Political Partisanship and Welfare State Reform in Advanced Industrial Societies

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In this article we evaluate two claims made in recent studies of the welfare states of advanced industrial societies: first, that welfare states have remained quite resilient in the face of demands for retrenchment; and second, that partisan politics have ceased to play a decisive role in their evolution. Addressing the first claim, we present analysis from a new data set on unemployment insurance and sickness benefit replacement rates for 18 countries for the years 1975–99. We find considerably more evidence of welfare retrenchment during the last two decades than do recent cross-national studies. Second, we examine the "end of partisanship" claim by estimating the effects of government partisanship on changes in income replacement rates in sickness and unemployment programs. Our results suggest that, contrary to claims that partisanship has little impact on welfare state commitments, traditional partisanship continues to have a considerable effect on welfare state entitlements in the era of retrenchment.

According to several recent cross-national studies, partisan politics have ceased to play a decisive role in the evolution of the welfare state. While many studies have suggested that the political power of social democratic and Christian democratic parties played an important role in the *expansion* of the welfare state (Esping-Andersen 1985; Garrett 1998; Hicks 1999; Hicks and Swank 1992; Iversen and Cusack 2000; Korpi 1989; Shalev 1983; van Kersbergen 1995; Western 1991), similar accounts, however, find little evidence of this partisan effect since the late 1970s (e.g., Castles 1998a; Huber and Stephens 2001a, b; Ross 2000).

This discrepancy can perhaps be seen most starkly in Huber and Stephens's recent assessment of partisan effects before and since the early 1970s (2001a). From 1960 to 1972, Social Democratic rule is strongly associated with increased transfer and consumption expenditure and with increased government employment, which has been overwhelmingly for "welfare" services like education, day care, health care, etc. But, they conclude:

> . . . *the overall pattern is one of a sharp narrowing of political differences in the 1980s.* Our interpretation is that this was a result of a shift in the political agenda: Once it was realized that the game had fundamentally changed as a result of the sea changes in the world economy, governments found themselves with dramatically fewer options. This contributed to shifting the politics of social policy to defending entitlements. (2001a, 221, emphasis added)[1]

More stridently, Paul Pierson has suggested that partisan theories of welfare expansion should no longer be expected to have much applicability, since "retrenchment is not simply the mirror image of welfare state expansion (1996, 151)":[2]

[1]Elsewhere Huber and Stephens appeal to Pierson's notion that the party effect is not picked up because partisan effects are constrained by "vested interests" on the right and "structural economic pressures" on the left (2001b, 125).

[2]Though they have not been recognized as such in the literature, the implications of Pierson's approach are very hard to reconcile with partisan theories of expansion. A more extended discussion of this can be found in Allan and Scruggs (2002).

A straightforward application of the power resource arguments to retrenchment would suggest that welfare states are in deep trouble. The power of organized labor and left parties has shrunk considerably in many advanced industrial societies . . . [H]owever, there is very little evidence that this decline has had a fundamental impact on welfare states. (1996, 150)

Is it really true that the welfare state has been as resilient as Pierson implies? And is it the case that partisanship tells us little about changes in the welfare state (be they large or small) in the last quarter century? In this article, we submit that such claims have run well ahead of the empirical data to support them. To address the first question, scholars continue to rely on aggregate or program-specific spending data to suggest that advanced democracies have not reduced welfare commitments. As we detail below, the appropriateness of these widely used spending data has been called into question for some time. Therefore, we reevaluate retrenchment over the last two-and-a-half decades using a new 18 country, cross-temporal data set of welfare state programs.[3] Employing these data we find considerably more evidence of welfare retrenchment.

Turning to the second question, we estimate the effects of government partisanship on our new measures of welfare state change. In doing so we confront what Green-Pedersen calls the "dependent variable problem"—how to operationalize the welfare state (Green-Pedersen 2002, n.d.)—and highlight weaknesses associated with the existing measures of welfare state generosity.[4] It is our contention that the use of welfare state entitlements data is essential for furthering our understanding of welfare state change. It has long been acknowledged that entitlements data, such as the kind we provide here, are a more appropriate measure of the distributional goals of the welfare state than is the absolute size of the welfare state. Contrary to claims that partisanship no longer matters, we find that partisanship exerts a considerable effect on welfare state entitlements in the era of retrenchment. We conclude by addressing the implications of our arguments and empirical findings for the comparative study of welfare state retrenchment.

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[3]The countries are: Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Japan, the Netherlands, New Zealand, Norway, Sweden, Switzerland, the United Kingdom, and the United States.

[4]Green-Pedersen (2002) provides a similar type of analysis in his study of retrenchment in Denmark and the Netherlands. He also provides a good overview of the problem of how to measure welfare state retrenchment.

Contemporary Welfare States: Resilience or Retrenchment?

Despite the nominal discussion of "retrenchment" in the literature, numerous accounts of the welfare state instead highlight its resilience. Indeed, a major starting point in the new politics literature is the continued *generosity* of the welfare state. For instance, the main empirical puzzle motivating Pierson's seminal work, *Dismantling the Welfare State*, was that avowedly antiwelfare administrations—Reagan's in the United States and Thatcher's in the United Kingdom—were so *un*successful in rolling it back in the 1980s. Other authors have reached similar conclusions (Stephens, Huber, and Ray 1999; also see Huber and Stephens 2001a, chapter 6; cf. Green-Pedersen and Haverland 2002).

Without going into a discussion here about the validity of specific *explanations* for welfare state austerity and retrenchment, it is worth prefacing our discussion of spending trends by pointing out that welfare state expansion is not monocausal. While there have been important changes militating towards contraction, many of the larger structural trends long alleged to increase demand for welfare programs—affluence, urbanization, secularization, democracy, "de-familialization," and economic specialization—persist (Scarborough 2000). These conditions are too often ignored, leading to the erroneous impression that some new "adverse" changes (globalization, union decline or ideological change) will fully and quickly lead to a collapse of welfare programs and institutions.

Are Expenditure Data Appropriate to Capture Welfare State Change?

Measures such as levels of (or changes in) total public social expenditure, total transfer payments as a percentage of gross domestic product, or, in the case of individual programs, programmatic expenditure as a percentage of gross domestic product (GDP) have all been used—and continue to be used—as indicators of "welfare effort" (e.g., Cameron 1978; Garrett 1998; Huber and Stephens 2001a, b; Korpi 1983; Stephens, Huber, and Ray 1999; Swank 2002; Wilensky and Lebeaux 1958). For example, to support his argument that conservative governments in Germany, the United States and Britain failed to tame public spending, even Pierson's (1996) more qualitatively oriented comparison ultimately relies on government program spending, employment, and social transfer data, all measured as a percentage of GDP.

In spite of their continued use in recent studies, reservations about the use of expenditure data are well

documented (Castles and Mitchell 1992; Clayton and Pontusson 1998; Esping-Andersen 1987, 1990; Gilbert and Moon 1988; Goodin et al. 1999). Such data cannot tell us very much about how, or on whom, the money is spent. In terms of understanding the impact of welfare state entitlements on individual life chances, this distinction can be crucial. As Esping-Andersen remarked, "it's difficult to imagine that anyone struggled for spending *per se*" (1990, 21). Indeed, insofar as the welfare state serves to insure against misfortune vis-à-vis the market, actual spending levels are not directly relevant to protection provided.

There are other reasons why social spending data are problematic as measures of welfare state generosity. First, changes in the structure of the dependent population can overwhelm (and hence mask) real cuts at the individual level. For example, *aggregate* social spending grew during the British recession of the early 1980s, even though the Conservative government slashed entitlements. As long as the percentage growth of dependents in a program (e.g., the unemployed,) exceeds the percentage per-capita reduction in benefits, aggregate social spending will be higher.[5]

Another problem with the use of spending as a ratio of GDP, perhaps the most often used measure of effort, concerns differences in economic growth rates. If, as many have argued, the welfare state performs services that have higher relative costs (Baumol's disease), or are paid by a postindustrial private sector whose productivity is inherently lower than in the past, the same level of services should require a *growing* share of government.[6]

Finally, differences in the tax treatment of transfers (either due to income tax exemptions or simply to different tax structures) distort the degree to which social spending, as measured in national accounts, translates into disposable income for program recipients. The role of taxation as an avenue for social transfers has been given more attention in recent years (Howard 1997), but while the tax system is increasingly being used as a transfer mechanism—the United States' Earned Income Tax Credit and the United Kingdom's Working Families Tax Credit being notable examples—it can also be used to claw

[5]This argument applies to the debate on income redistribution via the welfare state (Bradley et al. 2003; Kenworthy and Pontusson 2002). All else equal, doubling dependents with no market income, while cutting their benefit replacement rates by anything less than 50% results in an *increase* in government redistribution (i.e., the difference between prefisc inequality and postfisc inequality). Thus, it is possible to observe increased redistribution while individual benefit entitlements are retrenched substantially.

[6]Several recent studies identify causes for spending to rise faster than growth and the decreasing economic importance of high productivity manufacturing jobs (e.g., Iversen and Cusack 2000). Based on this logic, the things governments do entail growing *relative* costs.

back apparent increases in social spending. Adema (1998, 2001) finds that increases in gross expenditure levels can be offset considerably by changing the tax treatment of transfers (e.g., making benefits taxable), or by increasing consumption taxes. Either way, "net spending can often fall well below gross expenditures" (1998, 20).

Programmatic Measures of the Welfare State: Measuring Retrenchment with Income Replacement Rates

If the current reliance on aggregate spending in dynamic comparisons of welfare effort is problematic, how should we evaluate welfare state generosity? We believe that it is necessary to look at elements of welfare state programs that provide us with an indication of the likely impacts of programs on individual life chances. This is the approach taken in Esping-Andersen's influential study of welfare state regimes, *The Three Worlds of Welfare Capitalism*, and the desirability of his approach has been stated by various comparative welfare state scholars (Castles 1998b, 2002; Green-Pedersen and Haverland 2002; Hicks 1999; Kitschelt 2001). In this section we discuss our approach at greater length and, using data derived from it, reevaluate trends in welfare commitments for eighteen countries over the last three decades. We find considerable evidence of retrenchment, starting generally in the mid-1980s.

Although individual entitlement measures are preferable dependent variables if our chief concern is explaining changes in the welfare state's impact on individuals and families, problems remain. There is a real paucity of data reporting benefit replacement rates. Since *The Three Worlds,* few published studies have replicated the original study or examined whether the pattern Esping-Andersen reported in a single cross-section of countries (in 1980) is (or was) as generalizable as the welfare state literature has assumed.

Recent studies have made use of available data, such as the unemployment benefit replacement rate data published by the OECD (1995; e.g., Huber and Stephens 2001a; Iversen 2001). These data are less than desirable as a good indicator of welfare effort. First, this OECD data uses replacement rates without taking income and social insurance taxation into account. Large cross-national differences in the tax treatment of benefits make such gross replacement rates deceptive. Depending on the tax treatment and marginal rate of benefit income, very different gross replacement rates can translate into the same final disposable income. For example, in 1997, *gross* unemployment benefits replaced 97% of an average production worker's *net* income in Denmark, 89% in Norway, 79% in Finland, 76% in Canada, and 65% in Belgium.

Yet, after tax, the *net* replacement rates in all five countries were almost identical (between 63 and 66%). Such discrepancies undermine the value of *gross* replacement rates as a gauge of welfare state generosity not only across countries, but also over time: the tax treatment of benefits differs among and changes within countries during the 1970s and 1980s.[7] Different tax treatment of benefits reveals further difficulties with relying on spending as a basis for welfare state generosity. Since spending figures include gross program outlays, rather than outlays net of tax, relative program generosity across countries is distorted.

Given the discrepancies in the gross replacement rate data, the case for determining the *net* replacement rates of major social insurance programs as an indicator of individual welfare state entitlements is strong. While there has been limited work in this area, the existing data has several major shortcomings, particularly with regard to evaluating partisan effects. First, the Swedish Institute for Social Research (SOFI) also has a program calculating social rights (see Korpi and Palme 2001). Data like these were the basis for Esping-Andersen's (1990) decommodification indices. Unfortunately, these data calculate benefits only at five-year intervals and are not in the public domain. Second, the OECD Jobs Study (1994) estimated net unemployment replacement rates for four separate years: 1961, 1971, 1981, and 1991. The long gaps between data points make it difficult to estimate partisan effects. Moreover, our data address several major shortcomings of these estimates. Among other things, the OECD rates are calculated for single workers only, are based on imprecise projections regarding determination and tax treatment of benefits, and assume a relatively long period of unemployment. There are also more recent efforts by the OECD to calculate net replacement rates (OECD 1999). These results, however, do not provide any insight into recent trends, and they have only been calculated biannually since 1995. (For overlapping years our data are very close to theirs.)

The data reported in Table 1 cover two programs: unemployment insurance and sickness insurance.[8] Following the methodology discussed in Esping-Andersen (1990) and some recent efforts by the OECD (1999), we

estimated the benefits due for the first six months of the event and then annualized this figure in order to calculate a notional "tax owed" for the individual. We then deducted any income taxes or social charges due on that benefit and divided that figure by the net wage of the "average production worker" (APW). For benefits, we drew upon information from national government agencies responsible for such programs and the United States Social Security Administration's *Social Security Programs Throughout the World*. Wage and tax structure information are derived from the OECD's *Tax/Benefit Position* and *Taxing Wages* publications (which have been published since the mid 1970s), with additional information from various national governments. We calculated these net replacement rates for sickness and unemployment for two recipient groups: (a) a single worker, and (b) a married APW with a nonemployed spouse and two children. The replacement rates in Table 1 are the average for the two household types. An example of how the net replacement rate is calculated is provided in the appendix.

To give a sense of trends, data are presented for years 1975, 1985, 1999, and the highest recorded rate in the 1975–99 period. Table 1 also displays the change in replacement rate (a) between 1999 and 1975, the year often considered to be the end of the golden age of welfare expansion, and (b) between 1999 and the highest-recorded replacement rate. These replacement rates tell a very different story than studies examining spending data with respect to welfare state trends in OECD countries. First, there has been a great deal of retrenchment in benefit generosity in most countries. Second, there has been some convergence in replacement rates across countries.

Unemployment Insurance: in 11 of the 18 countries—Australia, Belgium, Denmark, Finland, France, Ireland, the Netherlands, New Zealand, Sweden, the United Kingdom, and the United States—cuts in unemployment insurance replacement rates have totaled five points or more compared to their post 1975 peaks. (If we went back to 1971, Canada would also appear in this list.) In eight countries, replacement rates in 1999 were more than 10 points below their peaks. Furthermore, Column 7 shows that in 10 of the 18 countries, replacement rates in 1999 are lower than they were in 1975. As the table suggests (and the annual data confirm), replacement rates peaked by the late 1980s in most countries.

Not only has there been considerable retrenchment in most countries, but there has also been considerable cross-national *convergence* in unemployment replacement rates. The coefficient of variation declines from .30 in 1975 to .22 in 1999. Consistent with convergence there is a strong negative pattern when change in replacement rate

[7]Unemployment insurance benefits became taxable during the 1970s in Belgium, Norway, and Sweden, and in the United States and Finland during the 1980s.

[8]We focus on sickness and unemployment benefits here because they are most directly related to insurance against labor market risks and thus are the best manifestations of Esping-Andersen's idea of welfare state decommodification. Current pension benefits, on the other hand, tend to reflect policy decisions made years before they are paid out.

TABLE 1   Trends in Program Replacement Rates

	Unemployment							Sick Pay						
	1975	1985	1999	Maximum	Last Year of Maximum	Difference 1999–1975	Difference 1999–Maximum	1975	1985	1999	Maximum	Year of Maximum	Difference 1999–1975	Difference 1999–Maximum
Australia	35.5	42.5	46	52	1989	10.5	−6	35.5	44.5	46	52	1989	10.5	−6
Austria	37.5	65	64	66	1987	26.5	−2	72.5	76.5	80	80	1999	7.5	0
Belgium	74	73	62	74	1983	−12	−12	86.5	76.5	80	86.5	1975	−6.5	−6.5
Canada	67.5	69	65.5	69	1985	−2	−3.5	67.5	69	65.5	69	1971	−2	−3.5
Denmark	69	79	63.5	84	1983	−5.5	−20.5	69	79	63.5	84.5	1983	−5.5	−21
Finland	45.2	70.5	64.5	70.5	1985	19.3	−6	60.5	88.5	73.5	91.5	1989	13	−18
France	46.5	72.5	71	76	1987	24.5	−5	58.5	62	61	65.5	1996	2.5	−4.5
Germany	69	66.5	65.5	69	1983	−3.5	−3.5	100	100	93.5	100	1995	−6.5	−6.5
Ireland	61.5	65	42.5	72	1979	−19	−29.5	61.5	65	42.5	72	1979	−19	−29.5
Italy	13	9	46.5	46.5	1999	33.5	0	72.5	80.5	74.5	81	1984	2	−6.5
Japan	53	55	55.5	55.5	1999	2.5	0	51.5	55	55.5	55.5	1999	4	0
Netherlands	84.5	87.5	75.5	89.5	1978	−9	−14	84.5	85.5	75.5	89.5	1978	−9	−14
New Zealand	48	52	42.5	56.5	1986	−5.5	−14	48	54.5	42.5	59	1986	−5.5	−16.5
Norway	54	70.5	70	71	1982	16	−1	54	100	100	100	1999	46	0
Sweden	79	82.5	72.5	86.5	1988	−6.5	−14	89.5	91	82.5	94	1981	−7	−11.5
Switzerland	75.5	78	76.5	79	1995	1	−2.5	81	82	81	84.5	1979	0	−3.5
UK	62.5	35	32	62.5	1975	−30.5	−30.5	62.5	36	31.5	62.5	1975	−31	−31
US	63	67	57.5	67.5	1984	−5.5	−10							
COV	0.31	0.30	0.22					0.25	0.25	0.28				

is graphed against initial replacement rates.[9] This convergence in unemployment replacement rates contrasts with the recent emphasis on path dependence in comparative welfare state programs. While our evidence does not directly refute all claims that welfare state trajectories are path dependent, it stands as an important result contradicting this general assertion.

Sickness Insurance: the results for sickness insurance replacement rates largely resemble those for unemployment replacement rates. As with unemployment insurance, sick pay replacement rates are more than five points lower than their post 1975 peak in 11 of 17 countries (the United States has no national sick-pay program). Replacement rates in 1999 are lower than they were in 1975 in eight of the 17, and the evidence suggests that the late 1970s or 1980s was the period in which sick-pay replacement rates peaked. Judging by the coefficient of variation, there is not much convergence (or divergence) among these countries with respect to sick-pay replacement rates, although the statistical analysis suggests more conditional convergence, leading us to conclude that even here there has been some convergence over time.

Our results suggest a pattern of welfare state retrenchment emerging in the last several decades. Moreover, the pattern of retrenchment is generally consistent with the conventional wisdom that the 1980s marked something of a watershed. Prior to the 1980s, we see expansion of generosity, while after that period, there seems to be retrenchment.[10]

Our results are consistent with income inequality trends, such as those compiled in the Luxembourg Income Study (2003). Their data on post-tax GINI coefficients and 80/20 splits suggests that most of the countries examined here have experienced increased inequality between the early to mid 1980s and late 1990s, in some cases considerable. Of course, inequality is affected by factors other than the welfare state (structure of skills, wage bargaining, etc.). Moreover, our results do not preclude the possibility that commodifying and inequality enhancing retrenchments in social insurance programs for the "typical worker" have been offset by existing means-tested programs for those in poverty.[11]

On a more qualitative level, we can point to other evidence of some major programmatic change in another major program: pensions. For example, in Denmark, Sweden, and Finland, all examples of what has traditionally been defined as a universalistic welfare regime, flat-rate pensions have been all but eliminated by the late 1990s. All have moved away from the universal citizenship pension, to primarily means-tested systems. Overall these systems *are* still dominated by state provision of pensions, but the character of earnings-related pensions has shifted somewhat more towards a conservative model relying on market earnings. Other evidence from our data set suggests that, not only are replacement rates declining, but the conditions for receiving unemployment (and pension) benefits are also becoming more stringent. The overall implication of these results is that workers are being asked to shoulder larger amounts of labor market risk compared with the social insurance systems of several decades past.

Using the replacement rate data thus not only has the advantage of allaying some of the concerns that apply to the expenditure data, but, more importantly, it also shows more clearly how changes in welfare state have impacted upon the life chances of "typical" individuals in the labor market. On the basis of this evidence, we should be less sanguine about the resilience of welfare states in the face of various pressures for retrenchment than has been suggested. With these data in hand, the next section of the article turns to the role of government partisanship in explaining these changes.

Another End of Ideology? Political Partisans and Welfare Retrenchment

In examining the partisanship argument, several studies have cited convergence pressures operating on both the left and the right. On the right, the *existence* of the welfare state is said to check retrenchment designs (via vested interests or constituents who now benefit from its programs). On the left, various structural factors (including the idea of "growth to limits" and globalization) are said to explain pressures for retraction or stasis. As noted in our introduction, Huber and Stephens (2001a,b) and Castles (1998a, 2001) at least partially concur with Pierson's claim that differences between left and right have narrowed.

Of course, push-and-pull factors may operate in opposite ways on the two sides of the partisan divide. "Constraints" for one side can bolster the argument for

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[9]This pattern persists even if the Italian case is excluded. Italian unemployment replacement rates are anomalous, in that there is a system in place (CIG) to provide workers in large plants with very generous benefits in the event of widespread layoffs. Other employees have historically had very low "regular" unemployment insurance benefits.

[10]Our preliminary results going back to the early 1970s reinforce this result; that is, expansion in the 1970s and into the 1980s and then retrenchment.

[11]While the retrenchment of benefits is consistent with increasing inequality, our data, based as they are upon "average production

workers" only, mean that an analysis of the broader *redistributional* consequences of welfare state reform is beyond the scope of this article.

expansion or contraction by the other side. For example, by itself, appealing to the pressures for globalization offers the justification for cuts to be used against opponents of such cuts. Likewise, opponents of retrenchment may use vested interests to resist retrenchment pressures resulting from globalization or other sources and to expand those programs. Thus, in and of themselves, competing constraints have an ambiguous effect on outcomes, do little to speak to partisan preferences, and say little about the effect of the latter on the former.

Iversen (2001) provides a stylized model of changing demand for welfare protection that allows for a constant level of differentiation between left and right parties. In this model, while the level of welfare generosity may change due to institutional and structural constraints, there is no reason that the unconstrained *differences* between the parties are narrowed. If the median voter shifts to the right, right parties demand bigger cuts than does the left but the difference between them can be the same. It could even increase. Kitschelt (2001) also suggests a model in which a left party's desire for a larger welfare state may be masked by strategic considerations that lead the left to make preemptive cuts or the right to resist cuts. Common to both authors, however, is the implicit presumption that underlying partisan differences over the nature of the welfare state remain.[12]

Empirical support for this idea can be gauged from other sources. For example, an analysis of national party manifestos suggests that, while there is considerable variation within most countries over time in the degree to which parties of the left/right support welfare state expansion/contraction (and evidence in some countries that parties are less expansionist than in the past), there is no systematic evidence of convergence among the major parties on welfare state expansion issues as the declining partisanship thesis implies (Budge et al. 2001). Additionally, case studies presented in Scharpf and Schmidt's (2000) assessment of changes in welfare and employment relations in Western Europe suggest persistent partisan differences.

Do Partisan Differences Matter?

That partisan differences *exist* does not necessarily imply that partisanship *matters* for explaining changes in welfare state entitlements. However, on two fronts, we show that partisanship does still matter critically. First, our reexamination of partisanship and retrenchment in the Germany,

Sweden, United Kingdom, and United States strongly rebuts conclusions drawn in initial studies (Pierson 1994, 1996). Second, our appraisal of the statistical evidence (particularly of changing replacement rates) suggests that the partisan nature of government matters for retrenchment. Moreover, its effect is completely consistent with the idea that the overall context for welfare state expansion has indeed shifted somewhat.

Pierson's case studies of the United States and United Kingdom sought to assess why two very neo-liberal/rightist administrations, with extensive plans to roll back the welfare state failed to do so convincingly. While Reagan and Thatcher probably got less retrenchment than they *wanted*, from a comparative perspective they were quite successful by many criteria.[13] Both countries spend less as a percentage of GDP (and the government employs a smaller share of the workforce) in 2000 than at any time since the early 1970s, and this occurred in the face of an increasing portion of the population that is retired and drawing a public pension. It is true that the United States looks slightly less like a case of retrenchment than the United Kingdom, but two things mitigate that fact. First, the United States was a smaller welfare state to begin with; second, its welfare state experienced a more rapidly aging population and its programs are more strongly weighted towards that segment of the population.

The extent of retrenchment in these two countries is even is more apparent if we look at comparative benefit replacement rates. While the 18-country average unemployment net replacement rate between 1980 and 1989 grew about one point (from 60.8 to 61.9), the decline in the United Kingdom in that period was 18 points (48 to 30)! While the average sickness benefit replacement rate rose from 69.7 to 71.9 across our entire group of 18 countries in this same period, it fell 16 points in the United Kingdom from 49 to 33. Combined with Thatcher's success in eviscerating trade union power, ending the state earnings-related pension, and privatizing public infrastructure (including 20% of the public housing stock) in a single decade, one could certainly cite this as a considerable roll-back of what is one of the oldest welfare states in the world.

Changes to unemployment insurance (and pensions) in the United States under the Reagan administration were also comparatively large. Unemployment insurance replacement rates fell from 65% in 1980 to 59% in 1989, after the benefit was made liable to federal (and,

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[12]More broadly, Iversen and Wren (1998) argue that partisan differences remain important for understanding divergent national responses to rising unemployment, inequality, and the need for fiscal restraint.

[13]Clayton and Pontusson (1998) also provide further evidence that these reforms constitute a much larger pullback from state protection that Pierson's data suggests.

more importantly, state and local) income tax.[14] Finally, as in the United Kingdom, the Reagan administration also targeted labor unions, a major proponent of the American welfare state (such as it was), and unionization fell rapidly.

To infer from these examples that the partisan nature of government has little overall impact on welfare state retrenchment seems questionable. The same can be said when the comparison is extended to Germany and Sweden. The United States, United Kingdom, and Germany lack variation on the critical partisanship variable.[15] All four have also experienced nontrivial cuts in their benefit replacement rates. The one country of the four with the least amount of time under right-wing governments, Sweden, is the only one to experience unambiguously higher growth in overall aggregate spending. Thus, even in terms of spending, the evidence against a partisanship effect seems weak.

Huber and Stephens (2001a) explicitly test for effects of Social Democratic (and Christian Democratic) executives on patterns of welfare spending during retrenchment. Their interpretation that a shift in the policy agenda to defending (rather than expanding) entitlements explains the insignificant estimate for partisan differences is problematic. First, a positive coefficient is consistent with either more spending *or* fewer cuts by left governments (vis-à-vis the right). (What would have changed was the intercept term of the model if the average change went from positive to negative.) Most of Huber and Stephens's estimates for 1972 to the 1990s have a positive sign for left parties. The fact that the estimates are not statistically significant may be the result of greater variance in their limited sample, not a zero effect. Moreover, and as we will elaborate upon below, finding that left governments are not clearly associated with *expansion*, does not necessarily imply that right governments are not associated with retrenchment.

One argument that is consistent with a partisan effect in both Huber and Stephens's and Pierson's explanation is that there are new constraints (e.g., unemployment, globalization) determining spending priorities, and that these are correlated with partisanship and the size of the welfare state. If for example, countries dominated by left

governments are more open to trade, and openness is increasingly associated with retrenchment, excluding trade openness from a model of welfare state generosity creates a downward bias in the estimated effect of left governments. On the other hand, estimating these omitted variables reduces the bias, while including them inflates the errors associated with estimates of partisanship effects. In Huber and Stephens's statistical analysis of the retrenchment period for example (1999, 202–21), with only 18 observations, model parsimony may necessitates excluding globalization as a regressor, but the downward bias in partisanship estimates may remain. These problems are as, if not more, pronounced when attempting to draw inferences from Pierson's four case studies.

Another specification problem with models examining the relationship between retrenchment and partisan explanations is, once again, the dependent variable. Advocates of the class mobilization and partisan thesis, like Korpi and Palme (2001) or Esping-Andersen (1990), point out that partisanship and class power should not necessarily explain aggregate spending but the degree of decommodification of risks in the welfare state. In this light, a "new politics of the welfare state" that focuses on distributional issues *within* the welfare state is not really at odds with "old" theories of class and class politics. That a relationship no longer exists between spending and partisanship may simply reflect that spending is a poor(er) proxy, *not* that partisanship has less impact on decommodification. This only underscores the importance of using entitlements data as the basis for evaluating the continued validity of partisan explanations.

On all of these counts, existing empirical work discounting partisan and class explanations of the welfare state are not necessarily well designed as tests either of the implications of partisanship or of critical causal forces in the new politics approach. Critics of partisanship explanations have generally failed to specify correctly what outcomes partisanship affects—life chances not aggregate spending—and not tested the purported causal mechanisms of "new politics" explanations—differences among overall configurations of forces affecting welfare states—on such outcomes.

Statistical Analysis

In this section we examine the determinants of changes in replacement rates for unemployment benefits and sickness insurance in 18 OECD countries over the period 1975–99. Our primary goal is to assess the importance of partisanship as an explanation of changes in benefits over this period.

[14]Replacement rates of social pensions, the minimum assured to the elderly under the Supplemental Security Income program, were largely unchanged over the period from 1975 to 1999, in contrast to *gains* in almost all other countries.

[15]Pierson (1996) does not consider the social costs to the state as a result of German reunification in his comparison of aggregate spending in the 1990s. While it is impossible to prove the counterfactual, it is almost certain that the German welfare state would have been further reduced had they not incurred these costs.

There has been some controversy among scholars of political parties as to how partisanship can be best operationalized. Probably most studies have used some measure of left-party strength in government, usually the share of cabinet seats. We follow this convention here, with the expectation derived from previous studies that left parties will be positively associated with *expansion* of welfare benefits. However, we also note Castles's (1982) suggestion that *right*-party strength—measured here as the average right-cabinet seat share—is the most appropriate indicator of partisan effects on welfare *retrenchment*.[16] We therefore expect the relationship with changes in replacement rates to be in the opposite direction; that is, right parties will be associated with *retrenchment* in benefit replacement rates.[17] By estimating the models alternately with left- and right-party strength variables, we can be more confident that any results—if consistent using both measures—are not overly sensitive to the choice of partisanship variable. Our expectation is that left parties will be positively associated with growing replacement rates, *particularly during periods of welfare expansion*. Conversely, parties of the right will be associated with welfare retrenchment, *particularly in periods of retrenchment*.

In addition to the partisanship variable, our statistical model includes several explanatory variables commonly found in other models of welfare state change. *GDP growth* is included, with the expectation that higher levels of growth will be associated with more generous benefits. The impact of globalization is measured using two variables: *trade openness* ([imports + exports]/GDP) and *financial openness*, the latter being a composite measure of the restrictions placed on financial transactions by states developed by Quinn and Inclán (1997). Based on most recent empirical studies, we do not expect substantial globalization effects, but include these measures because their impact on outcome measures such as ours has not been ascertained. We also include controls for *budget deficit* (as a percentage of GDP) in order to control for the possibility that partisanship is correlated with budgetary crises, and the latter are correlated with changes in replacement rates. In order to control for a possible effect

of the program population, the unemployment rate is also included as an independent variable.[18]

In order to account partially for possible cross-national convergence in benefit generosity suggested in the discussion of trends in the first section of the article, the *lagged replacement rate* is included as an independent variable. A negative coefficient would imply that above (below) average initial replacement rates are associated with more (less) severe cuts, conditional on the other variables in the model.

We include two different type of controls for political and welfare policy institutions. First, we include Huber, Ragin, and Stephens's (1997) measure of *institutional veto points* to assess the extent to which governments may find their "room to maneuver" restricted by general constitutional structures argued to affect public policy outcomes. The literature leads us to expect that more veto points will be associated with less retrenchment.

The second institutional variable measures the extent to which *corporatist* bargaining arrangements dominate within a country. We employ Siaroff's (1999) measure of "integration," which provides a unidimensional and ordinal measure based on levels of social partnership, industry-level coordination and national policy-making patterns. Higher scores are indicative of greater neo-corporatism and less pluralism, and vary by decade. In line with previous empirical findings, we expect more integrated/corporatist democracies to be more resistant to welfare state retrenchment.

Specification and Estimation

The basic model for unemployment and sickness benefits is:

Δ Replacement Rate (RR)

$= \beta_0 + \beta_1 \, \text{RR}_{t-1} + \beta_2 \, \text{Right Cabinet Share (RCS)}_{t-1}$

$+ \beta_3 \, \text{Trade Openness (TO)}_{t-1}$

$+ \beta_4 \, \text{Financial Openness (FO)}$

$+ \beta_5 \, \text{Veto Points (Veto)} + \beta_6 \, \text{GDP growth (Gro)}_{t-1}$

$+ \beta_7 \, \text{Budget deficit (Def)} + \beta_8 \, \text{Corporatism (Corp)}$

$+ \beta_9 \, \text{Structural Break (SB)} + \beta_{10} \, \text{SB*RR}_{t-1}$

$+ \beta_{11} \, \text{SB*RCS}_{t-1} + \beta_{12} \, \text{SB*TO}_{t-1}$

$+ \beta_{13} \, \text{SB*FO} + \beta_{14} \, \text{SB*Veto}$

$+ \beta_{15} \text{SB*Gro}_{t-1} + \beta_{16} \, \text{SB*Def}_{t-1}$

$+ \beta_{17} \, \text{SB*Corp} + \sum \beta_{n+17} \, \text{D}_n + \varepsilon$

$n =$ number of countries (1–18).

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[16] Party data are from Duane Swank's "Comparative Political Parties Dataset," which is available at: http://www.marquette.edu/polisci/Swank.htm. Because of their ideological profile, and because they are the most right-wing party in the Bundestag, Swank codes the German Christian Democrats as a Right (rather than a centrist) Party, and we follow this convention. Recoding them to be a nonright party does not affect our results.

[17] A number of studies have also found that moderate Christian democratic parties have also been associated with welfare state expansion (Hicks 1999; Huber and Stephens 2001a; van Kersbergen 1995). The right-party measure therefore also neatly encapsulates the divide between right and nonright parties based on their ideological affinity with welfare state "roll back."

[18] Ideally, the number of sick days claimed would be used for the sick-pay model. Lacking such a measure, the unemployment rate serves as a proxy.

Given the well-documented caveats associated with using regression analysis with cross-national and time-series data (Beck and Katz 1995), we estimate our models using OLS regression with panel corrected standard errors (PCSE). Several things about this model's specification bear mentioning. First, for each program replacement rate (unemployment and sick-pay insurance), preliminary tests on the levels of the replacement-rate series indicated problems with stationarity. This led us to use the first differences of the dependent variable.

Second, we attempted to reduce problems of simultaneity bias by using the lagged values of all time varying independent variables. Thus, independent variables in the equation have $(t - 1)$ subscripts in Equation 1. We also justify our use of a lagged measure of partisan composition of government based on a presumed lag between a policy change by a particular government and its substantive effect.[19]

Third, we use fixed country effects, which are denoted "D" in Equation 1. While there is no explicit theoretical reason to include them, fixed effects do allow us to reduce the possibility that the substantive estimates are in fact attributable to country-specific trends. In this sense, this is a rather conservative approach to testing our main hypotheses. We exclude the estimates for the country dummies, for reasons of space and ease of presentation. With the fixed effect Ds, β_0 and β_9 drop out of the equation.

The fourth point relates to the specification of interaction terms. In order to capture the "sea change" that occurred in the 1980s and suggested in much of the literature on contemporary welfare states, we needed to locate this structural break in a manner that was both nonarbitrary but consistent with the literature. We used the year of the major economic recession during the early 1980s in each country. We then interacted each of the substantive variables with that break.[20] This results in a dummy-variable-interaction model that allows us to evaluate relationships between the substantive variables before and after the critical break year.

For each of the two programs, we present results from two different methods of estimation. The first is the more or less standard OLS regression with corrections for het-erogeneous errors across panels. In order to demonstrate that the results that we are interested in (those for partisanship) are robust, we present estimates without interaction terms (thus treating the entire period 1975–99 as governed by the same slopes), without country dummies, and with only convergence effects.[21]

As a second approach we dichotomized changes in replacement rates, and estimated the model with a logistic regression function, coding cuts "1." Our expectations for the signs of the individual estimate in this model are opposite to those in the OLS model (with few exceptions, noted below), as a "positive" result is a cut in the replacement rate. As in the OLS regressions, we present estimates excluding some of the control variables.

Further specification changes were made to test the robustness of these results. This included dropping countries from the analysis, using alternative combinations of the control variables in the model, and using techniques to bound the influence of "large values" of the dependent variable. Unless otherwise noted, results for alternative specifications (which can be obtained from the authors) do not alter the substantive conclusions discussed in this section, except where noted in the text (e.g., for estimates of deficits or unemployment).

Results of Pooled OLS Regression

In this section we discuss the results of our statistical analyses. In short, we find consistent effects for partisanship and convergence, but not for the controls. Here, we focus primarily on the results for political partisanship and convergence effects in the two periods analyzed, since these are of main theoretical interest here, and assessing the robustness of what function as controls in this model is beyond the scope of the article.

Partisanship. For unemployment benefits, estimates for both of the partisanship variables (left-cabinet shares and right-cabinet shares) are in the predicted direction. Moreover, the pattern of the results corresponds to our expectations. Before the break points in the 1980s, left governments are associated with larger, statistically significant increases in replacement rates. Thus, from column 1 of Table 2, in the era of welfare expansion, a government composed of all left parties increased replacement rates three points faster than a government of all right parties. However, *since the recessions of the early 1980s* (column 2),

[19]There is ample evidence from our data collection that changes in benefits (or taxes) were announced a year before they went into effect or before their effect was measured.

[20]Specifically, for each country, the break is coded 1 for years after the last negative (or the lowest) growth year in the first half of the 1980s. Years before that event (inclusive) are coded 0. Individual break points were: 1978: New Zealand (results are not altered if the near recession in 1986 is substituted); 1981: Austria, Belgium, Denmark, Finland, Sweden, and United Kingdom; 1982: Canada, Germany, Italy, Netherlands, Norway, Switzerland, and United States; 1983: Australia, France, Ireland, and Japan.

[21]We also ran a variety of models excluding subsets of right-hand-side variables, including models with only partisan and interaction effects. The results for partisanship were all consistent with the ones reported.

TABLE 2 Regression Estimates for Changes in Unemployment Replacement Rates

		Left Cabinet Model*		Right Cabinet Model*		Right Cabinet, No Structural Break*	Convergence Term Only	Left Cabinet Model, No Country Dummies		Right Cabinet Model, No Country Dummies	
Independent Variables	Predicted	Pre-break	Post-break	Pre-break	Post-break			Pre-break	Post-break	Pre-break	Post-break
		1	2	3	4	5	6	7	8	9	10
Replacement Rate$_{t-1}$	neg	−.17**		−.17**	−.23**	−.18**	−.03**	−.039	−.051**	−.045	−.049**
		.04		*.04*	*.04*	*.04*	*.01*	*.027*	*.019*	*.028*	*.018*
Right Cabinet Share$_{t-1}$	neg			.19	−1.51**	−1.0*				.57	−1.39**
				.84	*.53*	*.42*				*.86*	*.43*
Left	pos	2.95**	.84					1.59*	.62		
		.83	*.64*					*.8*	*0.051*		
Trade Openness$_{t-1}$	neg	−.77	−5.23†	.74	−4.6†	−4.2†		3.92	−.70	4.41	−.96
		4.74	*2.77*	*4.9*	*2.7*	*2.4*		*2.87*	*1.25*	*3.14*	*1.28*
Financial Openness$_{t-1}$	neg	−.32	.02	−.41	−.11	−.016		.009	−.07	−.003	−.09
		.25	*.19*	*.26*	*.19*	*.17*		*0.212*	*.15*	*.235*	*.15*
Veto Points	pos	.74	.12	.47	−.09	−.009		.33	.04	.31	
		.73	*.66*	*.71*	*.63*	*.58*		*.3*	*0.14*	*.30*	
Corporatism	pos	1.98	3.63**	2.53*	3.60*	3.73*		−.40	.46	−.13	
		1.29	*1.05*	*1.26*	*1.10*	*1.03*		*.63*	*.3*	*.57*	
Unemployment Rate$_{t-1}$	neg	−.27	.03	−.26	.04	−.0048		−.31	−.025	−.31	−.008
		.23	*.12*	*.23*	*.12*	*.11*		*.21*	*0.086*	*.21*	*.09*
GDP Growth$_{t-1}$	pos	−.09	−.03	−.06	−.02	−.03		−.02	.022	−.01	.029
		.11	*.10*	*.11*	*.10*	*.08*		*.12*	*0.099*	*.12*	*.099*
Deficits	neg	−.16†	.08	−.10	−.05	−.09		−.02	−.054	.018	−.026
		.09		*.09*	*.08*	*.07*		*.08*	*0.061*	*.08*	*.060*
Constant							2.11*	1.39	2.32	1.41	3.88
							.79	*1.91*	*2.5*	*2.39*	*3.12*
R-squared		.21		.20		.17		.08		.08	
n		450		450		450	450	450		450	

Note: Model estimated with country fixed-effects (not reported).
†p < .10, *p < .05, **p < .01 (two-tailed).

left governments are not significantly associated with increases in replacement rates.

For right governments, we see the mirror image of this effect. That is, up to the 1980s (column 3), right governments are associated with changes in replacement rates that cannot be differentiated from zero. Since the 1980s, in the era of welfare retrenchment, governments made up of all right-wing parties are associated with larger *cuts* (about 1.5 points larger) in unemployment replacement rates. Alternative specifications of the model (several of which are included in Table 2) all lead to the same conclusion.

Our results may help to explain why previous studies, like Huber and Stephens, failed to find strong *left*-partisan effects during the 1980s and 1990s in their quantitative analyses. Because they only estimate effects for the "pro-welfare" parties (e.g., left), and not for the "antiwelfare" ones, they exclude what are the important partisan effects at work in retrenchment. We would expect that substituting right parties would result in larger estimates of partisan effects post 1980.

The estimated effects of partisanship on changes in sickness benefits in Table 3 are very similar to those for unemployment. The coefficients suggest substantive effects of political shifts to the right that are in fact greater than those found for unemployment benefits. Overall then, these results suggest reasonably strong partisan effects on welfare state retrenchment, even with controls for the macroeconomic context and other common factors generally held to impact the welfare state. Korpi and Palme's findings (2003), also based on an entitlements-based approach to examining welfare state reforms, are largely consistent with ours. Since the data they use are not publicly available, however, it is not possible to compare our results directly.

Globalization. Our results for the effects of economic internationalization are more or less consistent with previous work (Castles 2001; Garrett 1998; Pierson 2001). We fail to find widespread and robust effects of financial openness or trade openness on changes in benefits. The direction of the effects varies, depending on which dependent variable is used. The marginally significant estimates for effects of Trade Openness on unemployment program outcomes are due largely to a single case, Ireland. (Irish trade increases dramatically at the same time its replacement rate is falling.) There are, however, reasonably consistent results for the restraining effects of openness on changes in sick pay in the 1970s and early 1980s.

Convergence. There is a consistent negative relationship between changes in replacement rates and their initial levels ("lag replacement rate"). This result also obtains

without additional controls in the unemployment insurance model (Column 7 of Table 2), which implies that the result is not *simply* conditional on other variables. These results are consistent with the convergence in *levels* noted earlier in the discussion of unemployment replacement rates in Table 1. That changes in replacement rates are negatively correlated with their past level provides evidence for conditional convergence and against the notion of path dependence. In the sickness model, the convergence effect is conditional on other variables in the model as shown in column 6 of Table 3. The significant convergence estimate for sick pay obtains, however, if we control only for partisanship (column 7). This implies that, in the absence of partisan differences, there would also be convergence in sick-pay replacement rates.

Institutional Effects. Our results suggest that corporatist institutions may play a role in reducing welfare state retrenchment. Estimates for the corporatism variable are consistently positive (implying that more corporatist countries cut replacement rates less), and at least marginally statistically significant. Such results are consistent with the view that strong unions and policy concertation will tend to resist retrenchment, or retrench less. Moreover, estimates of the model that do not control for corporatist institutions (available from the authors) suggest that taking them into account lowers the estimated retrenching effects of right governments, especially since the 1980s. (For the unemployment model, the coefficient for right government falls from −1.80 if corporatism is excluded from the model to −1.51 when it is included.). However, the sign and significance of the corporatism estimates are sensitive to alternative specifications of the model, suggesting that they are not very robust. For example, if the corporatism variable is added to the specification shown in columns 8 and 9, the estimate is not significant in either period and the sign in the second period is negative. In contrast to the effects of corporatism, constitutional veto points seem to exert little discernible impact on welfare state outcomes (nor does it in alternative specifications).[22]

Structural Economic Crisis. Finally, the results in Tables 2 and 3 provide at best limited evidence that macroeconomic constraints systematically facilitate the speed of retrenchment. Unemployment and higher

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[22]An earlier version of the article included a dummy variable for the type of welfare regime, based on Esping-Andersen's threefold typology. Since we have included country dummies, the welfare regime estimates were subsumed. Using alternative specifications that we do not report, we found no "welfare regime" effects. This (non)finding can be taken as further evidence against "path dependent" effects of welfare regimes.

TABLE 3　Regression Estimates for Changes in Sick Pay Replacement Rates

Independent Variables	Predicted	Left Cabinet Model* Pre-break	Left Cabinet Model* Post-break	Right Cabinet Model* Pre-break	Right Cabinet Model* Post-break	Right Cabinet, No Structural Break*	Convergence Only	Convergence Conditional on Right Cabinet	Left Cabinet Model, No Country Dummies Pre-break	Left Cabinet Model, No Country Dummies Post-break	Right Cabinet Model, No Country Dummies Pre-break	Right Cabinet Model, No Country Dummies Post-break
		1	2	3	4	5	6	7	8	9	10	11
Replacement Rate$_{t-1}$	neg	−.13*	−.21**	−.11*	−.23**	−.14**	−.016	−.029*	−0.032	−0.042†	−.025	−.05*
		.053	*.04*	*.05*	*.04*	*.04*	*.012*	*.013*	*.044*	*.022*	*.049*	*.022*
Right Cabinet Share$_{t-1}$	neg			1.39	−2.09**	−1.11**		−1.52**			−.78	−1.52**
				.85	*.52*	*.4*		*.44*			*.88*	*.46*
Left$_{t-1}$	pos	2.82**	.73						2.57**	.47		
		.76	*.61*						*.73*	*.53*		
Trade Openness$_{t-1}$	neg	1.31	−2.70	1.74	−2.44	.76			3.38	−.25	2.83	−.43
		3.7	*2.44*	*3.73*	*2.42*	*2.22*			*2.46*	*1.15*	*2.4*	*1.16*
Financial Openness$_{t-1}$	neg	−.59*	−.20	−.85**	−.37	−.17			−.33	−.19	−.47	−.20
		0.28	*0.22*	*.29*	*0.23*	*.17*			*.31*	*.19*	*.31*	*.18*
Veto Points	pos	.22	−.42	−.15	−.70	−.42			.3	−.004	.24	.019
		.79	*.70*	*.76*	*.67*	*.64*			*.26*	*.14*	*.26*	*.139*
Corporatism	pos	.19	1.66†	.40	1.71†	1.74†			−.06	.58	.24	.53
		1.1	*0.96*	*1.04*	*0.91*	*.92*			*.63*	*.39*	*.58*	*.37*
Unemployment Rate$_{t-1}$	neg	−.47*	−.22†	−.42*	−.17	−.15			−.24	−.09	−.22	−.072
		0.21	*0.13*	*.20*	*0.13*	*.12*			*.18*	*.09*	*.18*	*.095*
GDP Growth$_{t-1}$	pos	−.22†	−.03	−.18	.008	−.05			−.067	.05	−.05	.054
		0.11	*0.1*	*.11*	*0.099*	*.08*			*.12*	*.11*	*.12*	*.11*
Deficits$_{t-1}$	neg	−.30**	−.14†	−.24**	−.12†	−.19**			−.14	−.09	−.11	−.06
		0.08	*0.07*	*.08*	*0.07*	*.07*			*.08*	*.07*	*.08*	*.06*
Constant							1.08	2.58	2.82	3.4	4.5	5.1
							.83	*1.03*	*2.07*	*3.2*	*2.14*	*3.0*
R-squared		0.22		.23		.15	.01	.03	.086		.085	
n		425		425		425	425	425	425		425	

Note: Model estimated with country fixed-effects (not reported); US is excluded as it has no national sick pay program.
† p < .10, * p < .05, ** p < .01 (two-tailed).

budget deficits are both consistently associated with larger cuts in replacement rates in both programs. However, these effects are sensitive to the model specification and to which dependent variable is used. We have no good explanation for why these results tend to be stronger in the estimates for sick pay than for unemployment insurance. Given the fact that unemployment insurance is perhaps a more salient factor than sick pay in affecting unemployment levels and the demand for social benefits, we would expect it to be more tightly linked to the macroeconomy.

Logit Results

Table 4 presents results of logit estimations of the basic models. We dichotomized the dependent variables using two rules. The first codes any negative change in the replacement rate as 1 and all zero or positive changes as zero. The second attempts to exclude "marginal" changes in replacement rates, which may accrue through no obvious intention to cut replacement rates.[23] The table reports only the coefficients for Right- and Left-Cabinet Shares, and, as in the earlier model, each of these effects are estimated in separate regressions. Two points should be kept in mind when viewing these results. First, the expected signs on the partisanship variables and the convergence terms are reversed, because a "positive" outcome here represents a cut in benefits. Second, since it is a dichotomous variable, the estimates are for the probability of making *any* cut in benefits (above the threshold specified). A cut of five points is treated here as the same as a cut of one point. Thus, these estimates tell us nothing about the *magnitude* of cuts.

For reasons of space, we report only the estimates for convergence terms and partisanship, and only for selected specifications. The results for right parties are very similar in sign and significance to those reported in Table 2. Up until the 1980s, we cannot conclude that right parties are more or less likely to initiate cuts in benefits. After the recessions of the early 1980s, however, right parties appear much more likely to initiate cuts than nonright parties. This reinforces our conclusion that parties of the right are more likely to cut benefits, not just raise them less, or cut them more than nonright parties in this period.

Estimates for left parties are generally negative over both periods, but these estimates are not robustly significant statistically. This pattern of results within the logit framework is actually consistent with the belief that right parties are the "anti-welfare" parties and that the post-1980s period is the era of retrenchment. Only when *both*

conditions are met—right government in an era of cuts—do we expect to see a strong negative effect; and the left-party effects in the 1970s are weak, in contrast to the results in Table 2, possibly because no parties (right or left) were likely to retrench during this period.

Conclusion: Politics as Usual?

In this article we have reexamined two key topics in the welfare state literature that have been the subject of considerable debates in recent years. Like many others, we believe that the entitlements approach to welfare state generosity, and the replacement rate data introduced in this article, provide a much greater purchase over questions concerning the welfare state's alleged resilience (or lack thereof) in recent years, and the role that partisanship plays in shaping welfare state entitlements.

Beyond the improvement in measuring welfare state generosity, we think our analytical results have a number of important implications for our understanding of welfare state politics. First, our analysis provides several good reasons not to abandon established theories of the welfare state. Not only is it not particularly productive to abandon theories because the things we wish to explain change their trajectory, but it is also the case that some old accounts hold up rather well. While some events may help us to focus more on what our categories and concepts are, this is quite different from abandoning links between them. Viewed in a broad comparative context, and over a number of years, there is evidence that welfare states have changed considerably since the early 1980s.

Second, at least one of the conventional explanations cited to explain welfare state expansion—the partisan makeup of democratic executives—also appears to work "in reverse," despite claims to the contrary found in the "new politics" accounts of welfare state adjustment. Just as government by the left tended to lead to more rapid expansion of the welfare state, particularly the expansion of rights to reasonable income outside of the market nexus, government by parties of the neo-liberal right since the 1980s (or even since the mid-1970s) has tended to result in greater retrenchment.

In a similar vein, our results cast some doubt on the idea that preexisting institutional arrangements necessarily constitute major barriers to change. To date, the evidence in favor of this idea has not been particularly persuasive when viewed in a proper comparative context. We find only limited evidence that more class-consensual institutions (i.e., neo-corporatism) has been more successful at resisting cuts in benefits during the last two or three decades, and we find almost no clear evidence that

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[23] Deciding what to label "retrenchment" and what to label a temporary cut is not as obvious as it might appear. The two rules chosen here are intended to indicate some robustness to alternative rules.

TABLE 4 Logit Estimates for Retrenchment

	1 = Any Cut					1 = Cut Greater than 1 Point				
	Full Model		Country Dummies Only, No Structural Break	Structural Break and Country Dummies Only		Full Model		Country Dummies Only, No Structural Break	Structural Break and Country Dummies Only	
	Pre-break	Post-break	Break	Pre-break	Post-break	Pre-break	Post-break	Break	Pre-break	Post-break
Unemployment Insurance										
Right Cabinet Share$_{t-1}$	.45	1.22**	.87**	.61	0.95**	−0.23	0.90*	.30	−.69	.73†
	.61	.43	.33	.54	0.37	0.68	0.46	.35	.61	.04
R-squared	.11		.08	.08		.14		.09	.09	
Left Cabinet Share	−1.09†	−.77†	−.73*	−1.00†	−.57	−0.56	−0.74	−0.22	0.41	−0.56
	0.65	.45	.33	.53	.39	0.73	0.5	0.35	0.57	0.43
R-squared	.11		.08	.08		.14		.09	.09	
Sick Pay										
Right Cabinet Share$_{t-1}$	−.26	1.04*	.60†	.28	.91*	−.32	1.13*	.62†	−.36	1.00*
	.73	.45	.35	.60	.40	.80	.51	.37	.66	.43
R-squared	.17		.11	.12		.17		.10	.10	
Left Cabinet Share	−1.32†	−.63	−.57†	−.22	−.74†	−1.08	−.75	−.60†	−.06	−.90*
	.75	.47	.34	.55	.41	.79	.54	.36	.58	.45
R-squared	.17		.11	.12		.17		.10	.10	

†p < .10, *p < .05, **p < .01 (two-tailed).

political systems with greater dispersion of power constrain welfare expansion or retrenchment.

The evaluation of the scope and dynamics of welfare state reform must pay more attention to the distribution of risks and life chances. Such features are not only beyond the scope of comparative spending data, but have generally been recognized as being so. We hope that by shifting the focus from expenditure measures to data that better reflect the *consequences* of welfare state policies, our research may contribute to correcting this lacuna in current welfare state research.

Appendix

Example of Computing Net Replacement Rates: Unemployment benefits for a single person in Denmark, 1999.

Gross and Net Wage: (APW) 274,200 kr and 152,999 kr.

Benefit: 90% of wages, up to a maximum daily benefit of 2760 kr per week. The maximum benefit for 26 weeks is thus $2760 * 26 = 71,760$ kr, which is less than 90% of APW in that period. Annualizing the benefit is simply doubling the 26-week benefit (143,520 kr).

Taxation: The benefit is fully taxable as income. Normal social charges are not assessed on transfers, but two flat rate contributions of 894 kr and 5139 kr are (for ATP pension scheme and unemployment insurance, respectively). These charges and a personal allowance of 32,300 are deducted to find taxable income. Taxable income is assessed at a rate of 7.5% (national tax) and 32.6% (average local tax rate).

Net Replacement Rate:

$$
\begin{aligned}
&\text{Replacement rate} \\
&= \text{Net benefit/Net wage} \\
&= [\underset{\text{benefit}}{143,520} - \underset{\text{taxable benefit income}}{(143,520\text{-}894\text{-}5139\text{-}32,300)} \\
&\quad * \underset{\substack{\text{tax rate} \\ \text{(national} \\ \text{\& local)}}}{(.075 + .326)}]/\underset{\text{net APW}}{152,999} \\
&= 93,507/152,999 \\
&= 62.3\%
\end{aligned}
$$

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