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Original Article

The Impact of Cohort Membership on Disposable Incomes in West Germany, France, and the United States

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Abstract

This paper uses age-period-cohort models to show that the living standards (total monetary incomes after public benefits and contributions, adjusted for household size and inflation) of successive birth cohorts in the United States and Germany are strongly correlated with general changes in disposable incomes. This means that, after introducing controls, virtually every successive birth cohort in Germany and the United States had increasing disposable incomes, similar to general rates of economic growth. For France, however, we find that, while disposable income increased through the 20th century, cohorts born before 1950 profited from this, while cohorts born after 1950 experience no improvement in living standards over previous cohorts, after adjusting disposable incomes for inflation and controls—mainly education and household composition. Thus, while economic growth benefits all birth cohorts in the United States and Germany, pre-1950 birth cohorts in France have monopolized lucrative positions and social transfers so that post-1950 birth cohorts do not benefit from economic growth.

Do some birth cohorts monopolize lucrative positions and social transfers, so that they are unduly advantaged over others? In a world without such intercohort inequalities, a cohort born into an economy that is, say, 2 per cent richer should have 2 per cent more disposable income over its life course. In reality, however, increasing prosperity could have bypassed some birth cohorts, while others disproportionately reaped the fruits of economic growth, appropriating lucrative positions and social transfers, thus disadvantaging other birth cohorts. We measure whether this happened for birth cohorts of the 20th century in West Germany, France, and the United States. We show how much belonging to a certain birth cohort influences incomes in these three countries and whether these countries advantage some birth cohorts while disadvantaging others. The existing literature has speculated on this question, but not answered it. This is largely owing to methodological limitations, as we show below.

We use an age–period–cohort (APC) analysis. This research tradition started with Karl Mannheim (1928), who proposed to conceptualize social change as the result of new birth cohorts replacing old ones. Each birth cohort shares a socialization, as it grows up in a similar historical period, which can durably mark or even collectively 'scar' a generation (Mannheim, 1928: p. 311f.). Notably, some cohorts may have had an easy entry into the labour market during an economic boom. However, a later generation may have had to establish itself during the slump that followed the boom, from which the preceding generation profited. For example, cohorts that reached adulthood during the Great Depression are less likely to invest in the stock market; they are scarred in the sense of being more risk averse (Malmendier and Nagel, 2011). Other cohorts grew up when entering the working market was difficult, which disadvantaged them economically. This led scholars to hypothesize about systematic cycles of 'lucky and less lucky generations' (Myles, 2002: p. 138).

We argue that the differential fate of cohorts may differ from country to country. Notably, while a succession of more and less favoured birth cohorts may be unavoidable, conservative welfare states, such as France and Germany, are known to use employment protection and seniority rights to protect workers that hold a stable job ('insiders') against those who do not hold a stable job ('outsiders'), to preserve social stability (Ferrera, 1996, 2010; Buchholz *et al.*, 2009). The post-1970s liberalization of conservative welfare states exacerbated this trend, arguably in France even more than in Germany (Buchholz *et al.*, 2009; Ferrera, 2010: p. 625; Palier, 2010: p. 96f.). One could therefore suppose that German and French cohorts that looked for their first job after 1970 are disadvantaged, as they entered a rigid labour market when economic growth ceased to generate room for outsiders, while labour regulations kept protecting insiders. To test whether this is the case, we contrast these two conservative welfare states with the United States, the quintessential liberal welfare state. While inequalities have increased in the United States, liberalization there did not protect a group of insiders against outsiders, but exposed everyone to the market, contrary to conservative welfare states (Thelen, 2012; Schröder, 2013). Thus, one would not expect that US birth cohorts that entered the labour market after 1970 are disadvantaged. While the welfare state literature furnishes these hypotheses, it is unable to answer them, as the following literature review shows.

Blossfeld (1986: p. 219) gives reason to argue that baby boomers are advantaged, as he shows that West German cohorts born around 1951 entered higher-prestige jobs than cohorts born around 1929. Lauterbach and Sacher (2001) in turn document that German men born between 1955 and 1970 are more often unemployed and precariously employed than men born between 1935 and 1940; but they document the contrary trend for women. For men, this illustrates our general hypothesis: cohorts that entered the labour market during favourable times have incomes above long-run trends, while cohorts entering the labour market in unfavourable times are disadvantaged for their entire work life. Boockmann and Steine (2006) compared West German cohorts born between 1925 and 1974, showing that monetary returns to education decreased over time. This also indicates that despite educational progress, it remains problematic to enter the labour market during unfavourable times. In a US–German comparison, Antonczyk, DeLeire, and Fitzenberger (2010) showed that German late- and early-born male cohorts (born close to 1930 or close to 1980) receive lower market wages—relative to overall wage levels—than cohorts born between 1940 and 1970. They find smaller cohort effects for the United States. However, they only look at market wages, while to understand whether certain cohorts are advantaged overall, one should measure actual living standards. Studies that do this lack the APC data necessary to disentangle cohort from age effects however, thus they remain speculative (Kohli, 2006; Liebig and Scheller, 2007). Lacking a methodology to disentangle cohort from age effects, scholars complain that 'as soon as one tries to describe generations as social collectives, one gets confronted with a set of rather severe difficulties that hinder a clear and concise operationalization' (May, 2012: p. 19).

For the United States, Kotlikoff (1992) popularized the argument that older birth cohorts monopolized lucrative positions so that younger birth cohorts are 'born to pay' for them (Longman, 1987). However, due to the aforementioned methodological problems, it is so far unclear whether this is true or just speculation. Bommier *et al.* (2010), only looking at transfers and not living standards, disagree by arguing that all cohorts born after 1930 benefited from public social transfers. Kopczuk, Saez, and Song (2010) document decreasing within-cohort inequality until the birth cohort born around 1950, and increasing intracohort inequality for later-born cohorts (also cf. Antonczyk, DeLeire and Fitzenberger, 2010). But while these studies show that inequality within birth cohorts increases over time, they leave open the question on how much belonging to a certain birth cohort influences one's income in the first place (Fitzenberger *et al.*, 2001; Osberg, 2003; Bönke, Corneo and Lüthen, 2012).

The literature on France supposes the strongest cohort effects on income. Baudelot and Gollac (1997) described how each new cohort entered the labour market with a higher starting salary until 1975. Chauvel (1997a) showed that cohorts born after 1955 are less socially mobile than pre-1955 cohorts. Post-1955 birth cohorts also participate less politically, are less healthy, and have higher suicide rates, possibly because of their difficult entry into the labour market (Chauvel, 1997b; Anguis, Cases and Surault, 2002; Koubi, 2003). After scholars even detected systematic downward intergenerational mobility of post-1960 birth cohorts (Peugny, 2009), others explained this by diminishing returns to increasing education (Bugeja, 2009; Chauvel, 2010a,b; Farges, 2012). However, while the French literature is not silent about intercohort-inequalities, it leaves important questions open. Some scholars argue, against the general trend, that French later-born cohorts have caught up (Bonnet, 2010). Also, existing research does not show to what degree education and other coping mechanisms compensate a difficult labour market entry.

Thus, the literature argues one needs to understand which generations are advantaged and disadvantaged, but it cannot 'answer the question of which generations get what, when and how' (Goerres and Vanhuysse, 2012: p. 1, also cf. Mayer, 2005: p. 18; 2009: p. 424). Though lacking reliable data, some argue that 'European societies, in whatever context, do not show signs of generational conflict' (Attias-Donfut and Arber, 2000: p. 18). We show that indeed France advantages mid-20th-century birth cohorts to the detriment of early- and late-20th-century cohorts. This is much less the case for Germany and not at all for the United States. In the following, we introduce the APC models that lead to these results.

Method

APC models explain outcomes through the combined effect of three influences: an individual's age a (variable α_a), cohort membership c (variable γ_c), and period of measurement p (variable π_p). This leads to the following equation:

$$y^{apc} = \mu + \alpha_a + \pi_p + \gamma_c \quad \text{(APC)}$$

An APC model can detect how an outcome is explained by position in the life cycle (age effect), time of measurement (period effect), and date of birth (= age at a certain period = cohort effect). While many have proposed empirical estimations of the three effects (Mason *et al*., 1973; Fienberg and Mason, 1979), an 'identification problem' besets all APC models (Glenn, 1976; Mason and Wolfinger, 2001). This arises from the equation: a = p − c. That is, each variable is a combination of the other two. To illustrate this problem, consider Table 1.

Table 1 shows fictitious average earnings (say, in hourly euros) for people with different ages at different times. Problematically, the same linear trend of income change can either be understood as 1) a combination of an age effect (income increases by 5 euro per 5-year age-group) plus a period effect (income increases by 1 euro per 5-year period) and 2) as an age effect (again, income increases by 5 euro per 5-year age group) plus a cohort effect (each cohort earns 1 euro more than the preceding one). In more general terms, if a variable linearly depends on age, period, and cohort, then an infinite number of decompositions between these effects fit the data and no statistical model can overcome this intrinsic indetermination (Holford, 1991; Luo, 2013).

Table 1. The age–period–cohort under-identification problem

a \ p	1985	1990	1995	2000	2005
25	5	6	7	8	9
30	10	11	12	13	14
35	15	16	17	18	19
40	20	21	22	23	24
45	25	26	27	28	29
50	30	31	32	33	34
55	35	36	37	38	39

The first solution to deal with this problem is to arbitrarily constrain an APC model. Many researchers hold the first and the last coefficient of the cohort vector equal. When one does this, the number of cases in these extreme cohorts is low, leading to large confidence intervals when trying to estimate effects. Yang and Land (2013: p. 65) call this solution the CGLIM (the 'conventional' constrained general linear model). They rightly criticize it as arbitrary.

The second solution is to suppress one of the linear trends (but not the non-linear bumps) of the set (a, p, c), and then suppress e.g. the period trend (p). This way, the cohort (c) trend absorbs all the long-term linear improvements one could attribute to either period (p) or cohort (c) (Mason and Wolfinger, 2001). We call this solution APC-trended (APCT). It attributes the long-term linear age-controlled trend to cohorts, not periods. This shows whether in absolute terms, later-born cohorts do better than previous ones at the same age. But while this shows whether subsequent cohorts are doing better or worse, e.g. in terms of income, this cohort trend is not weighed against a period trend but simply absorbs this trend. The model can thus not show whether a trend is due to a cohort or period effect. The model therefore also highly depends on the window of observation.

The third solution is the APC-intrinsic estimator (APC-IE) of Yang *et al.* (2008), which tries to solve the indetermination problem by a Principal Component Analysis of the age, period, and cohort vectors. This reduces the linear trend of the three variables to two dimensions. Yang *et al.* claim that this yields the intrinsic linear influence of each variable, so that the linear age, period, and cohort trends can be interpreted substantially.[1] However, O'Brien (2011) shows that theirs is an arbitrary choice as well, which fails to deliver substantive linear time trends. It also fails empirical tests. For example, the APC-IE model detects strongly declining educational levels by age (see the uploaded file with the heading 'Problems with APC-IE and HAPC' in the online annex), which makes no substantive sense, as individuals cannot lose their primary, secondary, or tertiary education over time.

The fourth solution is the hierarchical age–period–cohort (HAPC) model. It uses mixed multilevel models that conceptualize age as a continuous polynomial level-1 variable and period and cohort as categorical level-2 variables (Yang and Land, 2013). This model thus assumes that people with a certain age are embedded in a certain cohort at a certain point in time. Cohort coefficients from this model may present a non-zero cohort slope, which is difficult to make sense of, while the non-linearity of the model can be meaningfully interpreted (cf. Pampel and Hunter, 2012 and the uploaded online annex).

Owing to limitations of the existing models, we propose to use an APC-detrended (APCD) model.[2] The APCD acknowledges that linear trends in APC models cannot be robustly attributed to age, period, and cohort; so the model focuses on how the effects of age, period, and cohort fluctuate around a linear trend, which it absorbs.[3] Following the usual notation of APC models and Ordinary Least Squares expressions,[4] we consider a dependent variable y^{apc}, as well as the independent variables age a, period p, and cohort membership c. The equation $c = p - a$ indexes the vectors of coefficients α_a, π_p, γ_c. To provide accurate controls, we consider j covariates x_j (which can be continuous or binary). Including constraints, the model has the following expression:

$$\begin{cases} y^{apc} = \alpha_a + \pi_p + \gamma_c + \alpha_0 rescale(a) + \gamma_0 rescale(c) + \beta_0 + \sum_j \beta_j x_j + \varepsilon_i \\ \begin{cases} \sum_a \alpha_a = \sum_p \pi_p = \sum_c \gamma_c = 0 \\ Slope_a(\alpha_a) = Slope_p(\pi_p) = Slope_c(\gamma_c) = 0 \\ \min(c) < c < \max(c) \end{cases} \end{cases} \quad \text{(APCD)}$$

β_0 denotes the constant, β_j are the coefficients of control variables, α_a is the vector of the age effect, π_p is the period vector, and γ_c is the cohort vector. These vectors exclusively reflect the *non-linear* effect of age, period, and cohort, as we assign two sets of constraints: each vector sums up to zero and its slope is zero.[5] The terms α_0Rescale(a) and γ_0Rescale(c) absorb linear trends; Rescale is a transformation that standardizes the coefficients α_0 and γ_0: it transforms age from the initial code a_{min} to a_{max} to the interval -1 to $+1$. Finally, as the first and last cohorts appear just once in the model (the oldest age group of the first period and the youngest of the last), their coefficients are instable; we obtain better estimates by excluding them. With these constraints, the model becomes identifiable; it provides a unique solution.[6] The detrended cohort effect (DCE) coefficients γ_c are zero when non-linear cohort effects are absent. In this case, all cohorts behave according to their age and period characteristics, with no cohort-specific fluctuation. The APCD then provides no improvement compared with a simple age and period model (AP), which consists of:

$$\begin{cases} y^{ap} = \alpha_a + \pi_p + \alpha_0 rescale(a) + \pi_0 rescale(p) + \beta_0 + \sum_j \beta_j x_j + \varepsilon_i \\ \begin{cases} \sum_a \alpha_a = \sum_p \pi_p = 0 \\ Slope_a(\alpha_a) = Slope_p(\pi_p) = 0 \\ \min(c) < c < \max(c) \end{cases} \end{cases} \quad \text{(AP)}$$

If at least one γ_c coefficient is significantly different from zero however, some cohorts are above or below the linear trend. In this case, the AP model is insufficient, as some cohorts receive more or less than their expected share after period resources have been distributed according to age structures. Comparing the BIC (Bayesian information criterion, cf. Raftery, 1986) of the AP and APCD models offers another criterion for or against including non-linear cohort effects.

When explaining disposable income, substantive reasons exist to focus on deviations from linear trends. Namely, when disposable income increases by a rate of, say, 5 per cent every 5 years, and each cohort born 5 years later increases its income by 5 per cent, our model detects no cohort effect, as the long-run linear trend is absorbed by α_0 and π_0. Indeed, there is no cohort effect in the sense that each cohort profits from the overall linear trend in the same way. A second substantive reason to look at deviations from linear trends is that expectations about disposable incomes adapt to linear trends. No one is surprised if the living standards of one cohort after another increase with the general trend in living standards.[7]

The APCD thus diagnoses whether a certain cohort receives its relative share of period variations. However, even if later-born cohorts are below the long-run trend of income increases, they might still have a higher living standard in absolute terms, depending on the overall rate of income growth (compared with former cohorts at the same age). For example, if incomes grow by 2 per cent, and a later cohort has a disposable income that grew by only 1.5 per cent, then that cohort is below the trend, but it is still better-off than the preceding one.

Contrary to this, when a negative cohort effect is stronger than a positive linear trend, the *absolute* living standard of a cohort decreases.

The above-mentioned APCT model detects such absolute declines or progressions by using a variant of an APCD model without the zero-slope constraint in the cohort coefficients. Thus, the ACPT suppresses the γ_o rescale(c) term, so that it does not absorb the long-term trend. Therefore, the cohort coefficients absorb this long-term linear progression trend, while age effects and period fluctuations are controlled for in the same way as in the APCD. Thus, the parameters γ_c become a trended cohort effect (TCE), which denotes per-cohort change, controlled for age and other includable variables. However, this cannot resolve the (generally irresolvable) APC identification problem. Instead, APCT results show the systematic progression of cohorts, at a given age and controlled for period fluctuations. While the model can control period fluctuations, it cannot control long-run period trends but instead ascribes all linear progression of living standards to cohorts. Thus, in our case, the APCT model shows how inflation-adjusted living standards change on a cohort-by-cohort basis, while it cannot distinguish whether the linear part of this increase is due to cohort effects or period effects. In this sense, the model is more descriptive than the APCD. It is also dependent on the window of observation. Compared with APCD, APCT has the problem that long-term trends depend strongly on the period under study. For example, adding a year with an economic slowdown can substantially decrease the slope of the cohort trend. However, APCT is the only way to understand whether younger cohorts are better-off than former ones were at the same age. It thus answers Immanuel Kant's (1784) demand to understand whether later-born generations are better-off than earlier ones (see footnote 7), even if it cannot disentangle whether this is owing to a long-term period or cohort trend. The following formula defines the model:

$$\begin{cases} y^{apc} = \alpha_a + \pi_p + \gamma_c + \alpha_0 rescale(a) + \beta_0 + \sum_j \beta_j x_j + \varepsilon_i \\ \begin{cases} \sum_a \alpha_a = \sum_p \pi_p = \sum_c \gamma_c = 0 \\ Slope_a(\alpha_a) = Slope_p(\pi_p) = 0 \\ \min(c) < c < \max(c) \end{cases} \end{cases} \qquad \text{(APCT)}$$

Data

We use data from the Luxembourg Income Study (LIS). To make use of control variables and to have data available in harmonized form for all three countries, we use wave II (around 1984) to wave VI (around 2004) of the LIS data, excluding citizens from former East Germany, as no data for them is available before 1989. Our dependent variable is logged disposable income after public transfers and payments, adjusted for inflation[8] and equalized by household size (variable 'dpi' of the LIS, divided by the square root of household members). We have, however, run all analyses by not equalizing income by household members and the substantive results are similar.[9] We do not look at market income, as it is less helpful to understand the actual living standards of cohorts after welfare state effects, which we are interested in. Also, the problem with looking at market income is that we cannot look at any age groups that could have retired already, so that country comparisons using age groups >55 years become difficult. However, we have run all analysis with disposable market income, and the trends shown below are essentially the same.[10] Using disposable income lets us endogenize changing tax, transfer, and social policy environments for different cohorts. In addition, the disposable income variable of the LIS makes it possible to compare countries over time. It cannot capture in-kind benefits, but these tend to be low in conservative and liberal welfare states anyway—possibly with the exception of French childcare.

As control variables, we use International Standard Classification of Education codes for education (reference category is lower secondary or below), thus introducing a dummy each for secondary and tertiary education. We also use dummies for sex (reference is male), partner in household (reference is no partner), number of children (reference is no child, dummy each for one, two, and more than two children) and immigrant status. For the United States, we substitute 'immigrant' by 'African American' to indicate the effect of belonging to a disadvantaged minority. We have uploaded descriptive statistics for all variables as an online annex.[11] In the following, we first look at detrended cohort effects.[12]

Results

Detrended Cohort Effects

The following regression table (Table 2) shows the detrended cohort effects without and with control variables for German, French, and US cohorts.

The effects of belonging to different cohorts in model 1 (West Germany), model 3 (France), and model 5 (the United States) are displayed net of non-linear effects of age and period. The control models (model 2 for Germany, model 4 for France, and model 6 for the

Table 2. Cohort effects on disposable income in West Germany, France, and the United States

	West Germany				France				United States			
	1 no controls		2 controls		3 no controls		4 controls		5 no controls		6 controls	
Cohort 1920	−0.0487**	(−2.70)	−0.0339	(−1.92)	−0.0648***	(−4.93)	−0.0895***	(−7.23)	0.0126	(1.24)	0.0372***	(4.11)
Cohort 1925	−0.0137	(−0.94)	0.00103	(0.07)	−0.0305**	(−3.26)	−0.0494***	(−5.62)	0.00734	(0.92)	0.0253***	(3.48)
Cohort 1930	−0.0132	(−1.08)	−0.0190	(−1.66)	−0.0317***	(−3.67)	−0.0284***	(−3.48)	−0.0146	(−1.92)	−0.000440	(−0.06)
Cohort 1935	−0.0120	(−1.09)	−0.00172	(−0.16)	−0.00583	(−0.73)	0.00513	(0.68)	−0.0174**	(−2.61)	−0.0103	(−1.72)
Cohort 1940	0.0171	(1.85)	0.00340	(0.39)	0.0396***	(4.88)	0.0573***	(7.54)	−0.00938	(−1.38)	−0.0156*	(−2.49)
Cohort 1945	0.0744***	(7.08)	0.0584***	(5.89)	0.0847***	(10.32)	0.0930***	(12.21)	0.0156*	(2.52)	−0.0154**	(−2.79)
Cohort 1950	0.0600***	(6.25)	0.0462***	(5.14)	0.101***	(14.71)	0.101***	(15.93)	0.0206***	(3.61)	−0.0278***	(−5.40)
Cohort 1955	0.0180	(1.94)	0.00238	(0.28)	0.0401***	(6.58)	0.0542***	(9.67)	−0.00303	(−0.63)	−0.0378***	(−8.85)
Cohort 1960	−0.00189	(−0.20)	−0.00597	(−0.67)	0.00917	(1.59)	0.0339***	(6.40)	−0.0175***	(−4.19)	−0.0316***	(−8.37)
Cohort 1965	−0.0102	(−1.00)	−0.0165	(−1.70)	−0.0352***	(−5.55)	−0.0164**	(−2.85)	−0.00440	(−1.05)	−0.00413	(−1.09)
Cohort 1970	−0.0241*	(−2.15)	−0.0169	(−1.63)	−0.0563***	(−7.78)	−0.0586***	(−9.03)	0.00186	(0.36)	0.0184***	(3.93)
Cohort 1975	−0.0457**	(−2.85)	−0.0176	(−1.17)	−0.0499***	(−5.66)	−0.103***	(−12.85)	0.00841	(1.29)	0.0621***	(10.64)
Age 25 years	−0.0805***	(−6.45)	−0.134***	(−10.46)	−0.0109	(−1.51)	−0.0291***	(−4.17)	−0.0980***	(−16.38)	−0.134***	(−24.18)
Age 30 years	−0.0521***	(−5.86)	−0.0617***	(−7.16)	−0.0237***	(−4.32)	−0.0245***	(−4.87)	−0.0858***	(−19.89)	−0.0860***	(−22.12)
Age 35 years	−0.0188*	(−2.19)	0.00430	(0.52)	−0.0238***	(−4.42)	−0.00387	(−0.77)	−0.0353***	(−8.20)	0.00372	(0.94)
Age 40 years	0.0253**	(2.79)	0.0648***	(7.57)	0.000151	(0.03)	0.0232***	(4.15)	0.0452***	(10.31)	0.0773***	(19.51)
Age 45 years	0.101***	(11.50)	0.140***	(17.19)	0.0471***	(7.09)	0.0530***	(8.52)	0.124***	(24.27)	0.134***	(28.31)
Age 50 years	0.124***	(12.77)	0.140***	(14.91)	0.0564***	(7.72)	0.0479***	(7.07)	0.161***	(28.78)	0.145***	(28.85)
Age 55 years	0.0758***	(7.71)	0.0665***	(7.19)	0.0148	(1.93)	−0.00411	(−0.57)	0.104***	(16.91)	0.0771***	(13.81)
Age 60 years	−0.0242*	(−2.44)	−0.0455***	(−4.87)	−0.0196**	(−2.68)	−0.0414***	(−5.99)	0.0179**	(3.10)	−0.00533	(−1.00)
Age 65 years	−0.0610***	(−6.09)	−0.0846***	(−8.99)	−0.0137	(−1.95)	−0.0155*	(−2.35)	−0.0664***	(−12.07)	−0.0670***	(−13.39)
Age 70 years	−0.0899***	(−7.75)	−0.0893***	(−8.12)	−0.0268***	(−3.65)	−0.00557	(−0.81)	−0.166***	(−27.27)	−0.145***	(−25.91)
Period 1985	−0.0142**	(−3.19)	−0.0154***	(−3.64)	−0.00687*	(−2.04)	0.000918	(0.29)	0.0109***	(3.76)	0.0195***	(7.44)
Period 1990	0.0297***	(5.05)	0.0311***	(5.52)	−0.0274***	(−6.08)	−0.0289***	(−6.75)	−0.000158	(−0.05)	−0.00857**	(−2.92)
Period 1995	−0.0223**	(−3.06)	−0.0203**	(−2.94)	0.0459***	(11.32)	0.0376***	(10.07)	-0.0436***	(−14.89)	−0.0492***	(−18.42)
Period 2000	0.0121*	(2.39)	0.00885	(1.93)	0.0179***	(4.68)	0.00801*	(2.33)	0.0440***	(16.61)	0.0461***	(19.26)
Period 2005	−0.00540	(−1.30)	−0.00426	(−1.12)	−0.0295***	(−9.82)	−0.0176***	(−6.38)	−0.0111***	(−4.85)	−0.00782***	(−3.76)
Cohort trend	0.443***	(22.09)	0.389***	(20.39)	0.533***	(34.82)	0.361***	(24.47)	0.542***	(38.72)	0.420***	(32.93)
Age trend	0.183***	(16.88)	0.161***	(14.96)	0.231***	(28.17)	0.210***	(25.46)	0.235***	(32.03)	0.179***	(25.92)
Secondary education			0.131***	(17.09)			0.212***	(45.41)			0.379***	(79.48)
Tertiary education			0.393***	(41.23)			0.581***	(93.09)			0.751***	(149.85)
Female			0.00761	(1.28)			0.00377	(0.91)			−0.0434***	(−14.51)
1 child			−0.0889***	(−11.36)			−0.0515***	(−8.43)			−0.0576***	(−13.95)
2 children			−0.183***	(−21.96)			−0.132***	(−19.99)			−0.175***	(−40.77)
>2 children			−0.245***	(−24.71)			−0.220***	(−30.66)			−0.342***	(−64.74)
Partner in household			0.298***	(31.13)			0.259***	(42.42)			0.352***	(89.56)
Immigrant			−0.202***	(−23.97)			−0.146***	(−19.01)			−0.175***	(−47.44)
Constant	9.842***	(2,538.23)	9.544***	(785.84)	9.784***	(3,726.85)	9.484***	(1,368.60)	10.24***	(4,956.49)	9.699***	(1,674.37)
N	46,225		44,182		72,886		70,257		354,527		338,539	

t statistics in parentheses: * $P<0.05$, ** $P<0.01$, *** $P<0.001$.

United States) add control variables. Looking at the independent variables from top to bottom shows that, in the no-control models (1, 3, and 5), belonging to the first cohort, born around 1920, yields an average disposable income 4.9 per cent below the long-run income trend in West Germany (the label 'Cohort 1920' means having been born between 1915 and 1920). The 1945/1950 cohorts are most advantaged in all countries before controls are introduced. They are 7.4/6 per cent above the income trend in West Germany, 8.5/10.1 per cent in France, and 1.6/2.1 per cent in the United States. The age variables, which are also controlled for linear trends, have the expected values for all countries: disposable income always peaks around age 45–50. The period effects control that incomes during some times are above or below what one would expect, given long-run economic growth. The variables 'Cohort Trend' and 'Age Trend' control for linear increases and decreases of disposable income for different cohorts and during an individual's life; the APCT model will focus on these cohort trends.

The variables that follow in Table 2 control the effect on disposable income of being a female head of household, of having one, two, or more than two children, of having a secondary or tertiary education, of living with a partner, and of being an immigrant (of having non-White skin colour in the United States). To illustrate the regression results, Figure 1 plots the cohort effects for the three countries, before (graphs towards the left) and after (graphs towards the right) including control variables.

In Germany, before including controls, the average disposable income of cohorts born in 1920 and 1975 are, respectively, 4.9 and 4.6 per cent below the income trend, while the disposable incomes of the 1945/1950 cohorts are 7.4/6 per cent above it. After including controls, the only result that remains significant, however, is that the disposable income of the 1945 and 1950 cohorts is 5.8 and 4.6 per cent above the income these cohorts would have if they had participated equally in increases of disposable income (the linear trend). The comparison with France shows that these cohort effects of around 5 per cent are not strong.

In France, before including controls, the disposable income of the best-off 1950 birth cohort is 10.1 per cent above the trend, while the worst-off 1920 and 1970 cohorts are 6.5 and 5 per cent below the income trend. France is different from Germany, as French cohorts are even more above or below the trend after including controls, when the 1950 cohort is still 10.1 per cent above the trend, but the first and last 1920 and 1975 cohorts are 9 and 10.3 per cent below it. This means that, even with the same education and other characteristics, people born in 1950 compared with 1975 have 20 per cent higher disposable incomes than they should, compared with a situation where all cohorts profited similarly from increasing disposable incomes.

US cohort effects are minor compared with Germany and France. In the no-controls condition, the strongest effect is that the incomes of the 1950 cohort are 2.1 per cent above the trend, while those of the 1960 cohort are 1.8 per cent below it. In the controls condition, the income of the latest-born (1975) cohort is 6.2 per cent above the trend, while the income of the worst-off 1955 cohort is 3.8 per cent below it.

Therefore, before including controls, early- and late-born cohorts are disadvantaged in terms of disposable income in France and—to a lesser degree—in Germany, while in the United States there is no clear trend before including controls. After including controls, German cohort effects largely vanish—only the 1945/1950 cohorts are slightly advantaged. In France, however, after including controls, the incomes of mid-20th-century cohorts are about 20 percentage points above the linear income trend, compared with the incomes of the most disadvantaged 1920 and 1975 cohorts. The opposite is true for US cohorts after including controls. Here, early- and late-born cohorts fare better than cohorts born in the middle of the 20th century.

Including only education as a control variable, the graphs have similar shapes compared with when all control variables are included (not shown here). Education is the main effect that weakens cohort effects in Germany, strengthens them in France, and turns them around in the United States. To understand the strength of cohort effects relative to other significant control variables (thus not including sex), we compare them graphically for the three countries.

Figure 2 illustrates how cohort membership scarcely influences income in the United States and Germany, while having a strong influence in France. For Germany, the strongest cohort effect (5.8 per cent more income), of being born in 1945, is much smaller than all significant control variables.[13] In the United States, the strongest cohort effect is 10 per cent, which is the difference between the most advantaged 1975 cohort (6.2 per cent above the disposable income trend) and the 1955 cohort (3.8 per cent below). This is more than the effect of having one child (−5.8 per cent), but it is weaker than all other significant control variables. In France, members of the most advantaged 1950 cohort are 20.4 percentage points above the trend for disposable incomes, compared with the most disadvantaged 1975 cohort. This effect is comparable with having a secondary education compared with lower secondary (plus 21.2 per cent) and it is stronger

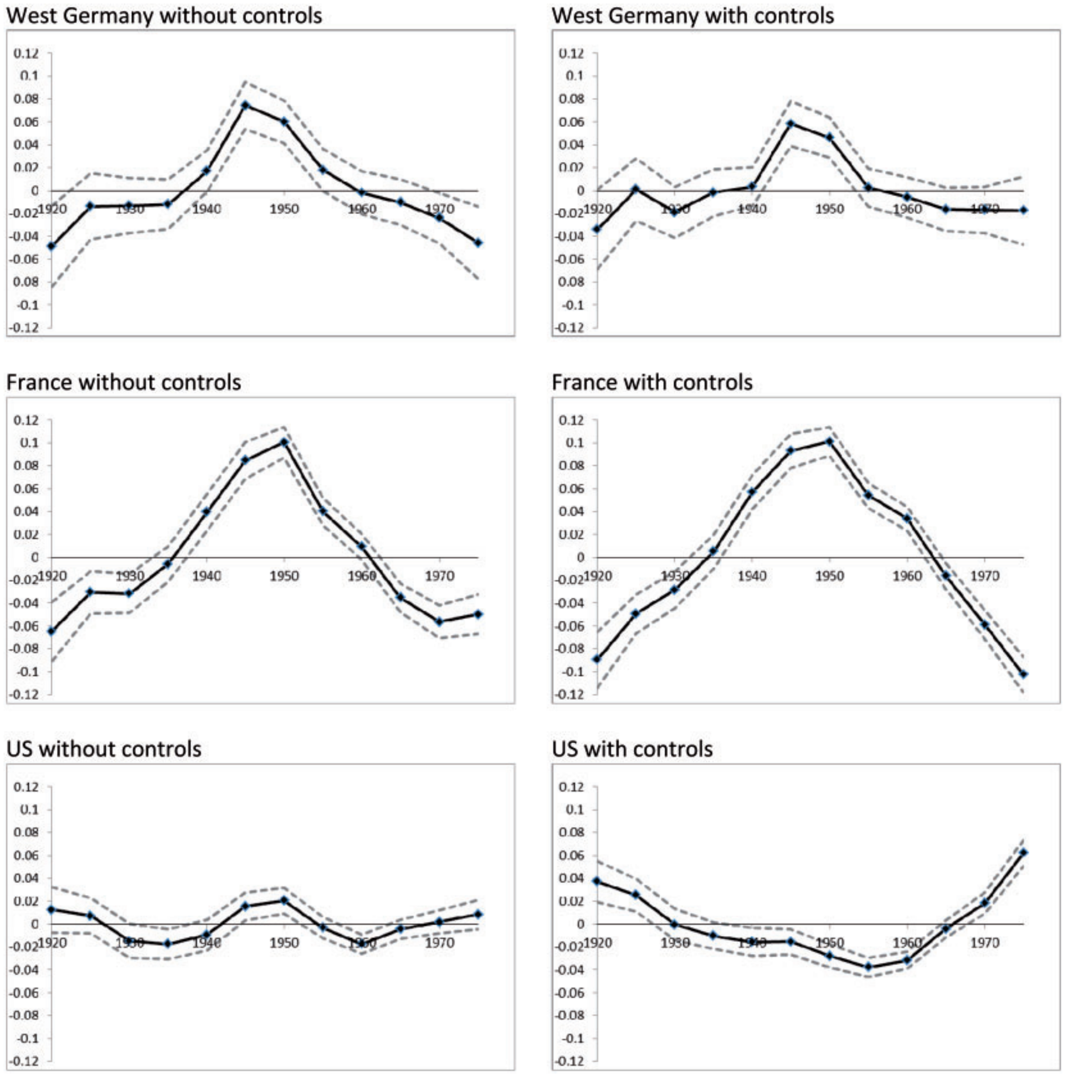

Figure 1. Detrended cohort effects with and without controls

than the effect of being an immigrant (−14.6 per cent). However, the French effect of having a tertiary education (+58.1 per cent) is almost three times as strong. Yet, in France, being part of the most disadvantaged cohort, compared with the most advantaged, diminishes disposable income almost four times as much as having a child (−5.2 per cent), clearly more than having two children (−13.2 per cent) and not quite as much as having three children (−24.5 per cent). Roughly speaking, the incomes of the most advantaged and disadvantaged cohorts are about 5 per cent apart in Germany, 10 per cent in the United States, and 20 per cent in France—after including controls. This means that cohort effects are weaker than all controls in Germany and the United States (apart from having one child), while they are stronger than three out of seven control variables in France.[14]

The APCD model that underlies these cohort effects absorbs linear cohort trends. Thus, it does not ascribe the effect of economic growth to successive cohorts, so that cohorts do not appear richer because of long-run linear economic growth. This makes sense when one is interested in how unequally cohorts participated in an overall trend of increasing incomes. However, this APCD model makes less sense to understand how the income of cohorts changed overall. The following section, therefore, includes the linear trend of increasing disposable income, to understand how it interacts with the cohort effects just shown.

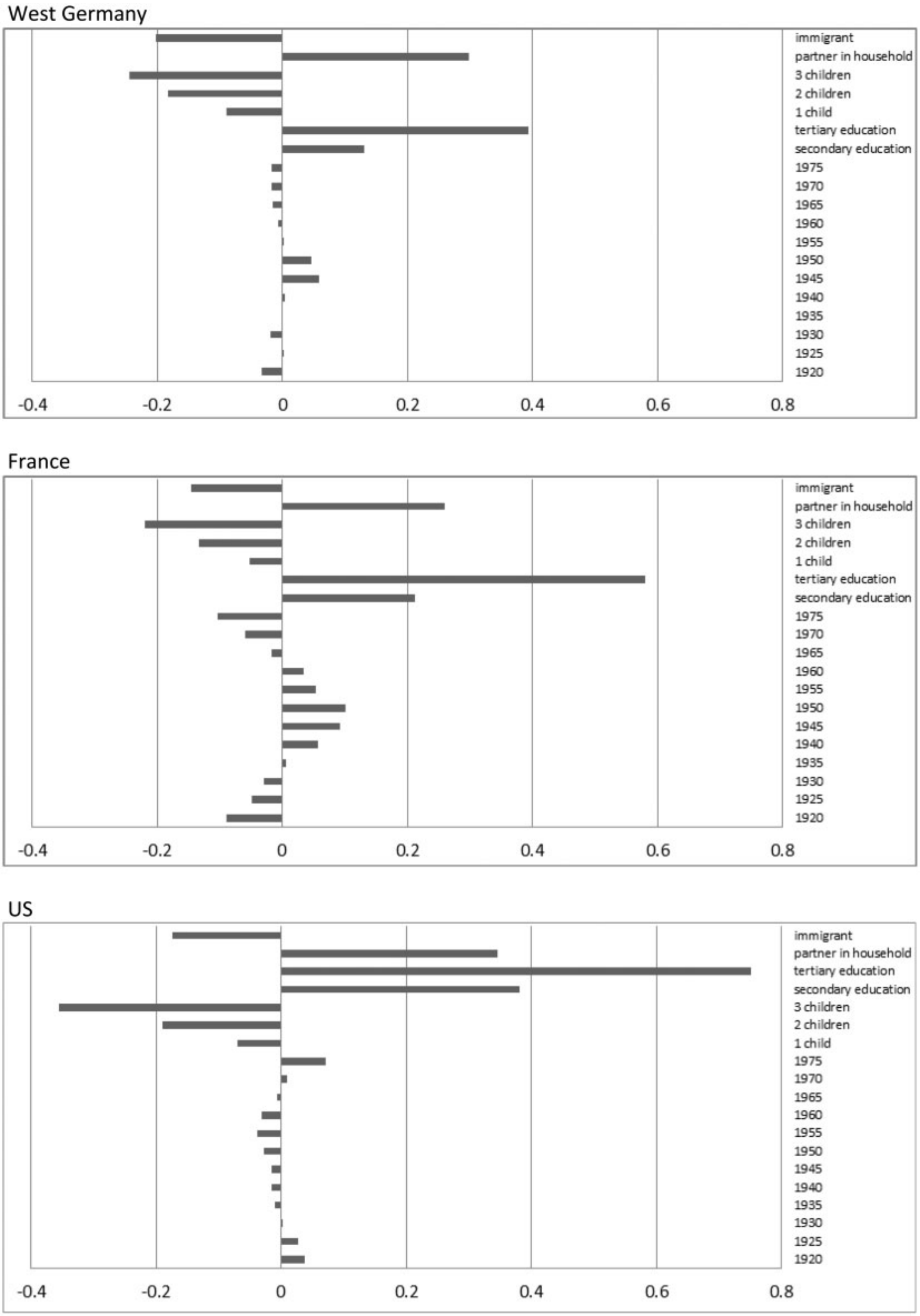

Figure 2. How cohort membership (born in years 1920–1975) and control variables affect incomes in West Germany, France, and the United States

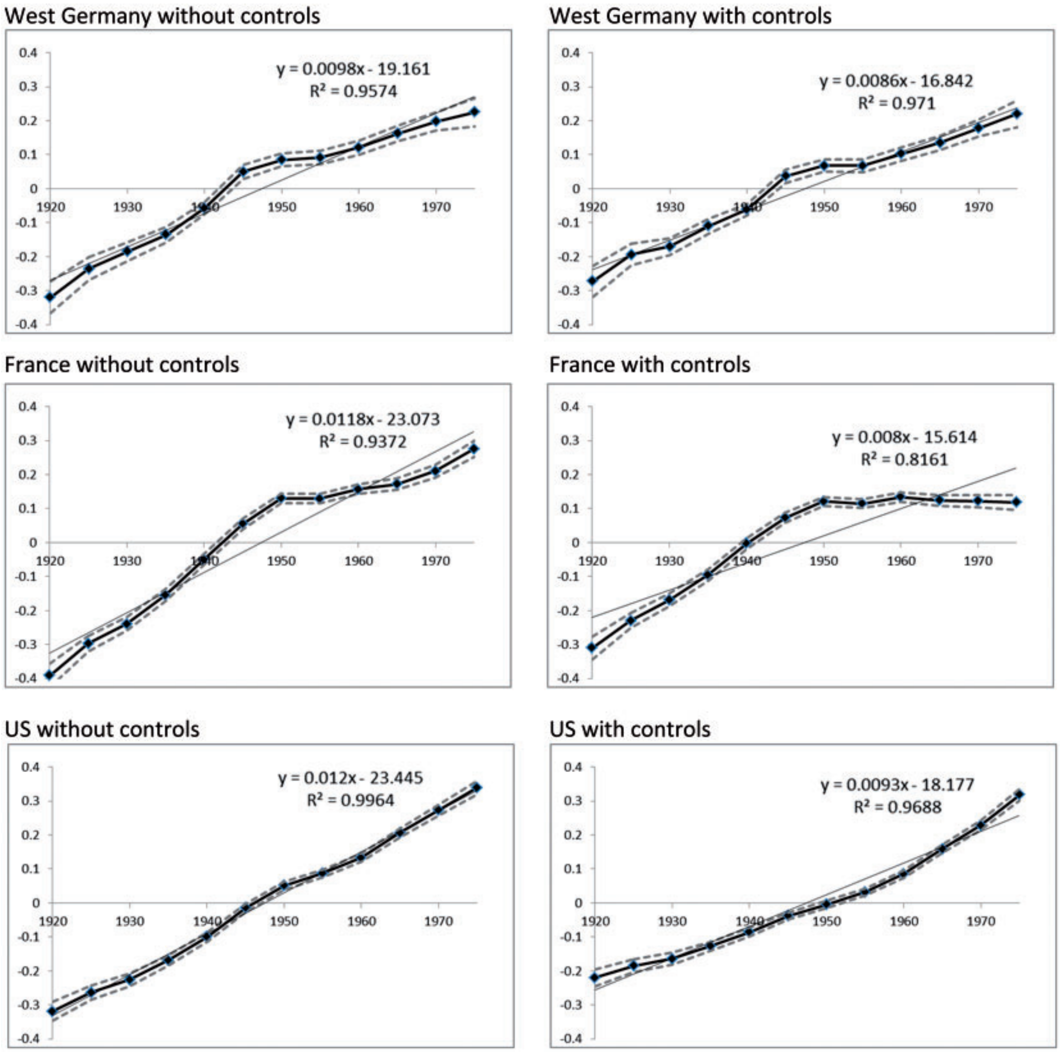

Figure 3. TCEs with and without controls

Trended Cohort Effects

The APCT model ascribes linear trends that are unabsorbed by age to a cohort effect. In the case of incomes, this makes sense in a descriptive way. It cannot tell us whether cohorts have a certain income owing to a period or owing to a cohort effect, but it shows which cohorts have what incomes owing to any possible combination of the two. Thus, the ACPT shows how the disposable incomes of successive cohorts changed due to long-term economic growth and specific cohort effects, while it cannot disentangle which of the two accounts for the changes in income. We do not include the APCT regression table here, as it is exactly the same as the APCD table, with the only difference that the long-term linear trend is now part of the cohort coefficients (the variable rescacoh is suppressed).[15] However, the cohort effect remains controlled for age trends and non-linear age effects. The Figure 3 shows the cohort effects after including the linear disposable income trend.

The linear trend of increasing disposable incomes, which we have controlled in the APCD model, is now captured by the regression line. As Figure 3 shows, inflation-adjusted disposable income increased considerably in all countries. The graphs towards the left, which represent the trend before controls, show that the disposable income of US cohorts correlates almost perfectly with general increases in disposable income, while correlations are weaker for France and Germany.[16] How much different cohorts are above or below the trend is identical to the APCD model. The additional benefit of the APCT model is that it shows that even in France, where cohort disposable incomes are furthest away from a linear increase, they are still strongly related to it when controls are not included ($r^2 = 0.94$).

The graphs towards the right show that, including controls, the incomes of German and US cohorts remain close to general increases in disposable incomes. However, the disposable incomes of French cohorts now clearly diverge from general income trends. While the bumps around the linear trend are not different from the APCD model, the APCT shows that the cohort effect is so strong that after controls, inflation-adjusted incomes for French cohorts born after 1950 are flat. This means that non-linear cohort effects in France are stronger than linear increases in economic growth. Looking at the slope from 1920 to 1950, it becomes apparent that for the cohorts born until 1950, inflation-adjusted incomes increase by an average of 1.5 per cent per year. For cohorts born after 1950, inflation-adjusted disposable incomes are flat. This means that if French cohorts had not improved their education and moved into dual-earner households (the two variables with the strongest income-increasing effect), they would have experienced no gains in disposable income since 1950. The following sections analyse and discuss these results.

Analysis

We find that, before including controls, French mid-20th-century birth cohorts are about 20 per cent above the long-run trend of increasing disposable incomes, compared with late- and early-born 20th-century cohorts. We also find that West German mid-20th-century cohorts are roughly 11 per cent above the disposable income trend when compared with early- and late-born cohorts, while the incomes of mid-20th-century cohorts from the United States are only 2 per cent above the trend of increasing incomes. However, German cohort effects largely result from changing educational levels. After controlling for education and other variables, the 1945/1950 cohorts are only about 5 per cent above the income trend, and no other cohorts deviate from the trend significantly. In France, however, including controls strengthens cohort effects, so that the most advantaged mid-20th-century cohorts enjoy a roughly 20 per cent advantage in disposable incomes over early- and late-born 20th-century cohorts. In the United States, including controls turns the cohort effect around. After controls, the latest, 1975 cohort has incomes almost 10 per cent above the trend when compared with mid-20th-century cohorts.

We thus show that the mere chance of being born into a fortunate versus an unfortunate French birth cohort influences disposable incomes as much as a secondary education, while being born into the most unfortunate versus the most fortunate cohort decreases incomes as much as being an immigrant and almost as much as having three children. Controls show that increased education and moving into dual-earner households increased the incomes of later-born cohorts in Germany, so that they follow the trend of increasing incomes. In France, education has also expanded, but later cohorts still have significantly less disposable income than they would if they had profited from increasing disposable incomes, as other cohorts did.

We then used a second model, which did not detrend disposable incomes for linear increases. This model showed that while the living standard of virtually each successive birth cohort in the United States and Germany increased (even if some cohorts had higher increases than others), French cohort effects are so strong that after controls, successive post-1950 cohorts had no gains in inflation-adjusted disposable income, even though incomes increased overall. Because we looked at disposable income, including public payments and benefits, it is likely that our findings result from cohort-biased social policy. The last section discusses possible reasons why French post-1950 cohorts did not increase their income as German and US cohorts did.

Discussion

Our introduction highlighted how the literature speculates that cohorts born around 1950 in Germany, the United States, and France are systematically advantaged, having profited from an expanding education system, low unemployment, and accommodating working markets. We also presented debates on whether mid-20th-century birth cohorts monopolized lucrative positions and social transfers, thereby excluding younger generations. Our models (apart from the US-controls model) indeed show that cohorts born between 1940 and 1950 have disposable incomes well above what one would expect if all cohorts had equally participated in long-run increases in disposable incomes. For Germany, our results not only correct estimates of scholars who claim that cohorts born in the late 1940s and early 1950s are most advantaged (Lauterbach and Sacher, 2001; Mayer and Hillmert, 2004). More importantly, our data show that much of this alleged cohort effect results from cohort correlates in Germany. In this sense, we agree with studies that claim that belonging to a certain cohort in Germany is not as influential for one's living standard as e.g. class membership (cf. May, 2012: p. 20).

For the United States, our data contradicts arguments that some cohorts are born to pay for others (Longman, 1987; Kotlikoff, 1992. In both the United States and Germany, net of education and other controls, cohorts seem to share fairly equally in economic growth. Our results thus indicate that Germany and the United States are fair in intergenerational terms. However, our data are alarming for France. After including controls, inflation-adjusted incomes of French cohorts born before 1950 increased on average by 1.5 per cent annually, while cohorts born after 1950 have no increase in disposable incomes after adjusting for inflation. These results complement the bleak literature on French cohorts (Baudelot and Gollac, 1997; Chauvel, 1997a,b, 2010a; Anguis, Cases and Surault, 2002. They contradict the literature that sees a catch up of later-born French cohorts (Bonnet, 2010), though our data only reaches until 2004.

While our aim was to show, and not to explain this, our data fit the mechanisms that the welfare state literature describes. The French welfare state is 'dualized', as the French social insurance system traditionally covered older cohorts fairly comprehensively, while newer cohorts have to cope with a system that covers fewer workers with fewer benefits (Palier, 2010: p. 96f.). In addition, France lacks Germany's vocational training system to integrate young cohorts into an already-difficult working market.

Overall, the insider–outsider dynamic that marks the Mediterranean welfare regime (Ferrera, 1996, 2010) seems like a good candidate to explain the pronounced cohort differences that separate France from Germany and the United States. Thus, while some studies deny generational conflicts in Europe (Attias-Donfut and Arber, 2000: p. 18), our study shows that generations in France have every reason to be in conflict, as the young and the old are disadvantaged compared with mid-20th-century birth cohorts.

In this sense, our results for France are alarming; they indicate that older generations have monopolized lucrative positions and social transfers, to the detriment of generations born after 1950. It does not seem unreasonable to assume that the 'young' (in 2014, this comprises everyone aged <64 years) will not accept the stagnation of their disposable incomes indefinitely. To better understand the reasons behind the cohort differences we show, more research on the effect of the welfare state on cohorts is needed. We hope our results will inspire such research and raise awareness of how much the coincidence of being born into a certain birth cohort influences living standards.

Notes

1 Yang and Land (2013) critique the 'conventional wisdom' of Holford (1985) and Rodgers (1982), who argue that only the non-linear components of APC models are estimable. They want to find interpretable trends of age, period and cohort, a quest that Glenn (1976) found 'futile'.

2 The APCD is available as a Stata ado file.

3 In biostatistics, this model had been developed by Holford (1983, 1991) on a Poisson model of vital statistics. His aim was to detect cohort deviations from linear trends.

4 Because APCD, like APC-IE, is based on a constrained general linear model, it allows any kind of standard specification, including Ordinary Least Squares, Log, Logit, or Poisson models; it also allows control variables that could mediate cohort effects (gender, education, occupation, etc.).

5 The constraint $\text{Slope}_a(\alpha_a) = 0$ means the trend of the age effect is zero and is true only if $\Sigma_a\ [(2a - a_{min} - a_{max})\ \alpha_a] = 0$. This constraint is easily expressed as a linear equation of coefficients. Holford (1991: p. 454) gave a similar expression for his zero-slope coefficients.

6 An alternative way to get the detrended coefficients is to run the model with a single identifying constraint and to regress the coefficients for each of the three factors on time, to then ask for the residuals (we thank an anonymous reviewer for this suggestions). This model yields essentially the same results as our APCD.

7 The underlying 'long-term generational progress' assumption argues that we expect later cohorts to benefit from technical, economic, and social progress of the past. Immanuel Kant (1784) was the first to highlight this.

8 We rely on measures of inflation from the World Bank and deflate all incomes to 2005 values.

9 For a comparison of results that are equalized by household members to results that are not, compare for the online annex file with the heading 'Comparison of equalized and non-equalized household incomes'. We cluster standard errors on the level of the individual and can also cluster them on the household level, which does not change our results. For a comparison, see the online annex with the heading 'Cluster errors at household-level'.

10 See the online annex with the heading 'Comparison market and dpi income'.
11 See the uploaded online annex file with the heading 'Descriptive statistics of the variables used'.
12 Stata code for the LIS calculations is uploaded in the online annex with the first line 'Code APC-D W Ger, Fr, US with and without controls'.
13 We checked our findings for Germany including the Eastern Länder and the results are similar (not shown here).
14 However, the control variables are relative to each opposed category, e.g. immigrants are compared with non-immigrants. Yet, the cohort variables are relative to the mean, e.g. the cohort variables add up to zero. This makes the cohort effect look smaller.
15 However, compare for the online annex that we uploaded, where we show the Stata code that we used for our calculations 'Code APC-T W Ger, FR, US, with and without controls'.
16 We narrowed the US sample to 20 per cent its real size, to check whether the small confidence intervals are due to the large US sample size, but we still got similar results. As in the APCD, our results remain the same when looking at Germany including the New Länder (not shown here).

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Supplementary Data

Supplementary data are available at *ESR* online.

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