

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

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1 INTRODUCTION

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, 1995a,b; Gouyette and Pestieau, 1999; Ravallion, 2001; Lindbeck, Nyberg and Weibull, 2003; Henrekson and Persson, 2004; Beaulier and Caplan, 2007). 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.¹ The literature points out that

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¹Benefit fraud seem to be particularly pervasive in the case of social benefits, sick benefits, and unemployment benefits. Greenberg, Moffitt and Friedman (1981); Greenberg and Halsey (1983) study transfer programs (negative-income tax plans) in the U.S. and find substantial income underreporting for up to 50 percent of certain subgroups of the population. Skogman Thoursie (2004) provides empirical

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.

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 at 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. Heinemann (2008) (henceforth FH) is the first attempt to empirically test the hypotheses on the dynamic relation of the welfare state and social norms provided by theory.² His analysis of *benefit morale* (i. e. the individual reluctance to exploit the welfare state via benefit fraud) puts forward that an increase in social benefits and in the unemployment rate over the preceding twenty years is associated with substantial decrease in benefit morale. Moreover, he claims that later birth cohorts have significantly lower levels of benefit morale whereas age has no effect.

In this paper we try to extend and deepen the analysis of FH. We start with his three basic hypotheses and highlight several methodological problems to overcome in order to test these hypotheses. For instance, we demonstrate the great 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 a second step we will suggest a refined econometric framework which is used to present our own empirical results based on an extended data set. Our empirical model allows, among others, public social expenditures and the level of unemployment to

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 U.S. 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.

²Algan and Cahuc (2008) study the impact of benefit morale on the design of public insurance policies within labor markets. They provide empirical evidence that countries displaying high benefit morale tend to insure their workers through unemployment benefits instead of using stringent employment protection.

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. 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 (partly) 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, in sharp contrast to FH 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 briefly summarize the hypotheses on the dynamic effects of the welfare state put forward by FH. Section 3 discusses several methodological problems to overcome in order to test these hypotheses. Our refined estimation strategy is presented in Section 4, and Section 5 discusses our new results. Section 6 concludes the paper.

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, 1995a,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 adjustments in basic behavior patterns of households, firms, interest-groups, politicians and publicsector 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 some time lags.

FH considers a particular channel, namely the role of benefit fraud in the hypothesis of hazardous welfare state dynamics. In particular, he empirically analyzes the effects of the extent of the welfare state on the social norm with respect to benefit fraud. To measure the individual

reluctance to exploit the welfare state via benefit fraud (i. e. benefit morale) he employs large scale international survey data from the *European and World Values Survey*.

Hypothesis 1 In order to test the fundamental supposition of Lindbeck (1995a,b) – suggesting that generous welfare payments in preceding periods reduce benefit morale today – FH first employs the change in social spending over the preceding twenty years derived from the *OECD Economic Outlook Database*. Firstly, we suggest to quantify the generosity of the welfare state by public social expenditures measured as a percentage of GDP derived from the *OECD Social Expenditure Database* (OECD, 2007). This database summarizes information on aggregated public social expenditure grouped along nine core social policy areas and allows us to measure the level of aggregate public social expenditure on an internationally comparable base. Secondly, and more importantly, we propose a more flexible empirical model to capture the dynamics suggested by the theoretical literature. In order to allow for varying effects of a generous welfare state on benefit morale in the short, medium and long run we consider the effect of public social expenditure in the current period, with a lag of five years and with a lag of ten years. This specification should allow us to test whether disincentive effects indeed materialize only with some time lag.

Hypothesis 2 In a second step FH analyzes the change in the unemployment rate over the preceding twenty years. This hypothesis can be derived from Lindbeck, Nyberg and Weibull (1999) who constitute an additional 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. Though the unemployment rate is not the only possible choice to quantify the population share living on benefits, we follow FH since the unemployment rate distinguishes itself from other measures by its wide availability and comparability across countries. Again, we will focus on the dynamic effects of unemployment.

Hypothesis 3 Finally, FH tests whether 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 decades, he hypothesizes that younger birth cohorts should have a lower level of benefit morale. To test this hypothesis empirically, he simply includes both the year of birth and the age of the respondent as explanatory variables. As we will show, in order to test *Hypothesis 3* accurately one has to disentangle different types of time-related variation in benefit morale: the effects of age, period, and birth cohort. After a thorough discussion of the identification problem created by the exact linear dependency among age,

period and cohort (Fienberg and Mason, 1985) we will use a cross-classified fixed-effects model (Yang and Land, 2006) to cleanly identify the effect of birth cohorts.

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 the findings of FH. In Section 4 we will suggest a refined econometric framework which is used to present our new set of results in Section 5.

3.1 Measurement of benefit morale

To our best knowledge there are two alternative data sources to study the phenomenon of benefit morale, the *European and World Values Surveys* (E/WVS) and the *International Social Survey Programme*. In order to guarantee comparability with previous papers we employ data from the E/WVS throughout the paper. 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. 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.

The original question in the E/WVS questionnaire gives a measure of benefit morale on a ten-point scale. FH and Algan and Cahuc (2008) use a re-scaled version of this measure as a dependent variable, and as an explanatory variable, respectively in their main analysis. 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.³ 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 FH.⁴ 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. However, the control variable income turns out to be a statistically significant determinant of benefit morale (detailed output available upon request). 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.

3.2 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 year of birth 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., Glenn, 1976; Mason, Mason and Winsbor-

³The reversed scaling does only change the sign of the coefficient in a regression analysis but leaves the absolute value unchanged.

⁴The correlation between the two measurements of benefit morale is about 0.74.

ough, 1976; Fienberg and Mason, 1979, 1985; 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 year of birth. Unfortunately, there are many cases of age and/or year of birth misreporting in the data.⁵ A border line case is given by observations where the sum of the respondent's age and his year of birth 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. 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.⁶ 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 the majority of these deviations is due to the timing of interview and the birthday rather than errors in the data.⁷ Therefore, we kept these observations throughout all our estimations, however, disregarded all observations with *strictly* impossible combinations of respondents' age and year of birth.

FH 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.⁸

In columns (Ia) and (Ib) in Table 1 we augment FH's two preferred specifications 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

⁵This 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.

⁶In these cases, the survey year is assumed to be the preceding year, since the majority of interviews took place then.

⁷A 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 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. Note, 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.

⁸It should be noted that FH controls in some specifications for period fixed-effects, however, not in the specification on which his results on birth cohort effects are based.

Table 1

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

	(Ia)	(IIa)	(IIIa)	(Ib)	(IIb)	(IIIb)
HYPOTHESIS 1						
Δ Social security benefits ^b	- 0.072** (0.034)	0.028 (0.028)	- 0.003 (0.014)			
HYPOTHESIS 2						
Δ Unemployment ^c				- 0.053** (0.019)	0.011 (0.028)	- 0.012 (0.012)
HYPOTHESIS 3						
Year of birth	0.0003 (0.081)	- 0.082 (0.067)	0.011 (0.037)	0.023 (0.064)	- 0.067 (0.066)	0.006 (0.037)
SOCIO-ECONOMIC CONTROL VARIABLES						
Age	0.019 (0.081)	- 0.065 (0.066)	0.028 (0.037)	0.040 (0.063)	- 0.051 (0.066)	0.023 (0.037)
Female	0.139*** (0.027)	0.126*** (0.022)	0.128*** (0.019)	0.140*** (0.028)	0.131*** (0.023)	0.133*** (0.019)
Married	0.186*** (0.030)	0.188*** (0.026)	0.186*** (0.024)	0.186*** (0.026)	0.191*** (0.021)	0.182*** (0.022)
Income	0.025* (0.012)	0.026** (0.010)	0.035*** (0.008)	0.024** (0.012)	0.028*** (0.008)	0.038*** (0.006)
School leaving age			0.005 (0.003)			0.006* (0.003)
Employed	0.169*** (0.048)	0.110*** (0.038)		0.129*** (0.044)	0.092** (0.036)	
Unemployed ^d			- 0.314*** (0.084)			- 0.295*** (0.079)
Self-employed ^d			- 0.014 (0.047)			- 0.022 (0.038)
Out of labor force ^d			- 0.058** (0.027)			- 0.053* (0.026)
Size of town			- 0.084** (0.035)			- 0.071** (0.029)
Number of children			0.002 (0.008)			- 0.005 (0.009)
ATTITUDINAL CONTROL VARIABLES						
Religious	0.023 (0.016)	0.025*** (0.006)	0.017** (0.006)	0.022 (0.019)	0.028*** (0.007)	0.019*** (0.006)
Confidence in parliament	0.107*** (0.026)	0.068*** (0.018)	0.056** (0.020)	0.095*** (0.022)	0.057*** (0.017)	0.046** (0.017)
Patriotism	0.127*** (0.032)	0.132*** (0.029)	0.117*** (0.031)	0.127*** (0.033)	0.136*** (0.032)	0.120*** (0.034)
Period fixed-effects ^e	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed-effects	No	Yes	Yes	No	Yes	Yes
Constant	8.496 (160.033)	171.315 (132.091)	- 13.730 (73.990)	- 36.531 (126.382)	141.671 (130.985)	- 3.231 (73.593)
No. of observations	53,303	53,303	44,517	62,862	62,862	50,400
R-squared	0.058	0.099	0.103	0.053	0.101	0.101

^aAll data sources can be found in FH and/or in the Data appendix in Halla, Lackner and Schneider (2009). The dependent variable is equal to benefit morale measured on a ten-point scale, where higher values indicate a higher level of benefit morale. Estimated using ordinary least squares.

***, ** and * indicate statistical significance at the 10-percent level, 5-percent level, and 1-percent level, respectively.

^b20 Year growth rate of social security benefits as percentage of GDP.

^c20 Year growth rate of unemployment rate.

^dBase group is employed.

^ePeriod fixed-effects are included as year dummies between 1981 and 2000 with the year 1981 as the base.

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) to cleanly identify the effect of birth cohorts.

3.3 *Unobserved country time-invariant heterogeneity*

In order to test *Hypothesis 1* to *3* it is indispensable to control for unobserved country time-invariant heterogeneity.⁹ 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 (Ia) and (Ib) with country fixed-effects, neither the coefficient of the measure of the size of the welfare state (see column (IIa) in Table 1), nor of the unemployment rate (see column (IIb) in Table 1) exerts any statistical significance. This suggests that both variables are correlated with unobserved factors.¹⁰ Therefore, we will control for country fixed-effects in all our specifications below.

3.4 *Socio-economic control variables*

FH 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.¹¹ 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

⁹In principal, one would prefer to control for unobserved individual time-invariant heterogeneity. However, to our best knowledge there is no panel data on benefit morale available.

¹⁰FH includes five country-level control variables (ethical fractionalization, latitude, legal origin, tax decentralization and autonomous regions) which effectively group together countries and mitigates the omission of country fixed-effects to some degree.

¹¹To capture the respondents’ level of educational attainment we use the school leaving age. For detailed information please refer to the Data Appendix in Halla, Lackner and Schneider (2009).

force' and compare them to the base group of employed individuals. 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 (IIIa) and (IIIb) 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.

3.5 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, FH controls for measures of religiosity, patriotism and confidence in parliament. Social psychologists (e. g. Tyler, 2000) reasonably argue that patriotism, for example, plays 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 benefit morale.

Is it therefore advisable to control for individual national pride when testing *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 (which are typically highly correlated with benefit morale) 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.

We find that the exclusion of measures of religiosity, patriotism and confidence in parliament does not change the qualitative results (detailed output available upon request) and abstract from attitudinal control variables in the following empirical analysis.

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),

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

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. After cleaning the data we have information on more than 30,500 respondents from 18 OECD-member countries for several years between 1990 and 2000.¹²

In order to test *Hypothesis 1* we include public social expenditures measured as a percentage of GDP S_{ct-5l} in the current period ($l=0$), with a lag of five years ($l=1$) and with a lag of ten years ($l=2$). To test *Hypothesis 2* we equivalently examine the effect of past and current unemployment rates U_{ct-5l} . In order to disentangle the effects of age, period and birth cohort (*Hypothesis 3*) we include age , age^2 , a series of binary variables $cohort_j$ capturing 13 different groups of birth cohorts as suggest by (Yang and Land, 2006), and period fixed-effects $period_t$.

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 (married or not), number of children, size of place of residence (measured on a three-point scale), 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).¹³ With respect to household income we

¹²Among others we have delete 4,712 observations with strictly impossible combinations of respondents' age and year of birth. The number of available observations per country and year, and the respective average levels of benefit morale can be found in Table 2 and Table 3 in Halla, Lackner and Schneider (2009).

¹³Some 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. For details please refer to the Data Appendix in Halla, Lackner and Schneider (2009).

suggest a very flexible specification and include a binary variable $income_{m,i,j,c,t}$ for each income category m , 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. Finally, we control for country fixed-effects $country_c$ to allow for unobserved country-specific

Table 2
Determinants of benefit morale: testing hypotheses 1 to 3.^a

	(I)	(II)	(III)	(IV)
HYPOTHESIS 1				
Public social expend. _{<i>t</i>}	0.0297*** (0.0055)	0.0313*** (0.0099)	0.0429*** (0.0025)	0.0354*** (0.0112)
Public social expend. _{<i>t-5</i>}	0.0205 (0.0125)		- 0.0613*** (0.0146)	
Public social expend. _{<i>t-10</i>}	- 0.2212*** (0.0267)	- 0.1204*** (0.0223)	- 0.3646*** (0.0089)	- 0.1775*** (0.0427)
HYPOTHESIS 2				
Unemployment rate _{<i>t</i>}	0.0625*** (0.0201)	0.0098 (0.0142)	0.2581*** (0.0194)	0.0680* (0.0353)
Unemployment rate _{<i>t-5</i>}	0.0945*** (0.0156)		0.1338*** (0.0070)	
Unemployment rate _{<i>t-10</i>}	- 0.0887*** (0.0178)	- 0.0615*** (0.0136)	- 0.2557*** (0.0119)	- 0.1298*** (0.0416)
HYPOTHESIS 3				
Birth cohort effects ^b	yes	yes	yes	yes
COUNTRY-LEVEL CONTROL VARIABLES				
GDP _{<i>t</i>} p.c. (in \$100,000)	- 0.0003 (0.0017)	- 0.0057*** (0.0017)	0.0303*** (0.0035)	0.0034 (0.0055)
GDP _{<i>t-5</i>} p.c. (in \$100,000)			- 0.0201*** (0.0050)	
GDP _{<i>t-10</i>} p.c. (in \$100,000)			- 0.0266*** (0.0013)	- 0.0159* (0.0083)
GDP deflator	0.3104*** (0.0308)	0.1848*** (0.0267)	0.3045*** (0.0235)	0.1992*** (0.0258)
Period fixed-effects ^c	yes	yes	yes	yes
Country fixed-effects	yes	yes	yes	yes
INDIVIDUAL-LEVEL CONTROL VARIABLES				
Age	0.0324** (0.0125)	0.0323** (0.0125)	0.0322** (0.0125)	0.0319** (0.0125)
Age ²	- 0.0002 (0.0001)	- 0.0002 (0.0001)	- 0.0002 (0.0001)	- 0.0002 (0.0001)
Female	0.1706*** (0.0235)	0.1696*** (0.0235)	0.1707*** (0.0235)	0.1696*** (0.0235)
Married	0.1813*** (0.0261)	0.1813*** (0.0260)	0.1829*** (0.0261)	0.1816*** (0.0260)
No. of children	0.0021 (0.0117)	0.0022 (0.0117)	0.0021 (0.0117)	0.0024 (0.0117)
School leaving age	0.0128** (0.0059)	0.0122** (0.0058)	0.0131** (0.0059)	0.0127** (0.0058)
Size of town	- 0.0743** (0.0303)	- 0.0739** (0.0302)	- 0.0760** (0.0305)	- 0.0742** (0.0303)

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Table 2. (Contd)

	(I)	(II)	(III)	(IV)
Self-employed ^d	- 0.0318 (0.0518)	- 0.0358 (0.0518)	- 0.0325 (0.0519)	- 0.0359 (0.0518)
Out of labor force ^d	- 0.0167 (0.0429)	- 0.0145 (0.0427)	- 0.0182 (0.0427)	- 0.0142 (0.0426)
Unemployed ^d	- 0.3897*** (0.0671)	- 0.3884*** (0.0672)	- 0.3927*** (0.0673)	- 0.3882*** (0.0674)
<i>Income</i> ^c	<i>yes</i>	<i>yes</i>	<i>yes</i>	<i>yes</i>
Constant	8.0434*** (0.5835)	9.4366*** (0.5299)	9.6978*** (0.7340)	10.0476*** (0.5713)
R-squared	0.096	0.095	0.096	0.096

^aThe dependent variable is equal to benefit morale measured on a ten-point scale, where higher values indicate a higher level of benefit morale. Estimated using ordinary least squares. The number of observations is in each estimation equal to 30, 582. 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.

^bCoefficients are displayed in Figure 1.

^cThe base group is equal to 1990.

^dThe base group is equal to employed individuals.

^eCoefficients are displayed in Figure 2.

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. Nevertheless, since the qualitative results of ordered probit estimations 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.¹⁴

5 ESTIMATION RESULTS

Our main estimation results are presented in Table 2. In order to test *Hypotheses 1* and *2* we include the levels of public social spending and the unemployment rate with a lag of ten years, with a lag of five years and in the current period. 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)

¹⁴As pointed out by Ai and Norton (2003) the interpretation of nonlinear models is quite cumbersome and not fully demonstrative.

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 *Hypotheses 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. Likewise, we observe a detrimental long run effect of high unemployment on 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 2) 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). 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 *Hypotheses 1* and *2* are no natural choice, however, it turned out to be the most robust specification among all.¹⁵ 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 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.

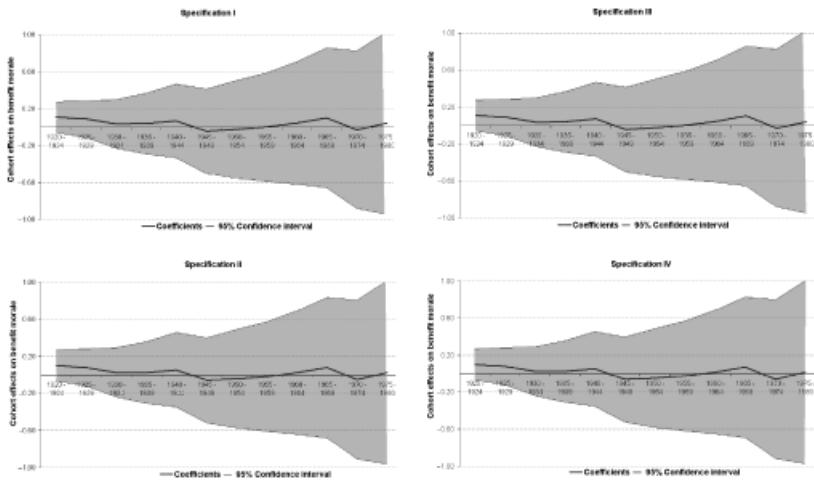
In sum, all our different specifications suggest that both an increase in public social expenditures and a rise in unemployment have small positive short run (or no) effects on benefit morale. These, however, are (partly) crowded out by adverse medium and long run effects. In the case of public social expenditures the negative medium and long run effects clearly dominate the positive effect in

¹⁵Notably, the results with respect to *Hypothesis 3* (see below) are completely unaffected by these modifications.

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Figure 1

Cohort effects on benefit morale

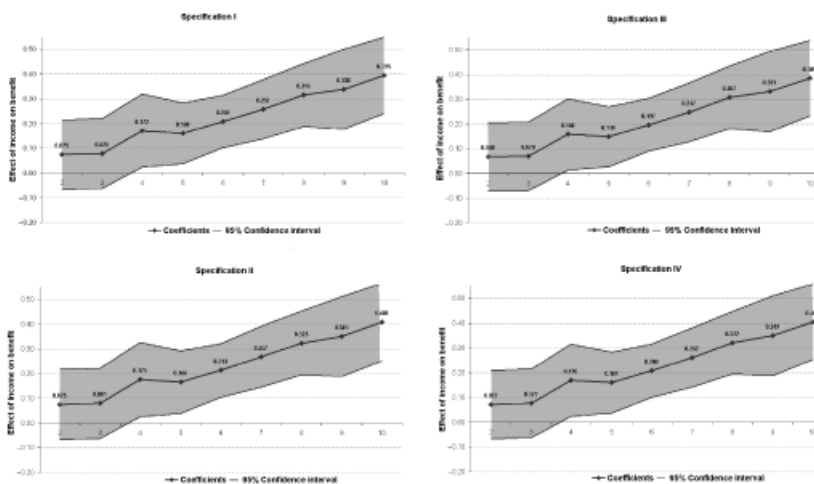


the current period. This result is in line with the idea that individuals have to experience generous welfare arrangements for quite some time until they adapt their social norm towards accepting benefit fraud, or at least considering it to be a minor offence. We therefore, interpret this as evidence in favor of *Hypothesis 1* and to a lesser extent of *Hypothesis 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 FH, 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 2. They show that for each binary variable capturing a birth cohort group, the 95 percent confidence interval includes the value zero. This means, that 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). We also substituted *age* and *age*² with a series of binary variables capturing different age groups. In any case, we find no significant effect of birth cohorts on benefit morale.

Figure 2

Income effects on benefit morale



This result is in sharp contrast to FH 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). Benefit morale has been increasing since the base year 1990.

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 2). 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 households 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 Visher, 1980). 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 morale (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.

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 (partly) 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. Therefore, our results suggest that the welfare state is at risk to destroy its own (economic) foundation and support 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 an increase in the probability of detection and a rise in the fine rate should lower the incidence of benefit fraud. Importantly, we do not agree with 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.

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SUMMARY

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 (partly) crowded out by adverse medium and long run effects. In contrast to earlier studies we do not find that younger birth cohorts have lower values of benefit morale.