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Markus Tepe and Pieter Vanhuysse

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Article

Elderly bias, new social risks and social spending: change and timing in eight programmes across four worlds of welfare, 1980–2003

Markus Tepe

Carl von Ossietzky University, Oldenburg, Germany

Pieter Vanhuysse*

University of Haifa and European Centre for Social Welfare Policy and Research, Vienna, Austria

Summary Over the past decades, all affluent welfare states have been coping with two major new trends: population ageing and new social risks resulting from de-industrialization. How have these demand-side trends, and their timing, affected welfare spending? We investigate up to 21 OECD democracies with respect to eight separate programmes and two composite indicators of aggregate welfare spending bias towards the elderly and new social risks. We find that welfare regime logics still matter crucially in accounting for variation between countries, as does the timing of the large-scale arrival of new social risks. Both Southern European welfare states and countries that entered the post-industrial society comparatively late spend less on programmes such as education and family allowances, and more on survivor pensions. However within countries, contemporaneous levels of new social risks conspicuously fail to affect spending on programmes that deal with these risks. These findings defy simple neo-pluralist expectations of social policy responsiveness: on their own, even dramatic demand-side trends influence welfare spending relatively little in advanced democracies.

Keywords comparative welfare regimes, demographic change, generational politics, service sector economy, social policy responsiveness

This article aims to investigate how individual welfare programmes and the general pro-elderly and pro-new social risks spending biases of mature welfare states respond to long-term demand-side changes deriving from population ageing and post-industrialization. Affluent democracies have been coping with two new, truly large-scale sociodemographic trends in the past four decades. First, accelerating population ageing, a combined result of longer life spans and lower fertility rates, has led to a rise in the demand for old-age cash and in-kind benefits and health care spending, and possibly also to a lower pressure for spending on younger generations. Second, the rise of the post-industrial economy and the massive entry of women in the labour market have led to new social risks and related social spending needs deriving from the rise of family instability, one-parent families and precarious employment or

Equal authors, listed alphabetically. *Author to whom correspondence should be sent: Pieter Vanhuysse, European Centre for Social Welfare Policy and Research, Berggasse 17, A-1090 Vienna, Austria. [email: vanhuysse@euro.centre.org]

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long-term unemployment for low-skilled workers.[1] Many studies have investigated changes in welfare spending towards either larger elderly voting groups or larger new social risks groups. However, few scholars have investigated how disaggregated social expenditure behaves if and when expenditure needs increase in both directions simultaneously, as is the case in most OECD democracies today. In a wider context of fiscal austerity since the 1980s, how have population ageing and the rise of the new social risk society affected the allocation of budgets to different welfare programmes?

We explore this question from two perspectives. First, are welfare states responsive towards changing demand-side pressures? Here we ask how changes in actual population demographics and post-industrialization affect 'supply-side' social spending within countries. Second, we investigate from a cross-sectional view the extent to which the timing of large-scale new social risks and membership in one of four 'worlds of welfare' can help to account for systematic differences in social spending between countries. We test the role of demand-side changes linked to population ageing and new social risks in driving a set of altogether ten supply-side spending variables in up to 21 OECD countries between 1980 and 2003. First, we compute two aggregate measures of relative welfare state spending bias: (a) the elderly/non-elderly spending share (henceforth ENSS) and (b) the new social risks share (henceforth NSRS). Second, we then disaggregate the analysis by accounting for spending on eight different welfare programmes separately: pensions, incapacity benefits, survivor pensions, health spending, family spending, unemployment benefits, active labour market policies and education. As Rose (1985: 1) suggested, scholars interested in the economic and political forces driving public spending growth should also focus on programme dynamics, rather than aggregate expenditure, as the most visible changes in government are its programmes. Using a disaggregated expenditure approach provides the possibility to track and compare how different welfare programmes with often very different underlying institutional and political logics respond toward changing demand factors (see also Castles, 2009).

Our main findings show that within countries, even dramatic socioeconomic trends such as population ageing and new social risks influence welfare spending relatively little. Contemporaneous levels of new social risks, in particular, conspicuously fail to affect spending on the programmes that deal with these risks. However the timing of the large-scale arrival of these risks, as well as institutional differences as captured by welfare regime membership, matter crucially in accounting for variation between countries. This article proceeds as follows. The following section reviews the literatures on elderly spending bias and new social risks. The subsequent section describes the data, variables and methods used, and derives hypotheses about the effects of demand-side social trends on the allocation of welfare budgets. This is followed by the presentation and discussion of the descriptive and multivariate analyses results within and between countries. The final section summarizes and concludes.

Theoretical background

Our theoretical starting point in linking changes in demand factors to the size of welfare programmes is the assumption of 'social policy responsiveness' (Brooks and Manza, 2006): all else being equal, higher social needs (deriving, in our case, from population ageing and new social risks) will generally translate into higher spending on the social programmes that deal with those needs. Social policy responsiveness means that social spending in competitive liberal democracies, especially when viewed on aggregate and in the long run, should be responsive to major changes in social needs, at least to the extent that these changes affect eligible voters.[2] This implies a neo-pluralist view of policymaking that also underpins many economic theories of democracy (e.g., Breton, 1996; Lake and Baum, 2001) and political economy theories of welfare states (e.g., IMF, 2004; Persson and Tabellini, 2000). These theories similarly assume that policy outputs in competitive democracies on the whole reflect (median) voter preferences rather well. In its most generic form, the implicit mechanism at work is the following: if voters vote according to their policy self-interest, then numerical changes in large ascriptive groups (e.g., elderly people) will compel parties across the ideological spectrum to drive up relevant programme spending. The particular ways in which social policy responsiveness is conditioned by factors such as interest representation, social movements and political participation, electoral models, party politics, collective action problems, and institutional

constraints and legacies remain black boxes in our account. However, we test how social policy spending responds to population ageing and new social risks on the demand side.

Population ageing

Population ageing in developed democracies is caused in large part because elderly cohorts today live significantly longer but still rarely retire significantly later, while fertility rates have hit post-war lows almost everywhere (e.g., Castles, 2003, 2004; Esping-Andersen, 2009; IMF, 2004; OECD, 2006). A small and somewhat eclectic literature has started investigating how this has affected welfare spending, and specifically the pro-elderly biases in social spending across the OECD. Pampel (1994) finds that larger aged populations increase the pro-elderly bias of public spending only in the absence of strong left parties and class-based corporatist institutions. Using a purely cross-sectional approach, Castles (2009) finds that population ageing generally increases old-age-related expenditure and decreases working-age related expenditure. Lynch (2006) argues that population ageing as such matters little in explaining variation in the pro-elderly biases of social spending – an argument we test directly (see H1 below). Instead, Lynch suggests that pro-elderly biases are better explained by the historical choices of welfare design and electoral competition at two critical junctures. First, in the early twentieth century, welfare programmes were designed along either narrow occupational or more universal citizenship-based lines. This led to the progressively more pro-elderly bias of social policies in occupational regimes, which focused from the start on protecting a core of powerful labour market insiders, who are today an ageing subpopulation. Citizenship-based regimes, by contrast, also focused on younger labour market outsiders, such as children and mothers. After World War II, a second bifurcation occurred within occupational regimes between particularistic and programmatic models of electoral competition. While all particularistic electoral models stuck to their occupational welfare design, some of the programmatic electoral models (e.g., the Netherlands) switched towards more citizenship-based welfare design. These constellations of electoral competition and welfare design, Lynch argues, have interacted over the course of the twentieth century to shape the electoral incentives of policymakers and have crystallized into distinct and relatively stable age orientations of social policy. Occupational/particularistic constellations, most prominently visible in Southern Europe, Japan, and the USA, fostered clientelistic and targeted policies that gave politicians electoral incentives to favour selective groups such as retired former labour market insiders. Italy's myriad pension systems catering for narrow groups are a case in point. By contrast, citizenship-based/programmatic constellations, as in Nordic Europe and the (non-U.S.) Anglo-Saxon world, gave rise to much more broadly cast safety nets, where the relative absence of clientelistic votes-for-benefits exchanges did not force politicians to increase the pro-elderly bias of social policies to the same degree. A case in point is Sweden: one of the demographically oldest OECD welfare states, yet also one of the least pro-elderly biased.

Lynch's (2006) historical–institutional theory has not been tested statistically, and it is bolstered with two qualitative case studies of the Netherlands (medium pro-elderly bias) and Italy (high bias). This conveys little information about the origins of the *least* pro-elderly countries, as none of these countries are covered. Yet Lynch's primary demarcation line between more and less pro-elderly social spending regimes has been broadly replicated by Esping-Andersen and Sarasa (2002) regarding general social spending; Tepe and Vanhuysse (2009) regarding pension spending; and Sabbagh and Vanhuysse (forthcoming) regarding inter-generational justice perceptions. Lynch and Myrskylä (2009) refute the 'self-interested pensioners' hypothesis when it comes to pension policy *attitudes*: pension recipient status does not lead to greater support for pension system status quo in 11 European countries. Kitschelt and Rehm (2006) and Busemeyer et al. (2009) find that the relevance of the age/retirement cleavage in determining welfare attitudes varies across programmes, with the strongest (negative) age effects to be found in the case of education spending. Others, too, have found qualified support for the 'self-interested pensioners thesis' when it comes to actual education or human capital spending (Berkman and Plutzer, 2004; Busemeyer, 2009). While our study does not tackle the self-interested attitudes thesis, we investigate whether and how the relative size of pro-elderly spending (the *ENSS*) and spending on individual programmes such as pensions, incapacity

benefits, survivor benefits, health spending, family spending, unemployment and active labour market programmes are affected by population ageing.

New social risks and their timing

A large body of literature has explored how affluent welfare states have adapted to the new social risks deriving from the rise of service sector economies, the rise of female employment, long-term unemployment or precarious employment among low-skilled workers, working poverty (often in service jobs), and more unstable family patterns including higher divorce rates and single parenthood.[3] Policies adopted to address new social risks are qualitatively and functionally different in both their aims and their beneficiaries from those addressing the 'old' social risks during the 'Golden Age' of OECD welfare states. New social risk policies, for instance, aim less to decommodify workers and are targeted less at male breadwinners in industrial jobs. Rather, these policies aim to improve the economic security and human capital of already highly commodified workers in the new post-industrial economy, which nowadays often tend to be women and low-skilled workers (Bonoli, 2007; Armingeon and Bonoli, 2006; Esping-Andersen, 1999, 2009). However in addition, new social risk policies today might actually compete for scarce state resources with policies for elderly populations such as pensions, elderly care and health care. The extent of such distributional clashes may in turn depend on the historical *timing* in individual societies of the large-scale arrival of new social risks relative to the arrival of population ageing. Specifically, Bonoli (2007) argues that the comparatively early emergence of new social risks in the Nordic countries, at a time when accelerated population ageing had not yet kicked in, resulted in larger spending to alleviate these risks. By contrast, in Continental and Anglo-American countries, the later arrival of new social risks in society happened to coincide with that of population ageing, thereby increasing budgetary pressures and reducing the prioritization of new social risk policies.

In other words, only those countries that were confronted not simultaneously but sequentially with accelerating population ageing and new social risks may have been able to significantly increase new social risks-oriented spending. Bonoli (2007) presents explorative descriptive evidence supporting this argument by juxtaposing levels of new social risk spending with the time lag with which different OECD countries had 'caught up' with Swedish levels of actual new social risks in society. By using cross-sectional regression models, this article tests the thesis that new social risk spending effort depends on the *timing* of the large-scale arrival of these risks in society (H5 below).

Measuring spending bias and programme dynamics

We investigate two sets of dependent variables. On the one hand, we use social expenditure data on eight individual welfare programmes to estimate the effect of our explanatory variables on programme expenditure per GDP. On the other hand, we construct two variables capturing the relative priority given by welfare states on aggregate to, respectively, pro-elderly social spending and new social risks spending. We define *ENSS* as follows:

$$ENSS_{i,t} = \left(\frac{Pensions_{i,t} + Survival_{i,t}}{Pensions_{i,t} + Survival_{i,t} + Incapacity_{i,t} + Family_{i,t} = ALMP_{i,t} + Unemployment_{i,t}} \right) * 100 \quad (1)$$

for country *i* in year *t*. Our *ENSS* is related but not identical to the elderly/non-elderly spending *share* developed by Lynch (2006: 29–31), which is operationalized as total spending on pensions and services for those aged above 65 (or those in formal retirement) adjusted for the number of elderly persons, as a share of total spending on unemployment benefits, active labour market policies, family allowances, and family services, adjusted for the number of working-age persons. As we are interested in the effect of population ageing on *ENSS*, we do not adjust for other social need factors. Nor do we incorporate

health and education spending, since health spending cannot be clearly attributed to elderly vs. non-elderly generations, whereas our data on education expenditure come from different statistical sources.[4] We do not include incapacity benefits in the numerator of our *ENSS* measure as we assume that these benefits primarily benefit employed persons, who for example suffer from an accident.

We do not include pensions in our *NSRS* measure as we consider old age to be neither an 'old' nor a 'new' social risk as such. Instead, we highlight family spending and active labour market spending as two key new social risk programmes. Both programmes deal directly with the new socioeconomic policy goals, set out, for instance, in the European Union's Lisbon 2000 Agenda, of supporting working mothers and actively investing in skills and human capital aimed at (re-)employing at-risk groups such as women and the long-term unemployed. Accordingly, we define *NSRS* as:

$$NSRS_{i,t} = \left(\frac{Family_{i,t} + ALMP_{i,t}}{Family_{i,t} + ALMP_{i,t} + Unemployment_{i,t} + Survival_{i,t} + Incapacity_{i,t}} \right) * 100 \qquad (2)$$

We employ expenditure data from a sample of up to 21 OECD countries covering a maximum time span from 1980 to 2003, from the OECD's (2008) SOCX database except for education (see also the Appendix). To be sure, social spending measures are an imperfect measure of governmental (let alone electoral) preferences: expenditures jointly reflect the nature of democratic decision making, informational signals from the policy environment, the preferences of incumbent parties, plus the traces of past governments as embedded in policy legacies and legal and institutional constraints. Nor can spending measures capture the full nature of welfare state effort. Depending on the research question asked, welfare effort can also be usefully gauged by measures of (changes in) social rights, entitlements, welfare generosity, or de-commodification (e.g., Esping-Andersen, 1990; Korpi and Palme, 2003). Yet spending measures clearly provide an adequate solution to the specific 'dependent variable problem' we aim to address (Green-Pedersen, 2007). We set out to investigate neither qualitative changes in the content or institutional structure nor quantitative changes in the micro-level generosity of welfare programmes, but rather the quantitative degree to which social spending at the macro level reacts to social needs on the demand side in the long term. For this purpose, the SOCX database is conceptually well suited (Castles, 2002: 613), in addition to being more widely available across time, cases and programmes.[5]

Hypotheses and measurement of demand-side changes

As regards the theoretical relationship between *ENSS* and *NSRS* and our independent variables, we test whether, all else being equal, higher social needs deriving from population ageing and new social risks generally translate into higher spending on the social programmes that deal with those needs. To illustrate, our demographic independent variable of main interest is the number of people aged 65 and more as a share of those aged between 15 and 64. This old-age dependency ratio admittedly does not capture the population up to 15 years of age. Since children cannot vote, it is a better measure for political analysis, as it is a proxy for the relative *numerical* strength of elderly relative to non-elderly (theoretically) eligible voters. Given the higher actual voting turnout rates of elderly people, this is probably a conservative measure of their political-electoral clout (but see Goerres, 2009). As regards the effect of population ageing on *ENSS*, Lynch (2006) argues that population ageing as such matters little, whereas a 'grey power thesis' along neo-pluralist lines sketched out above (e.g., Breyer and Craig, 1997; Disney, 2007; IMF, 2004; OECD, 2006; Persson and Tabellini, 2000) suggests that, faced with a growing proportion of retired or nearly retired voters, policy makers will respond by implementing policies in ways that boost elderly voters' welfare interests relative to those of younger voters. Accordingly, we hypothesize that

H1: ENSS increases with a larger share of elderly voters relative to younger voters in society.

Confronted with a multi-dimensional umbrella concept such as post-industrialization, we try to proxy this process by three independent variables. To account for the growing relative importance of the service sector, we use the measure of *service sector workers* as a share of total employment. The other two variables are intimately related to the new role of women in the post-industrial economy. Higher *female labour force participation rates*, another key element of the new social landscape (Esping-Andersen, 1999, 2009; Bonoli, 2007), are tapped by the percentage of women who participate in the active labour force as a share of the percentage of men who participate. Women's labour market participation is crucial today not just as it increases their individual social security and pension rights, but also as it is likely to increase their economic and political power, loosely defined. Job opportunities for women today directly increase both their bargaining power within the family (Iversen and Rosenbluth, 2006) and their wage relative to that of their partners (Huber et al., 2009). More female jobs also boost women's representation in political life, by combating traditional voter attitudes toward women and by bolstering the supply of women with professional experience, skills, and resources (Iversen and Rosenbluth, 2008).

To account directly for this political-representational dimension, we explore the effect of the share of *female parliamentarians*. While not a new social risk as such, this variable has co-evolved temporally with the rise of the post-industrial society. The share of women in parliaments has gone up steadily and uniformly since at least 1980 (Bolzendahl, 2009: 45), perhaps in part as a result of political value changes due to larger shares of working women and larger populations of female elderly voters. The share of women in parliaments can be a proxy for the degree to which increased female economic clout (e.g., through higher relative wages and labour force participation rates) is translated into female political influence, at least in PR electoral systems (Iversen and Rosenbluth, 2008). Parliamentary representation, in turn, is likely to shape social policy outcomes in women's favour. Bolzendahl (2009) finds that while the effect on total social spending of gender-specific variables such as divorce rates, female labour participation, women in leftwing parties and women in parliament is highest when all these traits work in conjunction, it is the latter variable which has the single most powerful overall effect (Bolzendahl and Brooks, 2007; but see Huber et al., 2009). Moreover, female legislators tend to be significantly more likely than men to be assigned to education, health and welfare related parliamentary committees (Thomas, 1994: 66; Wängnerud, 2009), to embrace legislative priorities dealing with issues of women, children, and the family (Thomas, 1994: 7) and to have more pro-welfare preferences (Wängnerud, 2009: 62; Bolzendahl and Brooks, 2007). As regards the effect of these three variables on *NSRS*, we therefore hypothesize that

H2: NSRS increases with a larger share of service sector workers.

H3: NSRS increases with a larger share of female labour market participation.

H4: NSRS increases with a larger share of women in national parliaments.

Lastly, we investigate the thesis that the comparatively late arrival of new social risks in society may reduce new social risks spending shares, by means of a *New Social Risks Timing* variable taken from Bonoli (2007: 513). This variable measures the average time lag in years with which any given country had 'caught up' with the particular 1970 level of Sweden, the OECD frontrunner, with respect to three new social risk variables: service sector employment (which stood at 54% in Sweden in 1970), female labour market participation rates (58%), and divorce rates (30%). As regards the effect of this variable on *NSRS,* to test the Bonoli (2007) thesis we hypothesize that

H5: *NSRS* decreases with a later 'arrival' of a given country at the particular levels of new social risks in Sweden in 1970.

We also include economic control variables. *GDP growth* and *annual deficit* take into account the financing side of the welfare state. The *unemployment rate* accounts for the demand side of the welfare state. Finally, on the programme level each model includes a variable measuring the development in *all other programmes* together.[6] Here we

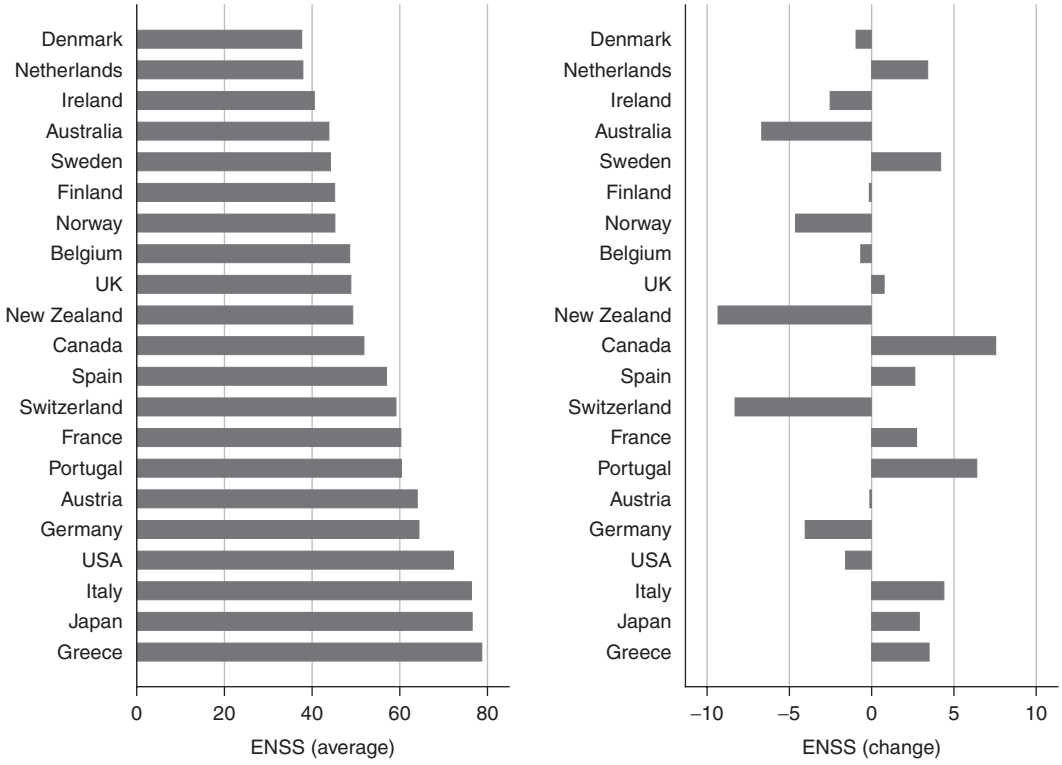

Figure 1 Elderly/non-elderly spending share (*ENSS*)
Note: Averages refer to 1980 to 2003. Change refers to differences between 1998–2003 and 1986–1991.
Source: OECD (2007) own calculation.

check for the possibility that, even after controlling for socio-political, timing and economic variables, spending on any one given welfare programme may not change independently from spending on the eight-programme welfare state as a whole, causing welfare states to resemble herds rather than individual elephants on the move, to paraphrase Hinrichs (2000).

Descriptive analysis

Figures 1 and 2 show average values for *ENSS* and *NSRS* between 1980 and 2003 and change in these shares between the last (1998–2003) and the second (1986–91) period (descriptive statistics of all other variables are available on request). Very much in line with similar estimates by Lynch (2006), who considers needs-adjusted ratios rather than spending shares, we find that the countries most heavily biased in public policy spending towards elderly as opposed to non-elderly generations tend to be the USA, Japan, Switzerland, Austria, and all members of the Southern European welfare regime.

By contrast, Scandinavian and Anglo-Saxon countries (the USA excepted) all figure among the least pro-elderly regimes. Relative public policy effort towards new social risks follows a somewhat less clear-cut regime pattern, with one exception: seven of the nine countries with the highest *ENSR* levels also record the lowest *NSRS* levels. To the extent that different individual welfare programmes vie for scarce resources within limited welfare state budgets, one would indeed expect a negative correlation between relative priorities for levels of *ENSS* versus *NSRS*. Testing the bivariate correlation, this

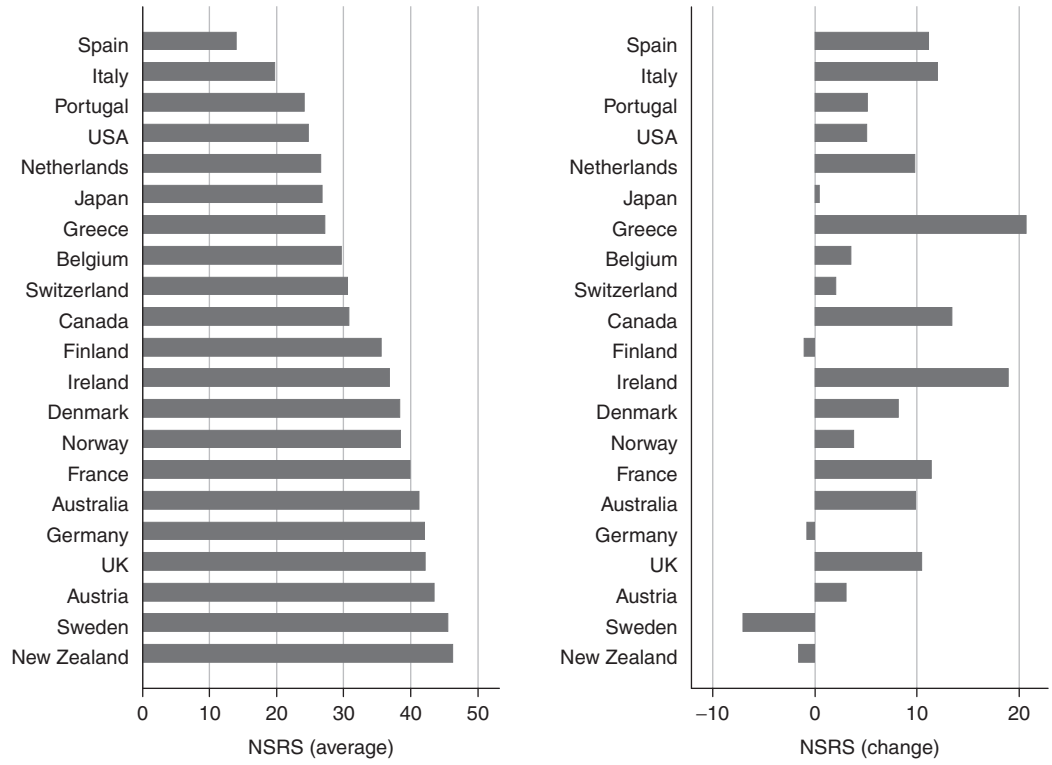

Figure 2 New social risk spending share (*NSRS*)
Note: Averages refer to 1980 to 2003. Change refers to differences between 1998–2003 and 1986–1991.
Source: OECD (2007) own calculation.

is precisely what we find (–0.33***; not shown). Similarly, the bivariate correlation between Bonoli's (2007) value for *New Social Risks Timing* and *NSRS* (–0.45***) provides preliminary support for H5.

Exploring change in *ENSS* and *NSRS* between the second period (1986–1991) and the last period (1998–2003), Figure 1 shows that except for Canada and Portugal (large increase), and New Zealand and Switzerland (large decrease), there have been mostly small changes in *ENSS* in both directions. In contrast, Figure 2 indicates that all countries in our sample except one (Sweden) have either reported at least a status quo, and often substantial increases, in *NSRS* levels. Clearly, OECD democracies have more firmly entered the new social risk society between the late 1980s and the turn of the century. This is clear also in Table 1, which shows descriptive statistics for our four main independent variables and our two dependent variables in the last observation period specifically (1999–2003).

Table 1 also shows values for a new social risks *timing* variable taken from Bonoli (2007: 513), who lists the specific year in which any given country had, on average, reached the particular level of Sweden in 1970, in terms of service sector employment, female labour market participation rates, and divorce rates. Thus, countries such as Austria and Belgium were first confronted with these particular levels of new social risks respectively 18 and 23 years after Sweden had been confronted with these levels. Today, all OECD countries are confronted with at least Sweden's 1970 levels of new social risks: Spain was the last country to catch up, in 1996. Not surprisingly, however, Sweden and other

Table 1 New social risks timing, and levels of four 'demand side' variables, *ENSS* and *NSRS*, 1998–2003

Country	*New social risks timing (year)*[a]	*Old-age dependency ratio*[b]*	*Female employment*[c]*	*Service sector*[d]*	*Female parliamentarians*[e]*	*ENSS*[f]*	*NSRS*[f]*
Sweden	1970	27.0	93.2	73.5	43.5	46.8	40.2
Denmark	1972	22.3	89.4	71.0	37.7	37.4	42.2
USA	1975	18.7	83.9	79.0	13.7	72.2	27.4
Norway	1977	23.3	90.1	73.8	36.1	43.3	40.1
Canada	1978	18.4	85.6	74.5	20.6	57.8	38.8
New Zealand	1979	18.0	80.8	68.3	29.2	41.0	43.9
UK	1979	24.3	81.7	73.7	18.1	49.3	50.5
Finland	1980	22.5	92.4	66.9	36.2	46.3	35.7
Japan	1980	26.0	69.4	64.3	6.4	78.4	27.0
Australia	1982	18.7	78.8	74.2	23.7	40.7	47.3
Switzerland	1982	23.5	81.2	70.5	22.9	55.2	32.7
Austria	1988	22.8	78.2	64.0	29.1	65.2	45.0
France	1988	24.6	83.1	72.1	11.3	61.1	45.6
Germany	1989	26.0	80.3	64.0	31.3	62.6	43.0
Ireland	1989	16.7	69.5	64.0	12.4	39.6	49.7
Netherlands	1990	20.1	78.3	76.7	35.8	40.7	34.5
Belgium	1993	25.6	75.8	72.6	23.5	48.9	32.3
Italy	1994	26.2	62.3	62.2	10.5	77.9	28.1
Greece	1995	24.8	64.0	60.6	7.9	81.6	37.4
Portugal	1995	24.0	80.5	52.9	17.9	63.8	28.1
Spain	1996	24.5	65.4	62.3	26.1	58.8	23.6

Note: * average values.
Source: [a]Bonoli (2007: 513) Japan, Ireland and Greece own calculation based on Bonoli (2007:513) benchmark for Sweden in female employment and service sector, [b]Armingeon et al. (2007), [c]OECD Labor force statistics, [d]OECD Labor force statistics, for the USA Agostino et al. (2006), [e]Paxton, Green and Hughes (2009), [f]OECD (2007) own calculation.

new social risks frontrunners have not remained stable since 1970. Much variation consequently remains today in the degree to which countries face population ageing and new social risks. At the turn of the century, the share of service sector employment ranged between 53% in Portugal and 79% in the US. Sweden remains the leader on no less than three dimensions, with female labour market participation shares of 93% (compared to 64% in Greece, the laggard country), 44% of female parliamentarians (compared to 6% in Japan), and elderly citizens representing 27% of non-elderly citizens (17% in Ireland).

Regression analysis

To separate within-unit and between-unit effects we follow Jackman (1985) in applying two different models: one focusing on change over time within countries, and one focusing on variance between countries.[7] Theoretically, we try to answer two kinds of question. First, to explore the long-term effects of population ageing and new social risks on the fiscal size of welfare programmes, we use fixed-effects models exploring variance in the programme size *within* countries over time (Table 2). Our second question is cross-sectional in nature, as we are interested in the effect of welfare regime affiliation and new social risks timing on variation *between* countries. Here we use a between-effects or cross-sectional estimation approach (Table 3). The inherently limited scope of aggregated data analysis allows only stylized regression analysis, rather than full-fledged causal models in the strict sense.[8] We employ aggregate data models mainly to point out statistical connections between variables based on the heuristic framework of hypotheses set out above. In order to allow easier interpretation of the effect sizes of the estimation coefficients, all metric independent variables have been z-standardized. Thus, all estimation coefficients

presented below indicate the effect of the independent variables on the dependent variables when the former change by one standard deviation.[9]

Within-country effect models

Our first set of models investigates the degree to which *ENSS, NSRS* and 'supply-side' spending on the eight individual programmes responds to 'demand-side' increases with population ageing and three measures of new social risks. Since we are interested in structural macro-changes rather than annual fluctuations, we have grouped the annual observations into six-year averages over four periods (1980–1985, 1986–1991, 1992–1997 and 1998–2003).[10] The dataset's panel structure suggests using the random or fixed effects estimator.[11] Without plausible empirical evidence for the random effects assumption, bias and consistency considerations alone would lead to a fixed-effects model.[12] Theoretical considerations also strongly suggest employing the fixed-effects (or within-country) estimator, since their predictions concern how changes within countries affect changes in social expenditure. Table 2 presents OLS estimation results for our first set of ten country fixed-effects regressions models, exploring the impact of our independent variables on welfare spending within a given country. To explore the robustness of our OLS estimations, we also employed an alternative estimation approach based on quasi-maximum-likelihood estimates (not shown, available on request). Since there are no substantive deviations, we rely on the more easily interpretable z-standardized OLS estimation coefficients below. Note also that to reduce the risk of an over-fitted model, we have kept our models as parsimonious as possible. Thus, we began with a broad model that also included two socio-political control measures, leftwing partisanship and strike days, which was excluded from the baseline models in Table 2, as neither variable showed an effect on *ENSS* and *NSRS*.[13]

GDP growth is significantly associated with increases in *NSRS*, indicating that new social risks spending mainly increases when there is budgetary surplus during economically 'good times'. Health spending, by contrast, appears to be counter-cyclical. Within this six-year-averages framework, annual *budget deficits* have no substantial impact on *NSRS* but a significant positive impact on *ENSS*, indicating that increasing pro-elderly spending biases are likely to be partially financed by deficit spending. On the programme level, the estimated coefficient for *deficits* is generally negative, though it is significant only in the case of active labour market programmes. This (weakly) supports the 'good times' interpretation, as governments appear to exhibit a modicum of fiscal conservatism with respect to individual programmes, education excepted. Predictably, after controlling for all other variables, growing *unemployment* decreases both *ENSS* (-0.13) and *NSRS* (-0.25) and increases unemployment expenditure (0.62). But unemployment levels do not significantly affect other 'natural' candidates for related expenditure increases such as (early) pensions, incapacity benefits, and, especially, active labour market programmes. The effects of spending on *all other programmes* show that health, education and family spending do not grow independently. Whenever the welfare state (the herd of elephants) generally grows, these three programmes (the individual elephants) troop along. When comparing the effect size of *all other programmes* with that of control variables such as GDP growth, budget deficits and period effects, the size of this first variable is stronger (0.92 in the case of health expenditure, 0.82 for education expenditure and 0.43 for family expenditure).

Turning to our macro-societal trends of primary concern, we find that higher old-age dependency rates tend to increase pension expenditure, which is in line with much prior research (Breyer and Craig, 1997; Castles, 2004; Disney, 2007; Pampel and Williamson, 1989; Tepe and Vanhuysse, 2009). Population ageing also increases *ENSS*, which provides tentative support for H1. This effect is stronger than that of budget deficits and GDP growth. But the old-age dependency ratio has no substantive effect on *NSRS*, nor does it appear to affect family allowances or spending on the other welfare programmes. Interestingly, our three measures of new social risks 'on the ground' conspicuously fail to affect programme expenditure. *Female employment* rates have no significant effect on any of our ten dependent variables, and the same is true for the effect of *service sector* employment, old age pensions excepted. Surprisingly, a larger share of *female parliamentarians* is associated with non-significant decreases in *ENSS* and with significant increases in incapacity spending and decreases rather than increases in *NSRS*. Unpacking expenditure analysis into two aggregate spending shares and then into individual

Table 2 Determinants of welfare spending: within-country (fixed) effects

	Model 1 ENSS	*Model 2 NSRS*	*Model 3 Pension*	*Model 4 Incapacity*	*Model 5 Survivors*	*Model 6 Health*	*Model 7 Family*	*Model 8 Unempl.*	*Model 9 ALMP*	*Model 10 Education*
GDP growth	–0.049	0.17***	–0.046	–0.0016	–0.074	–0.18**	0.11***	–0.061	0.025	0.038
	[0.034]	[0.057]	[0.041]	[0.056]	[0.064]	[0.073]	[0.028]	[0.041]	[0.085]	[0.066]
Deficit	0.090***	–0.0011	–0.018	–0.081	–0.036	–0.052	–0.078	–0.074	–0.22**	0.13*
	[0.030]	[0.090]	[0.033]	[0.072]	[0.074]	[0.071]	[0.048]	[0.067]	[0.098]	[0.074]
Unempl. Rate	–0.13*	–0.25*	0.079	0.031	0.17	–0.32	–0.067	0.62***	0.16	0.007
	[0.065]	[0.12]	[0.100]	[0.12]	[0.18]	[0.23]	[0.12]	[0.15]	[0.17]	[0.099]
ODR	0.17**	0.11	0.36***	–0.22**	0.04	–0.24	–0.014	0.043	0.0033	–0.021
	[0.074]	[0.11]	[0.080]	[0.086]	[0.079]	[0.28]	[0.13]	[0.15]	[0.25]	[0.18]
Female empl.	–0.21	0.37	–0.17	–0.39	–0.12	0.62	0.16	0.011	0.47	–0.13
	[0.15]	[0.33]	[0.19]	[0.28]	[0.35]	[0.36]	[0.30]	[0.24]	[0.38]	[0.24]
Service share	0.15	0.75	0.59**	–0.34	–0.47	–0.32	–0.0079	0.05	–0.057	0.1
	[0.21]	[0.50]	[0.24]	[0.29]	[0.29]	[0.31]	[0.19]	[0.23]	[0.24]	[0.23]
Female parl.	–0.01	–0.37**	0.037	0.32*	–0.12	–0.24	0.00086	0.11	–0.24	0.18
	[0.11]	[0.16]	[0.14]	[0.17]	[0.16]	[0.28]	[0.18]	[0.14]	[0.24]	[0.21]
Period 2	–0.014	–0.54**	–0.081	0.41**	0.11	0.18	–0.11	0.081	0.11	–0.44*
(86–91)	[0.10]	[0.20]	[0.12]	[0.17]	[0.27]	[0.39]	[0.14]	[0.18]	[0.24]	[0.24]
Period 3	–0.13	–0.57*	–0.32	0.59*	0.19	0.44	–0.1	–0.05	0.36	–0.43
(92–97)	[0.20]	[0.28]	[0.22]	[0.29]	[0.44]	[0.65]	[0.23]	[0.26]	[0.49]	[0.38]
Period 4	–0.19	–0.57	–0.42	0.77**	0.47	0.82	–0.021	–0.19	0.46	–0.7
(98–03)	[0.26]	[0.50]	[0.31]	[0.36]	[0.52]	[0.81]	[0.35]	[0.33]	[0.67]	[0.50]
All other prog.			0.21	0.31	–0.078	0.92***	0.43**	0.15	–0.089	0.82***
			[0.12]	[0.22]	[0.15]	[0.27]	[0.18]	[0.14]	[0.23]	[0.28]
Observations	76	76	76	76	76	76	76	76	76	76
Adj. within R2	0.202	0.579	0.749	0.363	0.407	0.553	0.314	0.722	0.311	0.364
Countries	21	21	21	21	21	21	21	21	21	21

Note: Robust standard errors in brackets. Constant included but not reported, ***$p < .01$, **$p < .05$, *$p < .1$.

programmes thus leads us to strongly qualify previous findings on the strong effect of both female labour market participation and women's representation on *general* social spending (e.g., Bolzendahl, 2009; Bolzendahl and Brooks, 2007). To test for the possibility that these three independent variables may not adequately capture the mechanisms by which new social risks lead to supply-side spending changes, we also added single parenthood, as proxied by the divorces-to-marriages rate, to our models. This variable did not show a significant effect on either *ENSS* or *NSRS*.[14] This bolsters our confidence in rejecting Hypotheses 2 through 4.

We investigate *time period* effects by including dummies for the three most recent six-year periods in Table 2. For all three most recent periods, the effect sizes are negative though non-significant for *ENSS* and negative but larger for *NSRS*. Once we control for population ageing, new social risks, economic and socio-political variables, OECD democracies have witnessed significantly less pro-new social risks spending between the mid-1980s and the mid-1990s. As regards individual programmes, time period effects are negative for pensions, unemployment benefits, family allowances, and education, in the latter two cases possibly as a result of declining fertility rates in most Western democracies. Period effects are positive for health spending, which is strongly driven by spiralling technology costs, and for active labour market programmes and incapacity benefits.[15] These period effects are consistently significant only in the case of incapacity benefits, which may increasingly replace unemployment benefits as alternative ways to deal with labour market outsiders. However, in seven out of eight programmes the direction of correlation remains the same across the last three periods, and the effect size is larger, often markedly, in the fourth period than in the second period. This is the case, for instance, with incapacity benefits (almost twice as large) and with health spending (four and a half times larger) and pensions (five times as large). Welfare programmes at the turn of the century have thus continued more strongly along the pathways which they were following in the late 1980s, whether these paths were towards expansion or retrenchment.

Between-country effect models

Table 3 investigates the respective roles of two time-invariant determinants of welfare spending: welfare regime type (upper panel) and new social risks timing (lower panel). The between-effects estimator draws solely on the cross-sectional information in the data using period averages and can be considered to provide a long-term perspective on the effect of certain demand factors. These parsimonious models only retain the old-age dependency ratio, the share of service sector jobs, GDP growth, and budget deficits. The main aim is to compare the ability of welfare regime type dummies and the timing of the advent of the new social risk society to explain social expenditure change cross-sectionally.[16] Following the 'Southern addition' made by Ferrera (1996) and others to Esping-Andersen's (1990) original three-world typology, we divide the OECD sample into four *welfare regime* types: the liberal regime in the Anglo-Saxon world, the conservative-corporatist regime in continental Europe, the social-democratic regime in Nordic Europe, and the Southern European regime. *New social risks timing* in turn tests Bonoli's (2007) argument that a crucial element in explaining new social risks spending priorities is the early versus belated 'arrival' of large-scale new social risks in society.

Model 2 in the lower panel supports the thesis on new social risks timing (H5). Countries that entered the new social risk society comparatively late devote significantly fewer resources to new social risk policies. On the programme level, this result is primarily driven by the lower outlays on education and family policies and the higher outlays on survivor pensions. In some ways, this finding on new social risks timing mirrors an oft-cited observation on 'institutional timing' by Myles and Pierson (2001: 318): *none* of the OECD nations where extensive, mature pay-as-you-go pension systems were in place by the mid-1970s have subsequently moved towards fully funded systems; only the smaller group of seven pay-as-you-go latecomers had the electoral and budgetary leeway to do so. Our findings indicate that 'demand-side' timing ought to be seen as interacting with the 'supply-side' timing of particular institutional and policy choices studied by comparative institutionalists in determining budgetary leeway in social policy.

Regarding institutional effects, Table 3 indicates that welfare regime affiliation still appears to matter crucially in accounting for expenditure variance. The adjusted *R*-squared values show that the regime models (upper panel) generally outperform the new

Table 3 Determinants of welfare spending: between-country effects for welfare regime models (upper panel) and new social risk timing models (lower panel)

Upper panel	*Model 1 ENSS*	*Model 2 NSRS*	*Model 3 Pension*	*Model 4 Incapacity*	*Model 5 Survivors*	*Model 6 Health*	*Model 7 Family*	*Model 8 Unempl.*	*Model 9 ALMP*	*Model 10 Education*
ODR	0.13	0.31*	0.19	0.00034	0.54	0.069	0.34	–0.19	0.23	–0.22
	[0.20]	[0.17]	[0.27]	[0.27]	[0.38]	[0.35]	[0.23]	[0.38]	[0.32]	[0.30]
Service share	–0.59**	–0.67***	–0.5	0.37	0.56	–0.21	–0.35	0.27	0.16	0.38
	[0.24]	[0.21]	[0.29]	[0.29]	[0.41]	[0.37]	[0.24]	[0.40]	[0.35]	[0.32]
GDP growth	–0.66**	–0.33	–0.57	0.36	0.66	–0.36	–0.12	0.38	0.43	–0.12
	[0.25]	[0.21]	[0.34]	[0.33]	[0.47]	[0.43]	[0.28]	[0.46]	[0.39]	[0.37]
Deficit	–0.22	0.0011	–0.42	0.21	0.71	–0.65	0.4	0.81	–0.05	–0.12
	[0.28]	[0.24]	[0.45]	[0.44]	[0.63]	[0.57]	[0.38]	[0.62]	[0.44]	[0.49]
Continental	–0.2	–1.18***	0.64	1.40**	0.98	0.043	–0.17	1.05	0.76	0.054
	[0.41]	[0.36]	[0.54]	[0.54]	[0.76]	[0.69]	[0.46]	[0.75]	[0.65]	[0.59]
Nordic	–0.82	–0.64	0.62	2.19***	–0.6	–0.27	0.72	1.18	1.42*	1.19*
	[0.46]	[0.40]	[0.60]	[0.60]	[0.84]	[0.77]	[0.51]	[0.83]	[0.73]	[0.66]
Southern	0.27	–3.28***	–0.36	1.14	2.24*	–1.45	–1.76**	1.03	–0.11	–0.53
	[0.56]	[0.48]	[0.77]	[0.76]	[1.08]	[0.98]	[0.65]	[1.06]	[0.88]	[0.84]
R2	0.823	0.855	0.696	0.692	0.447	0.369	0.783	0.349	0.535	0.632
Baseline R2	0.599	0.626	0.484	0.644	0.249	0.205	0.686	0.136	0.472	0.553
Adj. R2	0.711	0.762	0.519	0.513	0.124	0.001	0.656	–0.031	0.264	0.418
F-Test	7.33**	9.25***	3.92**	3.86**	1.38	1	6.18***	0.92	1.97	2.95**
Lower panel	*Model 1 ENSS*	*Model 2 NSRS*	*Model 3 Pension*	*Model 4 Incapacity*	*Model 5 Survivors*	*Model 6 Health*	*Model 7 Family*	*Model 8 Unempl.*	*Model 9 ALMP*	*Model 10 Education*
ODR	–0.026	0.042	0.41	0.57*	0.48*	0.11	0.57**	0.15	0.57*	0.029
	[0.17]	[0.25]	[0.24]	[0.30]	[0.26]	[0.30]	[0.23]	[0.31]	[0.30]	[0.23]
Service share	–0.59**	–0.14	–0.29	0.21	0.61*	0.3	–0.056	0.21	0.24	0.23
	[0.23]	[0.33]	[0.28]	[0.35]	[0.30]	[0.35]	[0.27]	[0.36]	[0.37]	[0.27]
GDP growth	–0.71***	0.098	–0.60*	0.16	0.21	–0.24	0.17	0.18	0.4	0.019
	[0.22]	[0.32]	[0.30]	[0.38]	[0.33]	[0.39]	[0.30]	[0.39]	[0.38]	[0.30]
Deficit	–0.38	0.017	–0.13	0.69	0.11	–0.42	0.98**	0.99*	0.25	0.35
	[0.28]	[0.41]	[0.43]	[0.54]	[0.47]	[0.55]	[0.43]	[0.56]	[0.47]	[0.43]
NSR Timing	0.27	–0.67**	0.027	–0.05	0.97***	0.074	–0.41*	0.16	–0.17	–0.52**
	[0.17]	[0.25]	[0.23]	[0.29]	[0.25]	[0.29]	[0.23]	[0.30]	[0.29]	[0.23]
R2	0.777	0.471	0.591	0.341	0.544	0.149	0.596	0.233	0.317	0.599
Baseline R2	0.349	0.453	0.032	0.074	0.344	0.008	0.271	0.002	0.121	0.537
Adj. R2	0.691	0.268	0.445	0.106	0.381	–0.155	0.452	–0.041	0.073	0.456
F-Test	9.05***	2.32*	4.05**	1.45	3.34**	0.49	4.13**	0.85	1.3	4.18**
Countries	19	19	20	20	20	20	20	20	20	20

Note: Robust standard errors in brackets. Constant included but not reported, ***$p < .01$, **$p < .05$, *$p < .1$

social risks timing models (lower panel) in explaining *ENSS* and, especially, *NSRS* (where the variance explained jumps from around 27% to around 76%). The same applies to the majority of the individual programme models. Our between-effects models necessarily suffer from the well-known problem in cross-sectional regression analysis of relatively few cases compared to the number of theoretically relevant independent variables (Kittel and Obinger, 2003: 41). Nevertheless, they can explain a substantial share of variance in programme size between 1980 and 2003.[17]

The share of service sector workers has a significant negative effect on *ENSS* in both panels and on *NSRS* in the timing model (lower panel), rather than increasing it (as set out by H2). After controlling for welfare regime type or new social risks timing, the old-age dependency ratio no longer has a significant effect on pension spending or *ENSS* (H1). This appears to corroborate Lynch's (2006) claim that demographic variables matter less than institutional ones in accounting for cross-sectional variance in the pro-elderly bias of social policy. Consistent with the descriptive analysis in Figures 1 and 2, we find that, in contrast to Anglo-American countries (the reference category), Continental European regimes spend significantly more on incapacity benefits, and they have significantly lower *NSRS* values (see also Bonoli, 2007). Scandinavian countries spend significantly more on incapacity benefits and on human capital policies such as education and active labour market programmes. Southern European regimes provide in most respects a mirror image to the Scandinavian regimes and an enlarged image to the Continental regimes: they spend less on families and more on survivor pensions, and they record the largest (negative) effect sizes for *NSRS*. Even more than in continental European regimes, it is clearly Southern European social spending which has adapted the least to the new post-industrial society (see also Esping-Andersen, 1999; 2009; Ferrera, 1996; Lynch, 2006). This is not surprising given that these are the countries that were latecomers also with respect to actual new social risks in society (Table 1).

Conclusions

This article has explored how eight individual programmes and the general pro-elderly and pro-new social risk spending biases in mature welfare states respond to long-term demand-side changes 'on the ground' deriving from population ageing and post-industrialization. We hypothesized that higher old-age dependency rates would have a positive effect on the general pro-elderly welfare state bias (H1) within countries, and that service sector jobs, female labour market participation, and female parliamentarian representation would increase the new social risks spending bias (H2, H3, H4). Our main findings support the first but refute the last three hypotheses. Contradicting inferences from studies on gender and social policy (Bolzendahl, 2009; Bolzendahl and Brooks, 2007; Thomas, 1994; Wängnerud, 2009), female parliamentarians are associated with *decreases* in the relative new social risks spending share. A possible explanation for the lack of support of the latter three hypotheses would be that our indicators used for testing them do not adequately capture the demand-side of new social risk-related spending. We do not dismiss this possibility out of hand, but inserting another key new social risk variable (the divorces-to-marriages rate) into our models did not modify our results.

When analyzing eight individual welfare programmes, population ageing tends to increase pension spending and to decrease incapacity benefit spending. Yet levels of new social risks in society again fail to affect relevant programme expenditure in statistically significant terms. Social spending is clearly not mechanically responsive to social needs, not even in the long run and in the most advanced democracies. Causal black boxes need to be opened further, especially at the level of individual programmes across different institutional regime contexts, for instance as regards the macro-micro-macro transitions from risks to preferences to spending patterns (Coleman, 1990; Brooks and Manza, 2006), political power, elite agency, and collective action problems (e.g., Korpi, 2001; Vanhuysse, 2006) and the political reasons for why high institutional friction sometimes lead to radically punctuated equilibria after long periods of near-zero change (e.g., Jensen, 2009).

Lastly, our between-effects models indicated that the particular institutional logic of welfare states still accounts for variance in spending across OECD countries. Continental European and, still more strikingly, Southern European regimes remain the genuine new social risks laggards and the continuing old social risk champions. While we found little support for the thesis that levels of new social risks

affect within-country spending, we did find support for the thesis that the timing of these risks influences differences in spending between countries (H5). Countries which entered the new social risk society comparatively late have a lower new social risks spending bias, as they (could only afford to) spend significantly less on programmes such as education and family allowances. Beyond much-studied factors to do with period effects and institutional design choice, timing thus matters for social policy also in a third sense: the timing of the emergence of demand-side factors significantly influences the budgetary scope available for related expenditure increases.

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Notes

1. To some degree, these two social trends share causal antecedents. For example, rising female labour market participation rates (a defining trait of the service sector economy) were possibly correlated with a higher demand for elderly care services and also with lower levels of fertility until the 1980s, although the direction of the latter correlation was reversed in recent decades (Castles, 2003: 212; Esping-Andersen, 2009).
2. Voters in turn are assumed to be self-interested – an assumption that is present implicitly or explicitly at the heart of most contemporary research on new social risks (e.g., Kitschelt and Rehm, 2006; Armingeon and Bonoli, 2006) and population aging (see Tepe and Vanhuysse, 2009; Busemeyer et al., 2009, and Lynch and Myrskylä, 2009, for empirical tests).
3. See Armingeon and Bonoli (2006), Esping-Andersen (1999, 2009), Huber et al. (2009), Kitschelt and Rehm (2006), Pierson (2001), Taylor-Gooby (2004).
4. To cover a maximum time length, we used OECD public education expenditure data whenever available in the time series by Busemeyer (2007: 605), which was taken from various OECD sources (primarily OECD 1992: 84, and various issues of *Education at a Glance*). When there were missing values (which was only the case for Denmark and Spain in 1980–1985), we imputed those values by inserting the relevant public education expenditure data from Word Bank (2007). To explore robustness, we tested the pairwise correlation between the Busemeyer and World Bank sources whenever both values *were* available. The resulting value (0.86***) enhanced our confidence in the procedure. We are grateful to Marius Busemeyer for providing us with his data. Full dataset and analysis syntax are available for replication purposes.
5. Scruggs's (2005) Comparative Welfare Entitlement Dataset covers just three programmes (unemployment, sickness and pensions). Korpi and Palme's (2008) Social Citizenship Indicator Program covers one further programme (accidents).
6. If k is the size of a certain program measured as a share of GDP, then *all other programmes* is defined as the sum of all eight programs measured as a share of GDP minus k.
7. The reason we chose not to mix up within-unit and between-unit effects is that this would hamper a clear interpretation of our estimated coefficients, as we would be unable to distinguish whether the coefficient is the result of cross-sectional or over-time effects. See also Kittel (2008).
8. See Jackman (1985), Kittel (2008). For example, there are issues of endogeneity when we use macro-economic growth as a determinant of spending (Kittel and Winner, 2005; Kittel, 2008).
9. While standardizing does not as such add any information to data (King, 1986: 671), with respect to our research question, we assume that the benefit of being able to compare the effect sizes of conflicting demand factors on social spending outweighs the potential risk of comparing apples with oranges.
10. We average for theoretical and methodological reasons. Theoretically, we are interested in the effects of long-term macro-societal changes on the size of different welfare programs. These effects, we expect, play out over long periods of time rather than annually. Second, Fisher tests (not shown) of stationarity using the augmented Dickey–Fuller tests on our dependent variables indicate that non-stationarity is a problem in our dataset.
11. The random effect estimator is heavily influenced by cross-sectional variance and depends on the assumption that unobserved heterogeneity is mean-independent from the causal variable (Halaby, 2004: 511). This assumption would be defensible under randomized assignment but not in a sample consisting of 21 OECD countries, where each unit represents a distinct set of social security institutions.
12. See Halaby (2004: 521). Allison (1994: 181) asserts that fixed-effects estimators are nearly always preferable to the random effect estimator with non-experimental data.
13. *Leftwing party power* did not have a significant effect on any of our ten dependent variables. This appears to contradict the main 'power resources' thesis (Huber and Stephens, 2001; Korpi, 2001; Korpi and Palme, 2003) and to corroborate recent findings of waning partisan effects (Kittel and Obinger, 2003). *Strike days* did not significantly affect welfare spending either, except that it strongly increased active labour

market programme spending, perhaps because this programme (unlike unemployment benefits and incapacity benefits) also benefits labour market insiders directly.

14. Results not shown, available on request. For Australia and Ireland, data were missing for two out of three periods. For definitions, see Appendix. Full dataset and analysis syntax are available for replication purposes.
15. We explored the robustness of our findings by excluding these time effects from the fixed-effects models. Whereas the main findings remain robust, in the model without time period effects the share of service sector workers significantly increases *NSRS* (not shown, available upon request).
16. Note that these results are only indicative as the between-effect estimator draws on $N = 20$ in the programme models (Japan was excluded since it does not fit the 'four worlds' typology) and $N = 19$ for the *ENSR* and *NSRR* models (we further excluded the USA, which is an obvious outlier within Anglo-Saxon regimes on both measures; see Figure 1, Appendix; Lynch, 2006).
17. The adjusted *R*-squared values and F-tests indicate that our models are seriously underspecified only in two cases: health spending (which is more technology-cost driven), and unemployment spending (which may be more cyclical). To check whether the conceptual and control variables are essential with respect to model fit, Table 3 also reports 'baseline *R*-squared' values for reduced between-effects models (not presented) that *only* include either *welfare regimes* or *NSR Timing*. Comparing the *R*-squared values of the models presented in Table 3 with these baseline values indicates that *regimes* and *timing* on their own explain a great deal of cross-sectional variance in *ENSS* (respectively 60% and 35%) and *NSRS* (respectively 63% and 45%).

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Appendix 1 Definition and source of variables

Variable	*Definition*	*Source*
Spending shares		
ENSS	See equation (1)	
NSRS	See equation (2)	
Economic controls		
GDP growth	Annual real GDP growth rate	Armingeon et al. (2007)
Deficit	Annual total government deficit as percentage of GDP	Armingeon et al. (2007)
Unemployment rate	Annual unemployment rate	Armingeon et al. (2007)
All other programs	See Note 9	
Population ageing		
ODR	population 65+ as % of population 15–64	Armingeon et al. (2007)
Post-industrialization		
Female employment	Female employment participation rate as % of male employment participation rate	OECD Labor Force Statistic
Service sector employment	Service sector workers as a share of total civilian employment	OECD Labor Force Statistic, Agostino et al. 2006 for the USA
Female parliamentarians	Female parliamentarians as % of all parliamentarians	Paxton, Green and Hughes (2009)
Divorces/marriages ratio	Crude divorce rate (number of divorces per 1000 population) as % of crude marriage rate (number of marriages per 1000 population).	OECD (2009)
Socio-political controls		
Left government	Cabinet composition: social-democratic and other left parties in percentage of total cabinet posts, weighted by days.	Armingeon et al. (2007)
Strike days	Working-days lost due to strikes and lockouts over total employment (log)	Armingeon et al. (2007)
Time effects		
Period 2	Dummy for 1986–1991	
Period 3	Dummy for 1992–1997	
Period 4	Dummy for 1998–2003	
Institutional measures		
Welfare regimes	*Liberal (Anglo-Saxon):* Australia, Canada, Ireland, New Zealand, UK *Continental European:* Austria, Belgium, France, Germany, Netherlands, Switzerland *Nordic*: Finland, Denmark, Norway, Sweden *Southern Europe*: Greece, Italy, Portugal, Spain	
NSR timing	See Table 2	Bonoli (2007: 513), Japan, Ireland and Greece own calculation based on Bonoli (2007: 513) benchmark for Sweden