The State of the Art in Welfare State Research on Social Spending Efforts in Capitalist Democracies since 1960

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In the last generation, social scientists have made great advances in understanding the forces behind the welfare state—or public social spending and provision. Scholars have asked why those aided by the state get what they do in the ways that they do for a number of circumstances affecting income and life chances. Typically, the object of study has been the state’s efforts through spending or services to ameliorate routine and foreseeable predicaments that threaten income, such as those caused by old age, unemployment, ill health, disability, and industrial accidents. Often scholars have widened the focus to include programs aiding persons with family obligations and citizens having served in the military. Sometimes housing, nutritional, and educational needs have been included, expanding the definition of the welfare state to encompass almost all domestic public spending.

Researchers have explored many empirical settings to derive and appraise arguments about public social provision. Influential early work (e.g., Marshall 1963; Titmuss 1958; Peacock and Wiseman 1961) focused on post–World War II Britain’s adoption of comprehensive public social provision and the term the “welfare state.” These studies argued that the inevitable expansion of citizen rights or, alternatively, social solidarities forged in war promoted public social provision. Soon, however, social scientists began quantitative, cross-sectional studies of all the countries of the post–World War II world (e.g., Cright 1965; Wilensky 1975) and typically found that socioeconomic “modernization,” notably industrialization and the aging of the population, underlay social spending progress. In addition, U.S. states were used as laboratories to test propositions about social policy—mainly whether economic modernization or

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political “pluralist” factors like electoral competition promoted policy innovation and spending (e.g., Dye 1966; Sharkansky and Hofferbert 1969; Wright 1974). Inspired in part by Piven and Cloward (1971), later American studies focused on the impact of social protest on public policy (e.g., Isaac and Kelly 1981; Hicks and Swank 1983). Beginning in the 1970s, there have been numerous case studies and close comparisons of the policy trajectories and fates of a few carefully chosen, rich nations (e.g., Rimlinger 1971; Heclo 1974; Flora and Heidenheimer 1981; Orloff and Skocpol 1984; Esping-Andersen 1985, 1990; Castles 1985; Amenta and Skocpol 1988; Baldwin 1990; Orloff 1993). In these later studies, economic factors were held relatively constant and divergences in the adoption of policy innovations and in the form of social spending systems have been explained by a range of factors, especially aspects of the state and political institutions such as the form and nature of bureaucracies and party systems.

The main proving ground for theories of public social spending, however, has been the postwar experience of about 20 wealthy, democratic, capitalist countries. Begun in the middle 1970s (e.g., Stephens 1979), this work has centered on explaining social spending efforts (see Wilensky 1975). By effort researchers have meant the share of income that a country devotes to public social provision. In the last decade, Sweden, Netherlands, and Denmark have stood near the top and the United States, Japan, and Australia near the bottom in rankings of the social spending efforts of rich democracies. According to the International Labor Organization (ILO) measure of public “social security” spending—old-age pensions, medical care, unemployment and workers’ compensation, family allowances, public assistance, and related categories—Sweden, in 1982, devoted approximately 32% of its gross domestic product (GDP) to public social provision and Japan only about 10% (see Hicks and Misra, table 3 above). The countries that have been among the leaders in the early 1980s have also seen great increases since 1960, with Denmark more than tripling its spending efforts. Yet some countries that are ranked relatively low in 1982, such as Japan and the United States, have seen strong increases in social spending activity since 1960, while the efforts of Australia, New Zealand, and Switzerland have remained relatively stagnant.

Explaining continuities and changes in postwar spending efforts among capitalist democracies has been important for a number of reasons. Unlike all the countries in the world, the rich capitalist democracies have largely similar economic and political systems, which provides boundaries for empirical propositions. Unlike the states of the Union, rich capitalist democracies vary greatly on numerous economic and political dimensions—economic health, openness of economies, type of political
institutions, variety of political party systems and regimes, level of social protest, and so on—as well as on social spending outcomes. Just as important are the excellent and voluminous data collected in the postwar period on these countries by various international organizations, especially the Organization for Economic Cooperation and Development (OECD) and the ILO. These economic and political data have been supplemented by the industry of various researchers, who have compiled data from the individual countries. This work has brought an expansion of the number of cases and thus the degrees of freedom available to test simultaneously numerous propositions.

This informational territory has often resembled a battlefield, with researchers sending waves of hypotheses across it, hoping to claim sovereignty for one theory or another (see Skocpol and Amenta 1986). Typically, the rule of left-wing or “social democratic” political parties has emerged triumphant with the greatest explanatory power from regression tests (see, e.g., Shalev 1983). Nevertheless, factors derived from socioeconomic (e.g., Wilensky 1975), statist (e.g., DeVinney 1983), pluralist (e.g., Pampel and Williamson 1988), social insurgency (e.g., Myles 1989; Hicks, Swank, and Ambuhl 1989), and Christian democratic (e.g., Wilensky 1981) theories have each won the occasional explanatory battle. No theory has yet won recognition as the sole legitimate solution, because proponents of theories that lose a test often claim that their ideas were unjustly undermined by unfair operationalizations or inappropriate analyses.

CURRENT RESEARCH

The papers included here by Alexander Hicks and Joya Misra and by Evelyne Huber, Charles Ragin, and John D. Stephens surpass previous studies, taking a more comprehensive, even-handed, and environmentally conscious approach to this contested empirical terrain. Each employs a large base of cases—more than two decades’ worth of yearly data on 17 or 18 of the 19 rich, capitalist countries that have been democracies since World War II. In this respect, the industry of these researchers is unmatched. Both articles provide inclusive and integrated theoretical arguments about public spending outcomes and both “go the methodological distance” to resolve contradictory findings rather than single-mindedly dispatching theoretical enemies. In both articles, the authors examine theoretical arguments and propositions derived from case studies and “small-n” comparative analyses of public spending innovations, as well as recycling arguments and measures from quantitative postwar analyses. Both attempt to make sense of previous research and to win confidence in current efforts by employing several sophisticated method-
ological and empirical strategies—especially by using multiple indicators of outcomes and different techniques of estimation. Huber et al. call explicitly for a resolution of conflicting findings in this literature, and Hicks and Misra have the same end in view. Because both groups have amassed more information than previously employed and because both are well versed in this literature and in quantitative methods, their goal is within reach.

Although the authors from both groups have been associated with the social democratic and other political conflict models, these studies included here transcend earlier loyalties. Both articles attempt to make sense of politics institutionally, a concern typical of small-\(n\) studies of social policy origins and researchers of the U.S. welfare state, such as Skocpol (1992) and Mayhew (1986). Hicks and Misra do so with their "infraresources"—or political situations structured in ways that might aid different political actors. Huber et al. provide an innovative conceptualization and measurement of long-term constitutional structure, which, they argue, influences the ability of political regimes to enact policy. In both articles, the authors attempt to measure statist legacies—"bureaucratic paternalism" for Hicks and Misra, "authoritarian legacies" for Huber et al. And both examine economic factors, a concern of early quantitative research.

The articles have different emphases. Hicks and Misra’s contribution centers on the political resources theory, which is reminiscent in sweep and scope to McCarthy and Zald’s (1977) resource mobilization theory of social movements. Hicks and Misra create theoretical bridges between contending political models, joining statist, pluralist, social democratic, and social protest propositions in a single framework. These authors make the plausible case that regardless of their diverse origins these hypotheses are not bound to incommensurable paradigms. Hicks and Misra distinguish different political actors and their bids for influence and connect them with the concept of resources. This model is pluralist in the widest sense, including more than narrow interest groups, and, in ranging beyond class actors, the theory does more than augment Korpi’s (1989) “power resource” theory.

Huber et al. are chiefly concerned with making sense of previous research and do so in part by rounding up, without roughing up, the usual suspects among independent indicators. They also examine a number of different measures of public spending efforts and employ different estimation techniques. Huber et al. follow but go beyond Castles (1982), who conducted cross-sectional examinations of the correlations between numerous independent measures and welfare state outcomes. Like Hicks and Misra, Huber et al. examine the ILO-based measure of welfare state effort (see above). In addition, Huber et al. employ regression analyses
of a more restricted OECD count of transfer payments and a wider measure of the overall government revenues. They also examine correlations between key independent measures and measures of social rights, posttax and posttransfer income inequality, and economic redistribution. In their regression analyses, Huber et al. compare OLS results with models correcting for country-specific errors, models correcting for time-specific errors, and models correcting for both. This methodological strategy illuminates stable differences among countries—as did early cross-sectional research—in addition to exploring the dynamic processes and changes in social spending efforts on which analyses with many time points have centered.

This does not mean that Hicks and Misra settle for one measurement strategy or that Huber et al. dodge theory-building. Hicks and Misra's article examines ILO's public social security spending as a share in GDP, but they also look at percentage-change scores in this measure and in independent measures. This latter specification helps them elude standard statistical problems. In addition, they explore numerous statistical interactions, which, among other things, serve as tests for the robustness of their findings. Similarly, the Huber et al. article has theoretical bite in arguing that theories of public social provision identify not only specific causal forces, but also particular outcomes, accounting in part for conflicting findings. For instance, the social democratic model seeks to explain public spending outcomes that liberate wage earners from the power of capital—the "decommodification" of their labor power (Esping-Andersen 1991) in the current social science vocabulary—not just general transfer payments. By contrast, Catholic party regimes should boost transfers and other social programs insofar as they do not threaten much redistribution.

THE FINDINGS

With such agreement in theoretical outlook, methodological inquisitiveness, operational fairness, and concern for closure, it comes as no surprise that similarities in key findings abound. Despite somewhat different operationalizations, both articles report a strong influence of left-wing government—the standard predictor of the social democratic model—on different forms of spending efforts. In their investigations, Huber et al. find, as expected, that left-wing governments have large effects on government revenues and redistributive spending—though the data for the latter are sketchy—as well as smaller effects on welfare state efforts through transfer payments. Hicks and Misra find a similar positive influence of left-wing government on their dependent measure, in both incarnations.

All the same, both groups find strong evidence of statist and socioeco-
nomic influences. Huber et al. find negative effects across the board for political or constitutional structures that hinder slim electoral majorities from enacting policy and that abet obstructionism from small groups. Both articles report a positive effect of voter participation on most dependent measures, which suggests that practices and rules easing the registration of voters and the exercise of the franchise spur social spending. In addition, Hicks and Misra find these statist catalysts to social spending efforts: government revenues as a share of GDP, bureaucratic paternalism, and, to a lesser extent, state centralization. As for socioeconomic factors, each paper finds numerous stimulants to spending: gross domestic product per capita, the percentage of aged citizens, the unemployment rate, and the price level.

Even when the focus is the standard ILO measure of social security efforts, however, the two perspectives sometimes are at odds. Notably, Huber et al. find that Christian democratic parties drive spending, but Hicks and Misra find no robustly positive effect of "center" parties. Moreover, Hicks and Misra find that state centralization increases efforts, but Huber et al. do not. On another dimension of the state, Hicks and Misra’s bureaucratic paternalism proves significant, but Huber et al.’s authoritarian legacies do not. There are also inconsistent results concerning protest: Hicks and Misra find that strikes and other working-class protest increase social spending and petty bourgeois protest slows it. Huber et al. find no particular impact of strikes.

Some divergent results seem reconcilable. Huber et al.’s measure of constitutional structure overlaps with Hicks and Misra’s measure of state centralization, perhaps accounting for the poor performance of Huber et al.’s centralization measure. Hicks and Misra’s measure of bureaucratic paternalism combines a number of factors, one of which captures state bureaucratization, while the Huber et al. measure of authoritarian legacies concerns mainly historical hindrances to parliamentary institutions—a less likely catalyst for social spending. Huber et al. provide a plausible explanation for why Catholic party governments more strongly increase welfare state transfer spending than other parties often designated “centrist.” This calls for a rethinking of the reasoning (see Castles and Mair 1984) behind the labeling of partisan political parties. Why findings on labor strikes are so divergent is less clear, though protest of all kinds may have greater impacts in noncorporatist political systems or in periods of economic decline, as suggested by Hicks and Misra.

CRITICISMS AND SUGGESTIONS
Impressive as they are, these articles are not above criticism, and each group has much further work to do on its research agenda. And even
with capitalist democracies, the richest dispensers of data, and even with the most generous-minded of researchers, informational limitations still hinder the appraisal of some theoretical claims.

Because of its greater theoretical ambition, Hicks and Misra's article is subject to more criticism, much of which is acknowledged in the article's informative footnotes. In this initial statement, the theory is not fully worked out, and the authors must decide what is included in it. For instance, the authors second-guess themselves on the role of close electoral competition, which is sometimes claimed, by way of "pluralist" argumentation, to advance the cause of social spending. Close elections are either a type of political resource or they are not. Simply because a measure has proved important in previous research (e.g., Pampel and Williamson 1988) is no good reason to incorporate it in the political resources theory. In any case, the proposition seems program- or welfare state-specific. One would expect the median voter to favor "solidaristic" or inclusive programs—that is, programs that might benefit the median voter. Moreover, at the heart of political resources theory are interactions between actors and different forms of resources. Although their analysis of the influence of actors and resources in corporatist and noncorporatist political systems is suggestive, the model demands more sustained thinking about and specification of these contextual effects—perhaps program by program.

Hicks and Misra also need to distinguish better what is excluded by their theory. Propositions and measures logically linked to alternative theories are sometimes annexed to the political resources model. For instance, profits are considered an appropriate conceptualization of the political resources of capital. Yet to remain consistent with its conceptualization of other group actors, the theory should specify resources tied to political organizations of the Right—conservative political parties and capitalist-led political organizations such as the U.S. National Association of Manufacturers. Profit fits more easily into an economic theory of spending. Similarly, such indicators as economic growth and the percentage of aged citizens seem better suited to socioeconomic theories, but are here pressed into service as measures of political resources. As the authors note, they do not intend to provide a critical test of the theory, but they should ponder what might constitute one. An obvious alternative is a strictly socioeconomic model.

The strength of the Huber et al.'s discussion is its many analyses of different welfare state efforts, but they sometimes oversimplify complex issues. In particular, their use of revenues as simply another measure of welfare state effort diverges from Hicks and Misra's treatment of it as an independent "infraresource" for spending efforts, for which it proves
a robust predictor. Taxation is a political issue in itself, and the form of
most taxation systems were decided before 1960. These systems may
have peculiar effects on revenue generation for reasons having little to
do with public social spending policies. Perhaps revenues should be mod-
eled as an intervening variable. Given their desire to resolve conflicts in
previous research, it is difficult to fault Huber et al. for analyzing a
measure that others (e.g., Cameron 1978) have employed as an indicator
of the welfare state, but the strategy proves more rewarding for more
straightforward outcomes.

Some divergences in interpretation suggest that too many theoretical
propositions are being represented by too few measures. Hicks and
Misra, following standard pluralist theory, argue that the percentage of
aged citizens in the population and the unemployment rate indicate the
political influence of the elderly and the unemployed as groups. By con-
trast, Huber et al. more plausibly view the percentage of aged citizens
and the unemployment rate as controls, measures that isolate not the
organized presence of the elderly or unemployed, but the character of
social legislation, which typically grants benefits automatically to those
of specific age or employment statuses. To appraise the political resources
alternative would mean to create corresponding “interest group” mea-
sures, which in turn would require extensive investigation and data col-
lection.

Similarly, the agreement about the impact of economic factors is not
matched by a consensus over their meaning. Hicks and Misra are con-
cerned with economic factors in part because they have an impact on
political resources and in part because they reflect profits, which are not
easily measured, but are expected to increase spending efforts. By con-
trast, Huber et al. argue that economic level indicates position in the
world system, separating core countries from semiperipheral ones, and
their analysis demonstrates that cross-sectional differences in income are
more important than changes over time in explaining social spending
efforts. However, all 17 countries had presumably reached the core by
1960, and thus their comparisons seem to be within the core rather than
between the core and the semiperiphery. It is difficult to see how either
group would counter arguments by logic-of-industrialization theorists.
Measures sometimes converge on themselves, hampering parameter esti-
mation and interpretation. As pointed out by Huber et al., left-party
government is highly correlated with the percentage of aged citizens, an
inexplicable association between key economic and political factors. As
Huber et al. note and judiciously consider, the many correlations between
open economies, corporatism, left-party strength, and union density also
hinder multiple regression.
Statist propositions suffer disproportionately from data limitations. Huber et al.'s dismissal of some statist theoretical propositions seems based chiefly on roadblocks in data acquisition. The OECD publishes numerous economic statistics, the ILO publishes labor movement figures, and each country's political history is an open book. It is more difficult to test propositions about the role of bureaucrats in public spending areas because no international organization—or independent researcher—has seen fit to collect systematic information about them in theoretically relevant ways. In addition, Huber et al.'s main political institutional measure, constitutional structure, is open to contending interpretations. A federal system may indicate not only the veto possibilities of interest groups over policies as Huber et al. maintain, but also a conflict over authority among bureaucracies in charge of public social spending, as suggested by Hicks and Misra.

WHAT THIS KIND OF RESEARCH CANNOT DO

The authors have done us a great service in going far toward ending the conflict in quantitative studies of spending efforts in postwar capitalist democracies. Yet these articles are far from resolving the larger controversy over the determinants of public spending policy, and the main limitations of these articles have to do not with their few internal imperfections, but with the inadequacies of this type of research. The history of rich capitalist democracies from 1960 through the early 1980s is far too restricted a pool of experience, and social spending effort is far too narrow an outcome, to make strong generalizations about social spending policy and to answer questions researchers have been asking.

Overall spending efforts are of interest only to the extent that they capture phenomena sociologists want to understand. As Huber et al. demonstrate, it is difficult to get systematic evidence about the redistributive disposition of these policies, not to mention their effects. Investigating quantitatively the quality of different programs and systems requires entirely different dependent measures (see, e.g., Myles 1989; Ragin, forthcoming), which are typically unavailable for the number of time points used for overall spending efforts and required for strong quantitative tests. The research agenda of the present and future may impair the use of overall spending as an indicator of policy. Analyzing spending efforts for the elderly is reasonable, as pensions are generally relatively unrestricted cash payments, but spending efforts may not capture what we want to know about other programs. Notable here is health care, where larger layouts in money may indicate little about the quality of care, citizens' rights, or the decommodification of labor power.
Moreover, very few of these countries developed new and large social spending programs since 1960. Thus this type of research cannot explain critical phenomena such as the timing and content of innovations in public spending. Insofar as general theories of public policy aspire to explain the initiation of policies as well as incremental increases in them, this research site has little to offer. As Hicks and Misra point out, studies such as theirs focus on incremental changes in already existing public policies—not innovations or why some policies get on or stay off the political agenda. This sort of criticism was applied to early pluralist studies of political power (e.g., Lukes 1974). Research on postwar spending efforts cannot answer fundamental questions concerning why the systems are constructed the way that they are and what accounts for gaps in the networks of programs and innovations.

Huber et al. overreach in discussing what they misleadingly call "the quantitative research on the welfare state," when they mean quantitative research on spending efforts of rich, capitalist democracies since 1960. There have been numerous quantitative studies of groups of industrialized countries prior to the Second World War—a period that has the advantage of being able to address why countries adopted programs when they did (e.g., Hage et al. 1989; Schneider 1982; Collier and Mesick 1978). There have also been quantitative examinations of subnational units with extensive control over social spending policies, the states of the Union in particular (e.g., Amenta and Carruthers 1988; Steinmetz 1990; Skocpol et al. 1993). If they are to avoid becoming historical footnotes, theories of public spending will have to be configured to explain vast differences in old-age pensions between Arizona and Arkansas from 1935 to 1955, as well as differences between Sweden and Switzerland from 1960 to 1980.

Single-country experts may find inadequate the explanations provided by quantitative research on postwar capitalist democracies. For instance, this research does not much illuminate the impetus behind historical innovations and advancements in U.S. public spending policies (see, e.g., Skocpol 1992) or differences among states of the Union. As both articles show, U.S. spending efforts are hindered by its political institutions and restrictive practices concerning voter registration and voting. Yet these cross-national studies single out what is lacking rather than what is present in the U.S. setting. Social democratic and Catholic parties, isolated as cardinal forces behind social spending by Huber et al., are absent in the U.S. setting. Hicks and Misra suggest routes to social spending gains through interest group activity and protest, which, in noncorporatist polities such as the United States, are presumed to be inferior functional equivalents to left-wing regimes elsewhere. This seems at best incom-
plete. Strike activity has little influence on social spending in the states (Amenta and Poulson 1992), for instance, and the evidence about the impact of other forms of mass disruption is uneven (see Skocpol and Amenta 1986).

The authors have done us a great service in going far toward resolving the controversy over conflicting findings in quantitative studies of spending efforts in postwar capitalist democracies. In the next generation, advances in knowledge will continue to be made by case studies, close comparisons, qualitative comparative analysis, and quantitative analysis. Case studies and close comparisons of countries have been and will remain the main means for understanding the development of public spending policies and have provided numerous propositions and hypotheses for examination in quantitative data sets. It seems likely that only comparative historical research can untangle issues of causality for which quantitative indicators are too highly correlated to interpret, as in the examples given by Huber et al. Qualitative comparative analysis (Ragin 1987), a relatively unexploited technique, seems suited for examining the kinds of interactions of actors, interests, and resources indicated by the political resources theory. In addition the method is better equipped than pooled time-series regression to assess causal heterogeneity and contextual effects—the likelihood that there are different routes to social policy gains and that actors and collective action will be differentially effective according to the political or economic setting. More quantitative research is needed, but mainly on new units of analysis, in different time periods, and sometimes focusing on the quality and efforts of individual programs.

To advance knowledge it is best to shed the attitude that propositions developed in other settings require “testing” on this data base to prove their worth. The overreliance on one kind of information brings to mind the old story about the man who has lost his keys in the night. As he searches for them beneath a streetlight, a passerby approaches, hears the story, and asks the searcher if he is certain that the key chain is nearby. The man admits that he thinks the keys were lost on the next block, but the “light is better here.” The analogy is not exact, but by combining their own considerable power with what has been made available by international organizations, the authors of these articles have uncovered almost everything there is to find with these data. In the future, scholars of the welfare state will have to generate more of their own light in more empirically shadowy places, by way of data collection in different time periods and settings and by innovative research using all techniques in order to find the answers to questions about the origins, character, and quality of public policies.
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