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Bismarck in the Bedroom? Pension Reform and Fertility: Evidence 1870-2010

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Philipp Jäger[1]

Bismarck in the Bedroom? Pension Reform and Fertility: Evidence 1870-2010

Abstract

Rising public pension generosity has frequently been cited as one reason for the (persistently) declining fertility rates in many advanced economies. Despite the theoretical appeal, empirical evidence on the pension-fertility nexus is limited. To fill this gap, I study country-level fertility trends before and after 23 pension reforms using a long-run panel dataset starting in 1870. In addition to the raw fertility rate (birth per women aged 15-49), I examine the residuals of a fertility regression, which capture variations in the fertility rate that cannot be explained by alternative theories of the historical fertility decline. Contrasting pre- and post-reform trends of the raw fertility rate as well as the fertility regression residual across countries, I do not find robust evidence that pension reforms, on average, affect fertility in the way most theoretical models predict. On the individual country level, however, some reforms are indeed associated with a significant structural break in fertility trends that is in line with the old-age security hypothesis. Varying social ties might provide an explanation for the different country-specific fertility reactions to pension reforms.

JEL Classification: H55, J13

Keywords: Old-age security; fertility; pension reform

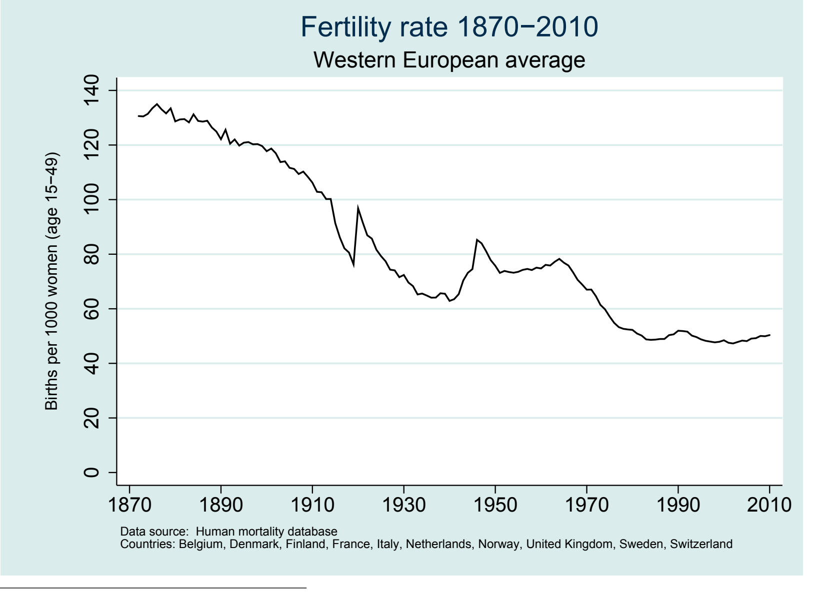
February 2017

1 Philipp Jäger, RWI. *– I thank Roland Döhrn, Gunther Bensch, Philipp Breidenbach, Torsten Schmidt and Sylvi Rzepka for their valuable comments and suggestions. All remaining errors are mine.* *– All correspondence to: Philipp Jäger, RWI, Hohenzollernstr. 1-3, 45128 Essen, Germany, e-mail: philipp.jaeger@rwi-essen.de*

1 Introduction

Fertility was high for centuries, before starting to decline in most advanced economies during the 19th century. Fertility rates have persistently fallen since then, only briefly interrupted by the post-World War II baby boom, as Figure 1 illustrates. Despite its significance, the underlying causes of the historical fertility transition remain controversial. While demographers tend to emphasize the role of cultural change and birth control techniques, economists typically stress the changing costs and benefits of children[1]. Identifying the actual drivers of the constant drop in fertility rates, however, is crucial given that it provides important policy implications for today's high as well as below replacement fertility countries. In this respect, the role of public old-age insurance is of special importance because pension systems are particularly affected by the demographic transition but, at the same time, might also drive fertility trends. Thus, in this paper, I empirically investigate whether the introduction and expansion of public pensions schemes have reduced fertility rates, given that these systems might crowd-out children as a form of old-age insurance. In the same vein, I examine whether a cutback in pension generosity boosts fertility.

Figure 1: **Evolution of the fertility rate**



[1] See Guinnane (2011) for an excellent discussion of the determinants of the fertility transition.

Among economists, rising public pension generosity has been frequently cited as one important reason for the persistent decline in fertility rates (Boldrin et al., 2015; Cigno and Rosati, 1996; Ehrlich and Kim, 2007; Fenge and Scheubel, 2017). According to this line of research, children naturally support their parents once they become needy in old age, and hence serve as old-age insurance (old-age security hypothesis). The introduction or expansion of the public pension system, however, potentially counteract these arrangements and, therefore, affect the cost-benefit calculation of having children. Given that public pension systems provide an additional (state-guaranteed) source of old-age income, children become a less attractive asset in the old-age insurance portfolio. Thus, the old-age security hypothesis implies that fertility is negatively correlated with the existence (or expansion) of a public old-age security system.

Despite the appeal of this line of argument, empirical evidence on the pension-fertility nexus is limited. More importantly, most empirical studies (e.g. Billari and Galasso, 2014; Cigno and Rosati, 1996, 1992; Ehrlich and Kim, 2007; Gábos et al., 2009; Hohm, 1975) investigate the relationship between the generosity of the pension system and the fertility rate after World War II only, typically uncovering a negative correlation. Given that public pension systems were introduced much earlier in most of today's high-income countries, these studies neglect the fertility response just after introduction of public pensions. Scheubel (2013) as well as Fenge and Scheubel (2017) are notable exceptions in that they specifically focus on the introduction period. These two studies, however, draw on the experience of only one specific case –the Bismarckian scheme in Imperial Germany– finding a negative fertility effect of its introduction. I will broaden the focus on a wider range of pension reforms to test whether this result may be generalized.

To my knowledge, this study is the first to investigate fertility effects of major pension reforms using a long-run cross-country panel dataset. I focus on reforms rather than gradual changes in pension generosity in order to reduce the potential for spurious correlation. In particular, I study country-level fertility trends before and after 23 pension reforms including the introduction, expansion, and cutback of the public old-age security insurance system. Drawing on the research design of Samwick (2000)[2], I estimate a fertility regression and use the regression residuals as a measure for variations in fertility that cannot be explained by standard fertility theories. The fertility regression is based on the specification proposed by Murtin (2012) and estimated for 21 countries for 1870 to 2010. Contrasting pre- and post-reform trends of the raw fertility rate as well as the fertility regression residual, I do not find robust evidence that pension reforms, on average, affect fertility in the way most theoretical models predict. On the individual country level, however, some reforms are indeed associated with a significant change in fertility trends that is in line with the old-age security hypothesis. These differential effects cannot be explained by the type of pension system, geographical factors, the timing, nor the intensity of the reform. Varying social ties, in contrast, might provide an explanation.

In the next section, I briefly elaborate on the theoretical foundations of the pension-fertility nexus. Section 3 introduces the data as well as the empirical approach, while section 4 presents the results. Finally, section 5 concludes.

[2]Samwick (2000) investigates the effect of pension reforms on savings rates instead of fertility rates.

2 The pension-fertility nexus

Institutions affect individual behavior. The organization of the public pension system has been shown to influence saving rates as well as labor supply (e.g. Bloom et al., 2007; Mastrobuoni, 2009). Hence, it is plausible that the generosity of the public pension systems also affects couples' fertility decision-making. The theoretical relationship between the organization of old-age insurance systems and fertility rates has been investigated in several studies using overlapping generation (OLG) models (Boldrin et al., 2015; Cigno and Rosati, 1996; Ehrlich and Kim, 2007; Sinn, 2004). In general, these models predict a negative relationship between an increase in public pension generosity[3] and fertility rates, given that these schemes replace the need for children as a vehicle for old-age insurance.

The specific design of the pension system is likely to play a role for the strength of the pension-fertility nexus, though. In general, pension systems can be broadly categorized as either funded or pay-as-you-go (PAYG), even though many hybrid forms exist. While the current working-age population pays for current pensioners in PAYG regimes, an actual capital stock is accumulated in funded schemes from which future pensions are paid. Most OLG models center on PAYG pension schemes, since they are prevalent today and the fertility effect is likely to be more pronounced. Pensions can also reduce fertility in fully-funded pension systems if capital markets are imperfect (Fenge and Scheubel, 2014). Under the assumption of perfect capital markets, however, the introduction of a fully funded system might simply crowd out private savings and hence leave fertility unaffected. Fenge and Scheubel (2017) show that from a theoretical point of view the public pension effect is also ambiguous in PAYG systems. On the one hand, public pension systems reduce lifetime incomes under the assumption that the internal rate of return in the PAYG system falls short of capital market returns. Hence, assuming that children are normal goods, the demand for them should decline with rising replacement and contribution rates. On the other hand, increasing contribution rates reduces net wages and hence lower the opportunity costs of children, resulting in a higher demand for children. Therefore, the overall effect is ambiguous and depends on the size of the income vis-à-vis the substitution effect.

The theoretical analysis is complicated by the fact that pension systems also differ in various other respects, for instance in the way pension benefits are linked to the contributions paid. While pension contributions as well as benefits are both related to earnings in schemes that follow the Bismarckian tradition, Beveridgean systems typically provide (sometimes means-tested) flat-rate minimum pensions which are mostly independent of the actual contributions paid. Given that these minimum pensions are often financed via (progressive) taxes, contributions and benefits can even be inversely related in Beveridgean schemes. Thus, the pension systems' rate of return, as well as the level of income replacement during retirement, differs substantially among income groups in Beveridgean schemes, while they are relatively homogeneous in Bismarkian ones. Given that these parameters are crucial from a theoretical point of view, fertility might react differently to reforms of Bismarckian style systems compared to Beveridgean ones.

[3]Throughout this paper, generous refers to a high level of income replacement. Thus, all systems that guarantee a large proportion of previous labor earnings during old-age pension are characterized as generous irrespective of their internal rate of return.

The observation that parent-child ties varied considerably among countries further challenges the premise that the pension-fertility relationship is universal. In rural England, for instance, children were not expected to support their parents, at least from the early modern period, starting in the 16th century, onward (Guinnane, 2011). Therefore, old-age security may not have been an equally important motivation for childbearing before public pension systems were introduced. Given that the theoretical predictions are in fact mixed, I analyze the effect of pension reforms on fertility rates empirically. Based on theoretical considerations, the fertility effect of pension reforms is likely to be country-specific and might depend on the cultural background or the specifics of the pension system.

3 Empirical strategy and data

3.1 The empirical strategy

Since fertility rates have been declining over time, I compare fertility trends rather than levels. In the benchmark specification, I contrast the 10-year period before a reform was implemented with the 10-year period after the introduction. The length of the pre- and post-reform period is a compromise: while it is is difficult to identify trends for shorter time periods, trends calculated for longer time spans are increasingly polluted by other influences or may not be available for a wide range of countries. As a robustness check, I analyze whether results differ if trends are based on 20 pre- and post-reform years.

Throughout the analysis I focus on raw fertility rates, operationalized as births per 1000 women aged between 15 and 49, given that total fertility rates are not consistently available over such a long time span. Besides raw fertility rates, I also investigate the residuals of the following regression inspired by Murtin (2012):

$$log(raw\,fertility\,rate)_{it} = a_i + b_t + \beta\,(\mathbf{X})_{it} + \varepsilon_{it} \tag{1}$$

where i and t denote the country and time dimension. In line with Murtin (2012), I regress fertility rates on country (a_i) and time-fixed (b_t) effects as well as on the following explanatory variables $\mathbf{X}$ that capture some of the main explanations for the historical fertility decline: education, log(infant mortality), log(overall mortality), log(GDP per working-age (20-59) adult) as well as log(GDP per working-age adult) squared.

As argued in Murtin (2012), the extension of the average period that individuals spend in schools has decreased fertility, as it changed the cost-benefit calculation of children. In the same vein, falling mortality reduced birth rates since fewer births are needed to ensure a fixed number of surviving children with a sufficient probability. Moreover, fertility is likely to be affected by the increase in average incomes. In line with Murtin (2012), I use GDP per working-age adult instead of per capita GDP in order to reduce reverse causality issues caused by the direct link between fertility and population size.

Given the rich demographic information in my dataset, I employ raw fertility rates (births per 1000 women aged 15-49) instead of crude birth rates (births per 1000 inhabitants), which have been used by Murtin (2012), as dependent variable[4].

Furthermore, I augment Murtins original specification by employing a war dummy. Wars can influence fertility rates through several channels since they impact contemporaneous economic as well as demographic conditions and also induce uncertainty. In this vein, Vandenbroucke (2014) argues that World War I reduced French fertility rates substantially, because the probability of a husband's death and hence a severe income shock increased considerably during this period. Moreover, given that men are vastly overrepresented in the military, wars also reduce the availability of males and therefore, the potential for procreation. Based on these considerations, I include a war dummy which equals one if annual battle deaths exceed 0.1 percent of the male population in the age group 18 to 49. As a robustness check, I also control for the fertility-war nexus using the average annual battle death toll as a percentage of men aged 18 to 49 as a continuous measure of war exposure. In an alternative specification, I exclude war periods.

The regression residual ε_{it} constitutes the part of the fertility rate that is not explained by some of the standard theories of the historical fertility decline. Following the strategy of Samwick (2000), who studies the effect of pension reforms on savings rates, I use this residual and contrast its pre-reform trend with the post-reform evolution. In order to check the robustness, I augment the model using a range of additional variables that might be correlated with pension reforms including democracy, female labor supply, religion, a migration proxy, public debt, the real interest rate and financial crises. An in depth discussion of the robustness checks is provided in chapter 4.3.

The analysis is executed in two steps. First, I analyze the cross-sectional averages of the raw fertility rate as well as the fertility residuals for each type of pension reform (introduction, expansion, cutback). Secondly, I investigate the pension reform effect for every country separately given that pension systems have been quite diverse, especially at the introduction period. While most Anglo-Saxon and Nordic countries relied on Beveridgean flat-rate minimum pensions, continental European states favored Bismarckian earnings-related benefits schemes. Moreover, children-parent ties vary significantly across countries. Thus, the same pension system could still induce differential fertility reactions.

3.2 Data and definition of reforms

The fertility regression presented in equation (1) is estimated using annual data from 21 countries for the period from 1870 to 2010[5]. For 10 countries, data is available for almost the entire sample period. For the remaining countries at

[4] In order to capture changes in the demographic composition, Murtin (2012) includes the share of females aged 20 to 29 and 30 to 39 as additional control variables. Using Murtins original specification does not change the results.

[5] See appendix for a list of countries and the first sample year per country.

least 60 annual observations are in the dataset. Data on births and other demographic information, including deaths and population size by age and gender, stem from the Human Mortality Database. Average years of schooling comes from Murtin (2012). Given that schooling data is only available at a ten-year period interval, I linearly interpolate the data in order to obtain annual figures. GDP per capita is taken from The Maddison-Project (2013). Information on battle deaths per war and country stem from the Correlates of War database (Sarkees and Wayman, 2010). In order to obtain annual figures, I divide the number of overall deaths by the duration of the respective war. Therefore, I stipulate that all combatants were males and that the number of casualties is the same in each war year. Both assumptions are necessarily approximations. Table B1 in the appendix provides summary statistics.

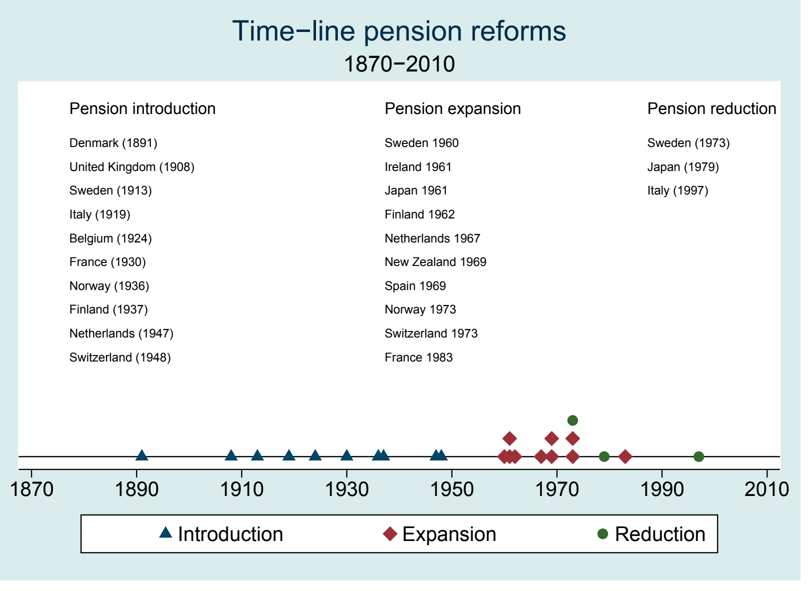
Figure 2 shows the timing of the 23 major pension reforms used in the analysis. Pension reforms are grouped into three different categories: introduction, expansions and cutbacks of the system and are collected from different sources.The specific dates of the introduction of the first nation-wide compulsory public pension system that covered a substantial number of workers are taken from Cutler and Johnson (2004). For the introduction period, I focus on 10 countries only, because sufficient pre-introduction years are lacking for the remaining countries[6].

Major expansions and cutbacks of the pension system are identified using the whole sample from 1960 onwards based on two different sources. Firstly, I use PAYG replacement rates calculated by Bloom et al. (2007). According to my definition, a major pension reform occurs whenever the pension replacement rate changes permanently by at least 10 percentage points from one year to another. Permanently means that the change is not reversed within the following decade. The replacement rate is defined as "the size of the annual pension, as a percentage of the recipient's pre-retirement income, for a worker of average income (which we take to be income equal to two thirds of GDP per worker) who works from age 17 to the reported normal retirement age in the system, under the system's current rules."(Bloom et al. (2007), Page 103). Hence, a 10-percentage point variation in the replacement rate corresponds to a substantial change of the retirement to pre-retirement income ratio of an "average" worker.

Secondly, I augment the pension reform dataset based on a literature review that also captures reforms for which no before-and-after comparison is possible based on the Bloom dataset, since they have been implemented at the beginning of the 1960s. Four additional reforms (Sweden, Ireland, Japan, and Finland) have been identified based on this endeavor. In order to assess the robustness of the results, I also implemented and tested different reform definitions (see chapter 4.4). Expansion and cutbacks are only considered from the 1960s onwards, because prior to this not all sample countries had basic public pension schemes in place for at least 10 years.

[6]The results do not change if the fertility residual is calculated based on a specification using the balanced sample of 10 countries instead of the full sample. Among the ten countries, Belgium is excluded for the period 1914-1918 because fertility data is missing. Excluding Belgium completely, however, does not change the results.

Figure 2: **Timing of pension reforms**



4 Results

4.1 Fertility regression

First, I discuss the estimation results of the fertility regression (1) used to obtain the residuals (see Table C1 in the appendix). Given that the specifications differ only marginally, it is not surprising that the results are in line with the findings by Murtin (2012: Table 4-5). Schooling is negatively correlated with the birth rate, while the infant mortality as well as overall mortality is positively related to the number of births. The only difference to Murtin (2012) concerns the coefficients of the GDP per working-age adult variables. This is due to the fact that I only use GDP per working-age adult as well as GDP per working-age adult squared and hence abstain from including their cubic forms, since they render each GDP per working-age variable insignificant. However, the empirical pension-fertility relationship does not depend on the functional form specification of the GDP variable. The war dummy, not included in Murtin (2012), exhibits the expected negative coefficient implying that wars contribute to lower fertility rates.

4.2 Cross-country results

Figure 3 displays the cross-sectional average of the fertility rate (dots) as well as the pre- and post reform trends around the introduction date of the public pension system. In contrast to the predictions of the old-age security hypothesis, fertility trends are not negatively affected by the introduction of the pension system. In fact, fertility declines slower after the reform. In order to assess whether trends change significantly, I follow Samwick (2000) and estimate the following regression:

$$raw\,fertility\,rate/residual_t = a_0 + \beta_1 year_t + \beta_2 after_t + \beta_3(after_t * year_t) + \beta_4 during_t + \varepsilon_t \quad (2)$$

where *year* is the year relative to the introduction of the pensions system and hence ranges from - 10 to 10. *after* indicates a dummy that equals one after the introduction of the system while *during* is a dummy variable for the year of the reform (year=0), which captures potential distortion during the implementation of the reform. However, β_4 is not discussed in detail, since it is never statistically significant and quantitatively less important. Table 1 gives an overview over the estimated coefficients of equation (2) for all types of pension reforms.

Figure 3: **Fertility rate before and after the introduction of the public pension system**

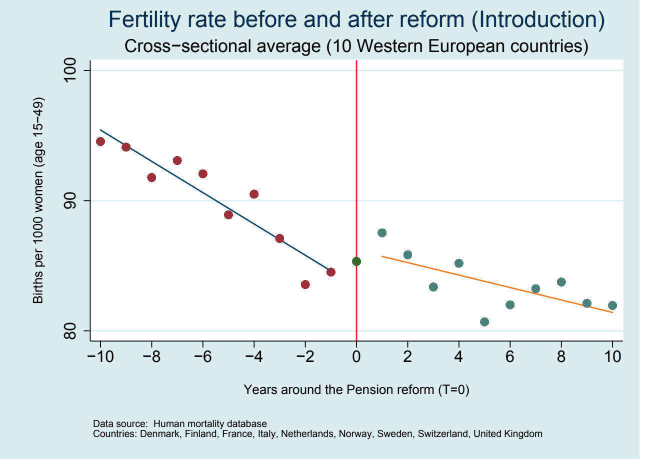


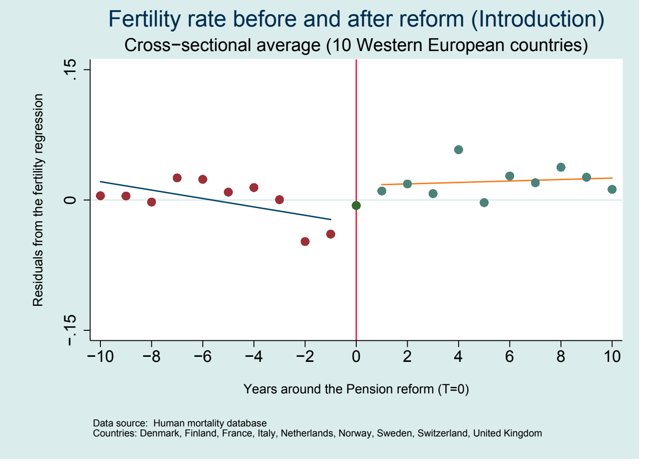
Table 1: **Results of the trend regression (2)**

	β_1(trend before)	β_2	β_3(trend change)	β_4
		Introduction		
Raw Fertility rate	-1.20 (-7.19)***	2.80 (1.91)*	0.72 (3.06)***	1.93 (1.05)
Fertility residual	-0.48 (-2.26)**	4.41 (2.35)**	0.57 (1.88)*	2.09 (0.89)
		Expansion		
Raw Fertility rate	-1.22 (-20.81)***	1.58 (3.07)***	-0.24 (-2.90)**	-0.26 (-0.40)
Fertility residual	-0.55 (-6.23)***	1.35 (1.75)*	0.23 (1.89)*	-0.63 (-0.66)
		Cutback		
Raw Fertility rate	-0.79 (-5.28)***	-1.25 (-0.95)	0.08 (0.37)	-0.63 (-0.38)
Fertility residual	0.48 (2.20)**	0.44 (0.23)	-0.91 (-2.98)***	0.28 (0.12)

Note: Coefficients for the fertility residual all multiplied by 100, *t*- statistics in parentheses. ***,**,* Significance at the 1,5 and 10% Level.

Before the introduction, fertility rates were on average declining by 1.20 births (per 1000 women aged 15-49) each year (β_1). After the introduction, fertility decreased only by 0.48 births per year ($\beta_1 - \beta_3$) and hence 0.72 births less (β_3). The β_3 coefficient is statistically significant at the 1%-level, indicating that the trend change is not a fluke. Thus, the introduction of public pensions seems to have rather stimulated fertility instead of attenuating it, a result completely at odds with the old-age security hypothesis.

Figure 4: **Fertility residual before and after the introduction of the public pension system**



Using the residuals of the fertility equation (1), instead of raw fertility rates, as the dependent variable, shows a similar picture (Figure 4 and Table 1). In fact, the fertility trend even becomes positive after the introduction ($\beta_1 + \beta_3 > 0$). Note that the raw fertility rate is measured on an absolute scale, while the regression residual is based on a logarithm scale. Hence, a β_1 of - 0.48 as indicated in the second row of Table 1 can be interpreted as a 0.48%-annual decline in the unexplained fertility rate, given that the presented coefficients for the fertility residuals have already been multiplied by 100.

The analysis of pension system expansions also offers no comprehensive evidence for the old-age security hypothesis. The evolution of the raw fertility rate, presented in Figure 5, suggests only a modest trend reduction after the expansion of the pension system. Fertility declines by 0.24 births per year slower than it did in the pre-reform period (Table 1). This is in line with the old-age security hypothesis. Moreover, this trend change is also statistically significant. If we consider the fertility regression residual, however, the results change. Fertility tends to fall significantly less (0.2% per year) after the expansion (Figure 6). Hence, once alternative explanations are accounted for, fertility rates do not decline after the generosity of the pension system increases.

Figure 5: **Fertility rate before and after the expansion of the public pension system**

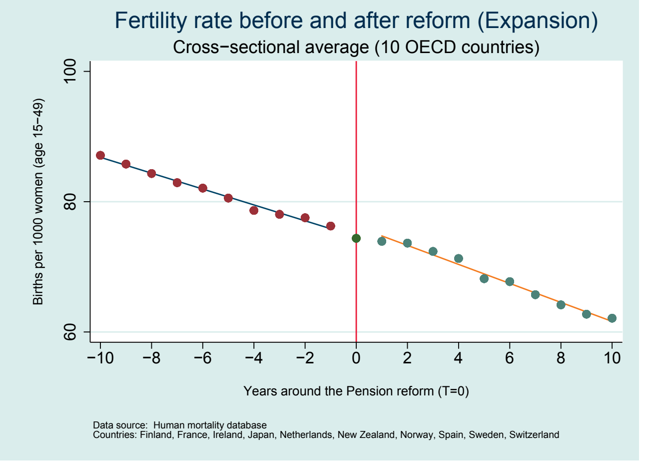
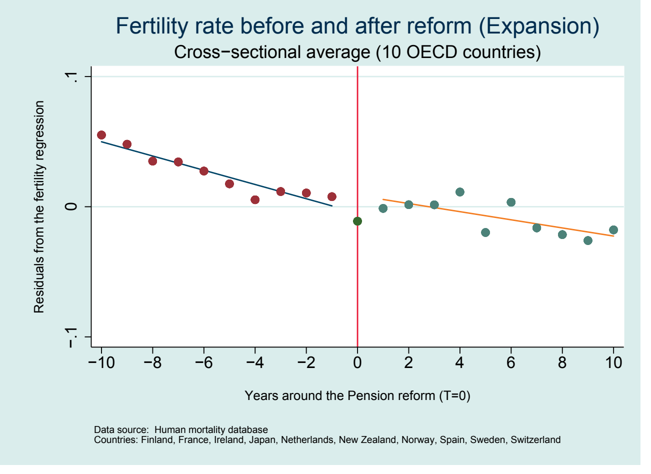


Figure 6: **Fertility residual before and after the expansion of the public pension system**



The examination of cutback reforms yields similar results. The old-age security hypothesis suggests an increase in fertility rates after a reduction in public pension generosity, since parents should increasingly rely on children for old-age support. The results presented in Figure 7 and Figure 8 do not support this theory. Even though the raw fertility rates decline less after the cutback, the difference is not significant. Moreover, once additional determinants are accounted for, the fertility trend becomes negative after the reform, although pre-reform fertility had been increasing.

Figure 7: **Fertility rate before and after the cutback of the public pension system**

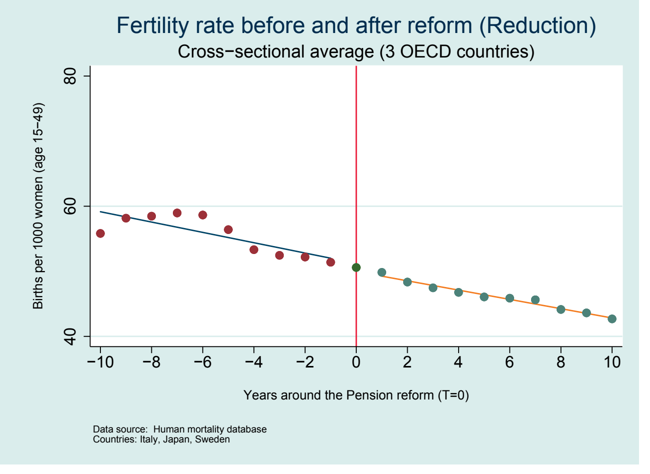
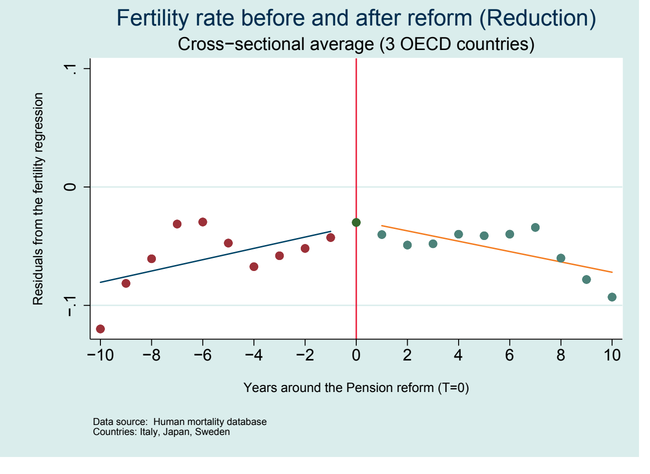


Figure 8: **Fertility residual before and after the cutback of the public pension system**



All in all, I find no evidence of a negative relationship between public pension generosity and fertility rates. Average fertility rates are neither negatively affected by the introduction nor by expansion of the public pension systems. Furthermore, cutbacks do not stimulate fertility. In the next chapter I discuss the robustness of these results.

4.3 Identification issues & Robustness checks

Despite the extensive set of controls, the presented results may still not allow for a causal interpretation. Other events that affect fertility trends could have happened just before, after or simultaneously with the pension reform. In order to mitigate the problem of coinciding events, I augment the fertility regression by several control variables. Specifically, I successively add the following variables: a continuous war proxy (battle deaths as a percentage of men aged 18 to 49) instead of a simple war dummy, the polity2 score as a measure of the democracy level, the gender balance, female labor force participation, the proportion of Catholics, the public debt-to-GDP ratio, real interest rates and economic crisis dummies. Data sources, summary statistics as well as the rationale behind the use of these variables are provided in appendix B.

All in all, four specifications of the fertility regression are used, the results are provided in Table C2 of the appendix. Among 12 specifications (4 fertility regressions times 3 types of pension reforms) the fertility trends behave contradictory to the intuition of the old-age security hypothesis in all but two case. Fertility trends are only negatively affected by the introduction and expansion of the public pension system in a setting where all control variables are included simultaneously. However, the evidence for the old-age insurance hypothesis seems to be mainly driven by a selective loss of observations based on missing data. Only 3 introductory respectively 5 expansionary reforms with sufficient pre- and post-reform data points[7] are left after the public debt-to-GDP ratio and real interest rates data are included. The country-specific results in section 4.4. indicate that the countries are selected. Most countries with sufficient remaining observations, show evidence for the old-age security hypothesis, while most countries that drop out do not. This is supported by the analysis of the cutback reforms. Results for this reform type do not reverse after all controls are included, which is probably based on the fact that no country drops out. Hence, all in all, the average results are robust to the inclusion of additional controls.

Another threat to identification concerns the reasons for the implementation of pension reforms. If pension reforms are introduced because fertility trends change, the research design provides flawed results. However, fertility seems to have played no relevant role in the implementation of pension reforms at least for the introduction period. The first public pension system introduced by Bismarck in 1889, was implemented in order to reduce the support for socialist parties in Germany (Cutler and Johnson, 2004). According to Cutler and Johnson (2004) the most important reason for the introduction of the public pension in other rich countries has been a preference for poverty relief or the German demonstration effect (e.g. in France after the return of Alsace-Lorraine). Cutler and Johnson (2004) also confirm

[7] I set 16 as the minimum number of observations, given that less observations impede the calculation of pre- and post-reform trends.

the negligible role of economic and demographic fundamentals in their empirical analysis. None of the explanatory variables included (industrialization, urbanization, democracy, elderly share, income etc.) are able to robustly predict the introduction of the public pension system in rich OECD and Latin American countries.

Moreover, the research design hinges on the assumption that a clear pre- and post-reform dichotomy exists. Even though this is generally the case for the identified pension reforms, a few limitations apply. First of all, public pension insurance systems usually replace existing systems such as the Poor Law in England introduced 1601 that already provided some old-age assistance. The newly established public pension systems, however, were usually much more generous than the existing systems (Cutler and Johnson, 2004; Guinnane, 2011). Thus, the introduction of the public pension systems constitutes an expansion of the existing old-age insurance system rather than a completely new concept. Furthermore, it can't be generally ruled out that minor pension reforms affected fertility trends before and after major pension reforms to some extent. In order to evaluate this potential bias, I drop expansion and cutback reforms, for which, based on the Bloom dataset as well as a literature search (first and foremost Immergut et al., 2007), minor pension reforms occurred during the pre- and post-reform period of the actual big reform. This procedure resulted in a loss of eight reforms[8]. The results do not change qualitatively.

Results might be also affected by coinciding reforms in other areas such as family policy. Based on Gauthier (1998), I identified major family policy reforms (introduction of paid maternity leave, child/family allowances and mother/children assistance programs) that have been implemented in the period surrounding the introduction of public pension systems. Dropping the five pension reforms[9] for which this is the case does not alter the results.

Given that the classification is not uniformly established in the literature, I also modified the definition of pension reforms in order to check whether the results depend on the specific coding. Therefore, I altered the reform definition and considered only expansion and cutbacks where the PAYG replacement rate, calculated by Bloom et al. (2007), increased or decreased by at least 50% instead of an absolute change of 10 percentage points. Seven expansions[10] satisfy this conditions, but no reform qualifies as a cutback reform. The average residual fertility trend for the seven pension expansions is negatively affected by the reform. However, the trend is not statically significant and the fertility level is generally much higher after the reform.

The relevance of a potential anticipation effect — couples foresee the reform and therefore adjust their pre-reform fertility— is evaluated by artificially antedating the reform three years prior to the actual reform. In this setting, residuals show a trend decline after the introduction. However, the trend is not significant. Moreover, expansions and cutback reform outcomes do not change.

[8] The following 5 reforms are excluded: Japan (1961), Finland (1962), Netherlands (1967), Spain (1969), New Zealand (1969), Sweden (1973), Japan (1979), Italy (1997).

[9] The following 5 reforms are excluded: United Kingdom (1908), Italy (1919), Belgium (1924), France (1930), Finland (1937).

[10] Australia (1967), France (1983), Japan (1967), New Zealand (1969), Norway (1973), Spain (1969), Switzerland (1973)

The last block of robustness checks concerns the sample selection. Results do not change if, instead of 10 years, the pre- as well as the post-reform period is extended to 20 years each. Similarly, excluding war periods or the countries Bulgaria and Hungary, which might be structurally different, does not affect the findings.

All in all, the robustness checks conducted in this chapter support the general finding that no clear evidence for the social security hypothesis exists.

4.4 Country-specific effects

Given the institutional and cultural diversity of the countries studied, next I examine the evolution of country-specific fertility trends. Therefore, I do not average the residuals from regression (1) across countries, but study them separately using the framework presented in equation (2). In contrast to the previous cross-country analyses, I find evidence for the old-age security hypothesis in some countries and reform types (see Table C3 for the country-specific results). Table 2 indicates reforms which resulted in a significant trend break of the regression residual in line with the old-age security hypothesis. Most reforms, however, show either no significant trend changes or shifts that are at odds with the old-age insurance hypothesis, irrespective of the specification of the fertility regression.

Table 2: **Reforms that affected the fertility rate in line with the old-age insurance hypothesis**

Introduction	Expansion	Reduction
France (1930)	Finland (1962)	Italy (1997)
Sweden (1913)	France (1983)	
	Netherlands (1967)	
	Norway (1973)	

Note: The total number of studied cases is 10 for introduction as well as expansion reforms, and 3 for reduction reforms. Figures in parentheses refer to the years when the reforms were adopted.

In the following, I explore empirically if country and reform characteristics explain why the fertility impact differs by reform. Therefore, I regress the *t*-values of the interaction term β_3 of regression (2) from the country-specific results presented in Table C3, which indicate a potential post-reform trend break, on the type of the pension system, geographical factors, the timing, the intensity of the reform and cultural aspects (results are presented in Table C4). For the introduction and expansion reforms a negative *t*-value of β_3 indicates a fertility reaction in line with the old-age security hypothesis. Given that cutback reforms should induce opposite fertility reactions, I multiply the *t*-value of these reforms by -1. Hence, a negative correlation with the *t*-value suggests that an increase in the explanatory variable tends to facilitate a fertility reaction that is in line with the old-age security hypothesis.

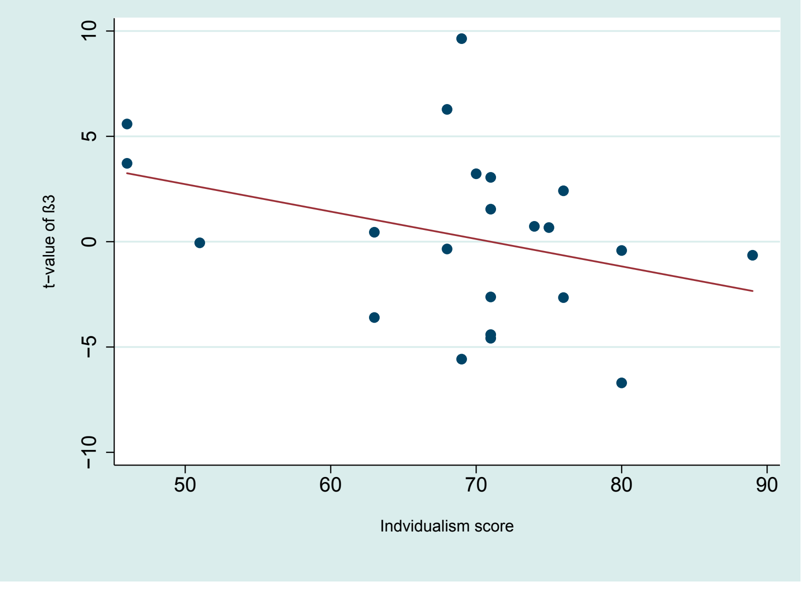
First, different types of pension systems are considered. Theoretically, the fertility effect likely differs between funded and PAYG schemes, since funded systems might simply crowd out private savings, at least under the assumption that individuals are not credit constrained, and hence leave fertility unaffected. Following these considerations, I investigate whether the introduction of originally funded systems in Italy, Switzerland, France and Belgium had different fertility effects than the introduction of mainly PAYG systems. Moreover, I test whether being a primarily Bismarckian system is correlated with the t-value of β_3. Bismarckian pension schemes are characterized by relatively constant levels of income replacement independent of the individuals' position in the income distribution. Beverdigean systems, in contrast, are more redistributive and offer higher replacement rates to poor individuals than to affluent ones. Given that the pension systems' replacement level is a decisive factor for the pension-fertility relationship at least from a theoretical point of view, results may differ between Bismarckian and Beveridgean regimes. Based on the features of their pension system, I classify Belgium, France, Italy, Spain and Switzerland as mainly Bismarckian[11]. Being funded as well as mainly Bismarckian is negatively correlated with the t-value, but not significantly so. Hence, these pension system specifics offer no explanation for the differential fertility effect.

Geography, which I operationalize using three locational dummies (Northern Europe, Anglo-Saxon, Continental Europe), is also not significantly related to the t-value of β_3. The same is true for the year of the pension reform. Thus, countries that introduce these reforms earlier (or later) are not significantly more likely to show results in line with the old-age security hypothesis. The intensity of expansion and cutback reforms, measured by the absolute change in the PAYG replacement rate, also provides no statistically significant answer. As a result, other determinants such as cultural differences might drive the cross-country variations.

In order to explore the role of social ties, I draw on the cultural dimensions theory introduced by Hofstede (e.g. 2001). Specifically, I focus on the individuality dimension to capture the degree to which persons are expected to take care of only themselves. Figure 9 plots t-values and individualism scores obtained from Hofstede et al. (2010). Countries with a higher individualism score tend to have lower t-values. This suggests that pension reforms tend to affect fertility negatively, in line with the old-age security hypothesis, if social ties are rather loose between individuals. While this seems surprising at first, two rather opposing explanations could drive this result. Parent-child ties might be especially pronounced in individualistic countries because there is nobody else to rely on. Alternatively, parents could derive less consumption utility from children. Hence, the main motivation for childbearing is "selfish" old-age support. Once the state provides public pension, fertility would fall quicker under these circumstances. The relationship between the t-values and individualism scores is only statistically significant at the 15 percent significance level, though. However, the individualism scores provide the best explanatory power among the hypotheses tested. The evidence is nonetheless far from bullet-proof and so far suggestive at best. An in-depth analysis of the pension-culture-fertility link is a fruitful alley for future research.

[11] Recoding Switzerland as non-bismarckian, does not affect the results.

Figure 9: ***t*-value of β_3 and Individualism score**



5 Conclusion

Comparing pre- and post-reform trends, I argue in this paper that pension reforms do not generally affect fertility in the way most theoretical models predict. This result, however, is not at odds with empirical studies showing that specific pension reforms e.g. in Italy in the 1990s (Billari and Galasso, 2014) have induced fertility effects in line with the old-age security hypothesis. In contrast, the estimations based on 23 pension reforms over a period from 1870 to 2010 suggest that the relationship between public pensions and fertility is country-specific and depends on the cultural context. These findings are not driven by major events occurring around the years of the pension reform including wars, democratization or secularization, nor by economic factors such as income, migration or female labor force participation. Moreover, Cutler and Johnson (2004) show that preferences for poverty reduction or the German demonstration effect (e.g. in France after the return of Alsace-Lorraine) have been the main motivation for the introduction of public pension system. Hence, pension reforms seem to have been introduced irrespective of pre-reform fertility trends.

The results imply that introducing public pension systems is no panacea to decrease fertility rates in high-fertility countries such as Sub-Saharan Africa. Furthermore, cutbacks in public pension generosity per se, even though fiscally demanded, will also not necessarily reverse fertility trends. Family-friendly policies, such as the expansion of child care facilities and increases in child allowances, might be better suited for these purposes (van Groezen et al., 2003; van Groezen and Meijdam, 2008). However, further research is needed in order to robustly identify why fertility reactions to different pension reforms vary so much.

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A Sample countries

Table A.1: **Sample countries**

Country	First sample year
Australia	1921
Austria	1947
Belgium	1870
Bulgaria	1950
Canada	1921
Denmark	1870
Finland	1878
France	1870
Hungary	1950
Ireland	1950
Italy	1872
Japan	1947
Netherlands	1870
New Zealand	1948
Norway	1870
Portugal	1940
Spain	1908
Sweden	1870
Switzerland	1876
United Kingdom*	1870
United States	1933

*United Kingdom until 1922 without Northern Ireland

B Control variables and summary statistics

In the following paragraphs, I briefly discuss the reasons for the inclusion of the additional control variables mentioned in section 4.3. and specify the data sources. First and foremost, I add a proxy for the level of democracy. Basso (2015) argues that democratization facilitates technology adoption as well as the availability and diffusion of modern contraceptives and therefore, depresses fertility rates. Given that democratization and pension reforms partly overlap in time, I include the polity2 score from the Polity IV project which is line with the procedure in Basso (2015).

Moreover, I include the gender balance in order to proxy migration as suggested by Fenge and Scheubel (2017). Migration can affect fertility through several channels (e.g. through differing age structure or fertility rates of migrants vis-à-vis natives) and is potentially caused by the generosity of the pension system. Hence, I include the ratio of females to males both aged 15 to 49 which also captures the scarcity of males or females of childbearing age.

In addition, the fertility regression is augmented by the female labor force participation rate. Increasing employment opportunities of women raise the opportunity costs of children and thus, should reduce fertility. Female labor force participation rates before 1950 are taken from the dataset of Olivetti (2013) –which mainly draws on Mitchell (1998a,b,c). Post-World War II data is obtained from ILO-statistics. Missing values have been linearly interpolated.

Furthermore, I employ the share of Catholics as a proxy for cultural change and contraceptive use. The opposition to contraceptives is deeply rooted in Catholicism (Scheubel, 2013), hence a higher share of Catholics should increase the fertility rate. Since the Catholic share is highly endogenous –more children of Catholics should result in more Catholics overall (Fenge and Scheubel, 2014)– and data availability is limited, the Catholic share variable serves only as an additional robustness check. Additionally, most of the religious variation should be already captured by the country- and time-fixed effects. Data for the period around 1880 comes from Delacroix and Nielsen (2001), for 1900 from Barro and McCleary (2005) and from 1945 onwards from the World Religion Project provided by Maoz and Henderson (2013). Gaps between these data points are linearly imputed.

Additional controls include real government interest rates that influence the relative return of children, the public debt-to-GDP ratio as an indicator of the credibility of the pension system and an economic crisis indicator. All these variables potentially affect fertility and the generosity of the pension system simultaneously. Summary statics and additional data sources are provided in Table B1.

Table B.1: **Summary Statistics**

Variable	Obs	Mean	Std. Dev.	Min	Max
Raw fertility rate	2,193	78.04	28.98	31.94	157.55
War dummy	2,193	0.04	0.20	0.00	1.00
Battle deaths per men (age 18-49)	2,193	0.00	0.00	0.00	0.04
GDP per working-age adult (age 20-59)	2,193	17259	12446	2267	57264
Crude death rate (deaths per 1000 inhabitants)	2,193	12.41	4.65	6.01	35.84
Infant mortality (infant deaths per 1000 births)	2,193	54.32	55.15	2.28	240.45
Average years of schooling	2,193	7.82	2.76	0.55	13.37
Gender imbalance ratio (female/male)	2,193	1.01	0.05	0.92	1.60
polity2 score	2,165	6.52	6.18	-10.00	10.00
Share of Catholics	2,075	0.41	0.38	0.00	1.00
Female labor force participation	1,881	0.40	0.12	0.12	0.63
Public debt-to-GDP ratio	1,968	0.55	0.39	0.02	2.90
Real interest rate	1,648	1.98	10.35	-337.72	24.27
Domestic debt crisis dummy	2,072	0.00	0.07	0.00	1.00
Banking crisis dummy	2,072	0.09	0.28	0.00	1.00

Note: Debt and real interest rate data stems from Mauro et al. (2013).
Crisis data comes from Reinhart and Rogoff (2011).

C Fertility regression results

Table C.1: **Fertility regression results**

	(1)
	Full sample
War dummy	-0.07**
	(-2.37)
ln(GDP per adult 20-59)	-1.28***
	(-3.41)
ln(GDP per adult 20-59) squared	0.08***
	(3.61)
ln(Crude death rate)	0.19*
	(1.92)
ln(Infant mortality)	0.10*
	(2.02)
Years of schooling	-0.02
	(-1.40)
Constant	9.49***
	(5.61)
N	2193
adj. R^2	0.905

Notes: Dependent variable: ln(raw fertility rate). Includes country and time fixed effects. *t*-statistics in parentheses. ***,**,* Significance at the 1%,5%,10% Level. Standard errors clustered at the country level

Table C.2: **Fertility regression results (Additional controls)**

	(1)	(2)	(3)	(4)
War dummy		-0.08**	-0.08**	-0.08
		(-2.23)	(-2.36)	(-1.30)
Battle deaths per men (age 18-49)	-14.26***			
	(-5.14)			
polity2 score	-0.01**	-0.01**	-0.01**	-0.02***
	(-2.80)	(-2.39)	(-2.38)	(-5.60)
Gender imbalance	-0.07	-0.15	-0.15	-0.33
	(-0.32)	(-0.63)	(-0.68)	(-1.66)
ln(GDP per adult 20-59)	-0.33	-0.61	-0.65	-1.47
	(-0.64)	(-0.94)	(-1.05)	(-1.53)
ln(GDP per adult 20-59) squared	0.03	0.04	0.04	0.08
	(0.91)	(1.09)	(1.19)	(1.59)
ln(Crude death rate)	0.24**	0.19	0.22**	0.22
	(2.74)	(1.54)	(2.18)	(1.30)
ln(Infant mortality)	0.07	0.10*	0.11	0.07
	(1.37)	(1.78)	(1.68)	(0.93)
Years of schooling	-0.02	-0.03	-0.02	-0.02
	(-1.42)	(-1.12)	(-0.98)	(-0.35)
Female labor force participation		-0.24	-0.16	-0.22
		(-0.81)	(-0.68)	(-0.70)
Share of Catholics			-0.43	-0.22
			(-0.93)	(-0.58)
Real interest rate				0.00***
				(3.19)
Public debt-to-GDP ratio				-0.08
				(-1.69)
Constant	5.08**	6.62**	6.70**	11.12**
	(2.31)	(2.25)	(2.36)	(2.36)
Financial crisis	No	No	No	Yes
N	2165	1859	1859	1436
adj. R^2	0.903	0.867	0.869	0.891

Notes: Dependent variable: ln(raw fertility rate). Includes country and time fixed effects. *t*-statistics in parentheses. ***,**,* Significance at the 1,5 and 10% Level. Standard errors clustered at the country level.

Table C.3: **Results of the trend regression (2) for each reform**

Reform	β_1(trend before)		β_2		β_3(trend change)		β_4	
				Introduction				
Denmark (1891)	-0.07	(-0.39)	-2.43	(-1.56)	0.18	(0.73)	-2.22	(-1.14)
United Kingdom (1908)	0.07	(0.18)	0.25	(0.08)	-0.35	(-0.64)	1.10	(0.26)
Sweden (1913)	0.57	(1.09)	5.15	(1.12)	-1.94	(-2.62)**	-3.36	(-0.58)
Italy (1919)	-3.36	(-3.03)***	27.29	(2.81)**	3.78	(2.42)**	9.78	(0.80)
Belgium (1924)	0.49	(0.95)	1.29	(0.63)	0.37	(0.67)	2.78	(1.17)
France (1930)	1.27	(3.12)***	9.34	(2.60)**	-2.65	(-4.58)***	1.23	(0.27)
Norway (1936)	-2.09	(-6.87)***	3.90	(1.46)	4.14	(9.64)***	3.31	(0.99)
Finland (1937)	-0.18	(-0.17)	4.04	(0.43)	0.68	(0.45)	7.65	(0.65)
Netherlands (1947)	-0.15	(-0.21)	1.31	(0.22)	-0.41	(-0.42)	7.70	(1.01)
Switzerland (1948)	0.81	(1.17)	-10.81	(-1.79)*	-0.34	(-0.35)	-11.84	(-1.56)
				Expansion				
Sweden (1960)	-0.82	(-1.76)*	0.52	(0.13)	2.01	(3.05)***	-4.56	(-0.89)
Ireland (1961)	0.75	(3.06)***	1.01	(0.47)	1.12	(3.22)***	-1.59	(-0.59)
Japan (1961)	-3.78	(-4.78)***	2.85	(0.41)	6.26	(5.59)***	5.97	(0.69)
Finland (1962)	-1.13	(-3.29)***	4.91	(1.63)	-1.75	(-3.60)***	-0.43	(-0.11)
Netherlands (1967)	-0.83	(-2.87)**	5.73	(2.27)**	-2.73	(-6.70)***	-1.30	(-0.41)
New Zealand (1969)	-1.42	(-4.29)***	10.95	(3.77)***	-0.63	(-1.34)	6.25	(1.72)
Spain (1969)	1.02	(3.83)***	2.48	(1.06)	-0.02	(-0.05)	-1.03	(-0.35)
Norway (1973)	1.29	(4.28)***	-10.94	(-4.15)***	-2.37	(-5.58)***	-5.90	(-1.79)*
Switzerland (1973)	-1.59	(-5.98)***	-10.09	(-4.33)***	2.36	(6.28)***	-2.60	(-0.89)
France (1983)	1.46	(3.64)***	2.79	(0.79)	-2.50	(-4.41)***	-4.41	(-1.00)
				Cutback				
Sweden (1973)	0.50	(1.33)	0.85	(0.26)	-0.81	(-1.54)	3.13	(0.77)
Japan (1979)	0.68	(1.46)	-6.67	(-1.63)	-2.45	(-3.72)***	-8.71	(-1.7)
Italy (1997)	0.26	(1.89)*	7.15	(5.94)***	0.51	(2.65)**	6.41	(4.25)***

Coefficients all multiplied by 100, *t*-statistics in parentheses. ***,**,* Significance at the 1%,5% and 10% level.

Table C.4: **Bivariate regressions for the *t*-value of β_3**

Bivariate regression	Coefficient		R^2
Funded (Dummy)	-1.65	(-0.66)	0.05
Bismarckian (Dummy)	-0.80	(-0.45)	0.01
Northern Europe (Dummy)	0.40	(0.22)	0.00
Anglo-Saxon (Dummy)	0.26	(0.10)	0.00
Continental Europe (Dummy)	-2.07	(-1.25)	0.07
Year of the reform (continuous)	-0.01	(-0.45)	0.01
Intensity of pension reform (continuous)	0.18	(0.03)	0.00
Individualism score (continuous)	-0.13	(-1.57)	0.11

Dependent variable of all regressions is the *t*-value of β_3 from Table C3 (multiplied by -1 for cutback reforms). *t*-statistics in parentheses. Conclusion does not change if all locational dummies are included together.