Inflation, Unemployment, and Left-Wing Political Parties: A Reanalysis

TO THE EDITOR:

The errors and omissions in "Political Parties and Macroeconomic Policy" by Douglas A. Hibbs, Jr. (American Political Science Review, December 1977) are so serious that they ought to be called to the attention of APSR readers. Hibbs asserts the existence of several basic relationships between economic and political variables which, if true, would have important policy implications. No doubt many readers will have spotted the flaws in Hibbs' analysis on their own, but others may well be taken in. Hence the necessity for this consumer product safety report.

Hibbs advances and purports to test three hypotheses:
1. On a cross-national basis, the level of inflation is inversely associated with the level of unemployment;
2. On a cross-national basis, the level of left-wing government is positively associated with the level of inflation;
3. On a cross-national basis, the level of left-wing government is inversely associated with the level of unemployment.

An attempt to replicate Hibbs' tests, especially of hypotheses 2 and 3, reveals that he has committed a number of errors, namely:
1. biased selection of cases;
2. combining noncomparable data sets;
3. biased selection of the time period for the independent variables;
4. subjective (unreliable) definition of one part of the independent variable;
5. an invalid definition for the other part of the independent variable.

I treat the above points in order.
1. Cross-national unemployment data lack comparability. For this reason, apparently, Hibbs relies on an article in the Monthly Labor Review which made an effort to present comparable unemployment data for the U.S. and seven foreign countries. It would therefore be reasonable for him to have applied his tests to these eight countries for which his source supplied comparable unemployment data. Hibbs does not do this. Instead, he assembles his cases on what appears to be a subjective basis. The Monthly Labor Review article gives data for eight countries. Without a word, Hibbs discards Japan from this data set. As it happens, Japan dramatically contradicts hypothesis 3: it had extremely low unemployment and no left-wing government. Surely it is wrong to discard a hypothesis-contradicting case from a data set without advising the reader.

Next, Hibbs adds five selected countries to the seven remaining in the Monthly Labor Review group. The data for these countries come from ILO-OECD sources (see below). Hibbs does not include all countries given by these sources, nor even all democracies. He discards all but five: Denmark, Finland, Norway, Netherlands, and Belgium. His ILO-OECD sources, however, give data for many other perfectly respectable democracies, including Australia, Austria, Iceland, Ireland, Luxembourg, New Zealand, and Switzerland. These countries are never mentioned.

As one inspects these omitted cases, two points emerge. First, once we leave the Monthly Labor Review data set we encounter a swamp of noncomparable unemployment data. As I discuss below, the ILO-OECD data are unusable for cross-national analysis; Hibbs should not have depended upon them for any cases.

Secondly, if one does depend upon these data—as Hibbs does—it emerges that some of the cases Hibbs is discarding squarely contradict his arguments. Hypothesis 1, for example, is seriously challenged by the cases of Ireland (high inflation, high unemployment) and Australia (low inflation, low unemployment). Switzerland, with very low unemployment and little left-wing government, contradicts hypothesis 3.

The point is obvious: you should not discard cases from data sets without (a) saying you are doing so, and (b) cogently defending your decision. In an extreme case, violation of this principle will enable a researcher to claim support for any hypothesis simply by ignoring contradictory cases.

2. Using Hibbs' stated source, the ILO Yearbooks, I was unable to arrive at the same figures he presents. The discrepancy was especially sharp in the case of Denmark's unemployment figure: Hibbs reported 1.5 percent; I got 3.2 percent. I wrote him about this and received the reply that his footnote is incorrect. Instead of the ILO source which he said he used, he used an OECD publication, not cited, as the source for the unemployment data for the five additional countries. The following
point applies to both of these sources (ILO-OECD).

Cross-national unemployment data are notoriously noncomparable. Countries use different definitions: different age groups are involved, different questions are asked in surveys—if surveys are used—and so on. The *Monthly Labor Review* article represents an effort by a team of labor statisticians to present some meaningful cross-national data. The ILO-OECD sources contain figures which are not comparable with these.

For example, for the United Kingdom the unemployment rate calculated from OECD figures (according to the instructions Hibbs gave in his letter) works out to be 1.6 percent for 1960–69. The *Monthly Labor Review* gives 2.9 percent for the same period. Obviously the two sources are not supplying comparable data. Numbers called the same thing (e.g., “unemployment”) do not necessarily mean the same thing. Yet Hibbs uses the *Monthly Labor Review* figures for seven countries and the OECD data for five others.

Hibbs may believe that the OECD-ILO data are comparable for the five countries he includes. But with a question as complex as the cross-national comparability of unemployment figures, if one advocates an hypothesis, one should consider oneself a biased judge, likely to allow the needs of his hypothesis to influence one’s opinion of which cases are acceptable. The use of a poor data set in its entirety has at least the defense of objectivity; the use of subjectively selected cases from such a set is more difficult to countenance.

3. Hibbs chooses the period 1960–69 to measure unemployment and inflation. Once this period is chosen for unemployment and inflation data, what would be the reasonable period to use to measure the independent variable, left-wing rule? One answer would be 1960–69, the same period. Another possibility would be a slightly earlier period such as 1958–67 to allow for a lag between policies and their presumed effects. Hibbs eschews these obvious possibilities and adopts instead the period 1945–69. The effect of this decision is to increase the left-wing government scores of several of the European countries and thus bring them closer to the desired regression line. France, for example, has had no left-wing government (by Hibbs’ own declaration) since 1958. A zero left-wing score for France seriously disrupts the scattergrams. By counting Socialist presence in French governments before 1958, the French left-wing score becomes 35. The left-wing score of Norway is similarly increased by counting earlier periods of Labor party rule.

Hibbs explains this curious choice of time period in footnote 16, asking us to believe that long lags, of several decades, between party rule and macroeconomic result are implied in his formulation of hypotheses 2 and 3. While it is perhaps implausible, we might accept this contention if the author consistently adhered to it. But he does not. The latter section of the article is devoted to a time series analysis which purports to show that after the British Labour and U.S. Democratic parties take over, unemployment rates in their respective countries go down. In order to undertake that analysis at all, one has to assume that macroeconomic effects come relatively quickly. And Hibbs explicitly makes this assumption, postulating a three-month lag between change in party control and unemployment effects. The results themselves—and Hibbs, at least, is obligated to believe them—indicate quick response times. For the British case, for example, Hibbs claims that the effect of party control on unemployment is virtually exhausted after about six quarters, that is, a year and a half, and completely exhausted in four years. It is curious indeed to see an author repudiate in one place his own findings in another.

Consistency, in fact, is not one of Hibbs’ strengths. In footnote 15, just before the footnote 16 which argues for the “decades lag” interpretation of the hypotheses, Hibbs writes:

Actually there was one brief period of Socialist-led rule in France after 1951: Guy Mollet’s government of February 1956 to May 1957. Analysis of annual data shows that unemployment was lower and inflation higher during [italics added] Mollet’s government (as well as during [italics added] the subsequent Center-Left government of Bourges-Maunoury) than during the right-wing Gaullist governments of the late 1950s and 1960s.

Clearly, if Hibbs believes in the “decades lag” argument of footnote 16 he should not in footnote 15 be giving Mollet credit for an unemployment level during his own regime. Obviously Hibbs is playing both sides of the street. De Gaulle is blamed for a 1960s unemployment level which is relatively higher in one comparison; then his Socialist predecessors are given credit for exactly the same unemployment level which is now relatively lower in another comparison (with the U.S. and Canada).

4. Hibbs does not offer an explicit operational definition for a “left-wing” political party. In practice, it appears that he depends on a semantic definition, considering the labels “Socialist,” “Labor,” and “Social Democrat” as
being “left-wing.” The policy orientations and decisions of these parties go unexamined. Thus the moderate, centrist Norwegian Labor party and Danish Social Democrats get classified as “left-wing.” The American Democratic party is right-wing.

Here again Hibbs falls into inconsistency, for he contradicts his treatment in the latter part of the article. There he treats the Democrats, with “relatively close connections to organized labor and lower income and occupational status groups” as the left-wing party which allegedly lowers unemployment by 2.36 percent when it gains control of the executive.

The inconsistency can hardly be attributed to random carelessness. In the analysis concerning the cross-national hypotheses 2 and 3, it is necessary that the U.S. be zero percent “left-wing” governed if the correlations are to come out as hypothesized.

As Hibbs employs it, then, the left-wing, right-wing dimension is a rubber variable, a variable so poorly defined that the same case (i.e., the U.S. Democratic party) may be placed in opposite categories at the discretion of the researcher.

5. In his hypotheses and the arguments supporting them, Hibbs refers to the general notion of left-wing government, that is, left-wing control of governmental decisions. It is his argument that left-wingers, to the extent that their preferences prevail, opt for higher inflation and lower unemployment. Clearly, then, to test his hypotheses we need a measure of left-wing government which fairly reflects the degree of control of those designated parties over governmental decisions. Hibbs’ approach does not serve this purpose. First, he considers only the executive branch of government, and secondly, he treats any left-wing party presence in a parliamentary coalition executive as amounting to total left-wing control of government in that year. The result is a definition which is an invalid measure of the meaning of the term “left-wing government.”

For example, in 1962 and 1963 the tiny Italian Social Democratic party, with 4 percent of the seats in the lower house, was one of the minor members of the Christian Democratic party governing coalition. According to Hibbs’ practice, Italy is scored as having a “left-wing government” in those years. Similarly, Sweden is calculated by Hibbs to be 100 percent left-wing throughout 1945–69, even though it is widely known that a pattern of “consensus government” prevailed there in which all parties participated in compromise decisions. While the Social Democrats have formed the Cabinet, they typically have done so with a minority of the seats in the Riksdag and at the sufferance of the other parties.

What is the effect of adopting this curiously invalid measure of left-wing government? It exaggerates the left-wing scores of many European countries. The basic requirement for fitting the data to hypotheses 2 and 3 is to separate the U.S. and Canada from the European countries Hibbs has selected. In this period the U.S. and Canada had higher unemployment and lower inflation than the European countries. The U.S. and Canada, assigned zero left scores, anchor one end of the scattergram. Any technique which increases the left scores of the European countries will therefore produce some nifty-looking scattergrams—Japan having been excluded at the outset. Hibbs’ method of exaggerating left-wing government scores of European coalition governments, then, assists in making the data conform to his hypotheses.

The extent to which these systematic abuses aid Hibbs’ case is perhaps best illustrated by the results obtained in a replication free of these abuses. For this replication the following decision principles were observed:

1. The countries employed are the eight originally encountered by Hibbs in the Monthly Labor Review article for which meaningful cross-national unemployment data exist. The unemployment data for other countries are so manifestly noncomparable that it is difficult to expand the analysis to them.

2. The period for the inflation and unemployment variables is 1960–69, the period chosen by Hibbs.

3. The period for the party rule variable is also 1960–69, it being apparent that Hibbs, except for one lapse, believes short or negligible lag-times exist between party control and economic result.

4. Left-wing parties are, following Hibbs’ semantic approach, any party called “Socialist,” “Labor,” or “Social Democrat.” To the list of left parties we must add the U.S. Democratic party, following Hibbs’ own treatment in the second part of the article. Canada, again yielding to Hibbs, is considered 100 percent “right-wing,” Mr. Trudeau notwithstanding.

5. Party control is measured, again following Hibbs, only for the executive branch. For coalition governments in parliamentary regimes, a left-wing party is assigned the fraction of control according to the proportion of seats it holds in the lower house. The debatable Swedish case is resolved in Hibbs’ favor, being
considered 100 percent left-wing throughout 1960–69.

The data which result from the application of these decisions are shown in Table 1. The product-moment correlation between the inflation and unemployment figures (Hibbs reports −.45, N=12) is \( r = -0.49 \), which is not statistically significant at the .01 level (one-tail; \( t=-1.37, d.f=6 \)).

The two political hypotheses (2 and 3) are unsupported by the data. The correlation between the degree of left-wing government and inflation is \( r = -0.21 \) (Hibbs reports +.74). The correlation between the degree of left-wing government and unemployment is \( r = +0.09 \) (Hibbs reports −.68).

Naturally, other data sets could have been employed and other definitions of the terms adopted. I have explored as many of these other possibilities as I could think of, and I see no sign with any of them that the data, handled fairly, will support the hypotheses Hibbs has advanced.

The last section of Hibbs' article contains a multiple regression time series analysis which purports to show that the British Labour and American Democratic parties reduce unemployment in their respective countries when they gain control of the executive. Here again we encounter the same pattern of abuses and inconsistencies.

Consider Hibbs' treatment of the British experience. The yearly unemployment figures for 1948–1976 are given in Figure 1. Do these data support the contention that Labour is the party of lower unemployment? We notice, first, that unemployment shoots up to history-making levels after Labour comes to office in March 1974. Second, we notice a substantial increase in unemployment during Labour's earlier rule of October 1964-June 1970. Again, unemployment reaches levels not previously attained in the period. (The lowest unemployment level, in 1955, occurs under the Conservatives.) Finally, we notice a sharp increase in unemployment in the first part of the Conservative regime beginning June 1970, but the level falls back in 1973, leaving the case a draw. Surely, in the face of these data, an advocate of the proposition that the Labour party reduces unemployment would be confounded. But Hibbs is scarcely slowed down.

First, the figures of 1973 onward are discarded "intentionally," as he says in footnote 27, on the grounds of an "exogenously imposed economic crisis." Strangely, Hibbs fails to mention what this crisis is. The only one I can think of is the oil embargo and the doubling of oil prices by OPEC. But these took place at the end of 1973 and therefore should not be the basis for discarding 1973 data. Blotting out the 1973 data is, however, helpful to Hibbs' case: the Conservatives get the blame for the sharp 1970–72 increase in unemployment, but they are denied credit for the equally sharp decline in 1972–73.

To handle the sharp rise in unemployment under Labour in 1967–69, Hibbs adds a dummy variable to the equation taking a value of 0 prior to 1967 and 1 otherwise. The main effect of this decision is to let the dummy, not the Labour party, take the blame for the higher 1967–69 unemployment.

Hibbs' explanation for introducing the dummy variable is that in October 1966 the Labour party introduced an expanded unemployment compensation system. Hibbs argues that "the new compensation scheme increased the rate and duration of unemployment." Another contradiction. Hibbs starts out asserting and ends up claiming that the Labour party reduces unemployment. But now, to put inconvenient data in their place, he asserts the opposite: the Labour party increased unemployment. Thus Hibbs presents us with a beast which I thought, until now, was purely mythical: the rubber

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### Table 1.

<table>
<thead>
<tr>
<th>Country</th>
<th>Percent Left-Wing Executive 1960–69</th>
<th>Average Unemployment 1960–69</th>
<th>Average Rate of Inflation 1960–69</th>
</tr>
</thead>
<tbody>
<tr>
<td>United States</td>
<td>80</td>
<td>4.8</td>
<td>2.3</td>
</tr>
<tr>
<td>Canada</td>
<td>0</td>
<td>5.1</td>
<td>2.5</td>
</tr>
<tr>
<td>United Kingdom</td>
<td>50</td>
<td>2.9</td>
<td>3.5</td>
</tr>
<tr>
<td>Japan</td>
<td>0</td>
<td>1.3</td>
<td>5.0</td>
</tr>
<tr>
<td>West Germany</td>
<td>12</td>
<td>0.6</td>
<td>2.5</td>
</tr>
<tr>
<td>Italy</td>
<td>12</td>
<td>3.7</td>
<td>3.6</td>
</tr>
<tr>
<td>Sweden</td>
<td>100</td>
<td>1.7</td>
<td>3.7</td>
</tr>
<tr>
<td>France</td>
<td>0</td>
<td>2.3</td>
<td>3.8</td>
</tr>
</tbody>
</table>

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hypothesis, true or false depending on the researcher’s needs at the moment.

In conclusion, Hibbs has failed to support his hypotheses about inflation, unemployment, and left-wing parties. He has, however, resoundingly demonstrated the old saw, “You can prove anything with statistics.” The proper statement of this adage is, of course, “You can prove anything with the abuse of statistics.”

JAMES L. PAYNE

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TO THE EDITOR:

James Payne’s communication (1979) concerning my article “Political Parties and Macroeconomic Policy” (1977) gives the impression that he believes he is onto something really big. (Perhaps I should be flattered?) I will try to salvage my reputation by convincing the Review’s readers that the “errors and omissions” claimed in his “consumer product safety report” amount to pretty small potatoes.

Readers may recall that my principal argument was that governments pursue macroeconomic policies broadly consistent with the objective economic interests and subjective preferences\(^1\) of their class-defined core political constituencies. Labor-oriented, working class-based leftist parties typically attach greater weight to full employment than price stability, whereas the reverse is generally true for middle and upper middle class-based centrist and rightist parties. Most of the article was devoted to showing that these partisan differences in economic priorities have an impact on observable macroeconomic outcomes, particularly unemployment outcomes. Let me now respond to Payne’s charges.

Selection of Cases. Payne implies that I selected cases (countries) that supported my arguments and excluded those that did not—a rather serious accusation. Actually, the countries appearing in my article form the empirical base of a much larger project on the political economy of Western industrial democracies, and have

\(^1\)My article summarized the objective distributional consequences of and subjective preferences toward unemployment/inflation configurations. These topics are given more sustained analysis in Hibbs (1975, 1978a, 1978b).

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Figure 1. Unemployment in the United Kingdom, 1948–1976

Source: ILO, Yearbook of Labor Statistics
been used in several subsequent papers dealing with such diverse political-economic phenomena as trade union militancy and wage inflation, long-run trends in strike activity, and the response of monetary authorities to the OPEC supply shock. The countries were selected initially because of their obvious importance in the political economy of Western capitalism (the United States, West Germany, etc.) and/or because they figure prominently in the literature on important political phenomena of interest to me (e.g., the Scandinavian welfare states). For purposes of my work on macroeconomic policy in Western industrial societies, the countries identified in ILO-OECD publications by Payne—Australia, Austria, Iceland, Ireland, Luxembourg, New Zealand and Switzerland—satisfied neither criterion.

Non-comparable Data. I have corresponded with Payne about Danish unemployment, pointing out that Danish specialists agree that trade union figures provide an accurate count of unemployment in the private sector, that there is virtually no unemployment outside the private sector, and, therefore, that the trade union figures in conjunction with data on total labor force size yield reliable economy-wide unemployment rates (an average of about 1.5 percent for 1960–69). I stand on this statement.

Payne’s more general point, that there are serious problems associated with making international comparisons of unemployment, specifically when the data are based on registration statistics rather than labor force surveys, is well taken. However, as I will show ahead, if international comparisons are undertaken for all industrial democracies for which comparable data are available, my principal argument is supported.

Selection of Time Periods. Payne noticed that in my international comparisons I looked at average rates of unemployment and inflation during the 1960–69 period in relation to “left-wing rule” over the period 1945–69, whereas in my dynamic time-series analyses of unemployment outcomes in the United States and Great Britain the partisan political terms entered the intervention models with comparatively short lags. He concludes that this is inconsistent and contradictory, and is a major illustration of the systematic abuses in my article.

I have a higher opinion of my colleagues’ critical judgment than this (not so) sleight of hand implies. Payne’s remarks here apparently stem from his failure to grasp the distinction between long-run equilibria and shorter-run dynamic fluctuations and/or to understand my ideas about the political sources of economic outcomes, which are quite clearly stated in my article (pp. 1473–75). The political factors bearing on macroeconomic outcomes that I identified included:

1. partisan differences in the political authorities’ short-run manipulation of monetary and fiscal policy instruments;
2. the postwar role of the Left in shaping institutional arrangements (principally labor market and manpower policies) designed to minimize unemployment, which are very difficult to dismantle once they are in place;
3. the Left’s prior economic performance, particularly just after the Second World War when many feared the onset of another great depression, and continued emphasis on low unemployment in political discourse which generated widespread public expectations of sustained high employment and therefore constrained the behavior of all subsequent governments.

Since I argued that factors (2) and (3) make an important contribution to explaining cross-national variation in aggregate, equilibrium macroeconomic outcomes, I computed the “left-wing” government scores over the 1945–69 period in my international comparisons. Payne points out that this enhances the associations in the scattergrams, which of course is precisely what my reasoning predicts.

By the early to mid-1950s the structural-institutional factors just mentioned were largely fixed in most capitalist industrial democracies. The stochastic time-series analyses in the latter part of my article were designed to estimate movements in unemployment rates that plausibly could be attributed to interparty differences in monetary and fiscal policies, which indeed have relatively short lag times.

While I thought this distinction between long-run equilibria and short-term fluctuations was obvious in my article, apparently it was not. Let me try to illustrate the point. Suppose we observed a group of countries experiencing partisan variation in control of the executive and that the outcome variables were mean stationary. If we fit time-series intervention models to the outcome variables for each nation, the party intervention terms would estimate the impact of partisan changes on fluctuations in the outcome variables and the intercept constants would pick up the net effects of nation-specific, (relatively) time-invariant structural-institutional factors not cap-
tured by the ARIMA functional forms. Cross-
national analysis of the intercept constants or
location parameters would be akin to the
international comparisons of equilibrium out-
comes described earlier.3

Definition of the Independent Variable. Let me
identify what I believe are errors of fact and
judgment among Payne’s observations. Payne
asserts that I scored Italy as having a “left-
ing” government in 1962 and 1963 because
the tiny Italian Social Democratic party was a
minor member of the Christian Democratic
governing coalition. I did not. The PSDI is
socialist or social democratic in name only and,
notwithstanding Payne’s remark about my use
of a semantic political classification scheme, it
made no contribution to my computations.

Payne describes the Norwegian Labor party
and the Danish Social Democrats as moderate
and centrist and seems to find it odd that I
consider the American Democratic party more
rightist by comparison. Further on Payne char-
terizes the postwar rule of the Swedish Social
Democrats as “consensus government . . . in
which all parties participated in compromise
decisions.” These observations will no doubt be
the source of some amusement to the Review’s
Scandinavian readership.

From the time the Labor or Social Demo-
cratic parties in the Scandinavian welfare states
achieved political power in the 1930s or just
after the Second World War until the culmina-
tion of the development of the system of
collective consumption and public sector distri-
bution of income in the early 1970s, the
growth in the share of the national product
allocated by the state was 24, 28 and 30
percent in Denmark, Norway and Sweden,
respectively, as compared to a public sector
increase of just over 5 percent in the United
States.4 By 1972 the fraction of national
income (again exclusive of defense and na-
tionalized industries) ultimately distributed by the
state in these Scandinavian countries ranged
between 45 and 50 percent, but stood only at
about 25 percent in the United States. Average
and marginal rates of taxation tell a similar
story. By socializing the consumption and final
allocation (though typically not the produc-
tion) of an enormous fraction of the national
product, the Scandinavian Social Democrats
engineered a massive circumvention of the
private market, which fundamentally changed
the political economy of distribution. The
American Democratic Party has never enter-
tained a program as ambitious and the private
marketplace continues to dominate distribu-
tional outcomes in this country.

In international perspective, then, I view the
Democratic party as centrist (not rightist, as
Payne says). However, for purposes of intrana-
tional political analysis the Democrats clearly
should be treated as the party of the Left,
though, as I observe in my article, the two
dominant American parties are less distant
ideologically and have more heterogeneous so-
cial bases than the British Labour and Conserva-
tive parties or, for that matter, the major
political blocs in most advanced capitalist
democracies. I doubt many political scientists
would find this international/intrnational dis-
tinction unreasonable or difficult to grasp.

In any case, the abstract point is of little
moment. If my assumption that the Democrats
(as the party with the closer connections to
organized labor and lower status groups) gen-
erally assign higher priority to unemployment
than the Republicans was incorrect, my in-
trnational time-series analysis for the U.S.—
which essentially amounted to a dynamic linear
contrast of interparty unemployment perform-
ance—would not have produced the results
reported in my article.

The impression left by Payne’s remarks
about the consensual nature of postwar Swe-
dish political life is quite misleading and reflects
a touching political innocence. The basic post-
war strategy of the Social Democrats has been
to divide the bourgeois opposition—isolating
the Conservatives (now called the Moderate
party) on the right wing of the Swedish spec-
trum—by selectively forming coalitions and
making compromises on discrete political is-
ues. Throughout the period they have implicit-
ly relied on the Swedish Communists’ refusal to
be implicated in turning a Social Democratic
government out of office. It is important to
understand, moreover, that the terms of “con-
ensus” policies were for the most part imposed
rather than negotiated by the Social Democrats
and their extremely powerful trade union allies.
After all, the Social Democrats presided over

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2For another illustration in the context of a
different research problem, see Hibbs (1976), especial-
ly pp. 1046–47.

3Strictly speaking, for this analogy to hold, the
various country models should have similar stochastic
structures and regular oscillation in partisan control of
the government. Nonetheless, the example illustrates
the point.

4Exclusive of defense and the budgets of national-
ized enterprises. For further discussion and sources for
the figures presented here and below see Hibbs
(1978c).
the establishment of what is probably the most advanced welfare state in the West and there have been many socioeconomic losers during the process. Nonetheless, outbreaks of bitter confrontation politics over such issues as the state's role in economic coordination and planning, steep rates of taxation, and the national compulsory pension scheme have periodically rocked the political system. A major struggle over the LO/Meidner plan to transfer ownership and control of large privately held firms to the trade unions looms on the horizon.

Replication of the International Empirical Results. In his replication of my international comparisons, Payne computes leftist executive influence over the period 1960–69 and, more important, scores the United States as much closer to Sweden than, for example, to Canada. I do not think this makes much sense and therefore see no point in commenting on his results. However, I do accept in principle Payne's criticism of my computation of leftist executive participation scores (percentage of years during which leftist parties occupied cabinet posts). In subsequent work I have used more reasonable measures—the average fraction of cabinet posts held by the Left, and the Left's share of the popular vote—to measure executive participation and electoral political importance, respectively. Cabinet share scores reflect the Left's direct participation in executive policy making. Vote share scores are an indirect measure of the Left's political influence, since all governments are likely to be constrained significantly on issues of great importance (e.g., employment) to a trade union-based, socialist-labor political bloc supported by a large fraction of the electorate.

Table 1 reports the simple correlations of the two Left influence measures just described as well as the executive participation indicator used in my original article with the rates of unemployment and inflation in 11 industrial democracies. In response to Payne's legitimate complaint about the international comparability of unemployment rates for some of the countries appearing in my article, I used only nations for which (1) unemployment data adjusted by the U.S. Bureau of Labor Statistics were available and/or (2) unemployment was determined by labor force sample surveys, to compute the correlations. Since many of the underlying scatterplots exhibit nonlinearities, I report linear and semi-log linear correlations.

The results in Table 1 show the following:

5BLS officials do adjust the data of countries that rely on labor force surveys to ascertain unemployment, but averaged over several years the adjustment factors are negligible to vanishing. To illustrate, the (upward) 1960–69 average adjustment proportions for the countries using labor force surveys that have been studied by BLS statisticians are: Australia, 0.0; Canada, 0.0; Italy, 0.09; Japan, 0.02; Sweden, 0.0.

Denmark also appears among the countries listed in Table 1 for the reasons mentioned previously.

Table 1. Linear and Semi-Log Linear Correlations: Unemployment, Inflation and Political Importance of the Left in 11 Industrial Democracies

<table>
<thead>
<tr>
<th></th>
<th>Average Unemployment Rate 1960–69</th>
<th>Average Inflation Rate 1960–69</th>
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<tbody>
<tr>
<td>Average Left Vote Share 1950–69</td>
<td>-.70</td>
<td>.48</td>
</tr>
<tr>
<td>$\log(x+1)$</td>
<td>-.66</td>
<td>.46</td>
</tr>
<tr>
<td>Average Left Vote Share 1960–69</td>
<td>-.70</td>
<td>.48</td>
</tr>
<tr>
<td>$\log(x+1)$</td>
<td>-.66</td>
<td>.46</td>
</tr>
<tr>
<td>Average Left Cabinet Share 1945–69</td>
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<td>.36</td>
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<tr>
<td>$\log(x+1)$</td>
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<td>.28</td>
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<tr>
<td>Average Left Cabinet Share 1960–69</td>
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<td>.31</td>
</tr>
<tr>
<td>$\log(x+1)$</td>
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<td>.35</td>
</tr>
<tr>
<td>Average Percent Years Left in Cabinet 1945–69</td>
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</tr>
<tr>
<td>$\log(x+1)$</td>
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<td>.50</td>
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<td>Average Percent Years Left in Cabinet 1960–69</td>
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<td>.32</td>
</tr>
<tr>
<td>$\log(x+1)$</td>
<td>-.34</td>
<td>.32</td>
</tr>
</tbody>
</table>

Source: Computed from a data appendix prepared by Nicholas Vasilatos. Supplied upon request to the author.

Australia, Canada, Denmark, Finland, France, Italy, Japan, Sweden, U.K., U.S., W. Germany.
(1) My basic argument is supported by every correlation. The political indicators correlate negatively with the average unemployment rate and positively with the average inflation rate.

(2) Every correlation but one is consistent with my a priori reasoning concerning the time-wise conceptualization of the political indicators. The 1945–69 (1950–69) indicators exhibit stronger associations with the macroeconomic variables than the 1960–69 indicators. The vote share variables show virtually identical correlations across time periods because electoral support for the Left is extremely stable through time (correlating, for example, at 0.997 across the two periods in Table 1).

(3) A high Left vote share, even if it does not lead to frequent outright control of the executive, may exert substantial influence on macroeconomic policies and outcomes, at least with respect to issues of great salience to the organized working class.

Of course, the correlations in Table 1 as well as the international comparisons in my original article prove nothing. The underlying model and methodology are far too simple to support strong claims. The results of the international comparisons are merely consistent with the basic arguments I have made concerning the political economy of macroeconomic policy in industrial democracies.

Remarks About the Intratemporal Time-Series Analyses. In this section Payne again accuses me of trying to buffalo the Review's readers by

(1) adding a dummy variable to the British model, which had a value 0 prior to 1966:4 and 1 otherwise, so that the Labour party would not “take the blame for the higher 1967–69 unemployment,” and
(2) not carrying my time-series analyses through to 1973, a year during which the Conservatives were in office and unemployment fell in Great Britain.

Let me first clear up Payne's misunderstanding of the contribution made by the dummy variable ($C_T$ in my article) to the interparty unemployment performance estimate for Great Britain. The steady state effect on unemployment attributed to this variable (0.86 percent) in the U.K. model affects all subsequent unemployment observations equally. Since the numbers of Labor- and Conservative-administered quarters subsequent to the full impact of the dummy variable are almost identical, neither party was advantaged or disadvantaged with respect to the interparty unemployment performance implied by the model.

Moreover, as I stated in my article, I did not want the magnitude of my estimate of interparty unemployment performance in Britain to be influenced by the sharp rise in unemployment benefits introduced in 1966:4, which nearly all observers agreed reduced financial pressure on the unemployed to accept unattractive jobs and thereby increased average employment search time and hence the measured rate of unemployment. Similarly, I did not want the enormous fiscal stimulus associated with American intervention in the Korean and Vietnamese civil wars to influence my estimate of interparty unemployment performance in the United States, and so dummy variables for these wars were incorporated into the U.S. model. Unlike the British case just reviewed, the war dummies did serve to “disadvantage” the Democrats. Curiously, Payne does not mention this.

Payne is correct in surmising that I did not carry my time series analyses beyond 1972:4 because of the unprecedented OPEC-induced supply shock, which was simultaneously recessionary and inflationary, and the consequences of which are still being felt today. He is also correct in saying that I might have included 1973 in my estimation time range. The OPEC supply shock hit in 1973:4, but its terrible effects were not really felt until 1974 and thereafter. However, he is wrong in asserting that not including the 1973 observations critically affected my results.

Figures 1 and 2 show one-step and multistep simulation forecasts generated by the British unemployment model reported in my article (Hibbs, 1977a, Table 2, p. 1479). All of the actual unemployment observations lie within ± two standard errors of the one-step ahead forecasts and within ± one standard error of the multistep (continuous) forecasts, which is a much stronger test of the fitted model's adequacy. In other words, the model estimated through 1972:4 easily accommodates the 1973 observations. If anything, the forecasts tend to underpredict the actuals, which is quite contrary to Payne's line of argument. His conjectures about the bias introduced by excluding the 1973 data are, therefore, unfounded.

Conclusion

The monograph on which my Review article was based was written during 1974–75 when quantitative work in macropolitical economy was still in its infancy. It was intended to stimulate research on the political sources (especially class-linked political sources) of economic policies and outcomes as much as to establish firm propositions; certainly, I had no
illusions about demonstrating "iron laws." Unquestionably, the article has serious limitations. To enhance the usefulness of this exchange let me identify three that I think are particularly important.

1. The article takes almost no account of the international economic and political constraints on macroeconomic policies and outcomes.

2. It assumes a rather naive view of the unemployment-inflation trade-off and makes no mention of the recent "rational expectations" mini-revolution in economic theory, which has profound implications for unemployment and inflation policy.

3. It looks at macroeconomic outcomes rather than movements in monetary and fiscal policy instruments. Only the latter, of course, are controlled directly by political officials.

In subsequent work I have tried to make some progress on these problems. I have had distinguished company. Important recent studies by James Alt, Andrew Cowart, Heino Fassbender, Bruno Frey, Robert Gordon, and Edward Tufte, among others, have advanced the field significantly.

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References


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The Meaning of Property Rights

TO THE EDITOR:

It is with great concern that I read Norman Furniss’ "The Political Implications of the Public Choice-Property Rights School" in the June 1978 issue of the Review. While I agree with many of Furniss’ specific criticisms, it must be pointed out that they apply more to the new welfare economics than to property rights analysis as a whole. Furniss’ restricted interpretation of property rights analysis may...
discourage further investigations of a conceptual framework that can make potentially important contributions to the analysis of political phenomena. Two problems are apparent in Furniss’ analysis.

First and most importantly, Furniss restricts the definition of property rights analysis to include only the new welfare economics. This restricted interpretation is clear in the section headings of the article where Furniss jumps from property rights-public choice to the new welfare economics as well where he uses them interchangeably in the text. This would not be too important in and of itself, but Furniss also interprets the work of others as if the two terms were interchangeable. He notes that, “for Samuels the impetus of the school was ‘the effort to make welfare economics safe for the market system’” (Furniss, 1978, p. 400). Furniss’ truncation of Samuels’ sentence is unfortunate. The original sentence began, “the ascendency of the new welfare economics, while rooted to some unknown extent in an effort to make welfare economics safe for the market system, was grounded in more technical concerns ...” (Samuels, 1972, p. 93). Aside from ignoring Samuels’ rather substantial qualification, Furniss also seems not to realize that Samuels was referring to the new welfare economics, not property rights analysis. More to the point, Samuels’ treatment of property rights is certainly not the same as his treatment of the new welfare economics (Samuels, 1972, p. 67; 1973).

The question is, to what extent does the new welfare economics overlap with property rights analysis? Unfortunately, two schools of economics employ property rights analysis. The first starts with neoclassical orthodoxy and grafts on considerations of property rights. It is to this school, better identified as new welfare economics, that Furniss’ criticisms apply. The second approach starts with the critique of neoclassical analysis rooted in the institutionalist approach of John Commons (Samuels, 1973). Furniss’ criticisms do not apply to this approach. All of the criticisms made by Furniss have been made before by writers in this second school. Samuels and Schmid have identified and critiqued the normative biases of neoclassical analysis and its reliance on the Coase and Pareto rules (Samuels, 1972; Samuels and Buchanan, 1975; Schmid, 1978). In these works one finds consideration of the distributive consequences of rights modifications, a broader interpretation of externalities, and a ques-

Furniss notes that “the refurbished classical economic theory ... has not gone uncontested within the discipline of economics. Some have disputed the effectiveness of market solutions; others have attacked the neglect of any consideration of the structure of social power” (1978, p. 401). But Furniss fails to recognize that most of those criticisms have come from within the property rights school by those who would consider themselves property rights analysts.

Second, given that Furniss stresses the normative biases of property rights analysis, his own biases must be questioned. Furniss contrasts the property rights school to Pigouvian analysis and by rejecting property rights implicitly embraces Pigou as a guide to public policy. Furniss properly quotes Daly’s criticisms of Coase’s challenge to Pigou, but fails to note Daly’s conclusion that Coase legitimately called into question the normative biases in Pigou. Daly concludes that:

Writers seeking to justify or deny the relevance of collective intervention of markets characterized by external effects should concentrate their efforts not on the Coase theorem per se but rather on his view that, in practice, collective intervention may only worsen matters. As he notes, “all solutions have costs and there is no reason to suppose that government regulation is called for simply because the problem is not well handled by the market or the firm.” Specifically, the likelihood of market failure does not imply government success (Daly, 1974, pp. 211–12; see also Schmid, 1978).

Furniss’ implicit acceptance of Pigou and his begging the question of the dangers of big business suggest that he is not concerned with normative biases per se but with which normative biases govern political science research.

I do not know whether Furniss’ criticisms arise from political science’s unfamiliarity with the second strain of property rights analysis or the xenophobia that political science occasionally exhibits. More likely the problem is simply the diversity of the literature that falls under the heading of property rights analysis. The label “property rights” may not be useful as it includes elements of both neoclassical and institutional thought. Regardless of the cause, Furniss’ article may discourage fuller consideration of property rights analysis. This would be unfortunate, as the second stream of the property rights literature may contribute to our understanding of politics. Most importantly, property rights analysis as practiced by institutionalist economists brings to the center of attention the inherent relationships between structure, behavior and performance, linkages
that are often lost in the overly compartmentalized approaches of political science.

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References


TO THE EDITOR:

I am always pleased to respond to people who take the time to comment on my work. I should advise the reader at the outset, however, that David Lowery's criticisms have only a tangential relation to the main focus of my article which is, to recall, "the political implications of the public-choice-property rights school" (my emphasis). My reply must be similarly removed from this central concern.

Lowery has three complaints, two specific and one general. The first specific charge is that I do not quote the complete sentence from which I take Samuels' phrase "the effort to make welfare economics safe for the market system." He is correct. Indeed, the sentence is so long—158 words with substantive footnote, 108 words without—that Lowery does not quote it in its entirety either. Whether this action is improper, however, depends not on whether Samuels says more than I cite (which he does), but on whether what I cite misrepresents one of the things he says. The latter I do not think can be sustained. Whether Lowery does or not is hard for me to determine. In any case, I quote the final three sentences from Samuels' discussion of the "Coase Rule" (1973, pp. 145, 146) which demonstrates I believe that I have accurately portrayed a major theme of his paper:

It is part of the natural history of social myths (including scientific doctrine serving as social myths) that they tend to be used wherever they will accomplish desired ends, in this case the further transformation of welfare economics to make it safe for capitalism. The Coase Rule is but one example of how the new welfare economics has chosen its assumptions and basic paradigm in such a way as to rationalize the market economy of a particular society, a return to an old tradition which Pigou dared to challenge and interrupt. Optimality in such a context is quite relative indeed.

Which brings us to the second specific accusation, that I am a secret follower of Pigou. To this indictment I plead innocent. I had thought that in my section on "the challenge of Pigou" I made clear the powerful counterarguments of Coase and Buchanan in particular. My final section in this section reads (Furniss, 1978a, p. 401), "In short, market solutions are reimplanted at the center of welfare economics." How this sentence can be read to mean that they are not reimplanted I do not know. But if others were confused I am glad to be able to clarify the issue, especially because the central focus of my article is based on the proposition that the public choice-property rights school has succeeded in undermining Pigou's propositions. Else the perspective would have little standing, and I have no interest in discussing bodies of thought of minor importance.

Now I turn to Lowery's general message. He begins by positing two streams of property rights literature, neoclassical and institutional, and then concludes that in addressing only the former I sow confusion (leg us pass over "xenophobic" as a rhetorical flourish) in the minds of political scientists. Perhaps because of space limitations, this position is not developed (nor is its proposition defended) very systematically. In an effort to inject some order, I would call the criticism significant were any one of the following three conditions found. (To anticipate, none are.) These conditions are as follows:

A. That I make distinctions not present in economic literature (that is, the two streams form a single river). This is the most straightforward interpretation of Lowery's letter, and it is the one most easily disposed of. To begin with, my use of "public choice-property rights" was designed to remove any ambiguities about what I was covering, which is more, secondly, than is generally thought necessary. For example, in the references following their widely cited article in the Journal of Economic Literature,
entitled simply "Property Rights and Economic Theory: A Survey of Recent Literature" (1972), Furubotn and Peijovich list 19 separate works by either Alchian, Buchanan, Coase or Demsetz. There is one reference to a work by either Ayres, Commons, Samuels or Schmid. Similarly, they list 14 articles from the Journal of Law and Economics and none from the Journal of Economic Issues. Obviously, this is not to say that Ayres, Commons, Samuels, Schmid et al. have not done or do not do good work, merely that their contributions are seen as distinct. Finally, I had thought that this distinction was accepted (with my qualifying "public choice-property rights" label) among institutional economists as well. I refer Lowery and interested readers to Goldberg (1974).

B. That although public choice-property rights and institutional economics should be separated analytically, the position of the latter regarding the concept of property must be spelled out in any discussion because it is the unique, and viable, alternative (that is, there are only two streams). This notion can muster little support. I refrain from quoting numerous citations from Aristotle, Locke, Marx etc., each of whom has much that is valuable to say on the concept and rights of property, and mention only current theorists. Here we find property rights an essential element in the thought of figures as diverse in their normative conclusions as Rothbard, Nozick, Buchanan, Rawls, and Macpherson. (I pick these because they happen to be discussed in a very useful paper by Lehning, 1978.) We also find well-developed political positions based on a rigorous delineation of appropriate and inappropriate property rights. The most impressive effort, I feel, has been made by a group I term "functional socialists," whose arguments can be traced to R. H. Tawney and Ernst Wigforss (see Furniss, 1978b). Let us add that institutional economists contribute many insights into the nature of property rights as well; Lowery provides a list of references. The point, of course, is that all these contributions are best treated in separate papers or articles (as indeed I have done) or in a monograph detailing the implications of divergent perspectives on the concept of property. In this connection, a piece by Lowery that would argue that institutionalist economists "may contribute to our understanding of politics" might well be in order. But it is neither necessary nor relevant for me in the midst of a discussion of the political significance of the public choice-property rights school to pluck from among the myriad formulations of the concept of property those of institutional economics for sustained treatment. Unless it is claimed. . .

C. That although distinct from the public choice-property rights school and only one of many perspectives on property, institutional economics has so eclipsed "neoclassical" economics that (to mix metaphors) institutional economics has become a mighty river while neoclassical economics has been reduced to a small creek. In this case one might declare (as Lowery at times seems to do) not that I should have incorporated both perspectives but that I have written the wrong article. I will not belabor the obstacles facing anyone attempting to argue seriously for this proposition, as opposed to merely stating it. Fortunately, we need not investigate what might be meant by the statement that "property rights analysis as practiced by institutionalist economists brings to the center of attention the inherent relationships between structure, behavior, and performance." There is no need for invidious comparison; for our purposes it is enough to note that the importance and intellectual influence of the public choice-property rights perspective can be exaggerated only with difficulty—the pages of this Review, for example, are full of debts, both implicit and explicit, to its basic conceptual thrust. Briefly, I refer the reader to the able presentation of Ostrom (1977). See also his citations. I would be glad to provide further references.

In sum, I continue to feel that my delineation of the public choice-property rights perspective is appropriate and that my argument is potentially significant. (Since Lowery states that my "criticisms apply" to the school, his letter does not offer much opportunity to say more than this.) I conclude on a conciliatory note. Scholars working in both the traditions of public choice-property rights and institutional economics share a disdain for mere "data manipulation" and a concern for elaborating the essential issues of economic and political life. James Buchanan puts the matter very well, and it is proper that he should have the final word (1976, pp. 167, 168):

The hard questions are not readily formulated in terms of testable hypotheses. But this offers no cause for not thinking about such questions, for not discussing them, for not searching for an appreciation and understanding. . . . We do not "solve" the "problem" of social order by producing a unique "solution," regardless of the sophistication of empirical techniques. There is no objective "truth" to be established here. The "problem" of social order is faced eternally by persons who realize that they must live together and that to do so they...
must impose upon themselves social rules, social institutions. Economics and economists cannot evade their responsibility in the continuing discourse over such rules and institutions by shifting attention to trivialities. To the extent that they do so, their functional roles can only be filled by the charlatans and the fools, whose presence around us requires no demonstration.

NORMAN FURNISS

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References


TO THE EDITOR:

As a person who once wrote a dissertation on Locke, I was intrigued to read Robert H. Horwitz's review of the Clarendon edition of Locke's Essay Concerning Human Understanding (APSR 72:651–52). I was especially struck by the account of the difficulties Locke had with printers. Given Horwitz's statement that "the Clarendon edition has been suspiciously inaugurated" it seems that the spirit of the seventeenth century lives on in printing as well as in philosophy.

JOHN O'CONNOR

American Philosophical Association

EDITORIAL NOTE

The following articles have tentatively been scheduled to appear in the June, 1979, issue:

David O. Sears, Carl P. Hensler and Leslie K. Speer, University of California, Los Angeles, "Whites' Opposition to 'Busing': Self-Interest or Symbolic Politics?"
Michael Johnston, University of Pittsburgh, "Patrons and Clients, Jobs and Machines: A Case Study of the Uses of Patronage"
Richard L. Cole, George Washington University, and David A. Caputo, Purdue University, "Presidential Control of the Senior Civil Service: Assessing the Strategies of the Nixon Years"
Edward T. Jennings, Jr., State University of New York, Buffalo, "Competition, Constituencies, and Welfare Policies in American States"

David John Gow, Rice University, "Scale Fitting in the Psychometric Model of Judicial Decision Making"
Arend Lijphart, University of California, San Diego, "Religious vs. Linguistic Class Voting: The 'Crucial Experiment' of Comparing Belgium, Canada, South Africa, and Switzerland"
David M. Lampton, Ohio State University, "The Roots of Interprovincial Inequality in Education and Health Services in China since 1949"
William Pang-yu Ting, University of Michigan, "Coalitional Behavior among the Chinese Military Elite: A Nonrecursive, Simultaneous-Equations, and Multiplicative Causal Model"