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A New Approach to the Study of Partisan Effects on Social Policy

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Abstract

Much research has analyzed the influence of government partisanship on social policies. This paper addresses three challenges in that literature. First, it advocates a dynamic linear modeling (DLM) approach well suited to address time-varying partisan effects. Second, the DLM makes it possible to examine partisan effects country-by-country thereby avoiding the cross-national pooling approach dominating this literature. Third, it offers a new approach to the challenge of choosing the right control variables. Empirically, we demonstrate the approach using data from 1972 to 2011 from 14 Western European countries. The main conclusion is that the relationship between government partisanship and welfare generosity does not appear to vary over time according to any pattern predicted by existing theories. More generally, findings indicate that in the countries and years covered by this study government partisanship has very little or no association with the five different social policy measures included in the analyses.

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This paper contributes to the long-standing scholarly debate about how partisan government shapes social policy in advanced industrial societies. The debate has centered on the question of whether government by Left parties will produce significantly different policy outputs than government by Centre-Right or Right parties (Hibbs 1977, Schmidt 1996, Schmitt 2015). The classical argument is that trade unions and left parties represent working-class interests, which means left parties will promote policies protecting low-income workers against market vicissitudes and decreasing their dependence on employers (Korpi 1983). While some empirical studies report support for this general partisan hypothesis (Allan & Scruggs 2004, Schmitt 2015) most conclude that: (1) partisan politics do not matter (Garrett & Mitchell 2006, Swank 2002, Kittel & Winner 2005, Busemeyer 2009*a*, Jensen 2011*a*, Jensen 2011*b*); (2) the effect of government partisanship has declined over time (Allan & Scruggs 2004, Busemeyer 2007, Huber & Stephens 2001, Huber & Stephens 2014); or (3) the effect varies depending on contextual factors such as globalization, union strength, type of social spending, or institutional setup (Jensen 2011*c*, Jensen & Mortensen 2014, Kwon & Pontusson 2010, Iversen & Cusack 2000, Cusack, Iversen & Rehm 2006, Korpi & Palme 2003, Busemeyer 2009*b*, Garritzmman & Seng 2016).

This mixed evidence has been accompanied by vivid scholarly debates about measurement (e.g. Clasen & Siegel 2007, Wenzelburger, Zohlnhöfer & Wolf 2013) and methods (e.g. Kittel & Winner 2005, Schmitt 2015, Garritzmman & Seng 2016). This paper contributes three innovations to this ongoing debate about (dynamic) partisan effects on social policies. First, we introduce a familiar but underutilized modeling approach to test for dynamic partisan effects: the dynamic linear or state space model (DLM). Theoretically, the idea of time-varying partisan effects has generated much attention, but estimating dynamic effects presents major challenges to conventional econometric methods and many of the methods applied to test for dynamics have not been well-suited to detecting them. Our preferred method has provided insight into time-varying relationships in monetary policy making, public opinion, and international conflict (Beck 1983, Gerber & Green 1998, Green, Gerber & de Boef 1999, Mitchell,

Gates & Hegre 1999, Brandt, Williams, Fordham & Pollins 2000, McAvoy 2006, Enns & McAvoy 2011, Fariss 2014), but has not previously been deployed to study partisan politics.

Second, used in combination with the extended length of the most recently released time series data on social spending and social entitlements, DLMS makes it possible to study dynamic partisan effects *country-by-country*, avoiding the necessity of pooling data across countries. Pooling has characterized (almost) all previous quantitative studies of welfare policies (see Schmitt 2015, table 2). The potential drawbacks of pooling have not, however, featured as prominently in this literature. Foremost among these drawbacks are various ways in which panel regressions can mask inferential problems. Omitted variables, important either for conceptual and purely econometric reasons, can give rise to Simpson’s paradox – the phenomenon in which relationships observed at a grouped level (cross-national in our case) reverse or disappear at individual level (i.e. country-level) and vice versa.

Third, we use a new approach to selecting control variables. Previous work has adopted statistical controls from among a broad set of variables, differing both in theoretical justifications and in operationalizations. Given that there is not yet a consensus in the literature regarding the ‘correct’ controls, we are concerned that results relying on any one set of controls advocated by previous work will be model dependent. We minimize this risk by estimating the relationship between government partisanship and social policies using every combination of a list of candidate control variables found in previous work. We then enumerate every combination of them, iteratively including and excluding each in a series of regressions for each country in our data. This yields an average of 1,500 estimates of the relationship of interest per country, each using a distinct set of controls. We summarize all estimates by country together to draw our main conclusions.

Given the “dependent variable” discussion (e.g. Clasen & Siegel 2007, Wenzelburger, Zohlnhöfer & Wolf 2013) and the increased specialization of the welfare state literature, it is impossible to cover every aspect of the partisan government effect question in just one article. Guided by our substantial interest in the general claims about partisan effects,

we focus on the generosity measures from the Comparative Welfare Entitlements Dataset (CWED) (Scruggs, Jahn & Kuitto 2014). As argued by Wenzelburger, Zohlnhöfer & Wolf (2013, 1230) entitlements may be less sensitive to ‘demand factors’ than expenditures and more clearly connected to political decisions, which should make it a more likely place to trace partisan effects, should they exist (see also Green-Pedersen 2007). More particularly, since labor market policy taps into a key dimension of the classical argument behind the partisan hypothesis (Jensen & Mortensen 2014), we choose the “unemployment generosity index” (*uegen*) as our main dependent variable. However, to assess the robustness of our results, we rerun all main analyses using three other generosity measures as well as the OECD measure of social expenditures collected by Brady, Huber & Stephens (2014). Our time series span approximately 1972 to 2011 and include 14 Western European democracies. Our focus on Europe reflects the theme of this special issue, but note that since we estimate individual country models the reported results are not, unlike pooled studies, influenced by the choice of countries.

Our preferred measure of government partisanship is the proportion of cabinet parties coming from secular right parties. Excluding Christian democratic parties reflects that these parties have demonstrated left-leaning social policy preferences (Esping-Andersen 1999, Huber & Stephens 2001, van Kersbergen 1995). However, as we discuss later, this simple measure has (also) been subject to criticism. Consequently, we further employ two alternative measures of government partisanship, neither of which alters our conclusions.

Our results point to the conclusion that the relationship between government partisanship and social policy does not, in fact, appear to vary over time according to any pattern predicted by existing theories. Furthermore, the vast majority of our estimates of partisan effects are statistically indistinguishable from zero, implying that we also do not find support for a strong static partisan effect on social policy.

Partisan effects on social policies

One of the most prominent questions in comparative public policy regards the effect of partisan politics on policy outputs. In particular, the question of how government partisanship influences social rights and social spending has gained much scholarly attention. The classical and most general hypothesis is that partisanship matters because parties of different ideological orientations strive to favor their core clienteles, and the clienteles of left and right parties have salient differing interests on questions of redistribution and welfare state generosity (Hibbs 1977, Stephens 1979, Korpi 1983).

The argument for strong partisan effects on social policies has, however, been challenged by several counter arguments. First, it (at least partly) contrasts to Downsian theories of party competition, in which a main argument is that public policy is determined by the median voter's preferences rather than by the preferences of core constituencies of parties in power (see also Kwon & Pontusson 2010). Second, the argument for strong partisan effects is challenged by theories emphasizing the power of organized interests, bureaucrats, and the influence of the institutional structures and barriers of government (Immergut 1992, Schmidt 1996, Tsebelis 2002). According to the inheritance perspective advocated by Rose and colleagues, one such barrier stems from the fact that the policy agenda of every new government is heavily influenced by policy obligations inherited from the former government, which is argued to leave little room for partisan effects (Rose & Davies 1994). Third, scholars have argued that external pressure on governments arising from international competition have constrained national governments' ability to pursue ideological policies (Garrett & Mitchell 2006, Kwon & Pontusson 2010). Fourth, it has been forcefully argued by proponents of the "new politics of the welfare state" perspective that the effect of government partisanship has declined over time (Pierson 1996). A main reason, according to Pierson (1994, 1996), is that welfare states have contributed to the development of strong groups and program beneficiaries cutting across traditional party lines who can forcefully mobilize against cutbacks. In other words, retrenchment is a game of blame

avoidance and not simply the mirror image of welfare state expansion. While this argument may be most relevant to policies directed at particular strong groups, such as retirement and pension policies, there are also more general claims about declining partisan effects. Kwon & Pontusson (2010), for instance, expect partisanship’s effect to decline with increasing globalization, and Huber & Stephens (2014, 9) have ascribed it to increasing complexity of social policies (see also Gingrich 2011).

Turning to the empirical evidence does not provide much direction with respect to the empirical validity of the various claims. As summarized by Schmitt (2015, table 1) in a recent review of studies focusing on social spending measures as the dependent variable, the general picture seems to be one of either weak or no effects from partisanship. It should be noted, though, that most of these studies use data from after 1980, making it difficult for them to evaluate the potentially dynamic partisan effects.¹ Disaggregating social spending to spending on health care, Jensen (2011*c*) reports no statistically significant effect of government partisanship on health care spending, but a long-term and negative effect on replacement rates when government is dominated by right-wing parties. Partisan effects on replacement rates were also found by Allan & Scruggs (2004), and Korpi & Palme (2003) report strong effects of partisanship on various types of transfers. Iversen & Cusack (2000), on the other hand, reveal no direct partisan effects on transfers, and Jensen (2011*a*) finds no effects of government partisanship in studying public expenditures on transfer programs as a percentage of GDP (see also Jensen 2011*b*).

With respect to dynamic effects, a quantitative study of social spending by Huber & Stephens (2001) provided preliminary support for the decline-of-partisanship hypothesis. This conclusion is reiterated in a recent paper by the same authors (Huber & Stephens 2014). Using 16 different dependent variables and comparing effects of social democratic and Christian democratic governments before and after 1985, they conclude that partisanship had bigger effects on a larger range of dependent variables before, compared to after, 1985.

¹Schmitt (2015) herself actually reports persistent partisan effects. We return to her study in the methods section in discussing the proper unit of analysis.

In a more conventional single-dependent, multivariate study, Kwon & Pontusson (2010) use 10-year moving windows to explore changes in estimated effects of Left governments on social spending across OECD countries. The analysis provides some evidence on increasing partisan effects during the 1970s and early 1980s, which disappear in the 1980s and 1990s. Busemeyer (2007), studying public education spending in 21 OECD countries from 1980-2001, reports stronger partisan effects in the 1990s than in the 1980s. In a recent study, however, Garritzmann & Seng (2016) find no systematic partisan effects on education when using an alternative estimation approach.

As should be clear from this summary of important quantitative studies of government partisanship's effect, it is not easy to draw clear conclusions about the (dynamic) effect of government partisanship of policies of social rights and social spending. Naturally, given the importance of the research question and the disagreement across studies, there has been a vivid debate about how the dependent variable should be conceptualized and measured (e.g. Clasen & Siegel 2007) as well as a debate about how to improve the methods used to analyze these data (e.g. Kittel & Winner 2005, Schmitt 2015, Garritzmann & Seng 2016). Regarding the latter question, however, three central challenges have received little attention in this literature. Two of these relate to effect heterogeneity (over time and across countries) and the third to how the control challenge has been handled.

First, as shown above, theories of time-varying partisan effects have garnered much scholarly attention, but the estimation challenges involved in testing dynamic theoretical expectations have been underappreciated. One commonly chosen option is to split single series into meaningful time periods and estimate effect coefficients in separate regressions or via interaction terms by time periods. This approach to estimating dynamic effect parameters has two crucial weaknesses. The first, and likely lesser, issue relates to statistical efficiency and power. Subdividing time series means our estimates are less efficient in the sense that they do not use all of the information in the data. It also weakens the strength of our inferences by working with smaller samples, increasing the risk of both Type I and Type II

errors. Low statistical power is also a difficulty for CUSUM and CUSUMSQ plots as tests for parameter stability (Wood 2000, Baltagi 2011).

A second, and potentially more challenging, difficulty is that subdividing time series requires researchers to identify and motivate their choice of relevant time periods *a priori* (see, for example: Huber & Stephens 2001, Allan & Scruggs 2004, Huber & Stephens 2014). Examining, for instance, the decline of partisanship hypothesis, the timing of changes is crucial to the question under investigation. If our model requires that we select them, the theoretical disagreement among researchers regarding cutoff points for the relevant time periods presents a substantial problem. If we test multiple cutoff points, there is no principled method for choosing among possible models. Furthermore, subdividing time series still requires assuming constant effect parameters within periods, which is not very plausible. Although a theoretically motivated choice of time periods may be available for certain applications, there is nothing categorical about the theoretical arguments behind the various hypotheses regarding government partisanship and social policy. Thus, theory is badly aligned with standard empirical methods in this case.²

These problems with statistical inefficiency and motivating *a priori* information extend to analyses using moving windows (Kwon & Pontusson 2010, Finseraas & Vernby 2011). Moving window analyses ignore large portions of data to estimate each bit of the dynamics in effect coefficients. Though eventually all data is considered, observations at the beginning and end of the time series are less influential than those in the middle, and each individual estimation is less efficient and has less statistical power than would be achieved using all of the data. Furthermore, decisions such as the size of window and which observations to include in each analysis can be consequential for the results, yet they are left to the researcher to decide without benefit of principled guidelines.

In the second place, there is the possibility of effect heterogeneity across countries. We

²Similar difficulties with motivating *a priori* information apply to the Chow test, which can reveal if shifts in effect parameters occur at particular points (Beck 1983, Wood 2000).

consider this another important validity threat for inferences about the relationship between government partisanship and social policy. The countries we study vary in political party systems, government institutions, norms and traditions of cabinet formation, electoral systems, and social welfare systems – to name only a few. Pooling over such variety risks confounding from various sources. To ensure our inferences are not subject to Simpsons paradox – the phenomenon in which group-level associations are reversed or non-existent at individual level, and vice versa – we should observe the hypothesized relationship in individual countries. This point is more than purely cautionary. Pearl (2014) provides guidelines for when to favor disaggregated over aggregated data for inference. In our context, this occurs in two notable cases. Consider some factor, Z , varying systematically across countries. One occurs when Z influences both government partisanship and social policies. The other happens when Z and government partisanship are influenced by the same prior factor, and then both Z and government partisanship go on to influence social policy. Our concerns regarding confounding from political party systems, electoral systems, etc. are of exactly these structures.

Although there exist modeling techniques for time-series cross-sectional data that allow effect parameters to vary over cross-sections (i.e. individual countries), we argue that a more appropriate test for the partisanship argument is to examine countries separately. Based on the theoretical argument that the relationship of government partisanship to changes in welfare state generosity has changed over time, it should be the case that most or all countries normally addressed by that argument individually exhibit evidence of the trend. If the effect spans industrialized societies, then it does not follow from existing theories that cross-sectional data is necessary to detect it. Furthermore, testing at the country level allows great flexibility in detecting changes. Variation in the timing of election cycles and short-term economic changes almost certainly prevent changes in the effect of partisanship from being synchronized across all countries in our sample. Analyzing countries separately avoids averaging over out-of-sync changes.

Finally, we confront the challenge of choosing valid control variables. Previous empirical work has adopted control variables relying both on different theoretical arguments and different lag structures from one another. That is, some studies argue for a risk of confounding from certain theoretical concepts, such as globalization or union strength, while other studies do not consider the same concepts as confounders. Other controls, such as trade openness, have been measured either contemporaneously or lagged by one period. To be as confident as possible in our inferences about the relationship between government partisanship and welfare policy, we wish to avoid making inferences dramatically affected by the inclusion or exclusion of particular controls. Of course, if a result is very different from all others, that may be because it has the “right” controls or it may be because the result is wrong and highly dependent on the selection of control variables. To ensure that our results depend as much as possible on the data and as little as possible on our choice of model specification, we draw on the careful theorizing in previous work and adopt (almost) all control variables and lag structures used in a set of recent analyses. Instead of making a single theoretical argument about which specification is “right,” we estimate models using every possible combination of these controls for every country in our data. This implies estimating a large number of models for each country. We report final results combining all estimations for each country to show the degree to which the various models agree. We consider this a useful step forward in a field featuring such variety in modeling choices.

A More Direct Approach to Time-Varying Partisan Effects

The dynamic partisan effects question is one case of a general problem that has proven tricky to address directly with statistical analyses, namely: how does the relationship between an outcome and explanatory variables change over time? Estimating dynamic relationships between variables is a challenging econometric problem that has been approached in several

ways. In the most basic sense, we wish to estimate a model in which a parameter for the effect of some explanatory variable, x , on an outcome, y , is allowed to vary by time periods. For a single time series, with one observation per period, we might write the following model for an outcome y_t with constant intercept and a time-varying relationship to explanatory variable x_t :

$$y_t = \beta_0 + x_t\beta_{1t} + \epsilon \tag{1}$$

This ideal model, however, is unidentified without additional assumptions. That is, even with an infinitely long time series we could not estimate the true values of β_{1t} because there are always more unknown parameters than data points – at least $t+2$ for the model in equation 1, assuming constant variance. Various compromises from this ideal model have been used to identify and estimate more restricted models. For example, we might constrain β_{1t} in some way. Another option would be to pool data over spells of, say, k time periods to recover fewer $\binom{t}{k}$ time-varying estimates of β_1 . We might pool data across a panel, within time periods, to estimate each β_{1t} with several cross-sectional observations. In this section we introduce and motivate our preferred method of approximating the ideal time-varying coefficients model, relying on assumptions constraining how β_t varies over time. We then discuss why our choice is well suited to our application.

Several features of our research question recommend our choice. First, given the disagreement about the nature and timing of changes in effects of government partisanship on social policy, we prefer models offering maximum flexibility. That is, we would like our model to estimate dynamics of many shapes and sizes with minimal *a priori* information from the analyst. Secondly, previous work on dynamic effects of partisanship has relied on cross-sectional data. However, as argued above, using only pooled cross-sectional data can mask heterogeneity in these effects across countries (Greene 2003, pp. 318-334).

Using single time series, however, makes the econometric problems in estimating dynamic coefficients quite demanding. Borrowing information across countries by pooling over a panel

to estimate variation in each year’s coefficient on government partisanship is impossible, so we identify the model using assumptions about β_t . The solution we choose is the method of flexible least squares (Kalaba & Tesfatsion 1988, Wood 2000) or dynamic linear models (DLM) using Kalman filtering (Shumway & Stoffer 2010, Enns & McAvoy 2011).³ These methods are flexible with regard to detecting the timing of changes in parameter estimates, can handle multivariate model specifications, and demand minimal assumptions about the form of dynamic relationships. We apply the DLM framework – a type of state space modeling – since it also returns useful estimates of uncertainty.

Returning to the idealized model above, the DLM addresses the identification problem without pooling over time or cross-sections, instead constraining dynamic coefficients with distributional assumptions. We assume β_t are realizations of an observed and underlying “state” that varies as a Markov chain⁴ over time, with each new estimate (β_t) diverging from its previous realization (β_{t-1}) as a function of the model’s predictive error in the new period. The magnitude of β_t ’s divergence from β_{t-1} is determined by the prediction error and the ratio of uncertainty about the coefficients to the total model uncertainty. Where uncertainty about the coefficients is greater, their estimates move more in response to prediction errors. Where overall model uncertainty is greater, new estimates respond less. Thus, the model flexibly returns time-constant coefficient estimates or detects dynamic ones where appropriate.

$$y_t = \mathbf{X}_t\boldsymbol{\beta}_t + v_t \tag{2}$$

$$v_t \sim i.i.d.(0, \sigma_v^2)$$

$$\boldsymbol{\beta}_t = \boldsymbol{\beta}_{t-1} + \mathbf{w}_t \tag{3}$$

$$\mathbf{w}_t \sim i.i.d.(0, Q)$$

³These have been shown to be algebraically equivalent approaches to modeling dynamic coefficients (Montana, Triantafyllopoulos & Tsagaris 2009).

⁴A random process varying over time according to a certain pattern: the probability distribution over next period’s realization is only a function of the current period’s realization.

Equations 2 and 3 express this formally. The first line is the measurement equation. The generosity of welfare benefits, y_t , is related to a vector of explanatory variables $\mathbf{X}_t$. The coefficients on those explanatory variables are the vector of time-varying effect coefficients, β_t . Errors, v_t , are independent, normally distributed, and mean-zero with variance σ_v^2 . Equation 3 is called the state equation. The vector of effect coefficients, β , varies over time as a Markov chain. Disturbances in the state equation, vector $\mathbf{w}_t$, are independent, normally distributed, and mean-zero with covariance matrix Q . They are assumed uncorrelated to v_t .

Estimating dynamic coefficients in the DLM is recursive. We begin by estimating the fixed parameters and initial values of the dynamic coefficients (i.e. $\beta_{t=0}$) via maximum likelihood, conditional on a set of starting values for the dynamic coefficients. Then, for each time t starting at period 1, β_{t-1} is our initial expectation of the value of β_t , conditional on information observed up to time $t - 1$. Based on this conditional expectation for β_t , we calculate a prediction for the outcome in time t : $\hat{y}_t$. The period's prediction error, $y_t - \hat{y}_t$, determines how far the estimates of β_t shift from β_{t-1} that period. Large prediction errors generate larger shifts in coefficient estimates. This recursion is the “filtering” step in the Kalman filter. The final step in estimating our model is to run the filter in reverse from the last time period back to the first. Filtering in reverse, known as Kalman smoothing, reduces our uncertainty in those estimates and ensures that early estimates of the dynamic coefficients are not overly influenced by random noise.⁵

The estimation procedure implies that all past information about β_t is fully summarized by their point estimates and covariance matrix at time t . One potential drawback to the DLM arises from this. The model weights observations equally – so, even observations in the distant past influence parameter estimates at time t equal to that of the observation at time $t - 1$ (Lebo & Box-Steffensmeier 2008) and the model's certainty about those estimates increases with new data. At each point, all information from past observations is summarized in the distribution of parameters at $t - 1$, and updated each period with new information.

⁵The process we describe informally here is detailed in appendix B. Our estimating equations follow from the derivations in Shumway & Stoffer (2010).

Since our lengthiest time series contains only about four decades of data, we avoid situations in which estimates are based on so much data that they resist registering dynamic changes. Shifts must be of a certain magnitude to move parameter estimates, which is useful in filtering out minor random fluctuations from period to period that might generate false positives about the presence of dynamics. Nevertheless, with very long time series there is the opposite danger that estimates might become too conservative.

Another possible concern about using DLMs comes from its assumptions regarding the distributions of time-varying coefficients. For example, Wood (2000) argues that the DLM’s distributional assumptions are unwarranted and that the more agnostic assumptions underlying flexible least squares are preferable. Given the basic similarity between flexible least squares and the DLM, we are unconcerned that the DLM returns worse estimates. The distributional assumptions of the DLM, as with many models, allow us to estimate uncertainty, and in this regard we find them quite useful. To illustrate, appendix C presents the results of applying our model to simulated data. The uncertainty estimates are small enough to be useful given the true values and grow when there is greater variance in the coefficient’s value over time. Given this, we have no reason to suspect the assumptions of the DLM are inappropriate for our application.

Data and Research Design

Reflecting the theme of this special issue, our empirical analysis focuses on Europe. More particularly, the data span approximately 1972 to 2011 from 14 Western European democracies: Austria, Belgium, Denmark, Finland, France, Germany, Ireland, Italy, the Netherlands, Norway, Spain, Sweden, Switzerland, and the United Kingdom.⁶

Two methodological issues feature prominently in previous work on partisanship’s effect:

⁶Our selection of Western European countries and time frames in individual regressions is limited only by data availability. Ideally we would impute missing values to broaden our data. However, we are doubtful that the assumption of data missing at random applies in these cases. Therefore, we opt to use only existing data.

the choices of measurements and controls, and the choice of model. We have argued, thus far, that the latter is the choice that provides the most scope for building on previous work. It is on this point that this paper’s contribution diverges the most from previous work. We apply DLMS to estimate time-varying effects of government partisanship.

With regard to the other key methodological issue we raise above – selection of measurement and choice of controls – we adhere closely to the standards and practices in previous work. Our approach to these choices has been to take direction from previous research and to be as inclusive and flexible as possible in considering controls. In this regard, our test aims to build directly on past work and to minimize model dependence in our conclusions.

With respect to the response variable, we focus on the generosity measures from the Comparative Welfare Entitlements Dataset (CWED) collected by Scruggs, Jahn & Kuitto (2014). Entitlements may be less sensitive to ‘demand factors’ than expenditures and more clearly connected to political decisions (Wenzelburger, Zohlnhöfer & Wolf 2013, 1230), which should make it more likely to trace partisan effects should they exist (see also Green-Pedersen 2007). More particularly, since labor market policy taps into a key dimension of the classical argument behind the partisan hypothesis (Jensen & Mortensen 2014), we choose the “unemployment generosity index” (*uegen*) as our main dependent variable. To assess the robustness of our results, we rerun our main analyses using three other generosity measures as well as an OECD measure of social expenditures collected by Brady, Huber & Stephens (2014). The alternative generosity measures are the combined generosity index (*totgen*) and the “sickness generosity index” (*skgen*) from the CWED and the “net replacement rate” for unemployment insurance (*unempsi*) constructed by Van Vliet & Caminada (2012). The social expenditure variable is the “*sstran*” measure from Brady, Huber & Stephens’s (2014) CWS dataset.

We measure our main explanatory variable, government partisanship, in three ways. In the main analysis, we use the proportion of cabinet parties coming from secular right parties.⁷ Focusing on secular right parties versus all others allows us to escape the thorny question

⁷Definitions of right and left parties come from the CWS data (Brady, Huber & Stephens 2014).

of the preferences of Christian democratic parties, which have been demonstrated to have left-leaning preferences in social policy (Esping-Andersen 1999, Huber & Stephens 2001, van Kersbergen 1995). In particular, critics of straightforward right-left measures of partisanship have argued that Christian democrats and more traditional left-wing parties would pursue similar policies on areas related to the labor market, though they might differ considerably on other social policies like elderly care and child care.

Two primary criticisms of this measurement are possible. First, it may be the case that changes in partisan influence on the generosity of transfer payments are concentrated on one side of the political spectrum. If leftist parties have been the main locus of partisan shifts on welfare policy, then our preferred measure risks overlooking the effect of interest. Secondly, it is possible that partisanship measures based on parties' generic political affiliation overlook changes in the effect of parties' *actual* political preferences. That is, parties nominally classified into left or right party families express some variety of political preferences across countries and over time, according to any measure (Franzmann & Kaiser 2006). The labels of "left" or "right" can, therefore, mean different things across countries and over time, opening up the possibility that parties' substantive policy preferences might evolve in their relationship to welfare policy while the relationship between nominal party categories and welfare policy exhibits a constant relationship.

We address these validity threats by repeating our analysis using two alternative government partisanship measures. The first critique we address by operationalizing government partisanship as the proportion of cabinet parties coming from left parties. The second we address by replacing measures based on parties' nominal classification with a more direct measure of party preferences on welfare policy. We use Lowe, Benoit, Mikhaylov & Laver's (2011) scaled measure of party preferences on social welfare policy, derived from the Comparative Manifesto Project's coding of the policy content of party manifestos (Lehmann, Matthieß, Merz, Regel & Werner 2015). The measure's specificity to welfare benefits and its face validity alongside other measures of actual party preferences make it a good choice

for exploring whether parties' stated preferences, rather than their nominal political affiliation, are dynamically related to social policies. To derive a cabinet-level measure from party-level preference measures, we take the average of all cabinet parties' scores weighted by their proportion of cabinet seats. Results from these two additional tests are found in appendix F.

We take the year as the unit of analysis for all estimations, modeling percentage change in social policy as a function of the partisanship of the government in office the year of the change, lagged actual social policy, and controls. This merits discussion since recent work has argued that it is preferable to model changes in welfare policy using government terms as the level of analysis – averaging over the years a cabinet is in office (Schmitt 2015, Garritzmann & Seng 2016). We advance two arguments in favor of our approach. First, there is variation in how an annual level of analysis impacts inferences about the relationship between government partisanship and social policy. Schmitt (2015) argues that annualized data favor economic over partisan variables, while Garritzmann & Seng (2016) argue that annual data privilege partisan variables – the difference driven by theoretical focus. We are uncertain that either argument holds strictly in our case. In simple bivariate associations, annualized and government term data should give similar answers regarding government partisanship social policy changes since social policy does not vary dramatically up and down over cabinets' terms, though we agree with Garritzmann & Seng that standard errors would be artificially smaller in annualized data. However, we do not rely on bivariate associations. To Schmitt's (2015) point, using annual data should not necessarily privilege economic explanations because economic data is measured annually – as is the social policy data. If a multivariate, annual-level model indicates that benefits are more responsive to economic factors, then we consider this result reliable. Though cabinets are a reference point for political actors and voters, there is no guarantee that associations between economic variables and social policy are preserved if we average over government terms. This is especially the case because government changes do not occur in a vacuum in our sample countries – governments sometimes

fail and call new elections specifically because of economic factors. This possibility makes us hesitant to rely on government-level data. Additionally, since we analyze models using single-country data, there are too few government level observations in any single country to apply statistical methods.

With regard to specifying the right controls for confounding, we do not argue for specific controls. If the effects of partisanship are truly dynamic, we will ideally find that estimates of dynamic effects of government partisanship on social policy are robust to variation in model specification. Furthermore, given the wide variety of model specifications found in previous work, we are hesitant to favor one or a few specific sets of covariates over all others. Instead, we aim to test for (dynamic) partisan effects in a way that minimizes the degree to which our findings depend on model specification.

Therefore, we draw several statistical controls from selected recent research and run models using every combination of them. In each specification, we model percentage change in unemployment benefit generosity as a function of government partisanship and one-year lagged real unemployment generosity as a control. Table 1 lists the remaining control variables and their differing lags. Note this list only includes variables exhibiting variation over time within countries. Our research design relies on within-country inferences, so we exclude, for example, counts of veto players, which varies rarely over time. Summary statistics for all variables are found in appendix A.

Table 1: Key time-varying confounders from previous research

Concept	Measure and data source	Citation
Globalization	Composite index of economic integration (Dreher 2006) (interacted with partisanship or uninteracted)	(Kwon & Pontusson 2010, Jensen 2011 <i>b</i> , Schmitt 2015)
Union density	Union members as % of labor force (Brady, Huber & Stephens 2014)	(Kwon & Pontusson 2010, Schmitt 2015)

GDP/capita growth	% annual change in GDP per capita (contemporaneous or lagged one year) (Brady, Huber & Stephens 2014)	(Allan & Scruggs 2004, Kwon & Pontusson 2010, Jensen 2011 <i>b</i> , Jensen 2011 <i>c</i> , Schmitt 2015)
Public debt / Deficits	Net general government lending or borrowing, in millions of national currency units at current prices (Brady, Huber & Stephens 2014)	(Allan & Scruggs 2004, Kwon & Pontusson 2010, Jensen 2011 <i>b</i> , Jensen 2011 <i>c</i> , Garritzmann & Seng 2016, Schmitt 2015)
Dependency ratio / Aged and youth population	Proportion of population under 15 and over 65 years of age (Brady, Huber & Stephens 2014)	(Kwon & Pontusson 2010, Jensen 2011 <i>c</i> , Jensen 2011 <i>b</i> , Huber & Stephens 2014)
Unemployment	Annualized unemployment rate (contemporaneous or lagged one year) (Brady, Huber & Stephens 2014)	(Allan & Scruggs 2004, Kwon & Pontusson 2010, Jensen 2011 <i>c</i> , Jensen 2011 <i>b</i> , Huber & Stephens 2014, Schmitt 2015)
De-industrialization	100 minus total agricultural and industrial employment as a percentage of total employment (Brady, Huber & Stephens 2014)	(Kwon & Pontusson 2010, Jensen 2011 <i>b</i>)
Trade Openness	Sum of exports and imports as % of GDP at current prices (lagged one year) (Brady, Huber & Stephens 2014)	(Allan & Scruggs 2004, Garritzmann & Seng 2016)
Female employment	Total female civilian employment as a proportion of total civilian employment (Brady, Huber & Stephens 2014)	(Jensen 2011 <i>c</i> , Huber & Stephens 2014, Garritzmann & Seng 2016)

We estimate models including and excluding each combination of every version of these measures to arrive at a complete model space of just over 1,700 specifications per country. Depending on the country, not every specification features sufficient variation on all variables or sufficient statistical power to estimate. Therefore, the actual number of analyses is closer to 1,500 per country, per measurement of partisanship. The grand total of results presented in the next section is, therefore, specifications by country by partisanship measure: around $1,500 \times 14 \times 3 \approx 63,000$ estimations. This is an overestimate since Germany is excluded from analyses measuring government partisanship as percentage of secular right parties in the cabinet due to a lack of variation. Including robustness checks using alternative measures of social policy, the total number of analyses is around: $63,000 + 1,500 \times 14 \times 4 \approx 147,000$.

Given limits on the information we can summarize in tabular form, outcomes are presented as graphical summaries. We plot together confidence intervals around coefficient estimates for government partisanship over time, aggregated by country. Intervals from the approximately 1,500 regressions for every country are overlaid in single plots so that darker plot regions show where many regressions produce similar estimates. The purpose is to assess the robustness of our estimates across a variety of specifications.

Findings

This section presents graphical results of the models described above. Namely, we plot 95% confidence intervals of dynamic coefficient estimates for the proportion of secular right parties in the cabinet. Figure 1 plots intervals for all model specifications by country. Shaded regions of plots are confidence intervals plotted over one another at very low opacity. The shaded regions darken when many intervals cover the same area, thus the darkest regions indicate findings consistent across specifications. Dashed horizontal lines mark zero. Confidence intervals overlapping zero are not statistically significant. Estimates for Germany are missing from figure 1 because secular right parties did not appear in the federal cabinet during our period of analysis until the formation of Angela Merkel’s second government in 2009. The Free Democratic Party, recognized as having become a center-right party instead of a center party at that point, participated in that cabinet. This lack of temporal variation makes it impossible to run the analysis for Germany.

The first thing to note in figure 1 is the limited variation in results within countries. In virtually all countries, estimates from the many analyses are remarkably similar despite widely varying model specifications, evidenced by the few regions of the plots showing lighter shades of gray. Where many estimates overlap – i.e. agree – opacity converges from gray to black. This agreement across model specifications is encouraging evidence that the results exhibit relatively little model dependence due to choice of controls. Secondly, this test

Figure 1: Estimated confidence intervals on right cabinet parties

Note: Panels include confidence intervals of time-varying coefficients on gov. partisanship, measured by proportion of right parties in cabinet, controlling for lagged level of unemployment benefit replacement rates and every combination of controls. Around 1,600 intervals are overlaid in each plot. Dashed lines denote zero.

provides very little evidence in favor of the hypothesis that partisan effects are dynamic. In fact, the analyses show little evidence that government partisanship relates to changes in welfare generosity at all – most confidence intervals overlap the zero line most of the time. These plots show only confidence intervals, but stable associations are also visible looking at the central predictions of dynamic coefficients. These can be viewed in online appendix E.

Another, broader, point to take from figure 1 is that there is, indeed, heterogeneity across

countries in estimates of government partisanship effects and the timing of the few dynamic effects that are detected. France and Sweden are the only countries exhibiting statistically detectable variation in the effect of government partisanship on unemployment replacement rates in this analysis. In the case of France, secular right parties were associated with less generosity in the early 1990s and more in the early years of the 2000s. Swedish secular right parties were associated with no change in generosity until around 2005, at which point they became associated with declining generosity. In both cases, these findings are consistent across all, or the vast majority of, model specifications.

To assess the general robustness of the findings, we further present results similar to those in figure 1, but using the four alternative measures of social policy mentioned above. In order, these are presented in figures A.3, A.4, A.5, and A.6 in appendix D. Results reflect quite closely the main findings from our preferred unemployment generosity measure. Estimates are largely statistically indistinguishable from zero and exhibit little evidence of change over time. In some plots vertical striations in shading appear – these occur when confidence intervals are clipped because they exceed the size of the plot.

Lastly, we also repeat the models in figure 1 using alternative measures of government partisanship. These are found in figures A.8 and A.10 in online appendix F. The results across all 14 countries in our sample reveal no evidence of a relationship between government partisanship and changes in unemployment replacement rates and no evidence of any change in that null relationship over time using alternative measures of partisanship.

Conclusion

The importance of the question of how government partisanship influences public policy is reflected in the longstanding and still vivid scholarly debate about how to best answer the question. The fact that the debate continues reflects that it is a complex question and not easily settled. This article does not settle it either, but it has addressed a set of central

challenges that deserve more attention than has been offered in the literature thus far.

First, though the question of time-varying partisan effects has received much attention at a theoretical level, the challenges involved in testing for time-varying effects have not been given the same attention. In this paper, we have outlined and applied a dynamic linear modeling (DLM) approach and argued why it is better suited to tracing partisan dynamics than previously applied econometric tools.

Second, using the DLM makes it possible to avoid pooling observations across countries, which has dominated previous quantitative studies within this literature. In many cases, pooling data across countries may be desirable from a theoretical perspective, and to bring more data to bear on a problem by borrowing strength across observations from similar units. Nevertheless, because countries may differ in important aspects relevant to our research question, and given the lack of previous single-country studies of this question, there was a strong case for investigating partisan effects country-by-country.

Third, this paper offers an alternative approach to the challenge of selecting the ‘correct’ set of controls. We have advocated and implemented an approach that reduces model dependence by estimating the relationship between government partisanship and social policy using every combination of controls identified in relevant previous research.

Empirically, the dynamic analyses we report reveal little dynamics in partisan effects. In fact, we do not find much evidence of systematic partisan effects in the countries and the period studied here at all. In some estimations, the partisan coefficient comes out statistically significant and signed as expected by the general partisan hypothesis, but it is not a robust and strong effect. Furthermore, it should be noted that this result is not restricted to a specific measure of generosity, but holds across several measures of generosity as well as one prominent measure of social spending. It also holds across three different measures of government partisanship.

As pointed out in the literature review, many other studies have reported no or weak effects of partisan effects using data from after 1980. Given that these studies estimate parti-

san effects averaged over time and across countries they could, however, hide dynamic effects as well as heterogeneous effects across countries. The results reported in this paper indicate that this has not been the case. Furthermore, a main contribution of this study is to show that there is no clear sign of decreasing partisan effects over time. The empirical expectation derived from the decline-of-partisanship hypothesis would be a convergence towards zero.

Of course, a limitation of our analyses is that the pre-austerity period in our data time series is quite short, given that most of our time series begin around 1972. However, since other scholars have identified the beginning of the 1980s (Allan & Scruggs 2004) and even 1985 (Huber & Stephens 2014) as cutoff points, our time series should be sufficiently long to detect the presence of partisan effects in the 1970s. Furthermore, as illustrated by the simulation, our estimation approach is capable of detecting effects even when they rather quickly converge towards zero (see appendix C).

In combination with the relatively weak or null partisan effects reported in many other welfare studies, the results of this paper may be taken as further support of the scholarly interest in determinants of social policies besides government partisanship. We have focused on thoroughly investigating government partisanship effects, but in future studies the modeling approach we advocate and apply can also be relevant to examining other drivers of welfare policy, such as globalization and the economy.

Another option is to adapt part of the approach advocated here by, for instance, using the DLM to estimate partisan effects under particular, theory-driven combinations of control variables and in a way that comes closer to the mechanisms whereby partisanship may matter to social policy. Although partisanship does not appear to be a strong and persistent force behind social policy, it is still a variable of crucial interest in modern democracies, and a variable whose conditional effects are worthy of further investigation. Related to this, the DLM approach may also be used to investigate in more detail, for instance, if partisan effects tend to be lagged more in countries with more veto points compared to countries with fewer veto points.

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APPENDIX
NOT FOR PRINT PUBLICATION

A Appendix: Summary statistics

Summary statistics for the variables in all statistical models are included in the tables below.

Plots of all dependent variables by country are included in the figure.

	Min	Max	Mean	Median	Std Dev	Obs
Pct. Change Unemployment Generosity	-0.05	0.22	0.01	0.00	0.05	39.00
Pct. Secular Right Parties in Cabinet	0.00	0.50	0.07	0.00	0.14	39.00
Pct. Left Parties in Cabinet	0.00	1.00	0.60	0.57	0.34	39.00
Avg. Logged Gov. Welfare Stance	-3.20	-0.93	-2.01	-1.94	0.70	39.00
Lag Level of Unemployment Generosity	7.40	10.80	10.10	10.40	0.79	39.00
Lag Trade Openness	56.18	112.78	77.21	70.76	16.34	39.00
Globalization	-21.14	19.17	1.65	1.43	14.02	39.00
Unemployment Rate	1.08	5.17	3.47	3.66	1.05	39.00
Lag Change in GDP per capita	-0.03	0.14	0.06	0.05	0.03	39.00
Gov. Deficit as Pct. of GDP	-0.06	0.01	-0.03	-0.03	0.01	39.00
Female Employment rate	0.38	0.47	0.42	0.42	0.03	39.00
Dependency Ratio	0.32	0.38	0.34	0.33	0.02	39.00
Union Density	27.77	60.83	44.42	44.27	10.52	39.00
Deindustrialization	99.43	99.71	99.59	99.59	0.08	39.00

Table A.1: Austria Summary Statistics

	Min	Max	Mean	Median	Std Dev	Obs
Pct. Change Unemployment Generosity	-0.01	0.02	0.00	0.00	0.01	33.00
Pct. Secular Right Parties in Cabinet	0.00	0.50	0.26	0.23	0.20	33.00
Pct. Left Parties in Cabinet	0.00	0.56	0.37	0.48	0.20	33.00
Avg. Logged Gov. Welfare Stance	-2.92	0.72	-2.03	-2.62	1.05	33.00
Lag Level of Unemployment Generosity	13.00	14.00	13.62	13.60	0.23	33.00
Lag Trade Openness	100.57	169.01	132.35	131.17	16.75	33.00
Globalization	-9.02	9.00	1.76	2.54	5.42	33.00
Unemployment Rate	6.61	13.51	9.85	9.47	2.30	33.00
Lag Change in GDP per capita	0.02	0.13	0.05	0.05	0.02	33.00
Gov. Deficit as Pct. of GDP	-0.16	0.00	-0.06	-0.06	0.05	33.00
Female Employment rate	0.35	0.45	0.41	0.42	0.03	33.00
Dependency Ratio	0.33	0.35	0.34	0.34	0.01	33.00
Union Density	49.48	55.71	52.94	52.91	1.64	33.00
Deindustrialization	99.62	99.77	99.72	99.74	0.04	33.00

Table A.2: Belgium Summary Statistics

	Min	Max	Mean	Median	Std Dev	Obs
Pct. Change Unemployment Generosity	-0.13	0.37	0.02	0.00	0.07	38.00
Pct. Secular Right Parties in Cabinet	0.00	1.00	0.47	0.47	0.46	38.00
Pct. Left Parties in Cabinet	0.00	1.00	0.42	0.00	0.45	38.00
Avg. Logged Gov. Welfare Stance	-1.79	0.14	-0.87	-0.92	0.51	38.00
Lag Level of Unemployment Generosity	5.50	13.20	10.69	11.30	2.01	38.00
Lag Trade Openness	55.80	106.30	74.73	71.31	12.42	38.00
Globalization	-15.97	15.68	1.04	2.80	10.72	38.00
Unemployment Rate	0.87	11.55	6.64	6.29	2.30	38.00
Lag Change in GDP per capita	-0.05	0.16	0.07	0.05	0.05	38.00
Gov. Deficit as Pct. of GDP	-0.08	0.05	-0.00	-0.00	0.03	38.00
Female Employment rate	0.41	0.48	0.45	0.46	0.02	38.00
Dependency Ratio	0.33	0.36	0.34	0.34	0.01	38.00
Union Density	62.21	80.77	74.20	75.15	4.29	38.00
Deindustrialization	99.57	99.81	99.70	99.70	0.05	38.00

Table A.3: Denmark Summary Statistics

	Min	Max	Mean	Median	Std Dev	Obs
Pct. Change Unemployment Generosity	-0.05	0.18	0.01	0.00	0.04	39.00
Pct. Secular Right Parties in Cabinet	0.00	0.42	0.19	0.27	0.17	39.00
Pct. Left Parties in Cabinet	0.00	0.66	0.44	0.46	0.20	39.00
Avg. Logged Gov. Welfare Stance	-2.83	-0.09	-1.34	-1.17	0.88	39.00
Lag Level of Unemployment Generosity	5.90	10.60	8.33	9.00	1.53	39.00
Lag Trade Openness	44.28	89.92	62.65	59.08	12.28	39.00
Globalization	-21.57	21.03	1.66	3.41	15.24	39.00
Unemployment Rate	1.73	16.63	7.62	6.86	3.84	39.00
Lag Change in GDP per capita	-0.08	0.25	0.08	0.08	0.07	39.00
Gov. Deficit as Pct. of GDP	-0.08	0.08	0.02	0.03	0.04	39.00
Female Employment rate	0.46	0.49	0.48	0.48	0.01	39.00
Dependency Ratio	0.32	0.34	0.33	0.33	0.01	39.00
Union Density	61.37	80.65	71.71	70.65	4.92	39.00
Deindustrialization	99.48	99.75	99.64	99.68	0.08	39.00

Table A.4: Finland Summary Statistics

	Min	Max	Mean	Median	Std Dev	Obs
Pct. Change Unemployment Generosity	-0.16	0.10	0.00	0.01	0.04	33.00
Pct. Secular Right Parties in Cabinet	0.00	1.00	0.54	0.75	0.45	33.00
Pct. Left Parties in Cabinet	0.00	1.00	0.44	0.25	0.46	33.00
Avg. Logged Gov. Welfare Stance	-1.72	0.00	-0.86	-1.21	0.60	33.00
Lag Level of Unemployment Generosity	10.20	12.70	11.32	11.40	0.74	33.00
Lag Trade Openness	39.71	56.63	47.11	46.11	5.30	33.00
Globalization	-14.29	14.34	2.69	4.03	9.27	33.00
Unemployment Rate	4.61	10.62	8.39	8.65	1.59	33.00
Lag Change in GDP per capita	0.01	0.15	0.06	0.04	0.04	33.00
Gov. Deficit as Pct. of GDP	-0.08	-0.00	-0.03	-0.03	0.02	33.00
Female Employment rate	0.39	0.48	0.44	0.45	0.03	33.00
Dependency Ratio	0.34	0.37	0.35	0.35	0.01	33.00
Union Density	7.58	21.28	11.30	9.48	4.29	33.00
Deindustrialization	99.55	99.80	99.70	99.74	0.08	33.00

Table A.5: France Summary Statistics

	Min	Max	Mean	Median	Std Dev	Obs
Pct. Change Unemployment Generosity	-0.05	0.03	-0.00	0.00	0.02	35.00
Pct. Secular Right Parties in Cabinet	0.00	0.00	0.00	0.00	0.00	35.00
Pct. Left Parties in Cabinet	0.00	1.00	0.47	0.50	0.44	35.00
Avg. Logged Gov. Welfare Stance	-1.75	-0.14	-1.16	-1.20	0.52	35.00
Lag Level of Unemployment Generosity	10.10	12.10	11.45	11.50	0.46	35.00
Lag Trade Openness	33.27	85.41	51.31	48.06	12.03	35.00
Globalization	-12.09	15.16	-0.00	-1.06	9.28	35.00
Unemployment Rate	1.01	11.24	6.75	7.21	2.53	35.00
Lag Change in GDP per capita	-0.15	0.11	0.04	0.05	0.04	35.00
Gov. Deficit as Pct. of GDP	-0.09	0.03	-0.02	-0.02	0.02	35.00
Female Employment rate	0.38	0.46	0.41	0.41	0.03	35.00
Dependency Ratio	0.30	0.36	0.33	0.32	0.02	35.00
Union Density	19.89	35.99	30.35	32.43	5.22	35.00
Deindustrialization	99.46	99.71	99.60	99.59	0.07	35.00

Table A.6: Germany Summary Statistics

	Min	Max	Mean	Median	Std Dev	Obs
Pct. Change Unemployment Generosity	-0.12	0.49	0.01	0.00	0.09	39.00
Pct. Secular Right Parties in Cabinet	0.00	0.81	0.26	0.09	0.30	39.00
Pct. Left Parties in Cabinet	0.00	0.45	0.11	0.05	0.14	39.00
Avg. Logged Gov. Welfare Stance	-2.90	-0.93	-2.04	-2.01	0.65	39.00
Lag Level of Unemployment Generosity	6.50	11.10	9.12	9.00	1.55	39.00
Lag Trade Openness	67.42	183.62	124.73	111.72	32.37	39.00
Globalization	-10.49	8.52	0.84	0.52	5.57	39.00
Unemployment Rate	3.86	17.15	10.22	10.39	4.49	39.00
Lag Change in GDP per capita	-0.11	0.25	0.10	0.10	0.07	39.00
Gov. Deficit as Pct. of GDP	-0.31	0.05	-0.06	-0.06	0.07	39.00
Female Employment rate	0.27	0.47	0.36	0.36	0.06	39.00
Dependency Ratio	0.31	0.42	0.37	0.38	0.04	39.00
Union Density	32.08	58.91	47.39	51.32	9.67	39.00
Deindustrialization	99.47	99.79	99.63	99.64	0.08	39.00

Table A.7: Ireland Summary Statistics

	Min	Max	Mean	Median	Std Dev	Obs
Pct. Change Unemployment Generosity	-0.19	0.31	0.01	0.00	0.09	34.00
Pct. Secular Right Parties in Cabinet	0.00	1.00	0.33	0.16	0.37	34.00
Pct. Left Parties in Cabinet	0.00	0.97	0.24	0.20	0.24	34.00
Avg. Logged Gov. Welfare Stance	-3.27	1.33	-0.78	-0.76	0.92	34.00
Lag Level of Unemployment Generosity	2.60	5.90	4.04	3.60	1.12	34.00
Lag Trade Openness	35.53	57.95	45.60	45.20	6.27	34.00
Globalization	-15.69	19.05	3.79	8.29	12.28	34.00
Unemployment Rate	6.15	12.11	9.67	9.88	1.84	34.00
Lag Change in GDP per capita	-0.04	0.25	0.09	0.06	0.07	34.00
Gov. Deficit as Pct. of GDP	-0.13	-0.01	-0.07	-0.07	0.04	34.00
Female Employment rate	0.31	0.41	0.36	0.35	0.03	34.00
Dependency Ratio	0.30	0.34	0.32	0.32	0.01	34.00
Union Density	33.24	50.41	38.94	38.36	5.28	34.00
Deindustrialization	99.50	99.70	99.63	99.64	0.05	34.00

Table A.8: Italy Summary Statistics

	Min	Max	Mean	Median	Std Dev	Obs
Pct. Change Unemployment Generosity	-0.10	0.07	-0.00	0.00	0.03	34.00
Pct. Secular Right Parties in Cabinet	0.00	0.45	0.26	0.34	0.17	34.00
Pct. Left Parties in Cabinet	0.00	0.52	0.24	0.29	0.21	34.00
Avg. Logged Gov. Welfare Stance	-2.94	-0.02	-0.93	-0.76	0.89	34.00
Lag Level of Unemployment Generosity	10.70	13.10	12.03	12.00	0.67	34.00
Lag Trade Openness	90.08	144.24	114.98	112.13	13.55	34.00
Globalization	-12.36	12.25	3.00	2.69	6.32	34.00
Unemployment Rate	2.12	12.15	6.33	5.53	2.90	34.00
Lag Change in GDP per capita	-0.04	0.13	0.04	0.05	0.03	34.00
Gov. Deficit as Pct. of GDP	-0.09	0.02	-0.03	-0.03	0.02	34.00
Female Employment rate	0.28	0.46	0.39	0.40	0.05	34.00
Dependency Ratio	0.31	0.35	0.32	0.32	0.01	34.00
Union Density	19.33	37.18	25.73	24.38	5.19	34.00
Deindustrialization	99.64	99.82	99.74	99.75	0.05	34.00

Table A.9: Netherlands Summary Statistics

	Min	Max	Mean	Median	Std Dev	Obs
Pct. Change Unemployment Generosity	-0.03	0.15	0.01	0.01	0.03	39.00
Pct. Secular Right Parties in Cabinet	0.00	1.00	0.17	0.00	0.29	39.00
Pct. Left Parties in Cabinet	0.00	1.00	0.65	0.87	0.42	39.00
Avg. Logged Gov. Welfare Stance	-2.73	-0.91	-1.61	-1.30	0.68	39.00
Lag Level of Unemployment Generosity	9.20	14.50	12.92	13.40	1.47	39.00
Lag Trade Openness	66.71	83.22	73.60	73.46	4.21	39.00
Globalization	-12.27	10.31	1.05	2.60	6.37	39.00
Unemployment Rate	1.47	6.06	3.40	3.25	1.35	39.00
Lag Change in GDP per capita	-0.09	0.19	0.08	0.09	0.05	39.00
Gov. Deficit as Pct. of GDP	-0.07	0.19	0.04	0.01	0.07	39.00
Female Employment rate	0.37	0.48	0.45	0.46	0.03	39.00
Dependency Ratio	0.34	0.37	0.36	0.35	0.01	39.00
Union Density	52.79	58.53	55.78	55.47	1.80	39.00
Deindustrialization	99.55	99.79	99.70	99.73	0.07	39.00

Table A.10: Norway Summary Statistics

	Min	Max	Mean	Median	Std Dev	Obs
Pct. Change Unemployment Generosity	-0.03	0.06	0.01	0.01	0.02	15.00
Pct. Secular Right Parties in Cabinet	0.00	1.00	0.53	0.67	0.48	15.00
Pct. Left Parties in Cabinet	0.00	1.00	0.47	0.33	0.48	15.00
Avg. Logged Gov. Welfare Stance	-3.19	-0.95	-2.09	-1.95	0.83	15.00
Lag Level of Unemployment Generosity	9.70	11.70	10.85	10.80	0.67	15.00
Lag Trade Openness	44.77	61.23	55.00	55.87	4.94	15.00
Globalization	10.06	18.50	16.05	16.22	2.28	15.00
Unemployment Rate	8.29	22.15	14.09	11.52	4.75	15.00
Lag Change in GDP per capita	-0.04	0.08	0.05	0.06	0.03	15.00
Gov. Deficit as Pct. of GDP	-0.11	0.02	-0.02	-0.01	0.04	15.00
Female Employment rate	0.35	0.44	0.39	0.39	0.03	15.00
Dependency Ratio	0.31	0.32	0.32	0.32	0.00	15.00
Union Density	14.34	16.74	15.56	15.79	0.70	15.00
Deindustrialization	99.66	99.78	99.70	99.68	0.03	15.00

Table A.11: Spain Summary Statistics

	Min	Max	Mean	Median	Std Dev	Obs
Pct. Change Unemployment Generosity	-0.17	0.11	0.00	-0.01	0.05	38.00
Pct. Secular Right Parties in Cabinet	0.00	0.55	0.13	0.00	0.20	38.00
Pct. Left Parties in Cabinet	0.00	1.00	0.65	1.00	0.46	38.00
Avg. Logged Gov. Welfare Stance	-2.01	-0.51	-1.42	-1.55	0.45	38.00
Lag Level of Unemployment Generosity	7.90	13.10	10.85	11.15	1.38	38.00
Lag Trade Openness	46.20	100.31	69.94	66.72	13.80	38.00
Globalization	-24.47	17.71	1.72	6.39	15.26	38.00
Unemployment Rate	1.62	10.19	5.00	4.48	2.85	38.00
Lag Change in GDP per capita	-0.04	0.17	0.07	0.08	0.04	38.00
Gov. Deficit as Pct. of GDP	-0.11	0.04	-0.02	-0.00	0.05	38.00
Female Employment rate	0.41	0.49	0.47	0.48	0.02	38.00
Dependency Ratio	0.34	0.36	0.36	0.36	0.01	38.00
Union Density	68.77	87.44	78.65	78.66	5.03	38.00
Deindustrialization	99.57	99.80	99.70	99.71	0.07	38.00

Table A.12: Sweden Summary Statistics

	Min	Max	Mean	Median	Std Dev	Obs
Pct. Change Unemployment Generosity	-0.07	1.10	0.05	0.00	0.21	34.00
Pct. Secular Right Parties in Cabinet	0.29	0.53	0.39	0.33	0.10	34.00
Pct. Left Parties in Cabinet	0.23	0.29	0.26	0.26	0.02	34.00
Avg. Logged Gov. Welfare Stance	-2.15	-0.70	-1.69	-1.84	0.44	34.00
Lag Level of Unemployment Generosity	4.10	13.80	12.78	13.30	1.80	34.00
Lag Trade Openness	57.12	98.81	74.27	71.72	9.88	34.00
Globalization	-8.49	11.83	2.18	2.00	4.32	34.00
Unemployment Rate	0.18	4.16	2.08	2.39	1.48	34.00
Lag Change in GDP per capita	-0.01	0.07	0.03	0.03	0.02	34.00
Gov. Deficit as Pct. of GDP	-0.03	0.02	-0.01	-0.01	0.01	34.00
Female Employment rate	0.35	0.45	0.41	0.42	0.04	34.00
Dependency Ratio	0.32	0.35	0.32	0.32	0.01	34.00
Union Density	17.34	33.81	23.48	22.75	4.38	34.00
Deindustrialization	99.52	99.75	99.65	99.68	0.08	34.00

Table A.13: Switzerland Summary Statistics

	Min	Max	Mean	Median	Std Dev	Obs
Pct. Change Unemployment Generosity	-0.15	0.23	-0.00	0.00	0.05	38.00
Pct. Secular Right Parties in Cabinet	0.00	1.00	0.52	0.58	0.48	38.00
Pct. Left Parties in Cabinet	0.00	1.00	0.48	0.38	0.48	38.00
Avg. Logged Gov. Welfare Stance	-3.64	-0.92	-2.38	-2.32	0.77	38.00
Lag Level of Unemployment Generosity	7.90	11.80	9.13	8.70	1.08	38.00
Lag Trade Openness	42.17	61.92	53.91	53.89	4.17	38.00
Globalization	-16.15	10.77	1.49	2.34	6.84	38.00
Unemployment Rate	2.08	11.88	7.03	6.49	2.64	38.00
Lag Change in GDP per capita	-0.03	0.26	0.09	0.07	0.06	38.00
Gov. Deficit as Pct. of GDP	-0.11	0.04	-0.05	-0.06	0.04	38.00
Female Employment rate	0.38	0.47	0.43	0.45	0.03	38.00
Dependency Ratio	0.33	0.38	0.35	0.35	0.01	38.00
Union Density	27.10	51.95	38.71	39.69	8.94	38.00
Deindustrialization	99.56	99.81	99.70	99.71	0.07	38.00

Table A.14: United Kingdom Summary Statistics

Figure A.1: Dependent variables by country

Note: Scales on plots differ. Lines are the actual dependent variables, percentage annual changes in the respective variables.

B Appendix: Model details

This appendix briefly outlines the estimating equations and methods for estimating our dynamic linear models. See Shumway & Stoffer (2010) for a complete explanation of the methodology and for the derivations of our specific estimating equations. There is the general version of our specific model. To recap from the main text, we estimate the following model:

$$\begin{aligned} y_t &= \mathbf{X}_t \boldsymbol{\beta}_t + v_t \\ v_t &\sim i.i.d.(0, \sigma_v^2) \end{aligned}$$

$$\begin{aligned} \boldsymbol{\beta}_t &= \boldsymbol{\beta}_{t-1} + \mathbf{w}_t \\ \mathbf{w}_t &\sim i.i.d.(0, Q) \end{aligned}$$

Scalar y_t is the outcome variable in time t . A vector, $\mathbf{X}_t$, ($1 \times k$) contains measures of the explanatory variables in time t . Parameters $\boldsymbol{\beta}_t$ ($k \times 1$ vector) are estimates for each time t . The scalar v_t and $\mathbf{w}_t$ ($k \times 1$ vector) are mean-zero Gaussian white noise. Parameters σ_v^2 and Q ($k \times k$ covariance matrix) are their fixed variances. The model is estimated in according to the steps:

1. Set initial values for parameters: $\boldsymbol{\beta}_0$, Q , σ_v^2 , and covariance matrix of prediction errors Σ_0 .
2. Run Kalman filter to get prediction errors from the model, $\mathbf{v}$, and their covariances.
3. Use prediction errors from step 2 to estimate $\boldsymbol{\beta}_0$, Q , σ_v^2 , and Σ_0 using maximum likelihood.

4. Repeat step 2 using estimates from step 3 in place of starting values from step 1.
5. Repeat step 4 until estimates of β_0 , Q , σ_v^2 , and Σ_0 or the likelihood stabilize.

The Kalman filter recursion has the following steps. At time 0, these two steps are unique and occur in place of steps 1 and 2 in the next list:

1. Calculate expectation of β_1 , conditional on β_0 . We assume β is a Markov chain with a distribution centered around its current value, therefore our expectation of its next period value is its current value: $\beta_{t|t-1} = \beta_{t-1|t-1}$. Likewise: $\beta_{1|0} = \beta_0$
2. Calculate expectation of the covariance of prediction errors to β_1 , conditional on Σ_0 and Q . We name this P : $P_{1|0} = \Sigma_0 + Q$.

From time 1 to time t , these steps occur:

1. $\beta_{t|t-1} = \beta_{t-1|t-1}$
2. $P_{t|t-1} = P_{t-1|t-1} + Q$
3. Calculate predicted value of y conditional on expectations from time $t - 1$:

$$\hat{y}_{t|t-1} = \mathbf{X}_t \beta_{t|t-1}$$
4. Calculate prediction error in time t : $\eta_{t|t-1} = y_t - \hat{y}_{t|t-1}$
5. Calculate Kalman gain for period t , i.e. proportion of uncertainty in each parameter in β_t attributable to uncertainty regarding that parameter relative to the full uncertainty in the model:

$$K_t = \frac{P_{t|t-1} X_t'}{X_t P_{t|t-1} X_t' + \sigma_v^2}$$

6. Update estimates of effect coefficients, $\beta_{t|t}$, based on prediction error and Kalman gain:

$$\beta_{t|t} = \beta_{t|t-1} + K_t \eta_{t|t-1}$$

The value of $\beta_{t|t}$ is our estimate of β_t .

7. Update expectation of the covariance of parameters, $P_{t|t}$, based on X_t and σ_v^2 :

$$P_{t|t} = P_{t|t-1} - \frac{P_{t|t-1}X_t'}{X_t P_{t|t-1} X_t' + \sigma_v^2} X_t P_{t|t-1}$$

This can also be expressed as:

$$P_{t|t} = [I - K_t X_t] P_{t|t-1}$$

C Appendix: Simulated data test

The model descriptions in the text and in appendix B demonstrate the DLM is capable of estimating coefficients that change with time. However, we would also like to know that the dynamic estimates the model recovers are close to the “true” coefficients. We examine this question by simulating a time series of 37 observations of data on which to test the model. The number of observations reflects the typical length of one of our single-country time series. The outcome variable is a function of the five other factors and a normally distributed error term.

The simulated true coefficients are meant to reflect the types of effects we might expect to see in the case of cabinet partisanship’s relationship to welfare generosity based on previous work. The goal of this is to validate that the DLM will return true dynamics of this type or true constant effects.

Three out of five simulated explanatory factors have gradually shifting relationships to the outcome. The first (Beta 1 in figureA.2) begins with a positive relationship to the outcome and gradually converges to no relationship. One goes from negative to positive (Beta 2 in figureA.2). Another goes from slightly positive, to null, and then to larger and positive (Beta 3 in figureA.2).

Next, we simulate two variables that relate to the outcome variable through constants. One of these is a true constant (Beta 5 in figureA.2), while the other is basically constant net of some minor random noise (Beta 4 in figureA.2).

The factors themselves, our simulated explanatory variables, are a matrix of five random vectors drawn from standard normal distributions. The outcome is calculated deterministically from the data and the true effects we wish to recover, then we add a realization of mean-zero normally distributed random noise to each observation. There is no intercept term included in the model. The outcome is then standardized by subtracting its mean and

dividing by its standard deviation before running the model. There is no need to standardize the explanatory variables because they are generated as random draws from independent standard normal distributions.

Figure A.2: Estimated and true time-varying coefficients

Note: The solid line is the known value of the coefficient. The dashed lines plot central predictions of dynamic effect coefficients, and the gray regions are 95% confidence intervals around those predictions.

Figure A.2 summarizes results from the DLM with this simulated data. Solid lines plot the true coefficients and the dashed lines indicate our estimates of each of them with their confidence intervals in gray. The model is quite successful at tracking each coefficients' evolution over time, filtering out the random noise to estimate Beta 4 as a constant and capturing the movements in the dynamic relationships. These results indicate that DLMS are an appropriate tool for capturing the changes in partisan effects we might encounter in our real-world data.

D Appendix: Robustness to alternative response variable

Figure A.3: Estimated confidence intervals for *unempsi*

Note: Panels include confidence intervals of time-varying coefficients on gov. partisanship, measured by proportion of right parties in cabinet, controlling for lagged level of *unempsi* and every combination of controls. Around 1,600 intervals are overlaid in each plot. Dashed lines denote zero. Belgium is omitted due to missing data.

Figure A.4: Estimated confidence intervals for *ssran*

Note: Panels include confidence intervals of time-varying coefficients on gov. partisanship, measured by proportion of right parties in cabinet, controlling for lagged level of *ssran* and every combination of controls. Around 1,600 intervals are overlaid in each plot. Dashed lines denote zero. Norway is omitted due to missing data.

Figure A.5: Estimated confidence intervals for *skgen*

Note: Panels include confidence intervals of time-varying coefficients on gov. partisanship, measured by proportion of right parties in cabinet, controlling for lagged level of *skgen* and every combination of controls. Around 1,600 intervals are overlaid in each plot. Dashed lines denote zero. Switzerland is omitted because sickness benefits generosity does not vary.

Figure A.6: Estimated confidence intervals for *totgen*

Note: Panels include confidence intervals of time-varying coefficients on gov. partisanship, measured by proportion of right parties in cabinet, controlling for lagged level of *totgen* and every combination of controls. Around 1,600 intervals are overlaid in each plot. Dashed lines denote zero.

E Appendix: Central predictions from main models

Figure A.7: Estimated time-varying coefficients for proportion of right cabinet parties

Note: Each panel plots time-varying coefficients on government partisanship, measured by proportion of right parties in cabinet, controlling for lagged level of unemployment replacement rates and every combination of controls. Around 1,600 lines are overlaid in each plot. Dashed lines denote zero.

F Appendix: Alternative measures of partisanship

This appendix contains results for the main analyses using two alternative measures of partisanship. As discussed in the main text, two criticisms are possible of our measurement of partisanship as the proportion of right cabinets in the cabinet. First, it may be the case that changes in partisan influence on the generosity of transfer payments are concentrated on one side of the political spectrum. If leftist parties have been the main locus of the partisan shifts on welfare policy, then our preferred measure risks overlooking the effect of primary interest. Secondly, it is possible that any measure of partisanship based on parties' generic political affiliation overlooks change in the effect of parties' *actual* political preferences. That is, parties nominally classified into left or right party families express some variety of political preferences across countries and over time, according to any measure (Franzmann & Kaiser 2006). The labels of "left" or "right" can, therefore, mean different things across countries and over time, opening up the possibility that parties' substantive policy preferences might evolve in their relationship to welfare policy while the relationship between nominal party categories and welfare policy exhibits a constant relationship.

We address these two validity threats here by repeating our main analysis using two alternative measures of government partisanship. The first critique we address by operationalizing government partisanship as the proportion of cabinet parties coming from left parties. The second we address by replacing these measures based on parties' nominal classification with a more direct measure of parties' preferences on welfare policy. We use Lowe et al.'s (2011) scaled measure of party preferences on social welfare policy, derived from the Comparative Manifesto Project's coding of the policy content of party manifestos (Lehmann et al. 2015). The measure's specificity to welfare benefits and its face validity alongside other measures of actual party preferences make it a good choice for exploring whether parties' stated preferences, rather than their nominal side of the political spectrum, are dynami-

cally related to social policies. To derive a cabinet-level measure from party-level preference measures, we take the average of all cabinet parties' scores weighted by their proportion of cabinet seats. Results from these two additional tests are plotted in figures A.9, A.8, A.10, and A.11.

Figure A.8 repeats the analysis in figure 1 using the percentage of left parties in the cabinet as a measure of government partisanship. Important to note here is that our concern about variation in national party systems provoking different findings for secular right and left parties is confirmed in the case of Ireland. In the cases of France and Sweden, our findings regarding secular right parties are mirrored in analyzing left parties. However, results for Ireland differ markedly between the two analyses. We attribute this to Ireland's party system, unique relative to either France or Sweden, in which the right side of the political spectrum is shared by secular and religious parties who have often been in government. Though the Irish secular right shows no variation in its (non-)effect on welfare generosity, the presence or absence of the Irish left in the cabinet has been variously associated with increases or decreases in welfare generosity over time.

Despite the consistent findings in France and Sweden and the statistically significant variation in Irish left parties' associations to changes in welfare generosity, the overall story emerging from figure A.8 is that in most countries and for the large majority of the time government partisanship is unrelated to changes in unemployment generosity.

Finally, one might argue that government partisanship is more appropriately measured by reference to how parties position themselves and advertise their ideologies in their public statements and not simply according to their putative policy preferences based on historical party family associations. The test above has asked whether parties historically associated with the secular right or the left of the political spectrum in their national context are associated with different likelihoods of making changes to welfare generosity, net of confounding factors, and whether those rates change over time. Another way of examining the relationship between government partisanship and welfare generosity would be to ask whether

Figure A.8: Estimated confidence intervals on left cabinet parties

Note: Panels include confidence intervals of time-varying coefficients on gov. partisanship, measured by the proportion of left parties in the cabinet, controlling for lagged level of unemployment benefit replacement rates and every combination of controls. Around 1,600 intervals are overlaid in each plot. Dashed lines denote zero.

cabinet parties' public stances toward the welfare state are associated with changes in social policy and whether that association is constant over time.

To address this latter version of the question, we repeat the analyses using a third measure of government partisanship – one capturing parties' expressed preferences about the welfare state. We derive this from parties' stances on government welfare policy as communicated in their election manifestos, Lowe et al.'s (2011) scaled measure of party preferences on social

welfare policy. The results across all 14 countries in our sample reveal no evidence of a relationship between government partisanship and changes in unemployment replacement rates and no evidence of any change in that null relationship over time. Confidence intervals and point estimates can be seen in figure A.10.

Figure A.9: Estimated time-varying coefficients for proportion of left cabinet parties

Note: Each panel plots time-varying coefficients on government partisanship, measured by the proportion of left parties in the cabinet, controlling for lagged level of unemployment replacement rates and every combination of controls. Around 1,600 lines are overlaid in each plot. Dashed lines denote zero.

Figure A.10: Estimated confidence intervals on manifesto welfare stances

Note: Panels include confidence intervals of time-varying coefficients on gov. partisanship, measured by a weighted average of the governing parties' stance on welfare derived from their manifestos, controlling for lagged level of unemployment benefit replacement rates and every combination of controls. Around 1,600 intervals are overlaid in each plot. Dashed lines denote zero.

Figure A.11: Estimated time-varying coefficients for manifesto welfare position

Note: Each panel plots time-varying coefficients on government partisanship, measured by a weighted average of the governing parties' stance on welfare derived from their manifestos, controlling for lagged level of unemployment replacement rates and every combination of controls. Around 1,600 lines are overlaid in each plot. Dashed lines denote zero.