 1992).

The fact that officials act from a variety of motives is irrelevant to the construction of theory. Here again, Arndt appears to want it two ways. He claims that contemporary political economy is theoretically underdeveloped, but at the same time he chastises MacIntyre for beginning with a simplifying assumption concerning motivations. This simplifying assumption, I should add, is no less plausible than the maximization assumption that undergirds the entire field of economics itself and which was powerfully defended by Milton Friedman in his famous essay on positive economics (Friedman 1953).

The “Methodenstreit” Revisited

This brings me to a second broad set of issues, namely methodological ones. There can be little doubt that political economy has suffered from methodological shortcomings, though Prof. Arndt seems to downplay the limitations of his own profession in this regard. Yet this is by no means a necessary feature of political economy. First, there are a number of political variables that lend themselves to quantification, from voting statistics to levels of violence. These can easily be incorporated into econometric models, both as continuous and dummy variables, and an increasing literature is attempting to do just that. To cite but one small area of work, there has been substantial cross-national quantitative work on the political economy of the debt crisis (Sachs 1989; Siddell 1987; Snider 1990; Remmer 1990). Thumbing through recent issues of *The American Journal of Political Science* or *The American Political Science Review* confirms a growing interest and acceptance of the utility of quantitative approaches among North American political scientists.

Second, the existence of qualitative variables is problem for all social science, not just political economy: economics has simply chosen to ignore it. Thus it is misleading to say that the methodological problems in political economy are “insurmountable” or that “non-quantifiable variables” pose problems on a “panoramic scale.” Economists do have the advantage of having a variety of quantitative data readily at hand, but economic work that is driven solely the supply of existing data has produced some of the worst science in any field, bar none. The absence of readily available data has forced political scientists and sociologists into creative and sophisticated thinking about methodological problems such as coding and indicator and proxy construction, and has resulted in the creation of new data sets.

More generally, however, I reject the contention that the only goal of social science is the generation of predictive law-like statements. This battle has been fought periodically since the late-19th century, when German sociology was divided by the well-known “Methodenstreit.” The generation of law-like statements and generalizations is certainly one goal, but we also seek to explain particular cases through theoretically informed configurative or historical analysis; some of Prof. Arndt’s best work has been in this vein. Generally, I would argue that political scientists are far ahead of economists in taking seriously the problems of small-n and case study analysis.

Lurking in Prof. Arndt’s criticism are a number of unexamined methodological biases which are highly questionable, including in particular the equation of methodological sophistication with the use of quantitative data and econometric analysis. I have no objection to such analysis, and have done some of it myself (Haggard, Kaufman and Webb 1991). But these types of methods can be limiting, as well, and often result in a supply-driven approach to theory testing in which other sources of relevant data are ignored. How many mainstream American economists are willing to interview government officials, do survey work, or tap archival materials to gain an historical understanding of particular cases? Such monographic literature has been the backbone of regional studies, and though this work could certainly be informed by greater attention to theory, it would be wrong to underestimate its value for our understanding of social phenomena.

What Is Political Economy For?

Though Prof. Arndt does raise theoretical and methodological objections to the new political economy, it is clear that the main problem that he has with this line of work is normative. “Very few exercises in political economy,” he argues, “are undertaken in … a detached spirit.” (p.115)
The objective of political economy, he believes, is to call into question the tenants of neoclassical economics, which in the eyes of political economists “functions as a cover or smokescreen for nefarious forces.”

First, a disclaimer. There can be little question that not all political economists have an equal level of economic literacy. And that as a result, claims are sometimes made that fly in the face of existing knowledge, if not fundamental economic logic; lack of “detachment” may simply be the result of bad training. I frequently wince at statements made by some of my political science colleagues, and I have no doubt that there are economists who wince at mine. Yet this is partly a function of the newness of the field itself and the inevitable difficulties associated with mastering the fundamentals of different disciplines. I was not trained in economics and econometrics as a graduate student, and what I have learned I have done “on the job” and by returning to the classroom. My graduate students, by contrast, are required to take more economics and methods courses, and this will gradually be reflected in the quality of work in the field. It is only fair to add, however, that the lack of sophistication among some political scientists concerning economics is perfectly mirrored by the lack of understanding of politics among some economists, who nonetheless feel qualified to hold forth on the subject; there is guilt enough to go around on this score.

Yet I do not want to make this apology overly abject. Prof. Arndt himself confuses normative and positive lines of analysis, in part by treating certain issues as settled which are in fact still the subject of important debate, in part by confusing claims that political economists have in fact made.

Take the issue of the role of “the state” in the economic development of the East Asian NICs. Prof. Arndt argues that because I place an emphasis on the role of political institutions in the development of the “Gang of Four,” I am “combatting, or at least questioning, neo-classical advocacy of market-oriented development.” (p.115)

This is misleading. My interest in introducing “the state” into the analysis of economic development was not motivated so much by a questioning of neoclassical claims concerning the role of market-oriented policies in NIC development, though I had my doubt about that, too. Rather, my concern was that neoclassical prescriptions had not asked the question of the political conditions under which such reforms are undertaken. In Pathways from the Periphery (Haggard 1990), I argue that the initiation and implementation of relatively market-oriented policies themselves demand explanation, and that one curious feature of the countries that have pursued such policies is that they had “strong” and non-liberal states. I should add that this observation is not limited to the East Asian cases: radical examples of market-oriented reform in Turkey and Chile also occurred under authoritarian auspices. This claim may be wrong, but I have attempted to provide it with a theoretical underpinning and it is certainly testable, two of the key prerequisites for entering into scientific discourse.

The problem with Prof. Arndt’s analysis is somewhat deeper, however. He seems to believe that the question of why the East Asian NICs succeeded has more or less been solved. Economic theory and empirical analysis have demonstrated, according to a number of economists, that relatively open trading systems, realistic exchange rates, and limited governments were responsible for their success, and thus efforts such as those by Alice Amsden (1989), Robert Wadh (1990, 1992) and myself to re-open these cases for closer scrutiny are fundamentally misguided.

Yet for reasons that Wade, Amsden and I have spelled out in detail, there are both empirical and theoretical questions to be posed to the neoclassical economic interpretation, quite apart from the issue of whether appropriate attention has been given to politics. It is far beyond the scope of this essay to reopen these debates here, but two issues are at stake. The first has to do with the empirical characterization of policy in these cases. Wade, Amsden and others have provided detailed evidence that Korea, Taiwan, and Singapore have pursued much more interventionist strategies than has commonly been portrayed among mainstream economists. Second, there are unresolved theoretical issues that center on the conditions under which non-parametric interventions may in fact be efficiency-enhancing, such as the existence of externalities, market imperfections, or increasing returns. To dismiss critics of the neoclassical perspective as harboring some hidden agenda does a disservice to the cause of open debate.

It seems, however, that the real problem that Prof. Arndt has is that he doesn’t like some of the findings of the new political economy. For
example, he chides me for my finding concerning the role of political insulation in the effectiveness of policy in the East Asian NICs, and questions whether useful "lessons" can be drawn from the observation that these governments were authoritarian. But this is a little like shooting the bearer of bad news. It is highly plausible that political conditions in some countries are more conducive to growth than in others. To pretend otherwise is not only unscientific, but it courts wishful thinking, the belief that all countries could develop at the same pace "if only if..." Yet it is precisely the task of political economy to investigate what those conditions are.

Does this make political scientists hopelessly deterministic? Not at all. It is possible that changing institutions in certain ways may contribute to enhancing efficiency, and if we can show how this might be done, then we can have as much impact on advancing world welfare as the economists. In recent work with Robert Kaufman, for example, I have explored the question of how the party system in new democracies may affect the propensity for macroeconomic stability. (Haggard and Kaufman 1992b)

Unfortunately, as Arndt correctly notes, it is not clear that some of the changes in political institutions that might be "required" to yield higher growth are necessarily desirable ones. But this is a tradeoff which economists need to confront and ponder. It is worth remembering that the East Asian "models" which have been advanced by the development policy community are countries that have been characterized by political closure and at least during some periods, extensive abuses of human rights. We have a responsibility as social scientists to be clear-headed about the "models" that we champion. One may perhaps reach the conclusion that such tradeoffs are worthwhile, but the tradeoffs themselves should be faced squarely, and not simply assumed away by the happy claim that all social interactions constitute positive-sum games (Arndt 1991, 116). This seems to me to be a prime example of what Arndt accuses political economists of doing: conflating the normative and the positive.

References
Amsden, Alice. 1989. Asia's Next Giant: South Korea and Late