Politics as Usual? Political Parties and Welfare Regime Change in Advanced Industrial Societies*

by

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Introduction

According to most recent cross-national studies, partisan politics have ceased to play a decisive role in the evolution of the welfare state. While the power of social democratic and Christian democratic parties vis-à-vis parties of the secular center and right is viewed as playing an important role in the *expansion* of the welfare state (Castles 1982; Shalev 1983; Esping Andersen 1985; Korpi 1989; Hicks and Swank 1992; Western 1991; van Kersbergen 1995; Garrett 1998; Hicks 1999; Iversen and Cusack 2000; Huber and Stephens 2001a, b), the same or similar accounts find little evidence that partisan difference have had much to do with the widespread stagnation or retrenchment experienced since the late 1970s.

This fact can perhaps be seen most starkly in Huber and Stephens's results of partisan effects before and after the early 1970s (2001a). From 1960-1972, 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 sharp narrowing 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).[1]

How do we account for this discrepancy? Does partisanship matter going up, but not coming down?[2] The increasingly prevailing view is that there is a "new politics" of welfare state change, because retrenchment dynamics are fundamentally different than expansion dynamics. Pierson's work on welfare state retrenchment is the most elaborate

[1] Elsewhere Huber and Stephens's 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] While much of the literature in comparative political economy has focused on (one might say obsessed with) the effects of "globalization," this has tended to obscure other puzzles. Iversen and Cusack's work on de-industrialization as an explanation for welfare state expansion (and a role for the political center of gravity), for instance, points out that structural change has tended to be obscured (or *errantly* assumed to be subsumed) by this debate. However, their work sheds little light on welfare state stagnation or decline.

in this regard (1994, 1996, 2001; cf. Clayton and Pontusson 1998). In various places, one finds references to his book as the starting point of discussions about reforms from healthcare to housing (Taylor-Goodby 2001; Green-Pedersen and Haverland 2002).

Our contention in this paper is that the jury is still out on this new politics, and its implications for partisanship. Pierson's thesis is provocative, but we think too hastily accepted. In part, we think this is because the new politics perspective suggests a "clean break" with previous work. Rather than proclaiming that we have witnessed some kind of Foucaultian "epistemic shift" in which the old relationships and theories abruptly cease to work anymore (but let our explanations about expansion persist), we suggest that the "new politics" perspective—including its implications for welfare expansion—deserve a long critical look. Does the theory indicate the need and basis for a "new way" to think about the present? If so, does it really have no implications for the veracity of our old explanations about expansion?

Most briefly, we think the answers are "no" and "no." We look at these questions not only with the recent literature on the expansion and decline of the welfare state, but also with net income replacement data on pension, sickness, and insurance programs from 1977-1997. As this latter period relates primarily to the period of retrenchment, our empirical analysis tends to focus on that period. However, we also assess the implications of key concepts in Pierson's "new politics" approach for the past. While we are skeptical of how much of what is described in this literature actually constitutes a watershed between expansion and retrenchment, if true, the argument seems to suggest important "revisionist" puzzles in understanding the past. Finally, in resurrecting old explanations of differences in national welfare policies, throughout our paper we pay particular attention to one of the "old truths": the impact of partisanship on the character and extent of the welfare state.

The rest of the paper is divided into five parts. The first section describes and critiques the basis for the new politics argument and its implications. In brief, we claim that the main arguments underlying Pierson's original argument were methodologically flawed. First, Pierson (and much of the ensuing work) fails to clearly establish that the main causal factors alleged to operate in the current "retrenchment era" were *not* operative in the expansion era. We suggest that there is abundant evidence to suggest that they were. Second, the dependent variable is often un- or poorly-specified, and alternative rival explanations (e.g., what would constitute a distinctive "old politics" view) are largely undefined. Third, its conclusions about the (lack of) success of retrenchment in both countries in the original study of the UK and UK, generally, and fails to stand up to larger comparative scrutiny. The second section discusses more specifically the implications of the argument for the impact of partisan politics. Here we suggest that the few accounts of the role of partisanship and retrenchment give no basis for concluding that partisanship should be any less relevant for the period from 1980 to 2010 than it was for the period from 1950-1980. Next, we turn to a discussion of the so-called "dependent variable problem" in comparative analyses of the welfare state. We suggest that the widespread use of aggregate spending data is problematic theoretically, and make a case for using more detailed data on benefits and entitlements that more accurately reflect how policy changes impact on the life chances of individuals. With these data in hand, in the following section we examine the determinants of changes in net replacement rates for benefits in three programs between 1977 and 1997: unemployment insurance, sickness insurance, and social pensions. Contrary to a number of existing studies, we find that partisanship still matters. We conclude by addressing the implications of our arguments and empirical findings for the comparative study of welfare state retrenchment *and* expansion.

I. Is There a New Politics?

The most often cited explanation for a new politics of the welfare state was provided by Pierson (1994, 1996). He suggests that there is a fundamental difference between the politics of welfare state expansion and contraction. Based on a comparative detailed case study of welfare reform in the US and UK (and less detailed studies of Germany and Sweden, see 1996), his argument suggests that the expansion of the welfare state was relatively unproblematic as the process faced little entrenched resistance and politicians were all too willing to accept credit for offering programs that provided security against social risks. However, as welfare states have matured, and productivity has declined, the "politics of blame avoidance" and entrenched welfare state interests emerge as unintended consequences of expansion which fundamentally change the "rules of the game." In short, he maintains that factors that systematically explained expansion should be unable to explain retrenchment.

For statistical analyses that underlie a lot of comparative welfare state research, this new politics implies that the magnitude of the correlation between variables is subject to a "structural change," between eras of expansion and contraction.[3] Note, however, that this leaves a lot unspecified. First, there is no specific intuition provided as to why magnitudes of the old relationships (i.e., the size of estimated coefficients in a regression model) should be dampened. "Regime change" may augment the effects—of class power, partisanship, international economic integration, and employment structure—as well as reducing them.

Second, conventional statistical tests (often used implicitly even in non-statistical analyses) provide grounds for rejecting a null of no effect, *not of interpreting null effects as zero effects*. Many things (regime change among them) might explain non-effects. Much of the existing evidence interpreted as supporting the new politics argument (e.g., Stephens and Huber, 2001a, b, Iversen and Cusack 2000, Iversen 2001) is susceptible to one of these two errors.

(It should be stressed that many interpretations in this vein are in studies directly testing why welfare states expanded. Our criticism here does not necessarily contradict their main positive findings about expansion. However, to the extent these studies interpret (*post hoc*) the failure of their explanations to cover the entire range of years covered in their analysis as evidence that a new theory is needed to explain decline, this undermines their own explanations without provide direct evidence of the alternative.)

Furthermore, a convincing claim that there is a "new politics" of welfare state retrenchment cannot *simply* point to contemporary instances of its posited causal factors, i.e., entrenched resistance and blame avoidance. It must also demonstrate that the pre-existing era (i.e., the establishment and expansion phase of the welfare state) was devoid of those phenomena. If the era of expansion was politically conflictual, driven by particularistic interests and made extensive use of efforts by politicians to "avoid blame"

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[3] Actually, it is possible that the magnitude of the effects is the same, but that the variance has gone up for some reason. If that were the case, it still begs the question why the variance would change.

on matters germane to increased government commitments to social security, then it seems difficult to speak of a new or changed causes. For the most part, however, those things are not directly considered in the "new politics" literature. In fact, they sometimes overlook clear evidence to the contrary.

Vested interests for the welfare state

One of the main explanations why the politics of retrenchment differs from the politics of expansion is the presence of numerous vested interests in existing programs, most of whom are "well organized" compared to their retrenchment opponents. The micrologic corresponds closely to that for protectionist coalitions: groups receiving concentrated program benefits lobby harder and better than large latent coalitions of those who pay the costs: taxpayers. One immediate problem with this view is that it implies that cuts are *least* likely (once proposed) in programs with smaller, more intensely dependent groups (as is typically the case with protectionism). Yet, such small groups are not typically the examples usually cited as "vested interests." Instead, trade unions or pensioners are cited as the main blocking interest groups, though they are hardly small, and in the case of unions they have distinctly mixed motives.[4]

Welfare states have never represented a consensus. They have had many proponents and detractors since their creation. Unions were proponents (and sometimes providers) of welfare services prior to the creation of the welfare state. Worker movements were often internally divided over a range of issues – "reform" versus "revolutionary" strategies, Beveridge-style flat benefits versus earnings related ones. America's New Deal encountered a great deal of resistance and opposition by some business interests.[5] Resistance to the ideas of minimum wages and social security

[4] Unions generally represent current workers, not retirees, though in some countries (like Italy) they "represent" both, and in others (like France) their political power derives almost exclusively from being on pension boards (not having mass membership). (This partly explains, for example, the constraining effects of unions in pension the reform debates in Italy and France (cf. Myles and Pierson 2001: 330).

[5] Business interests themselves were divided between the so-called "welfare capitalists" who largely shaped the (limited) social insurance schemes, and opponents of any kind of government regulated programs. See, e.g., Gordon (1994:240-79).

taxation provoked constitutional challenges. The history of welfare state expansion in Sweden contains similar conflicts.[6]

Scarbrough (2000: 247) argues that those with vested interests in welfare state programs are generally not strong and well-organized as supposed, notwithstanding claims to the contrary.[7] The AARP in the United States may be alone as a very powerful lobby that can trace its existence almost exclusively to the pension program.

Blame avoidance

Blame avoidance strategies suggest that under conditions of austerity, politicians are driven to avoid the electoral blame for cuts. Thus, politicians will either avoid cuts or cut only under conditions of consensual "pacts" with possible opponents.

However, there is ample evidence that most of the programmatic expansion of the democratic welfare state required the consent of the middle class, and that reforms do the same (Scarbrough 2000: 247). In other words, in many countries electoral motives prompted initial choices about welfare state programs, such choices played a role in decisions about expansion in the same way that they prompt them in retrenchment.[8]

One can certainly make the case, which seems perfectly consistent with the literature on corporatist class compromise (perhaps with an ironic vindication of some of their neo-Marxist critics) that the crisis of the welfare state is the result of "blame avoidance." Fudging fundamental trade-offs between wages and/or profits in the 1950s and 1960s by promoting forms of deferred compensation (i.e., pension insurance) that turn out to be unsustainable could certainly be included in this category of action.[9]

Of course, this does not deny possibilities for genuine miscalculation and ignorance—e.g., that life expectancy would rise and retirement age would not, or that

[6] While recent accounts suggest more support from (some) employers than previously assumed (Mares 2001, Baldwin 1990, Swenson 1989), these accounts do not suggest that the scope and character of expansion was not contested.

[7] More generally, she also argues that in many respects the same theories and forces explaining the expansion of the welfare state also play a role in explaining the persistence of the welfare state.

[8] For instance, Pierson notes how politicians from the beginning of the program scrupulously *avoided* ending exclusive reliance on payroll taxation to fund Social Security in the United States (1994: 56-8). Moreover, this approach, widely regarded as a more conservative one was most stringently defended *by conservatives* in Congress, against Democrats.

[9] This point is made by Myles and Pierson (2001), though the connection to the larger theme of blame avoidance is not addressed.

productivity growth would not grow as fast as it did in the 1950s and 1960s. The point is simply that there may be enough "blame avoidance" to go around from the inception of the welfare state. Indeed, the perspective suggested by the "new politics" casts doubt on the "positive sum" nature of welfare state commitments associated with the "class compromises" of the 1950s and 1960s (e.g., Eichengreen 1996).[10]

The welfare state is inherently redistributive, even as the basis of the redistribution may vary. Increased "credit claimed" for extending benefits in the past and decreased "blame avoided" for not retrenching them today are both associated with issues that are also general: the social level of taxation. The idea that those taxed are more unorganized, more uninfluential than welfare program beneficiaries, or that politicians are inherently more unable to claim credit for cutting people's taxes (today or in the future) than for expanding their benefits (today or in the future) is hardly unassailable. Clearly, many welfare state reforms have been "sold" on the basis of short-term gains via tax reductions (or non-increases), or on the wage or employment implications of payroll (labor) tax reductions.[11]

If one accepts that retrenchment politics is different than expansion politics for the major reasons laid out in Pierson's argument—increased blame avoidance and the rise of vested interests—that argument is a complete one only insofar as one also agrees that these causes are largely absent in the expansion period.[12] Partly for the reasons above, we think such a case is hard to make. But accepting that there was an expansion consensus has implications for interpreting welfare state expansion. To take the example we will be most concerned with later, why do we find a strong partisan effect in our statistical models? If there was a cross-party consensus over expansion and its extent, there should have been no partisan effect on spending.[13]

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[10] One could argue that the current situation amounts to an abdication (necessary or not) of previous commitments.

[11] One can point to similar trade-offs made in welfare expansion: such as a tendency to privilege large firms over small ones.

[12] It is also the case that the main strategies of achieving reform under retrenchment that Pierson articulates—obfuscation, division, and compensation—are certainly pervasive in the history of welfare state expansion.

[13] There could be differences (e.g., the left prefers to spend more than the right), but those apply in reverse "on the way down" (i.e., the left prefers to *cut* less than the right).

We do find some distinctions in the empirical literature on expansion (e.g., that left parties are relatively more associated with consumption than transfers, which centrist Christian Democrats were willing to expand). Nonetheless, there is still an overall bias among the two for Social Democrats. Moreover, parties of the secular right have always been opposed to the degree of expansion. Thus, while the direction of trends in spending may have reversed, it remains true that distinct policy (and perhaps partisan) differences in the magnitude of the change (if not its direction) remain.

While the limited work on a “new politics” suggests some contemporary evidence that politics might be different in various ways, the overall evidence is at best limited, and on the basis of evidence we mention in the next section, and as reviewed by Scarbrough (2000), the evidence is still mixed. The reason is not that Pierson's observations about the present are incorrect, it’s the claim that blame avoidance and vested interests are fundamentally new to welfare state politics. Moreover, if accepted as they have been laid out, the full implications of this claim on our past knowledge needs to be explored.

II. Another End of Ideology? Political Partisans and Welfare Retrenchment

The rest of this paper re-examines one of the major accounts of welfare state expansion, one that has tended to be cast aside in the debate on retrenchment: traditional partisan politics. Here we will argue that to date there is at best limited theoretical reason to think that the partisan center of gravity no longer matters in explaining *differences* in welfare state retrenchment. We acknowledge at the outset that the effects of partisanship are only one explanation of expansion that deserves greater attention in the present environment as well, and we must allow for the possibility that considering other factors will qualify (if not upturn) our inferences.

Most explanations of why parties should no longer matter for welfare state expansion/retrenchment are in fact explanations of why it might be hard to *find* such differences (generally statistically), and not really a turn away from the belief that partisan differences are irrelevant.[14]

Arguments tend to suggest constraints from both ends of the spectrum. On the right, the existence of the welfare state is said to check constraints on retrenchment (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. Of course, all of these factors may play a role on both sides of the partisan divide. Indeed, the "constraints" operating for one side should bolster the ability of the other to pursue their brand of reform. In other words, by itself, appealing to the pressures for globalization offer the right "ammunition" to use against any entrenched opponents, just as opponents of retrenchment may use vested interests as to resist pressures from globalization.

Regarding the literature on partisanship in retrenchment, our conclusion contains two elements. First, most approaches to the role of parties in recent years do not really deny fundamental partisan differences that may create policy divisions. In other words, there are still reasons to believe that there are fundamental partisan differences. Second, the lack of empirical *evidence* for partisan differences might be best thought of as the result of overly crude indicators of the welfare state.

Pierson's study of the US and UK develops explicitly from the puzzle of why two very neo-liberal/rightist administrations, with extensive plans to roll back the welfare state failed to do so convincingly. While he is almost certainly correct that Reagan and Thatcher probably got less retrenchment than they wanted, from a broader comparative perspective they were quite successful by many criteria.[15]

This is particularly true of welfare state entitlements. In the UK, while the overall average decline in net unemployment benefit replacement rates between 1980 and 1989 was -1.1 points (from 65.1 to 63.9), the decline in the UK in that period was 21 points

[14] This seems to apply for other explanations as well. For example, Iversen and Cusack's conclusion that Pierson's "path dependence" explains why the effect of de-industrialization persists after the causal agent goes away is extremely ad hoc.

[15] Clayton and Pontusson (1998) provide further evidence that, considered in the context of increased need, the reforms constituted a much larger pullback from state protection that Pierson's data suggests.

(50% to 29%)![16] While the average net sickness benefit replacement rate rose slightly from 69.9% to 71.7% across our entire group of 18 OECD countries in this period, it fell more than 16 points in the UK from 50% to 33%. Finally, while the average net replacement rate for the minimum social pensions (that paid to retirees with little or no other source of income, and who do not qualify for a work-related pension) increased from 36% to 42% of average production worker earnings, the replacement rate was unchanged in the UK during Tha tcher's rule. Combined with her success in eviscerating trade union power, ending the state earnings related pension, and privatizing not only numerous public services, but also twenty percent of the public housing stock, one could certainly cite this as a considerable roll-back—in the course of a single decade—of what was one of the longest established welfare states in the world.

Changes in the United States to unemployment and minimum pensions under the Reagan administration were less dramatic than those in the UK, but they were nonetheless comparatively large. Unemployment insurance replacement rates fell from 66% (slightly above average at the time) in 1980 to 61% in 1986, when the benefit became taxable.[17] Net minimum pensions (full amounts under Supplemental Security Insurance) increased by 3.5 points over the period; however, this net replacement rate in the US fell further behind the rate average change of for other countries. As in the UK, the Reagan administration also targeted labor unions, a major proponent of the American welfare state (such as it was).

Pierson does not deny some of these changes, but he does infer from these examples that the partisan center of gravity has little overall impact on welfare state retrenchment. This conclusion may seem questionable. Most fundamentally, the comparative results presented suggest otherwise. For evaluating partisan effect, his comparison of UK and US results lacks variation on the key independent variable:

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[16] This data is described below. Unemployment and sickness replacement rates refer to the average of a fully insured single APW rate and single-earner APW with 2 children. For minimum retirement income, the amount is the maximum means-tested benefit and it was assumed that there were no longer any children at home.) With respect to the UK and US, the drop in replacement rates would be more dramatic if the calculations included a working spouse due to the way the benefit and tax systems changed over the period.

[17] The benefit is not subject to federal tax, but, since taxable income for state tax is calculated on the basis of federal income, it is subject to state tax in most places.

ideological color of the governing executive.[18] Also more or less overlooked in both of these case studies are the positions of opposition parties and the public in the period. The opposition parties in both countries favored either increases in the welfare state (Labour in the UK) or much more restrictive cuts (Democrats in the United States, Liberals/Social Democrats in the UK).[19]

Moreover, insofar as there is variation in the overall partisan "center of gravity" between these two cases, the differences corroborate a partisanship story. Of the two neo-liberal executives, the Thatcher government had a clear governing majority in an effectively unicameral Parliament. Reagan, on the other hand, faced a long-established (if not ideologically coherent) Democratic majority in the House of Representatives *and* a Democratic majority or blocking minority in the US Senate during his tenure. The fact that Thatcher was more successful than Reagan follows from the fact that the political center of gravity in the UK moved clearly to the right, while in the US it moved so much less decisively.[20]

Several other accounts of partisan "non-differences" also have problems. Huber and Stephens (2001a) explicitly test for effects of Social Democracy and Christian Democracy executives on patterns of welfare spending during retrenchment. They find statistically significant positive relationships only for the period 1960-1972. Their interpretation that a shift in the policy agenda to defending entitlements explains the insignificant estimate for partisan differences is only one of many possible ones. Shifting to "defending" rather than "expanding" entitlements does not imply a narrowing of partisan differences. If a notional upper bound of spending and the maturity of programs were generally reached, a structural shift in the annual change in benefits has no implication for the impact of partisan differences. Even if pressures of globalization mattered (something that has been disputed empirically in many studies) and caused a

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[18] Indeed, the largest level of variation in partisanship

[19] An inverse account of the UK and US in the 1980s might be Sweden in the 1950s and 1960s. Here we might ask all questions in reverse. Why did the Social Democrats not do more to socialize the economy? The answers: capitalists and farmers (vested interests) and the risk of massive capital flight and loss of business confidence ("blame avoidance"). Such barriers were overcome with compensation, obfuscation and division (cow trades, investment credits, non-taxed profit accounts).

[20] The issue of the different political institutions is perhaps relevant here, insofar as Thatcher's majority was "artificial."

shift to a reduced spending paradigm, all parties may reduce commitments, but left parties would do so less than right parties.

An alternative interpretation of their results is simply that there is not enough data to confidently estimate the effect. Most of Huber and Stephens's estimates for 1972 to the 1990s suggest a *positive sign* for left parties. The fact that the estimate is not statistically significant may be the result of greater variance in their limited samples (18), not a zero effect. (Indeed, only trivially sized, but statistically significant, coefficients would provide any positive evidence of a diminution of the effect. In such a case more (precise) observations are needed to get better estimates.

Several other factors should be considered vis-à-vis their results. First, Huber and Stephens divide the 1973-1995 period into three sub-periods periods and employ a simple cross-sectional analysis of each. The reason for this approach, in contrast to a pooled analysis of, say, five-year averages and period dummies, is not clear. We show results for the latter approach below. Second, as we explain below, virtually all of their main dependent variables are based on spending levels that are subject to some serious limitations (as we also explain) as indicators of the partisan distributive goals generally held to be at stake in partisan conflict over the size and shape of the welfare state.

Finally, one argument that is consistent with a partisan effect and Huber and Stephens's and Pierson's explanation is that there are new constraints (e.g., the welfare regime, unemployment or capital mobility) determining spending priorities, *and that these are correlated with partisanship and the size of the welfare state*.[21] If so, and if they are correlated with dependent variables *and* partisanship, then statistical controls for these effects could alter the point estimates for partisanship, though they will necessarily inflate the errors associated with the partisanship estimate. This is likely to be severe in a model with only 18 observations.

In short, the Huber and Stephens results are not designed as a test of Pierson's thesis, and what they infer from the "non-results" for partisanship may easily be attributable to limited data.

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[21] If the "new" constraints are not correlated with *both* partisanship and welfare effort, it will not affect the estimates or errors associated with partisanship.

A third issue in the relationship between retrenchment and partisanship may be more in keeping with the "new politics" idea. There seems to be general agreement among many that the future holds out the prospect of important distributional welfare state issues. If true, we may expect that the locus of partisan differences may shift from aggregate spending (if that ever was indeed a difference, see below) to program details. If true, we will expect to find evidence at the program level. The evidence we present below may provide some suggestive evidence in this vein.

Kitschelt (2001) offers a framework to suggest that retrenchment problems will depend on specific parameters of the party system, and strategic electoral alternatives/constraints available voters/parties given certain choices.[22] His argument is largely consistent with the idea that left parties desire a larger state, even though he allows for left parties to pre-empt greater retrenchment by the right. (His framework is ambiguous about whether the left prefers more spending than Christian Democratic parties.) As he also points out, aggregate spending data is generally a poor way to evaluate welfare state change. This is a matter to which we return below.

A final criticism of results to date is that they have tended to rely either on examples of retrenchment by left parties (New Zealand) or advocacy of such reforms by left parties. Given the recent nature of retrenchment, it is important to be cautious about concluding that because a left party cuts government spending, this implies an absence of partisan impacts. (After all, the first major state welfare programs were in distinctly non-leftist governments!) This may be an impossible counterfactual to examine now, but the problem will decline as time in the "retrenchment era" passes.

III. Measuring Welfare State Decline

Any attempt to explain expansion or retrenchment must confront one of the most enduring debates in the comparative welfare state literature: what has now been labeled the "dependent variable problem." As Christoffer Green-Pedersen (2000, n.d.) observes, this debate (like the "new politics" debate discussed above) has only intensified in the

[22] We think that his approach could be an intervening factor with respect refining to making predictions about partisanship. However, we were unable to consider these refinements in the current version of the paper.

wake of Pierson's work on retrenchment, although questions have been raised about measurement issues for a much longer period. A resolution of this problem remains elusive, since, in Green-Pedersen's words, it is essentially a "problem of [the] theoretical conceptualisation of retrenchment" (n.d.: 6). In other words, how we measure changes in the welfare state over time ultimately depends upon the aspect of the welfare state with which we are theoretically concerned.

In this respect we take the position that since the welfare state exists to a large degree (but not, of course, exclusively) in order to minimize the economic risks posed to individuals and families by short- and long-terms absences from the labor market through illness, old-age, or unemployment, a valid empirical measure of the expansion or decline of the welfare state should reflect the extent to which welfare state programs individually or collectively impinge upon the life chances of individuals exposed to such risks. In this section of the paper, therefore, we review some alternative approaches to the operationalization of welfare state change in more detail, and discuss why we believe them to be unsuitable for measuring expansion or decline in our terms. We then outline our own approach, which builds on Esping-Andersen's path-breaking *Three Worlds of Welfare Capitalism* (1990). Combined with previous criticisms of prevailing measures of welfare effort, our findings here point to further shortcomings of such data that could bias past results.

The most widely used approach to quantifying changes in the size of the welfare state during periods of both expansion and decline has been to use aggregate social expenditure data as an indicator of a government's "welfare effort." 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 GDP have all been used as indicators of "welfare effort" (e.g., Wilensky and Lebeaux 1958; Cameron 1978; Korpi 1983; Pierson 1996; Garrett 1998; Stephens, Huber and Ray 1999; Huber and Stephens 2001a, b).

Reservations about the use of expenditure data are well documented (Esping-Andersen 1987, 1990; Gilbert and Moon 1988; Castles and Mitchell 1992; Clayton and Pontusson 1998; Goodin et al. 1999). As Esping-Andersen makes clear, "their focus on spending may be misleading" (1990: 19). Aggregate spending data cannot tell us very much about how, or on whom, the money is (re-)distributed. In terms of understanding the impact of welfare state outcomes on individual life chances, this distinction can be crucial. For example, two countries may devote approximately the same portion of GDP to social security transfers, but if one devotes most transfers to means-tested programs, while the other has universalistic benefits, the levels of benefit received by recipients in each country may differ greatly.

In addition, changes in spending levels may simply be an artifact of factors that ultimately have little bearing on recipients. To take the example cited by Esping-Andersen (1990: 20), expenditures on unemployment benefits in the United Kingdom grew sharply in the 1980s under Thatcher, while replacement rates were sharply cut. The rising expenditure was simply a product of rising levels of unemployment – not of more generous benefits. As long as the growth of dependence exceeds the per-capita percentage reduction in benefits, spending will be higher. Similarly, increases in aggregate social spending may be a product of rising age-dependency ratios, as the elderly populations continue to grow, and the maturation of public pension programs. Once again, we cannot from expenditure data alone draw many conclusions about the longer-term changes in the living standards of recipients.[23]

The misleading nature of some expenditure measures can be further exacerbated when we take into account the role of a state's tax treatment of benefits. The role of taxation as an avenue for social transfers has been given more attention in recent years (see, e.g., 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 back apparent increases in social spending. Almeda (1998) notes that increases in gross expenditure levels can be offset considerably by changing the tax treatment of transfers (e.g., by making benefits taxable), or indirectly by increasing consumption taxes. Either way, "net spending can thus often fall well below gross expenditures" (1998: 20).

[23] Some recent studies, such as Iversen and Cusack (2000) take considerable strides in trying to control for effects like dependents.

Thus, we are skeptical of using expenditure data as an appropriate measure of how welfare state reforms impact upon individual life chances. If we are to examine the impact of the welfare state at the individual level, it is necessary to look at elements of welfare state programs that can be measured at the individual level too, namely, benefit replacement rates and qualifying conditions. Castles (1998), while regretting the widespread absence of such data, argues that for most research "the loss may be relatively minor. Partly that is because the new measures [i.e., replacement rate data] are effectively disaggregations of broader expenditure outcomes, so that conclusions arrived at by the two different measures may not be hugely different" (1998: 149). For the reasons outlined above and in light of the empirical data we present below, however, we would disagree.

Although we believe that individual outcomes measures are preferable dependent variables if our chief concern is explaining changes in the welfare state's impact on individuals and families, problems remain. In contrast to expenditure data, there is a real paucity of data reporting benefit replacement rates. Recent studies have made use of available data, such as the unemployment benefit replacement rate data published by the OECD (e.g., Iversen, 2001). Unfortunately, the OECD data only provide *gross* replacement rates, which, like the expenditure data, can be misleading. Once again, the tax treatment of benefits can distort the picture presented by looking at gross replacement data alone, since the taxation of benefits varies considerably across countries. In 1991, for example, the correlation between the OECD's gross replacement rates and our calculated *net* replacement rates in our sample of countries was strong (r=0.68), but not strong enough to suggest that the former measure is a good substitute for the latter (OECD, 1994: 175). In fact, depending on the tax treatment of benefit income, a group of countries with a wide spread of gross replacement rates can all report approximately similar net replacement rates. For example, for 1997, *gross* unemployment benefits replace 95% of an average production worker's net income in Denmark, 88% in Norway, 79% in Finland, 76% in Canada, and 65% in Belgium. Yet, after benefits are subjected to taxation, the *net* replacement rate in all five countries is between 60-65%. Not only does this create problems for comparison across countries using gross replacement rate data, but it also may produce misleading comparisons over time, since large increases in gross replacement rates can be absorbed by the introduction of the taxation of benefits. This is exactly what happened in Norway: between 1979 and 1993 the gross replacement rate of benefits jumped from around 41% of an APW's net income to over 62%. Over the same period, however, the net replacement rate actually fell one point, from 62% in 1979 to 61% in 1993. The main reason for this is that in 1979 benefits were not taxed, although they were in subsequent years.

Given the discrepancies found in the gross replacement rate data, the case for using net replacement rate data as an indicator of individual welfare state outcomes is strong. Of course, this approach is not new, and is featured heavily in Esping-Andersen's seminal *The Three Worlds of Welfare Capitalism*. But while Esping-Andersen's study is widely regarded as path-breaking for its conceptualization of welfare state regimes, based in large part on their impact on individual life chances through his "index of de-commodification," subsequent studies focused on his "welfare regimes" concept largely at the expense of any attempt to build upon the available replacement rate and other entitlement data. Moreover, although the regime typology has by now become prevalent in the comparative welfare state literature, few studies have attempted to replicate the original study (Esping-Andersen based his findings on a single year, 1980) to see if the regimes still exist today.[24]

In order to provide a corrective for this lacuna in the comparative welfare state literature, we are currently in the process of compiling a data set comprised of cross national and time series data on benefits and qualifying conditions for a number of major welfare state programs. It is from this data set that the replacement rate data used in our analyses below are drawn.

IV. Empirical Analysis

Dependent Variables

In this section we examine the determinants of changes in replacement rates for unemployment benefits, sickness insurance, and minimum (social) pensions in eighteen

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[24] For a replication of Esping-Andersen's "index of de-commodification" for 1997, which in fact largely confirms the continued existence of the "three worlds," see Allan and Scruggs, 2001.

OECD countries over a twenty-year period beginning in 1977 and ending in 1997.[25] The time period is divided into four five-year groups. For the reasons explained above, the dependent variables measure changes over the four time periods (1977-82, 1982-87, 1987-92, 1992-97) in *net* replacement rates. To get this value we calculated the post-tax replacement rates provided by the respective benefits for a single worker earning average production worker wages, and a married couple (single earner with APW income) with two dependent children.[26] The average of the two net replacement rates is used here.

Independent variables

Our primary task is to explain the role that partisanship has played in changes to benefits over the period in question. There has been some controversy among scholars of political parties as to how partisanship can be best operationalized. As Schmidt cautions, "the choice of a particular indicator of the party composition of government can make a very large difference to estimates of partisan influence on public policy" (1996: 162). Somewhat surprisingly in our view, this particular "independent variable problem" has not been addressed to a great extent in relation to the welfare state. Most studies have tended to focus on left party strength in government, usually measured in terms of the share of cabinet seats. Given the traditional association between social democratic parties and welfare state expansion, one might reasonably predict a positive relationship between left cabinet seats and an increase in replacement rates. However, as has been made clear in a number of studies, Christian democratic parties have also been associated with welfare state expansion, yet such a relationship would not be apparent if a simple left party strength measure or scale was adopted. For this reason, and following Castles (1982), there are strong theoretical reasons to believe that *right* party strength—measured here in terms of the average right cabinet seat shares for each five year period—is a more useful indicator than any based on left party strength.[27] Parties on the right in the

[25] 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. Our final data set will include data from three additional countries (Greece, Portugal and Spain), but the re is currently insufficient information to include them in this analysis.
[26] For old age pensions, replacement income exc ludes any adjustment for children.
[27] More precisely, the five-year average for right cabinet share does not correspond exactly with the time periods mentioned above. Since a change in government is unlikely to produce immediate changes in benefits, and governments enter in the year they are elected, no matter when in that year, a one-year lag has

conservative or neo-liberal mold are more typically associated with efforts to "roll back" the welfare state, which set them apart from both social democratic and Christian democratic parties that are more supportive of welfare policies. Thus, the right party strength measure more neatly encapsulates the distinction between parties based on their ideological approach to social policy.

In addition to the partisanship variable, we include several explanatory variables commonly found in other models of welfare state change. Average per capita 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). In order to account for initial differences in benefit levels, the replacement rate for the beginning of each five-year period is also included in the analysis as an independent variable. This variable also serves as an indicator of conditional convergence: a negative coefficient indicating that above (below) average initial replacement rates are associated with more (less) cutbacks. Schmidt's "Institutional Constraints of Central State Government" measure (1996: 172-3) is included to assess the extent to which governments may find their "room to maneuver" restricted by constitutional factors and other veto points.[28] For the analysis of change in unemployment benefit replacement rates, the average unemployment rate for each five-year period (again, lagged by one year) is also included as an independent variable. Similarly, for the analysis of changes to the social pension, the average share of the population that is over 65 is included as an additional control.

Estimation Technique and Results

Given the well-documented caveats associated with using regression analysis with cross-national and time series data (Beck and Katz 1995), we estimates our models using

been inserted. This is also the case for the other independent variables in our models. Parties data are drawn from Duane Swank's "Comparative Political Parties Dataset," which can be found at: http://www.marquette.edu/polisci/Swank.htm

[28] Higher values indicate more constraints.

OLS regression with panel corrected standard errors (PCSE). The basic models for unemployment benefits, sickness benefits, and social pensions are shown in Table 1.

[Table 1 about here]

For unemployment benefits, estimates for all of the independent variables are in the predicted direction, but only three are statistically significant. The negative and statistically significant (at the 90% confidence interval) coefficient for the initial replacement rate is indicative of conditional convergence among the eighteen countries in the sample. The unemployment rate is also significant and negatively correlated with increases in net replacement rates: a two point increase in the average unemployment rate will cut net replacement rates by another point over five years, *ceteris paribus*. Most significantly for our purposes, however, there is a statistically significant (at the 99% confidence interval) negative relationship between the presence of right parties in cabinets and changes in the levels of net unemployment benefit replacement rates. All else being equal, the difference between having a cabinet with no right-party members and a cabinet composed entirely of right party members translates to a decline in the net unemployment replacement rate of around six points over five years.

A similar pattern is found with regards to sickness insurance replacement rates. Once again most of the independent variables are in the predicted direction. Interestingly, the two globalization indicators, trade and financial openness, are in opposite directions, but only the financial openness measure is statistically significant (and negative). Thus, we find some evidence that financial globalization has negatively affected sickness replacement rates. The only other independent variable that is statistically significant is the right party cabinet share variable, which is again negative and significant at the .01 level. In this case, an entirely rightist cabinet will cut replacement rates by around seven points more than a leftist or centrist cabinet over five years, *ceteris paribus*. For both unemployment and sickness insurance, therefore, we find that the presence of right parties in government is strongly correlated with benefit retrenchment.

While there is clear evidence that parties still matter when explaining variations in policy outcomes relating to unemployment and sickness benefits since the late seventies, the same cannot be said for social pension programs. Although the estimate for right cabinet share was in the predicted direction (negative), the only two variables in our model that were statistically significant were the financial openness measure, and the aged population share measure, which were both strongly negative. The initial models for unemployment and sickness benefits would appear to provide support for the claim that partisanship remains significant even in the face of globalization and institutional veto points. Do partisan effects remain strong, however, when the natures of the pre-existing welfare regime are taken into account?

In order to test the historical institutionalist claim that pre-existing institutional arrangements can constrain current political incumbents, dummy welfare regime variables were introduced in our original models. The results for unemployment and sickness benefits are shown in Table 2.[29]

[Table 2 about here]

As expected, both the social democratic and (to a slightly lesser extent) the conservative/Christian democratic regime types are strongly and positively associated with more generous benefits in unemployment and sickness insurance programs. Indeed, assuming nothing else changes, a country with a social democratic regime sees unemployment replacement rates increase by twenty-five per cent more, and sickness replacements rate rise by more than a third more, than a country with a liberal regime over the same period of time. More notable, however, is that while the introduction of the welfare regime variables increases the total variance explained in each model, partisan effects, though diminished slightly, remain strong and statistically significant: regardless of the regime type, right party cabinets will still produce a decline of around five points more, for both unemployment and sickness benefits, than a non-right cabinet. Thus,

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[29] Since the introduction of dummy regime variables to the social pension models did not change the overall model to any great extent, and little additional variance was explained, the results are not reported here.

these findings run contrary to recent studies that argue that partisan effects on the welfare state declined considerably in the 1980s (cf. Hicks 1999: 220-1).

Given that some countries experienced dramatic changes in net replacement rates for particular programs during our period of study, we suspected that some outliers might have been driving our models. An examination of partial regression plots showed that for unemployment benefit replacement rates, Finland was somewhat of an outlier, while Norway was an outlying case with regards to sickness benefit replacement rates. When these countries were dropped from the respective analyses, the coefficients for the right cabinet share variable decreased marginally in both cases, although they remained statistically significant. However, the statistical significance of the social democratic regime variable disappears. Since, obviously, Finland and Norway both fall into the social democratic regime model, it would appear that it was the impact of historical institutional configurations, not partisanship, that was being driven by these outlying cases, and so our original conclusions about the impact of parties still hold.[30]

We argued above that using net benefit replacement rates as dependent variables was more appropriate than using aggregate welfare or transfer spending indicators, since replacement rates more directly reflect the impact of policy changes on individual life chances. Some scholars (e.g., Castles, *op. cit.*) have suggested that there may be little need for more detailed data about replacement rates since the overall trends may not differ by much from the already available spending data. Although on the basis of our above analyses this view would appear mistaken, it is important to remember that in addition to suggesting an important modification to the dependent variable, our approach also differs in its use of a less commonly used measure of partisanship compared to most existing studies, namely, the use of right, rather than left cabinet shares in government. In order to show, then, that our results really *are* different from previous analyses, and not just an artifact of using a different partisanship variable, we ran a similar model using total transfer spending as our dependent variable, using the same transfer data found in

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[30] An additional concern was that our choice of five-year periods (1977-82, 1982-87, and so on) may have been "exceptional" in that we might have produced differing results had we started our analysis in, for instance, 1976 or 1978 instead. Running the models for several years on either side of our chosen years, however, showed that the overall patterns and effects remained stable under these modifications.

Iversen and Cusack (2000). If our earlier results differed from existing studies only based on our choice of partisanship measure, we would expect a similar partisan impact on changes in total transfer spending over a similar period. Our results are shown in Table 3.

[Table 3 about here]

Table 3 shows the results of our analysis of the determinants of changes in total transfer spending between 1975 and 1995, with the data again being grouped into four five-year periods.[31] Just as previous studies found little partisan effects on spending patterns since the 1970s using the left cabinet measures, we also find no partisan impact using right cabinet seat shares as our partisanship measure. This would appear to lend support to our view that, not only is the use of replacement rate data preferable to using spending data on the basis of the factors we outlined above, but it also captures quite different dynamics in terms of the partisan impact on social policy outcomes for benefit recipients. While partisan impacts on overall spending levels may indeed be disappearing as all parties face similar budgetary constraints, our analyses show that social and Christian democratic parties still appear to have different priorities compared to their counterparts on the right when it actually comes to allocating these resources. And it is surely these ideological differences that have the greatest impact on benefit recipients across our sample of countries.

Conclusion: Politics as Usual?

Our contentious tone throughout this paper should not be taken to suggest that the idea of a new politics of the welfare state is out of the question. On the contrary, we think that the new politics thesis implies a fundamentally new approach to understanding both the future *and* history of the welfare state. The relevant themes of such a revision would include how vested interests and blame avoidance influenced alternative choices over programs and institutions during their inception and growth as well as their retrenchment and decline.

[31] We could not replicate the 1977-1997 exactly since we did not have spending data for 1997. In addition, New Zealand is excluded from this analysis due to a lack of data for the entire period.

However, we are skeptical of efforts to periodize the last 40 years into domains where the old politics works in "the past" and the new politics works in "the present." Advocates of either the new or old politics have tended to concentrate on evidence *in* "their" supposed temporal domains, and not on making a strong case for why we should restrict each set of explanations *to* those distinctive temporal domains.

Our empirical results support the idea of continuity of effect between the two periods, at least for political partisanship. We find that just as partisanship "mattered" in expansion, it appears to still "matter" in retrenchment. However, we have also suggested some reasons why ours is an incomplete test of that proposition. Our new twist on the problems with aggregate spending data, namely that the tax treatment of transfers may grossly distort real differences among countries, provides reasons to be cautious about the commonly accepted idea that differences in comparative spending *trends* are a good proxy for differences in comparative trends regarding the protection of individuals against exposure to economic risks.

Ultimately, we should be cautious about drawing domain boundaries around the recent period of welfare state development. As Pierson himself suggests (2001), we still know much less about retrenchment than we do about expansion. In that sense, it is not clear that we know enough to either jettison the old or embrace the new. Regardless of whether new explanations are ultimately needed, we should remember that one goal of social science is to have more rather than less encompassing explanations. Ideally, that means we would explain *both* the past *and* the present with similar, not different, theories.[32]

[32] Of course, one can argue that insisting on an absence of structural change in the relationships between the variables in our models is just as unjustified. Why should one assume that anything that seems to work yesterday should also work today and tomorrow?

Table 1: Regression Analysis of Change in Net Replacement Rates, 1977-1977

Program:	***Unemployment***	***Sickness*[a]**	***Social Pension***
Independent Variables:			
Initial Replacement Rate	-.205*	-.131	-.236**
	(.124)	(.111)	(.119)
Avg. Right Party Cabinet Share	-.059***	-.079***	-.015
	(.022)	(.025)	(.020)
Govt. Constraints	.175	1.078	-1.099
	(.376)	(.714)	(.679)
Avg. Trade Openness	-2.584	.500	-.607
	(2.646)	(3.690)	(2.036)
Financial Openness	-.352	-1.893**	-.944***
	(.483)	(.785)	(.350)
Avg. GDP Growth per capita	34.707	59.417	-33.209
	(101.482)	(83.515)	(46.443)
Avg. Unemployment Rate	-.501*		
	(.278)		
Avg. Over 65 Share of Total Population			-71.799**
			(32.994)
Constant	23.617***	31.890***	35.784***
	(7.715)	(8.882)	(11.646)
R^2	0.2544	0.2863	0.3287
N	72	68	72
T	4	4	4
C	18	17	18

Panel corrected standard errors shown in parentheses, *p < .1, ** p < .05, ***p < .01.
a: The United States has no national sickness insurance program, and is excluded from the analysis.

Table 2: Regression Models with Controls for Welfare Regimes

Program:	***Unemployment***	***Sickness***[a]
Independent Variables:		
Initial Replacement Rate	-.312**	-.326**
	(.155)	(.154)
Avg. Right Party Cabinet Share	-.044**	-.051***
	(.019)	(.019)
Govt. Constraints	.479	2.265***
	(.609)	(.832)
Avg. Trade Openness	-3.747	1.417
	(2.362)	(3.607)
Financial Openness	-.368	-1.750***
	(.396)	(.563)
Avg. GDP Growth per capita	18.844	31.594
	(93.904)	(71.971)
Avg. Unemployment Rate	-.247	
	(.235)	
Social Democratic Regime	6.398**	12.112**
	(3.235)	(4.904)
Conservative/Christian Democratic Regime	5.488***	6.139**
	(1.596	(2.975)
Constant	24.641***	34.029***
	(8.267)	(7.202)
R^2	0.3186	0.3934
N	72	68
T	4	4
C	18	17

Panel corrected standard errors shown in parentheses, *p < .1, ** p < .05, ***p < .01.
a: The United States has no national sickness insurance program, and is excluded from the analysis.

Table 3: Regression Analysis of Change in Total Transfer Spending, 1975-1995

	Model I	***Model II***
Independent Variables:		
Initial Transfer Spending Level	-.111 (.091)	-.189 (.119)
Avg. Right Party Cabinet Share	.002 (.005)	.004 (.004)
Govt. Constraints	-.441 (.166)	-.394* (.203)
Avg. Trade Openness	1.114* (.621)	1.110 (.700)
Financial Openness	-.170 (.104)	-.112 (.118)
Avg. GDP Growth per capita	-124.050*** (19.550)	-115.497*** (19.043)
Avg. Unemployment Rate	.006 (.069)	.121 (.101)
Social Democratic Regime		1.629** (.674)
Conservative/Christian Democratic Regime		1.409** (.610)
Constant	7.790*** (1.429)	6.224*** (1.121)
R^2	0.5165	0.5566
N	68	68
T	4	4
C	17	17

Panel corrected standard errors shown in parentheses, *p < .1, ** p < .05, ***p < .01.

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