

**An Empirical Analysis of the Dynamics of the Welfare State:
The Case of Benefit Morale**

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An Empirical Analysis of the Dynamics of the Welfare State: The Case of Benefit Morale*

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Abstract

Does the supply of a welfare state create its own demand? Many economic scholars studying welfare arrangements refer to Say's law and insinuate a self-destructive welfare state. However, little is known about the empirical validity of these assumptions and hypotheses. We study the dynamic effect of different welfare arrangements on benefit fraud. In particular, we analyze the impact of the welfare state on the respective social norm, i. e. benefit morale. It turns out that a high level of public social expenditures and a high unemployment rate are associated with a small positive (or no) immediate impact on benefit morale, which however is crowded out by adverse medium and long run effects.

JEL Classification: A13, I30, I38, J65, J68, H20, Z13.

Keywords: Welfare state, social norms, benefit fraud, benefit morale.

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1 Introduction

Modern welfare states have contributed to the solution of a number of potentially serious social and economical problems.¹ Economic scholars studying the welfare state often discuss disincentive effects of welfare arrangements, and of the taxes to finance them, on economic behavior, which may create new problems (Lindbeck, 1995*a*; Lindbeck, Nyberg and Weibull, 2003; Henrikson and Persson, 2004). The basic dilemma of the welfare state is attributed to the direct effect of more generous benefits on the number of recipients. This effect operates through three different channels: tax distortions, moral hazard and benefit fraud.

Tax wedges do not only distort relative prices in favor of leisure and against work, but also increase the attractiveness of household production and black-market labor. Moral hazard is often a more serious problem. While tax wedges usually result in adjustments on the intensive margin (fewer hours of work), moral hazard often leads to adjustment on the extensive margin at least for a period of time. The complete withdrawal from the labor market can be observed in the case of unemployment insurance, sick-pay insurance or early retirement (Lindbeck, 2008). Higher taxes and more generous benefits also create incentives to invest less in human capital (Boskin, 1974) and to save less for the ‘rainy days’ (Lindbeck, 1995*a*). There is also considerable evidence of explicit benefit fraud. Benefit fraud seem to be particularly pervasive in the case of social benefits, sick benefits, and unemployment benefits.²

Lindbeck (1995*a,b*); Lindbeck, Nyberg and Weibull (2003) point out that it is necessary to discuss the disincentive effect of welfare arrangements in the context of a dynamic interaction between market behavior and political behavior over time. It is hypothesized that individuals do not respond to changes in such economic incentives immediately, since they are constrained by social norms (Elster, 1989) for some time. Therefore, the disincentive effects may materialize only with considerable time lags. In particular, theoretical models predict that the strength of the norm is expected to fall with the share of the population living on benefits (Lindbeck, Nyberg and Weibull, 1999) and parents’ incentives to instill such norms in their children should fall as the generosity of the welfare state increases (Lindbeck and Nyberg, 2006).

The theoretical literature rests upon testable assumptions and offers concrete hypotheses on welfare-state dynamics involving endogenous changes in social norms and political preferences. However, little is known about the empirical validity of these assumptions and hypotheses. The empirical research in this area is still in an early stage. Lindbeck (2003) states that ‘*theory and speculation are far ahead of systematic empirical research in the field of welfare-state dynamics*’. This fact is surprising since there is a widespread concern about abuse and dishonesty in social welfare programs.

¹For instance, Alesina and Rodrik (1994); Persson and Tabellini (1994) provide empirical evidence that income inequality is harmful for growth.

²Greenberg, Moffitt and Friedman (1981); Greenberg and Halsey (1982) study transfer programs (negative-income tax plans) in the United States and find substantial income underreporting for up to 50 percent of certain subgroups of the population. Skogman Thoursie (2004) provides empirical evidence for abuse of the sickness insurance system in Sweden by comparing the change between the number of men and women who report sick during popular sporting events. The difference-in-differences approach shows that the number of men who reported sick increases considerably in order to watch the sporting event on television. Kingston, Burgess and St. Louis (1986) identify frequent overpayments in unemployment insurance systems of five US states, indicating that many claimants falsely certify that they have actively sought a job. Wolf and Greenberg (1986) find fraud rates of 2 to 4 percent in the Aid to Families with Dependent Children and Food Stamps entitlement programs.

Our paper is intended to contribute to the empirical research in the field of welfare-state dynamics. We are interested in the dynamic effect of different welfare arrangements on benefit fraud. In particular, we analyze the impact of the welfare state on the respective social norm, called *benefit morale*. Benefit morale captures the individual reluctance to exploit the welfare state via benefit fraud.

There are few papers on benefit morale, both empirical and theoretical. Algan and Cahuc (2008) study the impact of benefit morale on the design of public insurance policies within the labor markets. They incorporate benefit morale into a model in which unemployment benefits and job protection are shaped by a government that maximizes a social welfare function. This model shows that the government provides low unemployment benefits if benefit morale is low. In this case, the government provides insurance by using a less efficient institution-employment protection. Therefore, low levels of benefit morale impede the implementation of efficient insurance institutions. Accordingly, they provide empirical evidence that countries displaying high benefit morale tend to insure their workers through unemployment benefits instead of using stringent employment protection.

Halla and Schneider (2008) contrast the determinants of benefit morale with the determinants of *tax morale* – the social norm to abstain from cheating on the state via tax evasion – in order to explore differences between benefit fraud and tax evasion. Their empirical analysis shows that both social norms evolve endogenously and are determined by prices (i.e. the cost of acting morally). Citizens who have comparably more opportunities and low cost to commit a certain offense, develop the attitude that it is a minor offense. This suggests that citizens excuse or rationalize their own deviant behavior. Moreover, they analyze the impact of *current* fiscal policy measures on these social norms. Their results show that both social norms deteriorate with higher tax rates, however, they improve with current higher public social expenditures.

Most closely related to our research is Heinemann (2008). His paper is the first attempt to empirically test the hypotheses on the dynamic relation of the welfare state and social norms provided by theory. He explores whether the expansion of the welfare state over time has led to lower levels of benefit morale. His analysis puts forward that an increase in social benefits and in the unemployment rate over the preceding twenty years is associated with substantially lower levels of benefit morale today. Moreover, he claims that later birth cohorts have significantly lower levels of benefit morale whereas age has no effect.

In this paper we use a large micro data set from the *European and World Values Survey*, combined with information from the *Organization for Economic Co-operation and Development* (OECD) to provide a systematic analysis of the dynamic effects of welfare state arrangements on benefit morale. Based on the theoretical literature we derive testable hypotheses on the effect of the generosity of the welfare state and the share of the population living on benefits on benefit morale over time. For the first time we provide empirical evidence that a generous welfare state and a large share of the population living on benefits have very different short, medium, and long run effects. A high level of public social expenditures and a large number of unemployed in the current period have small positive (or no) short run effects on benefit morale, however, these are crowded out by adverse medium and long run effects. This finding is consistent with the hypothesis that disincentive effects of a generous welfare state materialize only with some

time lag.

Further, we demonstrate the absolute importance of controlling for country fixed-effects for this type of analysis. The generosity of the welfare state is correlated with unobserved country-specific time-invariant heterogeneity in a way that disregarding country fixed-effects can diametrically reverse results. In sharp contrast to Heinemann (2008) we can not support the hypothesis of the degeneration of younger cohorts' benefit morale. Once the problem of linear dependency among age, period and cohort is adequately addressed this effect vanishes.

The paper is organized as follows: In Section 2 we derive testable hypotheses on the dynamic effects of the welfare state and benefit morale. Section 3 highlights several methodological problems to overcome in order to test these hypotheses. Our estimation strategy is presented in Section 4. The estimation results are discussed in Section 5, and Section 6 concludes the paper. The Data appendix provides all details on data sources and definitions.

2 Hypotheses on welfare state dynamics and benefit morale

Does the supply of a welfare state create its own demand? Many economic scholars studying welfare arrangements refer to Say's law and insinuate a self-destructive welfare state. The theoretical literature (e. g. Lindbeck, 1995*a,b*, 2003) emphasizes that it is necessary to account for social norms and to discuss the consequences of welfare state arrangements in a dynamic context. This should reflect the interacting adjustment of basic behavior patterns of households, firms, interest-groups, politicians and public-sector administrators over time. The fundamental supposition is that individuals do not respond to changes in economic incentives immediately, since they are constrained by social norms for some time. Individuals are assumed to experience disutility when violating social norms. This can be explained by intrinsic factors, a subjectively felt resistance to violate social norms, or extrinsic factors such as a loss of reputation possibly accompanied by punishment. Put differently, new incentives created by the welfare state are in conflict with existing social norms. However, as time evolves individuals gradually stop obeying initially existing norms and disincentive effects may materialize with considerable time lags.

In this paper we are interested in a particular channel, namely the role of benefit fraud in the hypothesis of hazardous welfare state dynamics. We want to test these dynamic effects of the extent of the welfare state on the social norm with respect to benefit fraud empirically. To measure the individual reluctance to exploit the welfare state via benefit fraud (i. e. benefit morale) we employ large scale international survey data. This data allows us to test whether benefit morale evolves in response to different welfare state arrangements as presumed and predicted by the theoretical literature.

If the fundamental supposition of Lindbeck (1995*a,b*) applies we should observe that generous welfare payments in preceding periods reduce benefit morale today. In order to test this supposition we need to quantify the generosity of the welfare state. The *OECD Social Expenditure Database* summarizes information on aggregated public social expenditure grouped along nine core social policy areas: old-age, survivors, incapacity-related benefits, health, family, active labor market policies, unemployment, housing and other social policy areas. In 2003, public social expenditures accounted – averaged across OECD-member countries in our sample – for 21.54 percent of the gross domestic product. As expected, public social expenditures are highest

among Scandinavian and Benelux countries and are relatively low among Anglo-Saxon countries (see Table 4). This data allows us to measure the level of aggregate public social expenditure on an internationally comparable base and we derive the following hypothesis:

Hypothesis 1: High levels of public social expenditures over time decrease benefit morale.

A central part of our main empirical analysis in Section 5 is devoted to the modeling of the correct dynamic effects of the extent of the welfare state on benefit morale.

Lindbeck, Nyberg and Weibull (1999) constitute a related explanation why disincentive effects of generous welfare arrangements are likely to be stronger in the long than in the short run. They hypothesize that the level of benefit morale tends to fall with the number of individuals living on benefits, in the sense that the guilt or shame connected with breaking the social norms is then reduced. In other words, the authors take the existence of benefit morale as given and endogenize the intensity of this social norm.

In their theoretical model the individual first expresses his political preferences with respect to the government's tax and benefit policies as a voter. Thereby, he anticipates the consequences of the chosen policy for his own subsequent economic choice and for aggregate behavior. Secondly, the individual has the choice of whether to work or to live solely on benefits. Utility is maximized subject to after-tax wage, the level of benefits, and the expected population share of benefit recipients. Under the assumption that individuals do not differ in their sensitivity to the social norm there is a critical wage rate that separates individuals choosing to work and individuals choosing to live on benefits. This results either in a low-tax equilibrium supported by a majority of taxpayers or in a high-tax equilibrium supported by a majority of benefit recipients. Under the alternative assumption of heterogeneous preferences there exists a critical value of benefit morale that separates individuals. Therefore, at some point of time a society may flip from one type of economic-political equilibrium to the other. For instance, a severe negative macroeconomic shock may cause a large share of the population living on benefits and it may be difficult to get a political majority for less generous benefits.

While it is difficult to test all aspects of this model, we focus on the fundamental assumption that a large share of population receiving welfare benefits weakens the social norm to respect welfare arrangements and abstain from exploiting the system. In order to quantify the population share living on benefits, we use the unemployment rate. While the unemployment rate is not the only possible choice, it distinguishes itself from other measures by its wide availability and comparability across countries. Again, we focus on the dynamic effects of unemployment on benefit morale and state:

Hypothesis 2: High levels of unemployment over time decrease benefit morale.

Another hypothesis is that changes in social norms are particularly likely to occur when a new generation enters working life and forms its norms on the basis of a new incentive structure (Lindbeck, 1995a, p. 11). This means, that individuals who are born at different stages of the welfare state exhibit different initial levels of benefit morale. Since the welfare state in all OECD member countries has been expanding on average over the last decades³, one could hypothesize

³In 1980, public social expenditures accounted – averaged across OECD-member countries in our sample – for 17.31 percent of the gross domestic product. In 2003, this value has increased to 21.54 percent (see Table 4).

that younger birth cohorts should have a lower level of benefit morale:

Hypothesis 3: Younger birth cohorts have a lower level of benefit morale.

While the test of this hypothesis is a legitimate stand-alone empirical research question, it is indispensable to control for time-related variation in benefit morale in order to test *Hypotheses 1* and *2*. A sensible test of these hypotheses has to consider the possibility that changes in the levels of public social expenditure and unemployment rates are correlated with time-related variation in benefit morale.

In order to provide empirical evidence for *Hypothesis 3* one has to go one step further and disentangle different types of time-related variation in benefit morale. In particular, the effect of age, period and birth cohort. This is complicated by the identification problem created by the exact linear dependency among age, period and cohort (Mason, Mason, Winsborough and Poole, 1973).

3 Methodological issues

In this section we discuss several methodological challenges to overcome in order to test *Hypotheses 1* to *3*. To highlight the relative importance of different econometric issues we analyze the robustness of earlier findings. Heinemann (2008) tests straightforward variants of *Hypotheses 1* and *2*, where he employs the change in social benefits and the change in the unemployment rate over the preceding twenty years. To test *Hypotheses 3* he includes the respondent's year of birth as an explanatory variable.⁴

In Section 4 we will suggest a refined econometric framework which is used to present our own empirical results based on a new and extended data set in Section 5. Our empirical model allows, among others, public social expenditures and the level of unemployment to have varying effects on benefit morale in the short, medium and long run. This more flexible modeling reveals interesting dynamic effects of welfare arrangements on benefit morale. Moreover, we will show that that if one carefully disentangles the effects of age, period and birth cohorts the result implying a morally corrupt youth disappears.

3.1 Data on benefit morale

To our best knowledge there are two alternative data sources⁵ to study the phenomenon of benefit morale, the *European and World Values Surveys* (E/WVS) and the *International Social Survey Programme* (ISSP).⁶ We have decided to employ data from to E/WVS for two reasons. First, since all previous papers on benefit morale have used data from E/WVS, this provides the

⁴All estimations discussed in Section 3 are based on the data set employed by Heinemann (2008). We would like to thank Prof. Heinemann for providing us with the data.

⁵In fact, the *European Social Survey* asks, 'Suppose you planned to get benefits or services you were not entitled to. How many of your friends or relatives do you think you could ask for support?'. However, this question is not aimed to capture the individual moral motivation to abstain from cheating on the state via benefit fraud, but should rather measure the (perceived) benefit morale among the respondent's circle of friends and acquaintances.

⁶The ISSP questionnaire includes in the year 1991 and 1998 the following question: 'Consider the [following] situation [. . .]. Do you feel it is wrong or not wrong if a person gives the government incorrect information about himself or herself to get government benefits that he or she is not entitled to?'. The scale of answers ranges from 1 'not wrong', to 4 'seriously wrong'. The 2008 questionnaire does not include this question anymore.

comparability of results to some extent. Second, the E/WVS covers substantially more countries and years than the ISSP.

The E/WVS is an ongoing academic project organized as a network of social scientists coordinated by a central body, the *World Values Survey Association*. These surveys provide data from representative national samples (based on face-to-face interviews) of more than 80 countries. It contains information on basic attitudes, beliefs and human values covering religion, morality, politics, work and leisure. To date, four waves have been conducted in 1981 – 1984, 1990 – 1993, 1995 – 1997, and 1999 – 2004. The exact question on benefit morale in the E/WVS questionnaire reads as follows:

‘Please tell me for each of the following statements whether you think it can always be justified, never be justified, or something in between: Claiming governments benefits to which you are not entitled.’

Respondents are asked to evaluate on an ordered scale from ‘*never justifiable*’ (1) to ‘*always justifiable*’ (10). We use this question to construct our measure of benefit morale.

3.2 Measurement of benefit morale

The original question in the E/WVS questionnaire gives a measure of benefit morale on a ten-point scale. Heinemann (2008); Algan and Cahuc (2008) use a re-scaled version of this measure as a dependent variable, and an explanatory variable, respectively. They create a binary measure which takes on the value one if the respondent answers ‘*never justifiable*’ in the E/WVS, and zero otherwise. The authors argue that the re-scaling should ease the interpretation of results. However, this mapping from the ordered scale into a binary variable seems arbitrary. This specific mapping groups together respondents with the highest level of benefit morale and all other respondents. That means, this binary measure does not capture a variation from the lowest level of benefit morale (the answer ‘*always justifiable*’) to considerably higher level of benefit morale such as the answer ‘*nine*’.

We think that the most innocuous procedure is to stick to the original ten-point scale provided by the E/WVS questionnaire but to reverse the scaling such that a higher value of the variable also indicates a higher level of benefit morale.⁷ This measure makes use of the whole variation and has a straight forward interpretation.

In order to explore the sensitivity of the results with respect to mapping from the ordered scale into a binary variable we compare the results for both variables based on the preferred specifications of Heinemann (2008).⁸ If we switch from the binary measure of benefit morale to the ordinal variable no qualitative changes can be observed with respect to *Hypothesis 1* to *3*

⁷The reversed scaling does only change the sign of the coefficient in a regression analysis but leaves the absolute value unchanged.

⁸Heinemann (2008) employs one set of specifications to test his version of *Hypothesis 1* and another set to test his version of *Hypothesis 2*. Both sets are intended to test *Hypothesis 3*. Our specification Ia and Ib in Table 1 correspond with Heinemann’s specification (3) in Table 3a and specification (4) in Table 3b, but we exclude five country-level control variables (ethical fractionalization, latitude, legal origin, tax decentralization and autonomous regions). We disregard these control variables, since they interfere with country-fixed effects to be discussed in Section 3.4. However, the exclusion of these control variables does not change the qualitative results.

(compare columns Ia and IIa, and columns Ib and IIB in Table 1).⁹ However, the control variable income turns out to be a statistically significant determinant of benefit morale. Although there are no further substantial differences between the results obtained from the two different versions, we cannot find any argument in favor of reducing the full ten-point measure of benefit morale which is available in the underlying data.

3.3 Time-related variation in benefit morale

In order to test *Hypothesis 3* thoroughly one has to disentangle the effects of age, period, and birth cohorts. Age effects represent variation associated with different age groups brought about by experience of life (e.g. accumulation of social capital), physiological changes and/or role or status changes. Period effects are defined as variation over time periods that affect all age groups simultaneously. These may result from shifts in political, social or economical environments. Birth cohort effects represent changes across groups of individuals born in the same year (Ryder, 1965). A birth cohort moves through life together and encounters the same historical and social events at the same ages.

Since the sum of a person's age and his birth year is equal to the current year (i. e. the year of the survey), there exists an exact linear relationship between these variables. Put differently, one of the variables is an exact linear combination of the two others, and a general linear model cannot be used to identify all three effects. An extensive literature across disciplines discusses how to identify these three effects with different types of data and in different settings (see, e.g., Mason, Mason, Winsborough and Poole, 1973; Glenn, 1976; Mason, Mason and Winsborough, 1976; Fienberg and Mason, 1979, 1985; Sasaki and Suzuki, 1987; Glenn, 1989; Robertson, Gandini and Boyle, 1999).

The E/WVS collects information on both age and year of birth. The year of the survey should be equal to the sum of the respondent's age and his birth year. Unfortunately, there are many cases of age and/or year of birth misreporting in the data.¹⁰ A border line case is given by observations where the sum of the respondent's age and his birth year is by one lower or higher than the survey year. The first case may occur if the interview took place before the respondent's birthday in the given year.¹¹ The second case may occur if the survey lasted until the beginning of the next year and the interview took place after the respondent's birthday.¹² Since the available survey data do not provide information on the month of birth or on the month of the interview, it is not possible to definitely distinguish between observations that naturally deviate by one and corrupted observations. However, an auxiliary regression analysis provides evidence that that the majority of these deviations is due to the timing of interview and birthday rather than errors in the data.¹³ Throughout all our estimations we kept these observations, however, disregarded

⁹The correlation between the two measurements of benefit morale is about 0.74.

¹⁰This type of misreporting has several potential sources. Mason and Cope (1987) mention (i) ignorance about age, (ii) distortion of age to meet preconceptions about the relationship of age to other characteristics or events, (iii) communication problems between interviewers and respondent, and (iv) errors of recording or processing.

¹¹Note, only for the interviews conducted in the year 1999 the month of the interview is available. In all other cases only information on the month when the field work has started and ended is provided.

¹²In these cases, the survey year is assumed to be the preceding year, since the majority of interviews took place then.

¹³A simple estimation, regressing a binary variable – taking on the value 1 if the deviation from age, period and cohorts is either zero or minus one – on the month when the field work has started/ended (which varies across

all observations with *strictly* impossible combinations of respondents' age and year of birth.

Heinemann (2008) neither addresses the observations with strictly impossible combinations of respondents' age and year of birth, nor the identification problem of age, period and birth cohort effects. His identification of birth cohort effects rests apparently upon variation from (i) strictly impossible combinations of respondents' age and year of birth, (ii) the specific timing of interview and birthday, and (iii) the omission of period effects.

In columns IIIa and IIIb in Table 1 we augment our regressions by period fixed effects and we see that the statistically significant negative impact of the year of birth (i.e. the birth cohort effect) disappears in both specifications. In any case, this result has to be interpreted with caution too, since the identification is solely due to the small deviations resulting from the specific timing of interview and birthday. In Section 4, we will employ a cross-classified fixed-effects model developed for micro-level data in the form of repeated cross-sections (Yang and Land, 2006; Yang, 2008; Yang and Land, 2008) to cleanly identify the effect of birth cohorts.

3.4 Unobserved country time-invariant heterogeneity

In order to test *Hypothesis 1* to *3* it is indispensable to control for unobserved country time-invariant heterogeneity.¹⁴ This especially applies to *Hypothesis 1* and *2* where the variables of main interest are measured on a country-level. Given that one never can be sure that all relevant control variables are included, an estimation without country fixed-effects is less convincing, since unobservable factors may be correlated with the variables of main interest.

The importance of country fixed-effects can easily be demonstrated. If we augment specifications IIIa and IIIb with country fixed effects, neither the coefficient of the measure of the size of the welfare state (see columns IVa in Table 1), nor of the unemployment rate (see column IVb in Table 1) exerts any statistical significance. This suggests that both variables are correlated with unobserved factors.¹⁵ Therefore, we will control for country-fixed in all our specifications below.

3.5 Socio-economic control variables

Heinemann (2008) controls for the following socio-economic characteristics: age, sex, family status, labor market status and household income. We think that this basic set of socio-economic control variables should be augmented by any measure of educational attainment.¹⁶ The level of educational attainment is correlated with the level of public social expenditures, the unemployment rate and the birth cohort. Not controlling for educational attainment could result in omitted variable bias and misleading conclusions with respect to *Hypothesis 1* to *3*. Moreover, we suggest a more detailed specification of labor market status and split up the status non-employed in 'unemployed' and 'out of labor force' and compare them to the base group of employed indi-

countries and years) shows that the earlier fieldwork has started the more likely there will be a deviation from a perfect linear relation between age, period and cohort.

¹⁴In principal, one would prefer to control for unobserved individual time-invariant heterogeneity. However, best to our knowledge there is no panel data on benefit morale available.

¹⁵Heinemann (2008) includes five country-level control variables (ethnic fractionalization, latitude, legal origin, tax decentralization and autonomous regions) which effectively groups together countries and mitigates the omission of country-fixed effects to some degree.

¹⁶To capture the respondents' level of educational attainment we use the school leaving age. For detailed information please refer to the Data appendix.

viduals. Finally, we suggest including two additional control variables; the number of children and the size of the place of residence.

This augmented specification of socio-economic control variables provides additional insights, see specifications Va and Vb in Table 1. The level of education exerts a positive and the size of the place of residence a negative statistically significant effect on benefit morale. Further, it becomes clear that the negative impact of non-employment is mainly due to unemployment, and only to a lesser extent due to being out of the labor force.

3.6 Attitudinal control variables

Finally, there are a number of attitudinal variables for which one could easily put forward a hypothesis on their interrelation to benefit morale. For instance, consider national pride/patriotism.¹⁷ Social psychologists (e. g. Tyler, 2000) reasonably argue that national pride has an important role in shaping deference to authorities. People who feel pride in society and in its authorities are more likely to obey those authorities and to accept their decisions. Accordingly, we expect more patriotic citizens to exhibit a higher level of benefit morale.

Is it therefore advisable to control for individual national pride when we test *Hypotheses 1 to 3*? In our view, it can be misleading to control for attitudinal control variables. For instance, individual patriotism may also be influenced by public social spending. Conditioning on it would tamper with that part of the causal effect of a public social spending on benefit morale that operates through patriotism. If we would add more attitudinal control variables we would eliminate further potential channels through which public social spending affects benefit morale. Moreover, we would not gain any further insights if we find evidence that certain attitudinal control variables are statistically significant determinants of benefit morale. Firstly, these coefficients could only be interpreted as correlations. In each case, one has to suspect a simultaneity bias. For instance, we would not only expect that more patriotic citizens have a higher level of benefit morale, but also vice versa, meaning that citizens with a high benefit morale exhibit higher levels of national pride. Finally, these attitudinal variables are not easily amenable to policy interventions.

In any case, specification *VIa* and *VIb* in Table 1 show that the exclusion of measures of patriotism, religiosity and confidence in parliament does not change the qualitative results. We consequently abstract from attitudinal control variables in the following empirical analysis.

¹⁷Other attitudinal variables are trust in a country's institutions, such as the government, the parliament or the legal system.

4 Estimation strategy

In order to account for the methodological issues discussed above we estimate a cross-classified fixed-effects model (Yang and Land, 2006),

$$\begin{aligned}
 BM_{i,j,c,t} = & \alpha_0 + \sum_{c=2}^{18} country_c + \beta_1 age_{i,j,c,t} + \beta_2 age_{i,j,c,t}^2 + \sum_{t=2}^4 period_t + \sum_{j=2}^{12} cohort_j \\
 & + \sum_{k=0}^2 \zeta_k U_{ct-5k} + \sum_{l=0}^2 \eta_l S_{ct-5l} + \sum_{m=2}^{10} \theta_m income_{i,j,c,t} + \xi \mathbf{X} + \pi \mathbf{Y} \varepsilon_{i,j,c,t},
 \end{aligned} \tag{1}$$

where $BM_{i,j,c,t}$ stands for the benefit morale of individual i , of birth cohort j , from country c in period t . The information on benefit morale is based on wave two to four of the E/WVS.¹⁸ After cleaning the data we have information on more than 30,500 respondents from 18 OECD-member countries for several years between 1990 and 2000 available (see Table 2).¹⁹ An overview of the the average level of benefit morale across countries and years is given by Table 3.

In order to test *Hypothesis 1* we include the current and past levels of public social expenditures measured as a percentage of GDP S_{ct} derived from the *OECD Social Expenditure Database*. To test *Hypothesis 2* we examine the effect of the current and past unemployment rates U_{ct} on benefit morale.²⁰ In order to disentangle the effects of age, period and birth cohort (*Hypothesis 3*) we include *age*, *age*², period fixed effects $period_t$ and a series of binary variables $cohort_j$ capturing 13 different groups of birth cohorts as suggest by (Yang and Land, 2006).

In addition, we include on a country level macroeconomic control variables \mathbf{X} (GDP per capita and GDP-deflator from OECD sources) and on an individual level a set of socio-economic control variables \mathbf{Y} included in the E/WVS, comprising the respondent's sex, marital status, number of children, size of the place of residence, education (captured by the school leaving age), labor market status (employed, self-employed, unemployed, out of labor force) and the household income (measured on a ten-point scale).²¹ With respect to household income we suggest a very flexible specification and include a binary variable for each income category $income_{i,j,c,t}$, where the base group is equal to the group with lowest household income. This specification does not impose any functional form on the effect of household income on benefit morale.²² Finally, we control for country fixed-effects $country_c$ to allow for unobserved country-specific time-invariant heterogeneity. All standard errors are clustered by country and year (Moulton, 1990).

Since benefit morale is measured on a ten-point scale, it is strictly speaking an ordinal measure, which requires an ordered response model (Wooldridge, 2002). Nevertheless, since

¹⁸In particular, we retrieved the data from the *European and World Values Surveys Four-wave Integrated Data File*, 1981 – 2004 and restricted our sample to respondents from OECD-member countries, since comparable information on public social expenditures is not available for non-member countries. We could not exploit data from the first wave of the E/WVS since information on public social expenditures is not available before 1980.

¹⁹Among others we have delete 4,712 observations with *strictly* impossible combinations of respondents' age and year of birth.

²⁰Table 4 and 5 show the development of public social expenditures and unemployment rates for the countries under consideration from 1980 to 2005.

²¹For details please refer to the Data appendix.

²²Note, we have excluded 112 observations from our analysis with values of eleven for the household income, since according to the E/WVS questionnaire this category does not exist. All these observations are from the United States.

the qualitative results turned out to be equivalent and the scale is rather large we will for the ease of presentation focus on least squares throughout the paper.²³

5 Estimation results

Our main estimation results are presented in Table 6. In order to test *Hypothesis 1* and *2* we include the levels of public social spending and the unemployment rate in the current period, with a lag of five years and with a lag of ten years. This specification allows for different short, medium and long run effects of both variables on benefit morale. As column I shows, both a higher level of public social spending and a higher unemployment rate in the current period have a quantitatively small positive effect on benefit morale. An increase in public social spending by one percentage point (sample mean is equal to 20.83 percent) of GDP is associated with an increase in benefit morale by about 0.03 points. An equivalent rise in the unemployment rate (sample mean is equal to 7.52 percent) increases benefit morale by about 0.06 points. These positive effects are contrary to what *Hypothesis 1* and *2* predict. However, our estimates of the lagged values show that after a certain period of time an adverse effect of both variables kicks in. In the case of public social spending we observe no medium run effect, but a quantitatively important negative long run effect. An increase in public social spending by one percentage point today is estimated to decrease benefit morale by about 0.22 points ten years later. In the case of the unemployment rate we observe a detrimental long run effect of benefit morale. According to our estimation an increase in the unemployment rate by one percentage point decreases the level of benefit morale ten years later by about minus 0.09 points.

If we leave out the five year lag of both variables (see column II in Table 6) the negative long run effect of a more pronounced welfare state remains present, though the effect turns out to be a bit smaller in both cases. Notably, due to the exclusion of the five year lag of both variables the positive effect of the current unemployment rate vanishes. The effects of all other (control) variables are robust due to this modification. To test the robustness of our results we have augmented our two specifications by GDP with a lag of five years and with a lag of ten years as additional control variables, see columns III and IV in Table 6. Comparing specification II with specification IV we see that this extension has no impact on the qualitative results, however, it tends to increase the estimates of the negative long run effects. Similarly, we observe an increase in all effects in absolute terms in specification III compared to specification I. Moreover, the adverse effect of public social spending kicks in already in the medium run.

Of course the lag of five and ten years with respect to *Hypothesis 1* and *2* is no natural choice, however, it turned out to be the most robust specification among all.²⁴ Ideally, one would like to estimate the effect of lagged public social spending and unemployment for each year over the preceding decade. This would allow us to trace out the full adjustment path of benefit morale. However, due to the small number of country-years this can not be implemented yet and one has to wait for further waves of the E/WVS to be conducted.

In sum all our different specifications suggest that both an increase in public social expendi-

²³As pointed out by Ai and Norton (2003); Norton, Wang and Ai (2004) the interpretation of nonlinear models is quite cumbersome and not fully demonstrative.

²⁴Notably, the results with respect to *Hypothesis 3* (see below) are completely unaffected by these modifications.

tures and a rise in unemployment have small positive short run (or no) effects on benefit morale. These, however, are (partly) crowded out by adverse medium and long run effects. In the case of public social expenditures the negative medium and long run effects clearly dominate the positive effect in the current period. This result is in line with the idea that individuals have to experience generous welfare arrangements for quite some time until they adapt their social norm towards accepting benefit fraud, or at least considering it to be a minor offence. We therefore, interpret this as evidence in favor of *Hypothesis 1* and *2* and corroborate the theoretical literature which suggests that disincentive effect of welfare arrangements may materialize only with considerable time lags. Our results also affirm the basic empirical finding of Heinemann (2008), in the sense that it boils down to a disincentive effect of different welfare arrangements on benefit morale. However, our analysis highlights that this relationship is characterized by a specific dynamic structure which requires sensible econometric modeling.

Based on our cross-classified fixed-effects model we do not find any evidence for birth cohort effects (*Hypothesis 3*). The four charts in Figure 1 display the estimated coefficients for the birth cohorts effects for the four different specifications summarized in Table 6 and show that for each binary variable capturing a birth cohort group the 95 percent confidence interval includes the value zero. That means, none of the birth cohort group dummies is statistically significant different from zero at conventional levels. This finding is very robust and holds across all specifications. An equivalent analysis was also carried out using several other cohort structures (i. e. other than the five-year groups). In any case, we find no significant effect of birth cohorts on benefit morale.

This result is in sharp contrast to Heinemann (2008) who finds that younger birth cohorts tend to have lower values of benefit morale. Our results show that if one carefully disentangles the effect of age, birth cohort, and period effects the result of the morally corrupt youth disappears. With respect to age we find that benefit morale increases with age. An additional year of age is associated with an increase in benefit morale by about 0.03 points. Encouragingly, we find an upward trend in benefit morale over time (i. e. positive period effects). As the four charts in Figure 2 show, benefit morale has been increasing since the year 1990 (base category).

The results on the remaining individual socio-economic control variables are very robust across different specifications and are in accordance with our expectations. We find a statistically significant effect of household income on benefit morale. Our flexible specification – comprising a binary variable for each income category – reveals that benefit morale is lower among low-income households (see Figure 3). For instance, a household at the bottom of the income distribution has about 0.40 points lower level of benefit morale than a household at the top of the income distribution. This result is in line with Halla and Schneider (2008) who argue that citizens who have comparably more opportunities and low cost to commit a certain offense, develop the attitude that it is a minor offense. Low-income household seem to excuse or rationalize their own deviant behavior. Put differently, they self-servingly adjust their moral values. High-income households have comparably less opportunities to commit benefit fraud and consequently develop and report the attitude that fraudulently collecting benefits is wrong. Independent from income, labor market status is an important determinant of benefit morale. We find that compared to employed individuals, those who are unemployed have a much lower level of benefit morale

(about minus 0.39 points). Being self-employed or out of the labor-force yields (compared to being employed) no statistically significant differences in the level of benefit morale.

Females exhibit a higher level of benefit morale (about plus 0.17 points). This is in line with the criminological literature showing that in general females have lower probabilities and frequencies of committing criminal acts than males (e.g. Smith and Visser, 1980; Steffensmeier, Anderson and Streifel, 1989; Elliot, 1994). The honest behavior of married people (about plus 0.18 points) can either be explained by a true causal effect of marital status or by self-selection into and/or out of marriage. Education has a small but statistically significant positive effect on benefit morale. An additional year of schooling increases the level of benefit morale by about 0.01 points. Finally, the size of the place of residence is decisive. The bigger the place of residence (captured by the number of inhabitants, measured on a three-point scale) the lower the level of benefit (about minus 0.07 points). This can be explained by a different civic attitude in rural areas as compared to large cities and a stronger corporate attitude in rural areas.²⁵

6 Conclusions

Our findings corroborate the theoretical literature which assumes that disincentive effects of a generous welfare state materialize only with some time lag. In particular we show that a high level of public social expenditures and a high unemployment rate are associated with small positive (or no) immediate impact on benefit morale, which is crowded out by adverse medium and long run effects. Therefore, negative macroeconomic shocks not only increase the share of population living on benefits *per se*, but in addition a deterioration of benefit morale in the future has to be expected. These dynamic disincentive effects have not been empirically explored so far. Our results are consistent with the fundamental supposition that individuals do not respond to changes in economic incentives immediately, since they are constrained by social norms for some time. Essentially, our results suggest that the welfare state destroys its own (economic) foundation and we have to approve the hypothesis of the self-destructive welfare state.

As is the case with all empirical research, our analysis presents some caveats due to limited data. Firstly, in order to fully understand the dynamic and complex relationship between the welfare state and benefit morale more observations over time (i.e. a larger number of country-years) are needed. And, secondly there are other measurements and dimensions of the welfare state, which have a potential effect on benefit morale that has to be studied in order to fully understand the whole phenomenon.

Assuming that the modern welfare state – according to Lindbeck (1995a, p.9) ‘*a triumph of western civilization*’ – should be conserved, a clear, though hard to implement, policy implication follows. We need institutional designs which take these dynamic disincentive effects into consideration and aim to mitigate the hazardous feedback of (generous) welfare arrangements on benefit morale. In order to identify efficient institutional arrangements, clearly more empirical research on the determinants of benefit morale is needed. We hope that this paper will stimulate further research in this area. In any case, some disincentive effects can be reduced by an intensified enforcement policy. Referring to the classical economics-of-crime approach (Becker, 1968)

²⁵We also include the number of children as a control variable. However it exerts no statistically significant effect on benefit morale.

an increase in the probability of detection and a rise in the fine rate should lower the incidence of benefit fraud.²⁶

Encouragingly, we can not approve earlier studies putting forward that younger birth cohorts have lower values of benefit morale. We show that if one carefully disentangles the different time-related variation in benefit morale the finding of the morally corrupt youth disappears. Further we have identified a *ceteris paribus* increase in benefit morale in our sample over time. Notably, there are some indirect channels through which the welfare state may even improve benefit morale. For instance, higher public social expenditures may increase the level of educational attainment, which in turn has a positive effect on benefit morale.

²⁶The optimal deterrence policy is discussed by Kolm (1973).

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7 Data appendix

The information on the **individual-level (control) variables** benefit morale, age, sex, marital status (married or not), number of children, education (captured by the school leaving age), size of place of residence (measured on a three-point scale), employment status (employed, self-employed, unemployed and out of labor force) and household income (measured on a ten-point scale) is based on wave two to four of the *European and World Values Survey* (E/WVS). In particular, we retrieved the data from the *European and World Values Surveys Four-wave Integrated Data File*, 1981 – 2004 and restricted our sample first of all to all respondents from OECD-member countries (except South Korea) for which information on these basic individual characteristics was available and consistent. We have decided to exclude South Korea since no information on labor market status was available.

A special comment is in order for the measurement of education. The E/WVS includes two questions on education: ‘What is the highest educational level that you have attained?’ and (ii) ‘At what age did you (or will you) complete your full time education?’. The former question has considerably more missing response (49.87%) compared to the latter one (17.46%). In order to exploit all the available information on education and to save observations we construct a variable capturing the actual or the regular school leaving age. In particular, if information on the second question was available we used it. In the cases where the answer on the second question was missing, but information on the first question was available we have imputed the regular school leaving age of the respective educational level. We distinguished two cases: (i) If there was information on both questions for other respondents from the same country and year available, we have imputed the average school leaving age among those with the same highest educational level attained. (ii) If there were no respondents from the same country and year available with information on both questions we imputed the regular school living age of the respective educational level. Full details are available upon request. Some respondents have reported an unrealistically low or high school leaving age. We have decided to restrict the school leaving age to be within the age of 10 and 28 and disregard 4,263 observations.

Our **country-level (control) variables** are from different sources. As a measure for the generosity of the welfare state we use data on **public social expenditure** measured as a percentage of GDP from the *OECD Social Expenditure Database*. An expenditure item is classified as social if the benefits are intended to address one or more social purposes, and if programmes regulating the provision involve either inter-personal redistribution, or compulsory participation. The OECD groups benefits with a social purpose into nine policy areas: (i) old age (pensions, early retirement pensions, home-help and residential services for the elderly), (ii) survivors (pensions and funeral payments), (iii) incapacity-related benefits (care services, disability benefits, benefits accruing from occupational injury and accident legislation, employee sickness payments), (iv) health (spending on in- and out-patient care, medical goods, prevention), (v) family (child allowances and credits, childcare support, income support during leave, sole parent payments), (vi) active labor market policies (employment services, training, youth measures, subsidized employment, employment measures for the disabled), (vii) unemployment (unemployment compensation, severance payment, early retirement for labor market reasons), (viii) housing (housing allowances and rent subsidies), and (ix) other social policy areas (non-categorical cash benefits to low-income households, other social services). For further details, refer to OECD (2007). Data on the **unemployment rate**, the **GDP per capita**, and the **GDP-deflator** are based on the *OECD Factbook 2007*.

8 Appendix (tables and figures)

Table 1: Determinants of benefit morale: Methodological challenges to overcome in order to test hypotheses 1 to 3.^a

	(Ia)	(IIa)	(IIIa)	(IVa)	(Va)	(VIa)	(Ib)	(IIb)	(IIIb)	(IVb)	(Vb)	(VIb)
<i>HYPOTHESIS 1</i>												
Δ Social security benefits ^b	-0.010* (0.005)	-0.051* (0.026)	-0.072** (0.034)	0.028 (0.028)	-0.003 (0.014)	-0.004 (0.013)						
<i>HYPOTHESIS 2</i>												
Δ Unemployment ^c							-0.011** (0.004)	-0.050** (0.019)	-0.053** (0.019)	0.011 (0.028)	-0.012 (0.012)	-0.010 (0.012)
<i>HYPOTHESIS 3</i>												
Year of birth	-0.008*** (0.002)	-0.027** (0.011)	0.000 (0.081)	-0.082 (0.067)	0.011 (0.037)	0.013 (0.038)	-0.006*** (0.002)	-0.016** (0.007)	0.023 (0.064)	-0.067 (0.066)	0.006 (0.037)	0.008 (0.038)
<i>SOCIO-ECONOMIC CONTROL VARIABLES</i>												
Age	-0.003 (0.002)	-0.009 (0.010)	0.019 (0.081)	-0.065 (0.066)	0.028 (0.037)	0.031 (0.037)	-0.001 (0.002)	0.002 (0.007)	0.040 (0.063)	-0.051 (0.066)	0.023 (0.037)	0.026 (0.038)
Female	0.036*** (0.006)	0.140*** (0.026)	0.139*** (0.027)	0.126*** (0.022)	0.128*** (0.019)	0.138*** (0.019)	0.037*** (0.006)	0.141*** (0.027)	0.140*** (0.028)	0.131*** (0.023)	0.133*** (0.019)	0.146*** (0.019)
Married	0.050*** (0.006)	0.176*** (0.033)	0.186*** (0.030)	0.188*** (0.026)	0.186*** (0.024)	0.200*** (0.025)	0.051*** (0.006)	0.182*** (0.027)	0.186*** (0.026)	0.191*** (0.021)	0.182*** (0.022)	0.195*** (0.022)
Income	0.004 (0.003)	0.030** (0.013)	0.025* (0.012)	0.026** (0.010)	0.035*** (0.008)	0.035*** (0.008)	0.003 (0.003)	0.026** (0.011)	0.024** (0.012)	0.028*** (0.008)	0.038*** (0.006)	0.038*** (0.006)
School leaving age					0.005 (0.003)	0.004 (0.003)					0.006* (0.003)	0.005 (0.003)
Employed	0.035*** (0.010)	0.173*** (0.047)	0.169*** (0.048)	0.110*** (0.038)			0.027*** (0.008)	0.134*** (0.044)	0.129*** (0.044)	0.092** (0.036)		
Unemployed ^d					-0.314*** (0.084)	-0.331*** (0.084)					-0.295*** (0.079)	-0.316*** (0.079)
Self-employed ^d					-0.014 (0.047)	-0.022 (0.048)					-0.022 (0.038)	-0.028 (0.038)
Out of labor force ^d					-0.058** (0.027)	-0.053* (0.028)					-0.053* (0.026)	-0.048* (0.028)
Size of town					-0.084** (0.035)	-0.092** (0.037)					-0.071** (0.029)	-0.079** (0.030)
Number of children					0.002 (0.008)	0.004 (0.008)					-0.005 (0.009)	-0.003 (0.009)
<i>ATTITUDINAL CONTROL VARIABLES</i>												
Religious	0.003 (0.003)	0.019 (0.016)	0.023 (0.016)	0.025*** (0.006)	0.017** (0.006)		0.003 (0.004)	0.019 (0.019)	0.022 (0.019)	0.028*** (0.007)	0.019*** (0.006)	
Confidence in parliament	0.009 (0.007)	0.108*** (0.033)	0.107*** (0.026)	0.068*** (0.018)	0.056** (0.020)		0.007 (0.005)	0.092*** (0.024)	0.095*** (0.022)	0.057*** (0.017)	0.046** (0.017)	
Patriotism	0.049*** (0.008)	0.145*** (0.036)	0.127*** (0.032)	0.132*** (0.029)	0.117*** (0.031)		0.045*** (0.007)	0.136*** (0.038)	0.127*** (0.033)	0.136*** (0.032)	0.120*** (0.034)	
<i>Time fixed-effects</i>	No	No	Yes	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes
<i>Country fixed-effects^e</i>	No	No	No	Yes	Yes	Yes	No	No	No	Yes	Yes	Yes
Constant	17.055*** (4.679)	62.799*** (21.034)	8.496 (160.033)	171.315 (132.091)	-13.730 (73.990)	-16.792 (74.419)	12.938*** (3.307)	39.858*** (13.978)	-36.531 (126.382)	141.671 (130.985)	-3.231 (73.593)	-7.683 (74.783)
Observations	53303	53303	53303	53303	44517	44517	62862	62862	62862	62862	50400	50400
R-squared	0.063	0.050	0.058	0.099	0.103	0.100	0.062	0.049	0.053	0.101	0.101	0.098

^a The data used in this table was generously made available by Prof. Heinemann. All data sources are found in Heinemann (2008) and/or in the Data appendix in this paper. The dependent variable is in each case a measure of benefit morale from the E/WVS. Originally, benefit morale was asked on a ten-point scale based on the following question: 'Please tell me for each of the following statements whether you think it always be justified (10), never be justified (1), or something in between; Claiming governments benefits to which you are not entitled.'. The dependent variable used in columns Ia, Ib, IIa, and IIb is a binary measure which takes on the value one if the respondent answers 'never justified' in the E/WVS, and zero otherwise. The dependent variable used in all other columns is equal to 1 for the lowest level of benefit morale ('always justified') and equal to 10 for the highest level of benefit morale ('never justified'). In each case a higher value indicates a higher level of average benefit morale. Estimated using ordinary least squares. *, **, and *** indicate statistical significance at the 10-percent level, 5-percent level, and 1-percent level, respectively. ^b 20 Year growth rate of social security benefits as percentage of GDP. ^c 20 Year growth rate of unemployment rate. ^d Base group is employed. ^e Time fixed-effects are included as year dummies between 1981 and 2000 with the year 1981 as the base.

Table 2: Number of available observations per country and year.^a

	1990	1995	1996	1997	1998	1999	2000	Totals
Australia	0	1,504	0	0	0	0	0	1,504
Belgium	0	0	0	0	0	1,328	0	1,328
Canada	1,397	0	0	0	0	0	1,265	2,662
Denmark	766	0	0	0	0	796	0	1,562
Finland	0	0	0	0	0	0	642	642
France	0	0	0	0	0	1,126	0	1,126
Germany ^b	3,056	0	0	1,490	0	1,366	0	5,912
Greece	0	0	0	0	0	650	0	650
Ireland	864	0	0	0	0	722	0	1,586
Italy	0	0	0	0	0	1,180	0	1,180
Japan	639	788	0	0	0	0	986	2,413
Luxembourg	0	0	0	0	0	488	0	488
Netherlands	667	0	0	0	0	802	0	1,469
New Zealand	0	0	0	0	924	0	0	924
Spain	0	747	0	0	0	638	683	2,068
Sweden	0	0	757	0	0	450	0	1,207
Great Britain	0	0	0	0	0	560	0	560
United States	1490	1030	0	0	0	781	0	3,301
	8,879	4,069	757	1,490	924	10,887	3,576	30,582

^aThe estimations in Table 6 are based on these observations. ^bValues for Germany are a combination of Western and Eastern Germany.

Table 3: Benefit morale across countries for available years.^a

	1990	1995	1996	1997	1998	1999	2000
Australia		9.41					9.41
Belgium						8.60	8.60
Canada	9.17						9.16
Denmark	9.55					9.69	9.62
Finland							8.73
France						7.64	7.64
Germany ^b	9.09			8.84		8.91	8.99
Greece						7.22	7.22
Ireland	9.08					9.21	9.14
Italy						9.20	9.20
Japan	9.17	8.95					8.94
Luxembourg						8.24	8.24
Netherlands	9.47					9.53	9.51
New Zealand					9.29		9.29
Spain		9.10				8.47	8.94
Sweden			8.93			9.07	8.98
Great Britain						8.98	8.98
United States	9.09	9.43				8.91	9.15
	9.18	9.27	8.93	8.84	9.29	8.75	8.98

^a Benefit morale is measured on a ten-point scale based on the following question from the E/WVS: ‘Please tell me for each of the following statements whether you think it always be justified (10), never be justified (1), or something in between: Claiming governments benefits to which you are not entitled.’. Our re-scaled measure of benefit morale is equal to 1 for the lowest level of benefit morale (‘always justifiable’) and equal to 10 for the highest level of benefit morale (‘never justifiable’), i. e. higher values indicate a higher level of average benefit morale. The average values are based on the set of observations per country and year displayed in Table 2. ^b Values for Germany are a combination of Western and Eastern Germany.

Table 4: Public social expenditures across countries and years.^a

	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	
Australia	10.95	10.85	11.89	12.45	12.82	13.02	12.91	13.03	12.31	12.48	14.06	15.22	16.24	16.47	16.22	17.13	17.16	16.96	16.97	16.92	17.87	17.41	17.47	17.90	14.86
Belgium	23.50	25.08	25.68	25.97	25.18	26.12	25.95	25.71	25.53	24.67	24.97	25.75	25.90	27.05	26.46	26.35	26.87	25.81	26.06	25.93	25.30	25.72	26.13	26.48	25.76
Canada	14.13	14.58	17.04	17.29	17.01	17.27	17.20	16.90	16.65	17.04	18.43	20.56	21.30	21.23	20.18	19.20	18.36	17.72	18.01	17.05	16.73	17.27	17.31	17.27	17.74
Denmark	25.18	25.38	25.53	25.99	24.91	24.18	23.39	24.11	25.40	25.69	25.47	26.30	26.83	28.63	29.44	28.87	28.17	27.16	27.00	26.78	25.75	26.38	26.90	27.58	26.29
Finland	18.37	18.73	19.82	20.78	21.71	22.79	23.38	23.88	23.13	22.93	24.52	29.62	33.57	29.87	29.16	27.36	27.10	25.17	23.24	22.79	21.32	21.44	21.87	22.45	23.96
France	20.82	21.80	22.28	22.36	22.61	25.77	25.75	25.86	25.73	24.86	25.26	25.96	26.64	28.15	28.13	28.35	28.62	28.48	28.69	28.82	27.55	27.46	27.95	28.72	26.11
Germany ^b	22.99	23.73	23.81	23.38	23.06	23.63	23.58	23.94	23.96	22.98	22.48	23.70	25.68	26.09	26.09	26.60	27.10	26.45	26.31	26.35	26.25	26.32	26.99	27.25	24.95
Greece	11.48	13.82	16.23	16.86	17.23	17.89	17.74	17.70	16.42	17.51	18.61	17.96	18.12	19.07	19.05	19.30	19.96	20.01	20.62	21.38	21.30	22.29	21.33	21.30	18.47
Ireland	16.76	16.88	17.53	17.65	17.07	21.81	21.97	21.17	19.79	18.19	15.51	16.35	17.06	17.08	16.79	16.32	15.43	14.34	13.38	14.18	13.64	14.43	15.49	15.93	16.86
Italy	17.98	19.37	19.85	20.91	20.47	20.81	20.81	21.03	21.14	21.32	19.93	20.13	20.72	20.94	20.71	19.79	21.99	22.70	22.96	23.26	23.16	23.30	23.83	24.19	21.30
Japan	10.32	10.75	11.18	11.47	11.29	11.15	11.55	11.58	11.20	10.96	11.25	11.37	11.87	12.51	13.11	13.89	14.10	14.20	14.90	15.44	16.11	16.85	17.48	17.73	13.01
Luxembourg	23.62	25.34	24.55	24.74	23.32	23.14	22.30	23.38	22.45	21.72	21.94	22.29	22.70	23.07	22.86	23.77	23.75	22.49	21.56	21.74	20.43	19.77	21.58	22.25	22.70
Netherlands	24.15	24.97	26.35	26.62	25.44	24.22	23.63	23.55	23.31	22.95	24.35	24.41	24.94	25.09	23.63	22.79	21.76	21.19	20.62	19.91	19.33	19.47	19.92	20.67	23.05
New Zealand	17.10	17.29	18.22	18.07	17.40	17.97	17.79	18.60	20.05	21.38	21.78	22.16	22.03	20.28	19.49	18.95	18.89	19.86	20.02	19.34	19.11	18.40	18.43	18.01	19.19
Spain	15.55	16.69	16.74	17.44	17.21	17.78	17.50	17.41	17.90	18.09	19.98	20.71	21.83	23.15	22.10	21.48	21.42	20.83	20.73	20.36	20.36	20.17	20.24	20.31	19.41
Sweden	28.59	29.56	29.75	30.04	28.75	29.71	29.74	29.85	30.36	29.66	30.53	32.13	35.04	36.17	34.86	32.54	32.06	30.68	30.50	30.06	28.76	29.27	30.45	31.28	30.85
Great Britain	16.58	18.01	18.48	19.36	19.35	19.56	19.67	18.94	17.66	17.11	17.16	18.59	20.33	20.97	20.51	20.36	20.09	19.16	19.27	19.01	19.14	20.12	20.14	20.64	19.18
United States	13.28	13.56	13.94	14.13	13.15	12.91	13.08	13.03	13.02	13.00	13.39	14.38	15.08	15.31	15.30	15.35	15.20	14.87	14.81	14.59	14.59	15.15	16.01	16.20	14.30
	17.31	17.69	18.28	18.65	18.29	18.27	18.03	18.06	18.13	17.97	18.20	19.45	20.58	21.01	20.72	20.39	20.43	20.02	20.05	20.25	20.09	20.45	21.07	21.54	19.50

^a Total public social expenditures as percentage of GDP. Source: *OECD Social Expenditure Database* (data extracted on November, 29, 2007). For details please consult the Data appendix. ^b Values for Germany are a combination of Western and Eastern Germany.

Table 5: Unemployment rate across countries and years.^a

	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	
Australia	6.1	5.8	7.2	10.0	9.0	8.3	7.9	7.9	7.0	6.0	6.7	9.3	10.5	10.6	9.5	8.2	8.2	8.3	7.7	6.9	6.3	6.8	6.4	6.1	5.5	5.1	7.6
Belgium			9.8	10.7	10.8	10.1	10	9.8	8.8	7.4	6.6	6.4	7.1	8.6	9.8	9.7	9.5	9.2	9.3	8.5	6.9	6.6	7.5	8.2	8.4	8.4	8.7
Canada	7.5	7.6	11	12.0	11.3	10.6	9.7	8.8	7.8	7.5	8.1	10.3	11.2	11.4	10.4	9.5	9.6	9.1	8.3	7.6	6.8	7.2	7.7	7.6	7.2	6.8	8.9
Denmark			7.8	8.4	7.9	6.6	5.0	5.0	5.7	6.8	7.2	7.9	8.6	9.6	7.7	6.8	6.3	5.2	4.9	5.1	4.3	4.5	4.6	5.4	5.5	4.8	6.3
Finland					5.9	6	6.7	4.9	4.1	3.1	3.2	6.6	11.7	16.4	16.9	15.2	14.6	12.7	11.3	10.2	9.7	9.1	9.1	9	8.9	8.4	9.3
France			7.4	7.7	9.2	9.7	9.8	9.9	9.4	8.9	8.5	9	9.9	11.1	11.7	11.1	11.6	11.5	11.1	10.5	9.1	8.4	8.9	9.5	9.6	9.9	9.7
Germany ^b	2.6	4.0	5.7	6.9	7.1	7.2	6.5	6.3	6.2	5.6	4.8	4.2	6.4	7.7	8.3	8	8.6	9.2	8.8	7.9	7.2	7.4	8.2	9.1	9.5	9.4	7.0
Greece			7	7.3	7	6.6	6.6	6.6	6.9	6.7	6.3	6.9	7.8	8.6	8.9	9.1	9.7	9.6	11.1	12	11.3	10.8	10.3	9.7	10.5	9.8	8.7
Ireland			11.4	13.9	15.5	16.8	16.8	16.6	16.2	14.7	13.4	14.7	15.4	15.6	14.3	12.3	11.7	9.9	7.5	5.7	4.3	4	4.5	4.7	4.5	4.4	11.2
Italy			6.5	7.4	7.9	8.1	8.9	9.6	9.7	9.7	8.9	8.5	8.8	9.8	10.6	11.2	11.2	11.2	11.3	11	10.1	9.1	8.6	8.4	8	7.7	9.3
Japan	2.0	2.2	2.4	2.7	2.7	2.6	2.8	2.8	2.5	2.3	2.1	2.1	2.2	2.5	2.9	3.1	3.4	3.4	4.1	4.7	4.7	5	5.4	5.3	4.7	4.4	3.3
Luxembourg			3.0	3.4	3.0	2.9	2.5	2.5	2	1.8	1.6	1.6	2.1	2.6	3.2	2.9	2.9	2.7	2.7	2.4	2.3	2.1	2.8	3.7	5.1	4.5	2.7
Netherlands	4.3	5.8	7.7	9.2	8.9	7.9	7.8	7.7	7.2	6.6	5.9	5.5	5.3	6.2	6.8	6.6	6	4.9	3.8	3.2	2.8	2.2	2.8	3.7	4.6	4.7	5.7
New Zealand	1.9	3.1	3.1	4.9	4.9	3.6	4.1	4.1	5.6	7.1	7.8	10.3	10.4	9.5	8.1	6.3	6.1	6.6	7.4	6.8	6	5.3	5.2	4.6	3.9	3.7	5.8
Spain			13	14.4	16.7	17.8	17.5	16.8	15.8	13.9	13.0	13.0	14.7	18.3	19.5	18.4	17.8	16.7	15	12.5	11.1	10.4	11.1	11.1	10.6	9.2	14.5
Sweden	2.1	2.6	3.3	3.7	3.3	2.9	2.7	2.2	1.8	1.5	1.7	3.1	5.6	9	9.4	8.8	9.6	9.9	8.2	6.7	5.6	4.9	4.9	5.6	6.4	5.0	
Great Britain			10.1	10.8	10.9	11.2	11.2	10.3	8.5	7.1	6.9	8.6	9.7	10.2	9.3	8.5	7.9	6.8	6.1	5.9	5.4	5	5.1	4.9	4.7	4.8	7.9
United States	7.1	7.6	9.7	9.6	7.5	7.2	7.0	6.2	5.5	5.3	5.6	6.8	7.5	6.9	6.1	5.6	5.4	4.9	4.5	4.2	4	4.7	5.8	6	5.5	5.1	6.2
	4.2	4.7	7.7	8.7	8.6	8.5	8.3	7.9	7.4	6.9	6.7	7.1	8.3	9.3	9.4	8.9	8.6	8.2	7.6	7.4	7.0	7.0	7.3	7.6	7.6	7.5	7.8

^a Total unemployment rate. Source: *OECD Factbook 2007* (data extracted on November, 29, 2007). ^b Values for Germany are a combination of Western and Eastern Germany.

Table 6: Determinants of benefit morale: Testing hypotheses 1 to 3.^a

	I	II	III	IV
<i>Hypothesis 1</i>				
Public social expend. _t	0.030*** (0.005)	0.031*** (0.010)	0.043*** (0.002)	0.035*** (0.011)
Public social expend. _{t-5}	0.021 (0.013)		-0.061*** (0.015)	
Public social expend. _{t-10}	-0.221*** (0.027)	-0.120*** (0.022)	-0.365*** (0.009)	-0.178*** (0.043)
<i>Hypothesis 2</i>				
Unemployment rate _t	0.063*** (0.020)	0.010 (0.014)	0.258*** (0.019)	0.068* (0.035)
Unemployment rate _{t-5}	0.095*** (0.016)		0.134*** (0.007)	
Unemployment rate _{t-10}	-0.089*** (0.018)	-0.061*** (0.014)	-0.256*** (0.012)	-0.130*** (0.042)
<i>Hypothesis 3</i>				
Birth cohort effects ^b	yes	yes	yes	yes
<i>Country-level control variables</i>				
GDP _t p.c. (in \$100,000)	-0.000 (0.002)	-0.006*** (0.002)	0.030*** (0.003)	0.003 (0.006)
GDP _{t-5} p.c. (in \$100,000)			-0.020*** (0.005)	
GDP _{t-10} p.c. (in \$100,000)			-0.027*** (0.001)	-0.016* (0.008)
GDP deflator	0.310*** (0.031)	0.185*** (0.027)	0.305*** (0.024)	0.199*** (0.026)
Period fixed-effects ^c	yes	yes	yes	yes
Country fixed-effects	yes	yes	yes	yes
<i>Individual-level control variables</i>				
Age	0.032** (0.012)	0.032** (0.013)	0.032** (0.013)	0.032** (0.013)
Age ²	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
Female	0.171*** (0.023)	0.170*** (0.024)	0.171*** (0.023)	0.170*** (0.024)
Married	0.181*** (0.026)	0.181*** (0.026)	0.183*** (0.026)	0.182*** (0.026)
No. of children	0.002 (0.012)	0.002 (0.012)	0.002 (0.012)	0.002 (0.012)
School leaving age	0.013** (0.006)	0.012** (0.006)	0.013** (0.006)	0.013** (0.006)
Town size	-0.074** (0.030)	-0.074** (0.030)	-0.076** (0.030)	-0.074** (0.030)
Self-employed ^e	-0.032 (0.052)	-0.036 (0.052)	-0.033 (0.052)	-0.036 (0.052)
Out of labor force ^e	-0.017 (0.043)	-0.015 (0.043)	-0.018 (0.043)	-0.014 (0.043)
Unemployed ^e	-0.390*** (0.067)	-0.388*** (0.067)	-0.393*** (0.067)	-0.388*** (0.067)
Income ^d	yes	yes	yes	yes
Constant	8.043*** (0.583)	9.437*** (0.530)	9.698*** (0.734)	10.048*** (0.571)
Observations	30,582	30,582	30,582	30,582
R-squared	0.096	0.095	0.096	0.096

^a The dependent variable is equal to benefit morale measured on a ten-point scale, where higher values indicate a higher level of benefit morale (see Table 3). Estimated using ordinary least squares. Robust standard errors (allowing for clustering by country-years) in parentheses below. *, ** and *** indicate statistical significance at the 10-percent level, 5-percent level, and 1-percent level, respectively. ^b Coefficients are displayed in Figure 1. ^c Coefficients are displayed in Figure 2. ^d Coefficients are displayed in Figure 3. ^e The base group is equal to employed individuals.

Figure 1: Cohort effects on benefit morale

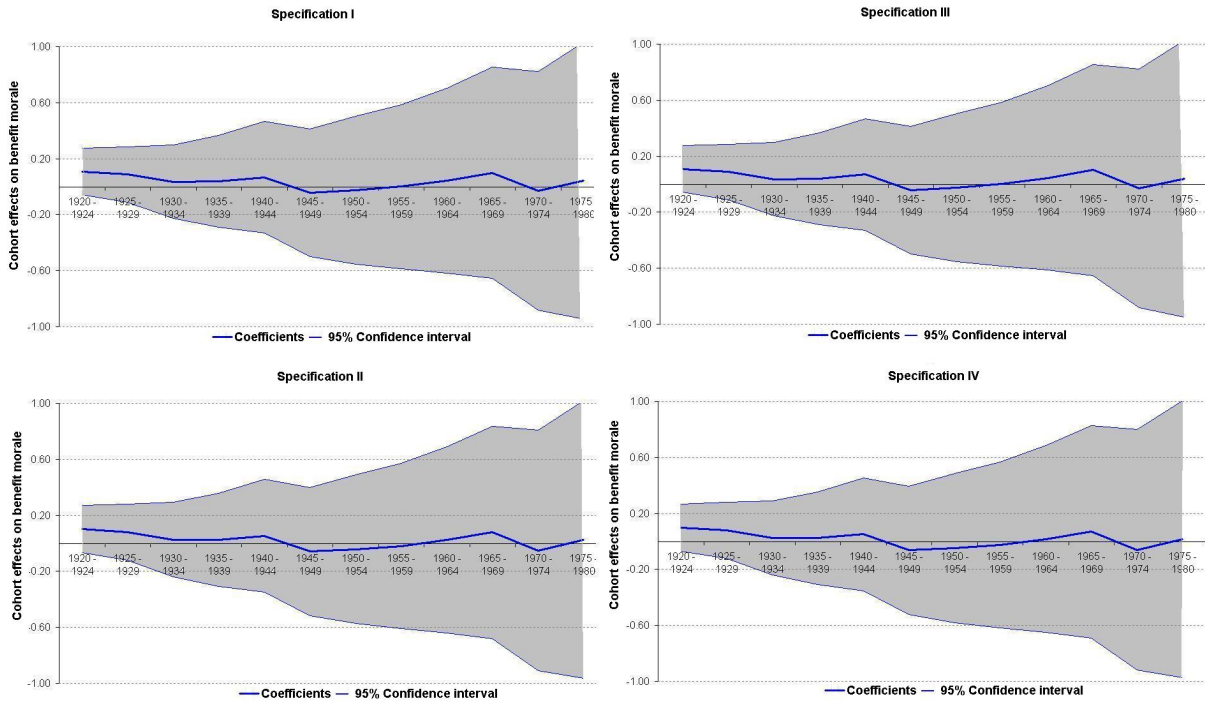


Figure 2: Period effects on benefit morale

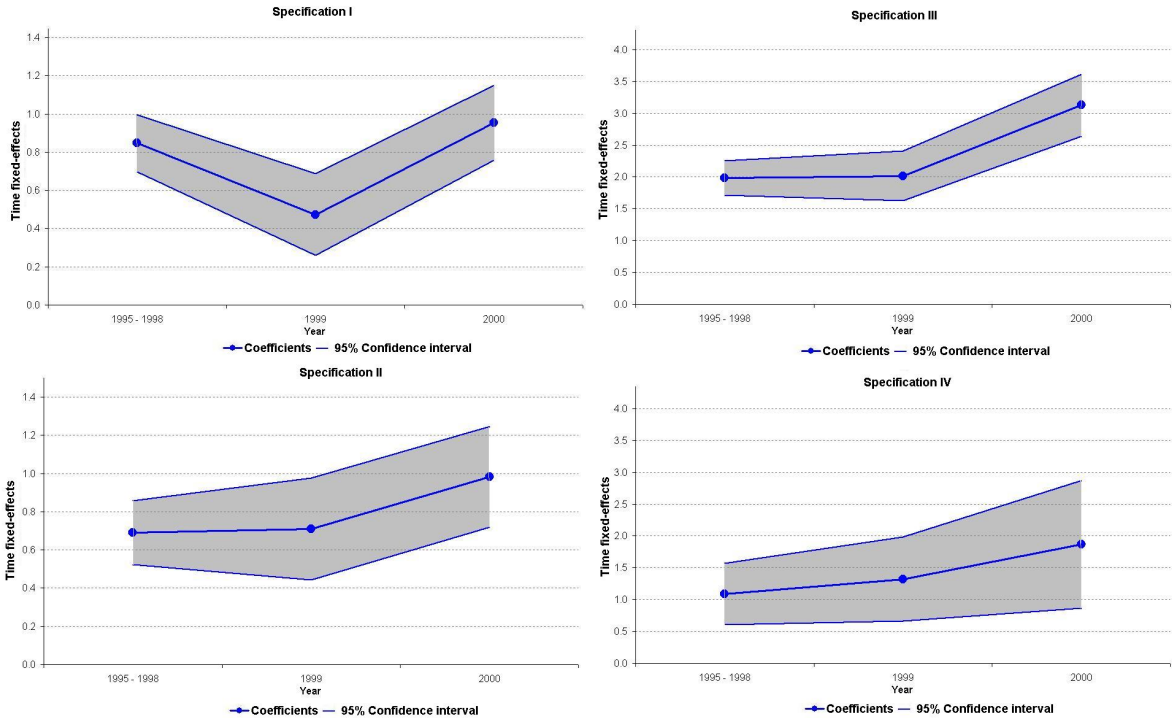


Figure 3: Income effects on benefit morale

