



Politicians in Uniform

The Harvard community has made this article openly available. [Please share](#) how this access benefits you. Your story matters

Citation	Ames, Barry, and Robert H. Bates. 1977. Politicians in Uniform. American Political Science Review 71, no. 3: 1052-1054.
Published Version	http://www.jstor.org/stable/1960106
Citable link	http://nrs.harvard.edu/urn-3:HUL.InstRepos:12211571
Terms of Use	This article was downloaded from Harvard University's DASH repository, and is made available under the terms and conditions applicable to Other Posted Material, as set forth at http://nrs.harvard.edu/urn-3:HUL.InstRepos:dash.current.terms-of-use#LAA

arrangements – since they actually exist.

The upshot of this is that the validity of Willhoite's thesis depends upon evidence about what *humans* do – not upon evidence about what primates do. And that is what we knew (or should have known) in the first place.

PHILLIP C. CHAPMAN

University of Arizona

TO THE EDITOR:

Professor Chapman's first criticism is that I illegitimately equated political *authority* with power, stratification, and dominance-deference relationships. I'm sorry if I conveyed that impression to any reader; I certainly did not mean to use "authority" in any eccentric way but simply as power or hierarchical relationships considered to be legitimate or rightful. Through my discussion of "attention structure" and "charisma," I was suggesting that hierarchy-forming is phylogenetically rooted in the human species and long preceded the emergence of hominids' capacity to distinguish symbolically between legitimate and illegitimate power. I would further suggest that we often tend to accord authority, in the strict sense, to those who are in fact our hierarchical superiors. It is also possible, of course, to believe that one is obliged to defer only to those to whom one has initially conceded authority. That I have a strong normative preference for this latter ordering of priorities should be evident from the concluding paragraphs of my article. My principal concern, however, was to outline a phylogenetic perspective on human hierarchies, the behavioral substructure of all authority relationships.

Professor Chapman's second critical point made me painfully aware that I had used the terms "nature" and "natural" equivocally and had thus unintentionally contributed to misleading readers about my meaning. In my central thesis statement (p. 1110), I used "by nature" in the traditional political philosophic sense. But on p. 1124, I used "natural" in Robert Bigelow's evolutionary-biological sense (as typified in the quote by him on pp. 1125–1126). I wish now that I had said "phylogenetically" instead of "by nature" in my thesis statement, because, as the remainder of the article makes clear, that is precisely what I meant. From an evolutionary-biological perspective I would continue to reject a dichotomy between "natural" and "artificial" in human behavior. Rather, the significant question is the degree to which specific types of behavior are constrained or channeled phylogenetically. From this standpoint, then, used car salesmen's

apparel choices are perfectly "natural" but very likely free of any significant degree of genetic control. I continue to believe that there are persuasive reasons for giving serious consideration to my basic hypothesis: the propensity to form hierarchies is not only, in the descriptive sense, "natural" to humans, but has also been strongly selected for in the phylogenesis of our primate species.

FRED H. WILLHOITE, JR.

Coe College

Politicians in Uniform

TO THE EDITOR:

The recent article by Robert Jackman ("Politicians in Uniform: Military Governments and Social Change in the Third World," *APSR*, 70 [December, 1976] 1078–1097) attempts to resolve the question of the relationship between military governance and social change. The core of Jackman's argument is a reconstruction of Nordlinger's "Soldiers in Mufti," (*APSR*, 64 [December, 1970] 1131–1148). Jackman identifies important limitations in Nordlinger's tests of various hypotheses concerning the effects of military rule and attempts to improve Nordlinger's work by using covariance analysis. We think, however, that Nordlinger made a number of errors which Jackman repeats, albeit in a slightly different form. More importantly, we believe that Jackman's article illustrates a major pitfall in the empirical literature on policy outputs, i.e., the danger of analyzing political economy problems without giving careful thought to their nonpolitical dimensions.

Jackman correctly identifies major errors in Nordlinger's article, including the misspecification inherent in zero-order correlations and the indeterminateness of the direction of causality which results when the dependent variable (social change) occurs before the independent variable (military involvement)! Jackman is right to correct these errors, but they do not exhaust the difficulties in Nordlinger's piece.

One of the primary problems of Nordlinger's analysis is the high probability of measurement error. Nordlinger measured the effects of military involvement on seven different indicators (most coming from Adelman and Morris's *Society, Politics and Economic Development*), including such items as "leadership commitment to economic development," "rate of improvement of human resources," "change in the effectiveness of the tax system," "change in the rate of gross investment," and "rate of growth of GNP per capita." "Control" variables included the size of the middle class and the

degree of "modernization of its outlook." Whether such a concept as "leadership commitment to economic development" makes any sense we're not sure, but in a situation where lending agencies like the International Monetary Fund and the World Bank require a plan as a precondition for aid, it is hopeless to think that the existence of a plan indicates the extent of such a commitment. Moreover, even were we to accept to validity of this measure, it still has problems. As defined it does not appear to represent any kind of scale, and even if it did, it would have but three levels, thereby engendering problems of limited variation.

Similar problems arise with Nordlinger's second index of economic development. Can one talk about the modern outlook of social classes? Adelman and Morris's category of the most "modernized" middle class included such countries as Argentina, Brazil, Chile, Greece, Rhodesia, South Africa, Taiwan and Uruguay. Perhaps the middle class is just fickle; perhaps the concept makes little sense in the first place. Moreover, even a cursory reading of Nordlinger's index reveals that it measures much the same thing as the dependent variables of several of the equations. This may account for the more frequently significant *t*-coefficients and the higher R^2 's in Table 1 and 2 than elsewhere in the article. By adopting Nordlinger's choice of variables, even while changing his specification, Jackman is thus vulnerable to major errors.

Let us now consider Jackman's own model. He specifies the problem to be explained as one of "social change" and utilizes four indicators of that concept: change in per capita energy use (a proxy for economic growth), change in the ratio of school enrollment to population, change in the number of doctors per capita, and change in the number of radios per capita. These four indicators are largely unrelated (see Table 5). As separate concepts, this would be fine, but since they are supposed to be measures of a single concept, their near-orthogonality raises major questions. How are we to know that the indicators are measuring the same thing? Perhaps there is no concept here at all. We agree with Professor Jackman that these are interesting variables in themselves, but we disagree with his apparent belief that he has got around the problem of multidimensionality by applying the same explanatory model to each variable. That this approach is unsuccessful is most evident in the results: R^2 's that average .196 with one control variable and .211 with the other. In the presence of evidence of misspecification of this magnitude, nothing can be concluded about the slopes of the included

variables. *In other words, the effect of the military's involvement in politics cannot be determined in these models.*

While Jackman may have corrected some of Nordlinger's problems, he thus did not correct all of them. Problems of measurement, contamination, and specification clearly remain. These problems are solvable in principle, but it is our conviction that Nordlinger's problems lay deeper than Jackman recognized and that this is his ultimate undoing. More importantly, it is at this point that the fundamental problems that bedevil this particular article can be seen to underlie much of the broader literature on policy outputs. Nordlinger's dependent variables include some that are directly controllable by the government (like the tax system), some that are partially controllable by the government (like the rate of gross investment), and some which the government influences much less, *even if it adopts "appropriate" policies.* The presence of these different kinds of dependent variables has important implications for this kind of work.

One consequence of recognizing these distinctions is that we should shy away from any analysis that uses the same set of variables and the same mathematical form to account for variations in different kinds of dependent variables. Not only is the government differently related to each kind of indicator, but some indicators may logically precede others and may, in fact, be parameters in a model determining others. Gross investment rates, for example, may depend on the tax system, while per capita GNP growth depends, at least in part, on the gross investment rate. Minimally one might conclude that identical models of these phenomena are inappropriate.

Suppose we want to examine the effect of government policy on agricultural productivity. We might consider such factors as the incentives to invest in new agricultural technologies, the domestic terms of trade between agriculture and industry, the international terms of trade and the relative price advantage thereby given to domestic agricultural producers, the structure of land law, and the efficiency of the transportation network. All these clearly have a considerable bearing on the rate of growth of agricultural productivity, and they might well be included in any regression model.

A very different model would be appropriate to explain change in school enrollment ratios. Such a model might include lagged measures of public expenditures on education, the previous level of enrollments (including the colonial heritage), population density and the number of languages spoken (which affect costs). Worth

noting, however, is that governments seeking to speed social change might choose not to spend money to accelerate enrollments, either because they already have a school-leavers problem, or because it might be cheaper to educate students abroad, or because the private return to education is sufficiently high relative to the social return that subsidies are economically unproductive.

Modeling these kinds of outcomes seems relatively easy compared to modelling changes in per capita product, partly because the underlying structure appears to be more obvious, and partly because some of these indicators are themselves logically precedent to other kinds of change. (There is, of course, the complication that changes in school enrollment ratios, for example, may depend on economic growth, but in such a model economic resources can be taken as a parameter.)

Perhaps this argument sounds so involved that crunching ahead is the only alternative to giving up the whole affair. We think not, and it seems to us that our earlier distinction indeed offers a simplifying key to the problem. First, one should separate results of (1) direct government activities like changing the tax structure, allocating public funds, or repressing and torturing; from (2) partially controllable results like increasing school enrollments and raising gross investment; and from (3) ultimate outcomes like changing GNP, reducing the Gini coefficient, or increasing the population's caloric intake. Immediately we see that in order to affect ultimate outcomes, directly controllable activities must vary. So we can start with these direct and more easily modelable policies: public spending, taxation, repression. If they do not vary according to the degree of military involvement, the problem is resolved right there. If they do, then we must move to the next (less controllable) activity, always modeling with a plausible specification.

Such an approach raises the question of whether we can expect government policy to have much of an effect on ultimate economic outcomes. Put another way, is it credible that policy instruments represent real control variables in determining the behavior of the dependent variables? A negative answer to this question may, in fact, be plausible. The 1973 rise in the U.S. agricultural prices had more to do with Russia's decision to enter the world market than it did with U.S. agricultural policy, whatever our feelings about Butz's response to this fact. And much of the third world's development in the early 1950s resulted not from the domestic policies of their governments but rather from the rapid expansion of demand

for primary products resulting from the Korean War. The importance of these arguments can be seen in Jackman's null finding. Methodological caveats aside, is this result to be taken as showing that military governments in fact do not differ from civilian regimes in their policy outcomes? Or does it simply mean that government policy in general does not have much influence over the phenomena of concern?

In sum, we wish to emphasize that the technical features of a complex analysis are significant and pose difficult problems, but the deeper problem is the danger of modeling complex political-economic relationships without giving primary attention to their distinctive structures.

BARRY AMES

Washington University, St. Louis
Visiting Scholar, Stanford University

ROBERT H. BATES

California Institute of Technology
Visiting Scholar, Stanford University

TO THE EDITOR:

Ames and Bates voice a series of muddled complaints with my paper. None of these complaints is relevant to my analysis, most are unoriginal, and some are quite silly.

The major objection appears to be that I did not give careful thought to the "nonpolitical dimensions" of "political economy problems." (I assume that this is intended to be *my* "principal undoing" rather than Nordlinger's.) This complaint is not without its logical and semantic difficulties: if *non-political* issues are the key to a problem, in what sense can that problem meaningfully be labeled one of *political* economy? More puzzling, however, is the very restrictive definition of "political" implied in their assertion (nowhere do Ames and Bates provide an explicit definition). Toward the end of their remarks, political is taken to refer to "controllable activities." Thus, "direct government activities" like allocating public funds are "political"; "partially controllable results [sic]" such as increasing school enrollment ratios are (presumably) semipolitical; while "ultimate outcomes" like altering the size distribution of income are "nonpolitical." This implied definition is too restrictive to be useful, and fits poorly with the thrust of most political economy. For example, it is naive to assert that the size distribution of income is nonpolitical, but Ames and Bates cannot be prevented from making that assertion for this reason alone.

Ames and Bates have had some difficulty representing what my analysis was all about.