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Age structure effects and growth in the OECD, 1950–1990

Thomas Lindh, Bo Malmberg

Institute for Housing Research, Uppsala University, Box 785, SE-801 29 Gävle, Sweden
(Tel.: +46-26-4206526 resp. 4206512; Fax: +46-26-4206501;
e-mail: thomas.lindh@ibf.uu.se, bo.malmberg@ibf.uu.se)

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Abstract. Economic growth depends on human resources and human needs. The demographic age structure shapes both of these factors. We study five-year data from the OECD countries 1950–1990 in the framework of an age structure augmented neoclassical growth model with gradual technical adjustment. The model performs well in both pooled and panel estimations. The growth patterns of GDP per worker (labor productivity) in the OECD countries are to a large extent explained by age structure changes. The 50–64 age group has a positive influence, and the group above 65 contributes negatively, while younger age groups have ambiguous effects. However, the mechanism behind these age effects is not yet resolved.

JEL classification: J11, O40, O57

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1. Introduction

Few economists would, in principle, deny that changes in age structure should affect economic growth rates. In practice, however, age structure variables have played a subordinate role in empirical macroeconomics. There are ex-

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ceptions though, for example Fair and Dominguez (1991). Generally, it has been assumed that changes in age structure are too small and too slow to have a noticeable effect on economic aggregates.

We challenge this assumption. Based on 5-year data our study shows that the age structure has substantial effects on per-worker growth rates. Between 1950 and 1990, in the OECD countries, there is a strong positive correlation between initial population shares of upper middle-aged people (50–64 years) and growth in the following period, and a strong negative correlation between growth and the population shares of old-age people (65 years and older).

The presence of age effects on economic growth rates cannot be dismissed as spurious correlations. Our results are not driven by outliers, they are robust to changes in the sample period, and there are no serious problems with serial correlation or heteroskedasticity. Nor are the results very sensitive to the inclusion of explanatory variables used in other recent studies of cross-country growth. If added to the regressions, some control variables – including measures of education, financial development, trade structure etc. – do have significant effects on growth, but the estimated age effects persist.

Potential problems emanating from country heterogeneity, endogeneity of non-age variables and the dynamic model structure are causes for concern. The basic pattern, however, survives in conventional fixed and random effects estimations. Instrumental variables estimations do not indicate any serious endogeneity problem. Attempts to circumvent the dynamic bias problem using generalized methods-of-moments estimation does tilt the pattern somewhat, but the total impact of the age structure is reinforced. Due to multicollinearity between the age variables the magnitude of the coefficients and the exact pattern remain somewhat uncertain.

In sum, the precision and consistency of our estimated coefficients are not beyond doubt, but the general impact of age structure on growth survives a very diverse array of sensitivity tests. That the age structure as a whole does have a both statistically and economically significant impact on growth in our sample is, in our view, hard to dispute.

Our empirical approach is based on a human-capital-augmented Solow (1956) model where age structure is a component of actually available human capital. We derive an expression for transitional growth rates conditioned on the saving rate, work-force growth, initial income, and the size of the human capital stock, using an approach inspired by Mankiw et al. (1992). We make one essential modification by allowing for heterogeneity in the rate of technical change. An important conclusion from the theoretical analysis is that changes in the explanatory variables will engender shifts in transitional growth rates. The sensitivity of transitional growth rates to changes in the parameters implies that growth regressions based on means taken over long periods is a less satisfactory approach for countries that experience substantial shifts in their age structure. Our analysis, thus, adds further to the arguments advanced by, for example, Islam (1995), Brander and Dowrick (1994), and Caselli et al. (1996), that a panel data approach should be preferred to pure cross-section regressions on international growth data.

In this paper we treat age structure change as an exogenous factor. Clearly, a complete model should have included not only demographic effects on economic growth but also the feed-back effects of economic change on demographic behavior. The development of such a model, however, falls outside the scope of this paper, where the aim is to provide new empirical evidence on the old question of how demographic change affects economic development. This simultaneity problem is taken care of by using predetermined age-share measures.

In Sect. 2 we present our model. Section 3 contains our empirical results and discussion. Conclusions are in Sect. 4.

2. Growth effects of a shifting age structure – A model

The transitional growth model developed by Mankiw et al. (1992) – henceforth MRW – posits that human capital is produced by educational investment alone. Microeconomic evidence, however, indicates that experience plays a key role in human capital formation. On the macro-level this implies that a country with an experienced work-force will, ceteris paribus, have more human capital than a country with an inexperienced work-force. We allow for an experience effect on aggregate human capital by a composite measure of human capital interacting the stock of educational capital with a Cobb-Douglas index, N, of the age structure,

$$N = \prod_i n_i^{a_i} \tag{1}$$

where n_i is the population share of age group i. This specification has the advantage of being tractable and easily incorporated with conventional specifications like MRW.

The index is designed to catch other potential mechanisms besides pure experience effects. Although this may be regarded a disadvantage, too, by not allowing us to discriminate between competing impact mechanisms, our main empirical purpose is to explore if there are any significant age effects at all. We, therefore, prefer a broad specification to a more specialized.

By choosing a Cobb-Douglas index we also acknowledge that there might be limited substitutability between age groups (Murphy et al. 1988). We furthermore include dependent age groups in this index to allow for the fact that the amount of human capital actually supplied to the market may vary with the dependency burden of households; for example, more children may decrease female labor force participation. This also catches other possible interactions between growth and age structure through, e.g., demand effects.

With this definition, a Cobb-Douglas production function in terms of output per worker, y, can be written

$$y = Ak^{\alpha}(hN)^{\beta} \quad 0 < \alpha < 1, \quad 0 < \beta < 1 \quad \text{and} \quad 0 < \alpha + \beta < 1 \tag{2}$$

where k is physical capital per worker, and h is educational capital per worker.[1] A denotes the technology level.

Following MRW we assume that capital accumulation, both physical and educational, is governed by the standard dynamic equation, taking the saving rates s_k and s_h as exogenous[2]

$$\dot{k} = s_k y - (\delta_k + w)k \quad \text{and} \quad \dot{h} = s_h y - (\delta_h + w)h \tag{3}$$

where δ_k and δ_h are constant depreciation factors and w is the exogenous growth rate of the work-force. MRW assume technology to be the same for all countries, but we assume that the technology factor A converges to an exogenous world technology[3], A^*, only gradually

$$\dot{A} = \gamma(A^* - A). \tag{4}$$

The adjustment rate γ is assumed to depend on the productivity gap between best-practice technology and currently used technology.[4]

Like MRW we simplify to a common depreciation factor $\delta = \delta_k = \delta_h$, but we also assume a common saving rate, i.e., $s = s_k = s_h$. Although a drastic assumption, it has some support in attempts to measure human capital investments.[5] This leads to equal steady state stocks of physical and human capital in real value terms:

$$h^* = \left(\frac{s}{\delta + w} A^* N^\beta\right)^{1/(1-\alpha-\beta)} = k^*. \tag{5}$$

The proportional growth rate for an economy in transition to steady state can be approximated (for details see the appendix) by

$$\frac{d \ln y}{dt} = \lambda(\ln y^* - \ln y) + u \tag{6}$$

with u denoting the error made and $\lambda = \tilde{\gamma}(\delta + w)(1 - \alpha - \beta)$, where $\tilde{\gamma}$ is proportional to γ, the technological adjustment rate.

Inserting steady state stocks and dividing through by $\Gamma = \tilde{\gamma}(\delta + w)$ the basic growth equation can be written

$$\frac{g}{\Gamma} = \ln A^* + (\alpha+\beta)[\ln s - \ln(\delta+w)] - (1-\alpha-\beta)\ln y + \beta \ln N + \frac{u}{\Gamma}. \tag{7}$$

The interpretation of (7) is straight-forward. If there is no change in age structure, saving rate, depreciation, work-force growth or potential technology level, the economy will eventually come so close to steady state that growth practically stops. However, changes in these variables shift the steady state income and, consequently, the transitional growth rate. One implication is that variations in age structure will also imply variations in the transitional short-run growth rate. Note that the impact on the growth rate is country- and time-specific since it is proportional to Γ. This is in fact an important motive for introducing technology adjustment since it allows for some country heterogeneity, albeit in a fairly restrictive form.

Empirical specification

Is the theoretical model consistent with empirical data? To answer this question a regression model based on equation (7) has been tested on five-year data for the OECD countries from 1950 to 1990:[6]

$$\frac{g_{tj}}{\Gamma_{tj}} = b_0 + b_1 \ln i_{tj} + b_2 \ln(\delta + w_{tj}) + b_3 \ln y_{tj} + b_4 \sum_m a_m \ln n_{mtj} + b_5 \frac{1}{\Gamma_{tj}} + \varepsilon_{tj}. \tag{8}$$

In addition to growth rate variation across countries, this specification also allows for variation over time. Shifts in steady state are thereby taken into account.

The growth rate of real GDP per worker over period t for country j is defined as $g_{tj} = \ln y_{t+1,j} - \ln y_{tj}$. This growth rate, divided by the country- and period-specific convergence term Γ_{tj}, is influenced by (*i*) the saving rate, measured by the average investment share i_{tj} over the period[7], (*ii*) the average growth rate of the work force, w_{tj}, over the period, and a fixed rate of depreciation δ (constant with a stylized value of 0.03), (*iii*) the initial level of GDP per worker, y_{tj}, in the period, (*iv*) the age group shares at the beginning of the period, n_{mtj}. We also add the inverse of the convergence term, $1/\Gamma_{tj}$, to take care of a non-zero mean in the approximation error, u. World technology, $\ln A^*$, is estimated in the constant and cannot be separately identified.

Our measures of age structure distinguish four important phases in the adult life cycle: young adulthood, prime age, middle age, and old age. Thus, population shares for the age groups 15–29, 30–49, 50–64, and 65+ years are included in the age index. Together these variables capture most of the age structure variation in the OECD countries in the post-war period and have sufficient individual variation to allow identification of distinct age effects. The youngest age group, children aged 0–14, had to be dropped in order to avoid high degrees of linear dependency among the age variables. Some arbitrariness in the definition of the age group variables cannot be avoided. Therefore, estimations with alternative specifications have been carried out that show essentially the same pattern of age effects as those presented below. These and other estimates referred to in the text but not reported are available from the authors upon request.

Moreover, the rate of technological adjustment is proxied by the relative gap in GDP per capita to the world technological leader, taken to be the United States during the post-war period. This implies that $\gamma_{tj} = \pi(\tilde{y}_{t,US} - \tilde{y}_{tj})/\tilde{y}_{t,US}$, where π is a constant proportionality factor. Our definition of γ_{tj} makes it necessary to exclude the United States and Switzerland (which in 1970–1975 had a higher GDP per capita than the United States) from the sample.[8]

3. Estimation results

The estimation results from pooled regressions, presented in Table 1, show that the model fits OECD data well. If we look first at the non-age variables, we note that the investment share, work-force growth, and initial income all have parameters with the expected sign. High investment rates have a positive

Table 1. Pooled regression estimates with the basic growth model

Full sample: 168 obs	Base regression				All variables
Dep. variable: g/Γ	Unrestr.	Restr.	Period	OECD	mult. by $\sqrt{\Gamma}$
Constant	2.98	6.61	8.00	35.35	−0.13
	(0.42)	(1.96)	(2.50)	(4.73)	(0.73)
$1/\Gamma$	0.015	0.016	0.016	0.016	0.019
	(5.80)	(6.03)	(6.96)	(6.68)	(2.34)
$\ln n_{15-29}$	−0.82	−0.83	−0.56	0.20	−1.68
	(0.90)	(0.93)	(0.69)	(0.20)	(2.50)
$\ln n_{30-49}$	0.25	0.38	0.65	0.99	−0.48
	(0.22)	(0.34)	(0.61)	(0.89)	(0.58)
$\ln n_{50-64}$	3.56	3.49	2.59	2.36	3.23
	(3.65)	(3.61)	(2.65)	(2.20)	(3.75)
$\ln n_{65+}$	−2.34	−2.03	−1.52	−1.11	−2.44
	(3.24)	(3.77)	(2.80)	(1.55)	(4.93)
$\ln i$	1.34				
Restr: $\ln i - \ln(\delta + w)$	(3.55)	1.60	1.54	1.50	1.70
$\ln(\delta + w)$	−1.92	(5.05)	(5.55)	(5.27)	(7.69)
	(2.97)				
$\ln y_0$	−0.64	−0.80	−0.95	−0.89	−0.45
	(1.93)	(4.02)	(5.02)	(4.14)	(2.66)
1960–1970, 1985–1990			0.78		
(dummy)			(4.74)		
$\ln n_{50-64}^{OECD}$				17.29	
				(4.32)	
$\ln n_{65+}^{OECD}$				−3.26	
				(2.65)	
adj R^2	0.566	0.567	0.622	0.610	0.437
χ^2 age shares	0.000	0.000	0.007	0.123	0.000
$\chi^2 \ln i = -\ln w$	0.478		0.492	0.512	0.013
F-test: country eff	0.403	0.331	0.374	0.440	0.252
F-test: time eff	0.001	0.001	0.850	0.436	0.000
$\alpha + \beta$ implied	0.677	0.666	0.620	0.628	0.792
$\tilde{\lambda}$ implied	0.015	0.017	0.020	0.019	0.011

Notes: Pooled OLS estimates with Newey-West error estimates corrected for heteroskedasticity and autocorrelation. Absolute t-values in parentheses. Wald test p-values for joint significance of the four age shares and the restriction $b_1 = -b_2$. F-test p-values for common country and time intercepts in the residuals. $\tilde{\lambda}$ is the mean convergence parameter with $(1 - \alpha - \beta)b_5$ added to compensate for the approximation error.

effect on growth, whereas work-force growth (because of capital dilution) and initial income (because of convergence[9]) have a negative effect on growth rates. The estimated mean approximation error is significantly different from zero. The point estimate 1.6% is in line with the theory since the mean of the error should mainly catch U.S. growth. See the appendix for details.

In Table 1 ordinary least squares point estimates with heteroskedastic and autocorrelation consistent errors – see Newey and West (1987) – are presented. White's heteroskedasticity tests indicate that more efficient estimates could, in principle, be obtained by re-weighting the regressions. Tests with our data have shown, however, that in practice such a procedure will not change the results appreciably.[10]

Nor is serial correlation any serious problem in our data. We have found significant first order autocorrelation only for a few countries and elimination of these countries from the sample leaves the results essentially unchanged.

The model has one directly testable restriction. According to equation (7) the investment share and work-force growth parameters should be of the same size, but with different signs. This restriction is not rejected. The estimated total capital share, $\alpha + \beta$, is close to the common stylized value of two-thirds. This is in line with the findings of MRW and other researchers. In addition, the estimate of the mean rate of convergence is in line with the findings of most other researchers, even if this may – as Caselli et al. (1996) claim – be due to a dynamic bias. When the approximation error is compensated for – see appendix – the implied rate of convergence lies between 1 and 2% in the pooled base regressions, increasing to around 4% in the panel regressions below.

With respect to the age-share variables we note, first, that they are jointly significant according to the Wald test. That is, our hypothesis of age structure effects on economic growth is not rejected by the data. The age parameter estimates also show the general hump shape pattern that micro-studies of human capital accumulation lead us to expect. Surprisingly, the two younger age groups do not have significant positive effects on growth. Instead, it is only the middle-age share which is significantly positively related with growth. If other variables are kept constant, the typical effect of a one percentage point increase in the middle-age share would be around a quarter to a half percentage point increase in growth per worker. The oldest age group, on the other hand, has a significant negative effect on growth.

A negative effect on growth of a large old-age population fits well with conventional ideas. The positive middle-age effect is more surprising. There are, however, many mechanisms by which a large middle-aged population may generate a high steady-state level of income and, hence, high levels of transitional growth. Cross-section studies show that labor income generally peaks around 50 years of age, indicating a peak of per capita human capital supply at that age (see, e.g., Ahlroth et al. 1997). High levels of self-employment among people over 50 years of age point in the same direction. There are also indications that people in their fifties work more intensely than younger colleagues (Ackum Agell 1994). Other mechanisms besides human capital cannot be excluded either. Net wealth is high in the middle-age group, and many middle-aged people have begun to transfer housing wealth into financial assets. In addition, middle-aged people pay high taxes and have a low demand for public services. On theoretical grounds, a positive middle-age effect on growth is, thus, not at all implausible. Moreover, earlier empirical studies of age effects, for example McMillan and Baesel (1990), or Malmberg (1994), support the hypothesis that the middle-age effect on growth is positive, as do a recent study by Bloom and Williamson (1997).

Are the estimated age effects due to omitted variables?

If important determinants of national growth rates are omitted, parameter estimates may be biased. The age effects we estimate might, therefore, be dependent on the exclusion of some fundamental factor affecting national growth. In order to check for this possibility, three different tests have been carried out. First, country-specific and time-specific effects in the residuals have been tested for (see also the next subsection on this issue). Second, models with explicit measures of educational capital have been estimated. Third, we have re-run our regressions with control variables taken from other recent growth studies. Our conclusion is that omitted variables do not explain the presence of age effects in OECD growth data.

The models estimated by pooled regressions do not have significant country effects in the residuals. This indicates that inter-country differences are already adequately accounted for by our proposed model. The country- and time-specific coefficients are obtained by multiplying with Γ_{it}.

Significant time effects in the residuals are still present, though, indicating the existence of OECD-wide influences on growth rates that are not captured by our base model. More specifically, the 1960–1970 and 1985–1990 periods are characterized by significantly higher growth rates than expected from the model. When a dummy for these periods is introduced, the magnitude of the age parameters decreases but the pattern and joint significance remain. Moreover, it may be the case that this time trend in OECD growth is associated with the overall age structure changes of the OECD population. As shown in column 4 of Table 1 an inclusion of OECD averages for the middle-age and old-age shares eliminates significant time effects in the residuals.

If, contrary to our assumption, the accumulation rates for physical and educational capital are not the same, then the exclusion of educational investments variables from our empirical model would become a possible source of bias in our age effect estimates. However, when we re-estimate our model using one stock and one flow measure of educational investment – average schooling years in the population and enrollment rates in secondary and higher education[11] – the age estimates are hardly affected at all (see Table 2). Due to a smaller sample the Barro-Lee data are only available from 1960 (and not for Luxembourg) the precision of the estimates are weaker, but the age variables are still jointly significant. Moreover, we confirm the results of Islam (1995) and Benhabib and Spiegel (1994) that educational variables do not perform as well in panel studies as they do in pure cross-country growth regressions.

In Table 3 the results of a more ad hoc approach to check for omitted variable bias is presented. This table reports selected results for the only ten variables that turned out significant at the 10% level when all the variables in King and Levine (1993) and a selection of demographic, trade, and government variables from Barro and Lee (1993) were included, one at a time, into our base regression.[12] The estimated age parameters are not completely stable when these variables are added. Especially trade variables – which are strongly correlated with time effects – tend to lower the middle-age coefficient. Only in one case of ten – average inflation – is the joint significance of the age variables pushed beyond the ten percent level. The case for claiming that age effects are fragile is, thus, rather weak.

What about other sources for bias?

There are (at least) four other reasons why our estimates may be biased. First, our proxy for the rate of technical adjustment can potentially introduce measurement errors in one of the regressors. Second, $1/\Gamma$ as well as investment

Table 2. Pooled regression estimates with distinct capital accumulation rates. Stock and flow forms

Full sample: 120 obs Dep. variable: g/Γ	Base with age	Human capital stock without age	 with age	Human capital flow without age	 with age
Constant	5.66	10.41	5.06	11.60	8.02
	(1.00)	(4.30)	(0.87)	(4.83)	(1.50)
$1/\Gamma$	0.016	0.016	0.016	0.016	0.016
	(6.15)	(6.51)	(6.17)	(6.41)	(5.90)
$\ln n_{15-29}$	−1.88		−1.88		−1.79
	(1.78)		(1.79)		(1.66)
$\ln n_{30-49}$	0.95		0.97		0.98
	(0.78)		(0.81)		(0.81)
$\ln n_{50-64}$	2.48		2.50		2.62
	(2.08)		(2.11)		(2.16)
$\ln n_{65+}$	−1.96		−2.00		−1.88
	(3.09)		(3.13)		(2.97)
$\ln i - \ln(\delta + w)$	1.94	2.28	1.95	1.79	1.41
	(4.31)	(5.39)	(4.33)	(3.89)	(3.07)
$\ln h^*$		−0.02	−0.12		
		(0.09)	(0.44)		
$\ln s_h - \ln(\delta + w)$				1.08	1.08
				(2.00)	(2.28)
$\ln y_0$	−1.03	−1.47	−0.95	−1.56	−1.17
	(3.59)	(6.44)	(3.12)	(7.06)	(4.33)
adj R^2	0.572	0.545	0.568	0.556	0.579
χ^2 age shares	0.003		0.003		0.003
χ^2 restriction	0.166	0.357	0.173	0.777	0.440
F-test: country eff	0.393	0.334	0.437	0.363	0.411
F-test: time eff	0.012	0.001	0.012	0.003	0.033
α implied		0.604	0.672	0.405	0.386
β implied		0.006	−0.040	0.244	0.295
$\alpha + \beta$ implied	0.653	0.610	0.632	0.649	0.681
$\tilde{\lambda}$ implied	0.021	0.028	0.020	0.028	0.022

Notes: Pooled OLS estimates with Newey-West error estimates corrected for heteroskedasticity and autocorrelation. Absolute t-values in parentheses.Wald test p-values for joint significance of the four age shares. The restriction tested in the first three columns is the same as in Table 1. In the last two columns the null is that the sum of coefficients on capital accumulation terms together are equal to the negative of the work-force growth coefficient. F-test p-values for common country and time intercepts in the residuals. The stock and flow models (see equations (12) and (16) in MRW) are derived with the obvious modifications. $\tilde{\lambda}$ is the mean convergence parameter with $(1 - \alpha - \beta)b_5$ added to adjust for the approximation error.

and work-force growth are likely to be simultaneously determined with GDP growth. Third, initial GDP in the period is also part of the definition of the growth rate causing concern about dynamic bias. Fourth, heterogeneity in parameters may in a dynamic model bias the estimates. All four of these potential problems bias the estimates by introducing a correlation between the regressors and the contemporary error term. In Table 4 we report some estimations designed to assess the importance of these problems. Our ambition here is only to show that there are no obvious indications that the general thrust of our results are affected by these problems.

Measurement error bias from $1/\Gamma$ can be avoided by moving that term to

Table 3. Control regressions

Definition of control variable	Restricted base regressions Control	t-value	χ^2 age p-value	$\ln n_{50-64}$	$\ln n_{65+}$	time effects p-value
Trade balance share	EX–IM	4.69	0.002	1.30	−1.87	0.462
Terms of trade	TOT	2.22	0.001	1.20	−2.25	0.562
Government consumption	GOVSH5	−2.00	0.005	3.05	−1.23	0.010
Growth rate of population	GPOP	2.20	0.022	1.98	−1.12	0.338
Growth private dom assets	GDCPT	2.45	0.060	2.51	−1.62	0.018
Growth rate of government consumption	GGOV	−8.23	0.002	2.16	−1.61	0.334
Initial government consumption share	GOVI	4.81	0.002	2.16	−2.16	0.023
Growth rate of trade share	GTRD	−2.96	0.003	2.14	−1.93	0.222
Growth of exports	GX	−3.90	0.004	2.00	−1.95	0.374
Average inflation	PI	−1.90	0.125	2.04	−1.62	0.054

Notes: The first four control variables are from Barro and Lee (1993), the rest from King and Levine (1993). Pooled OLS estimates with Newey-West error estimates corrected for heteroskedasticity and autocorrelation. Wald test p-values for joint significance of the four age shares. F-test p-values for common time intercepts in the residuals.

the left hand side. Since previous results indicate that 0.016 is a quite precise coefficient estimate, we simply deduct $0.016/\Gamma$ from the dependent variable and re-estimate the pooled base regression. Column 1 in Table 4 is very nearly identical to column 2 in Table 1 except that the overall fit deteriorates as would be expected. We conclude that this particular bias is no serious problem.

Column 2 in Table 4 reports instrumental variables estimates using generalized method of moments (GMM). The first and second lag of all non-age variables were used as instruments. Since 42 observations are lost by using second-order lags some overall precision is lost, but the estimates are remarkably close to the base regression. Sargan tests (as in Hansen 1982) of the over-identifying restrictions do not reject the validity of the instruments. We conclude that this regression gives no obvious reason to suspect that simultaneity in general is an important issue for our results. However, if there is a dynamic bias these instruments can be suspected to be endogenous in spite of the Sargan test.

Hence, a more troublesome issue – mostly ignored in the growth literature – is the dynamic bias that is associated with the use of initial GDP as regressor. The definition of the growth rate is $g_{tj} = \ln y_{t+1,j} - \ln y_{tj}$ so $\ln y_{tj}$ appears both on the left and right hand side of the estimated equation. The dependent variable is g/Γ where Γ also contain GDP measures – although measured per capita and not per worker. There is a large body of literature showing that individual effects in panels with few time series observations may cause serious bias in the estimation of dynamic relations. Nerlove (1971) and Nickell (1981) are standard references. Recent assessments of the problem can be found in e.g. Mátyás and Sevestre (1996).

In a growth context Islam (1995) and Caselli et al. (1996) study the problem, providing evidence that the initial GDP coefficient, and through that the convergence parameter, may be underestimated in conventional models. Islam

Table 4. Instrument and panel regressions

Dep. variable: g/Γ	Pooled regressions		Fixed country and time effects		
	$(g - 0.016)/\Gamma$	Instrum.	LSDV	Diff IV	Min. abs. dev.
Constant	6.60	9.48		−0.02	0.02
	(1.95)	(1.53)		(0.21)	(0.23)
$1/\Gamma$		0.018	0.015	0.011	0.016
		(4.95)	(6.60)	(2.12)	(5.94)
$\ln n_{15-29}$	−0.89	−1.57	−2.19	−4.20	−1.69
	(0.99)	(1.48)	(2.05)	(1.88)	(1.93)
$\ln n_{30-49}$	0.33	1.33	0.24	−2.69	−0.65
	(0.31)	(0.97)	(0.16)	(0.80)	(0.36)
$\ln n_{50-64}$	3.46	3.40	3.25	5.97	2.85
	(3.57)	(2.39)	(3.13)	(2.77)	(3.71)
$\ln n_{65+}$	−2.02	−2.37	−1.46	−0.22	−0.86
	(3.78)	(3.64)	(1.16)	(0.10)	(1.04)
$\ln i - \ln(\delta + w)$	1.59	1.20	1.67	2.22	1.70
	(5.12)	(1.90)	(3.80)	(2.58)	(6.44)
$\ln y_0$	−0.82	−1.10	−2.02	−2.46	−1.60
	(5.00)	(3.04)	(3.61)	(1.31)	(3.62)
Observations	168	126	168	126	168
χ^2 overid. restr.		0.379		0.285	
adj R^2	0.418	0.578	0.519	0.328	0.517
χ^2 age shares	0.000	0.000	0.004	0.002	0.000
$\chi^2 \ln i = -\ln w$	0.503	0.240	0.511	0.299	
F-test: country eff	0.344	0.456			
F-test: time eff	0.001	0.003			
$\alpha + \beta$ implied	0.660	0.522	0.452	0.474	0.514
$\tilde{\lambda}$ implied	0.018	0.025	0.038	0.043	0.032

Notes: Newey-West error estimates corrected for heteroskedasticity and autocorrelation. Absolute t-values in parentheses. Wald test p-values for joint significance of the four age shares and the restriction $b_1 = -b_2$. F-test p-values for common country and time intercepts in the residuals. IV-estimates from a GMM-procedure. The minimum absolute deviation estimate is an approximation implemented by iterated weighted least squares. Sargan test for over-identifying restrictions in the IV estimations. $\tilde{\lambda}$ is the mean convergence parameter with $(1 - \alpha - \beta)b_5$ added to compensate for the approximation error.

uses the π matrix approach of Chamberlain (1984) and Caselli et al. (1996) the Arellano and Bond (1991) estimator. Both these estimators belong to the class of GMM methods.

Although the residual tests of the pooled model reject the presence of country-specific effects on conventional significance levels there is, of course, still a positive probability that country heterogeneity may affect the estimates and we clearly have time-specific effects in the residuals. Estimates of a model allowing for both country- and time-specific fixed effects[13] are, therefore, presented in column 3 using the least squares estimator on deviations from country and period means, which is equivalent to using country and period dummies. This estimator is known to be biased for dynamic models. Thus, following Anderson and Hsiao (1982) we transform the variables by first-differencing the deviations from period means to sweep out fixed country effects without introducing the Nickell (1981) type bias through demeaning. This method introduces a moving average in the error process. To deal with this we report GMM estimates in column 4 using as instruments the first lag of the differenced non-age variables and the second lag of the level of non-age variables. The GMM procedure is implemented by reweighting the instrumented regression by Newey-West corrected estimates of the variance-covariance matrix that allow for potential serial correlation up to three lags and heteroskedasticity. This is a more parsimonious estimator than the Arellano and Bond (1991) estimator, which utilizes all possible lags as instruments, or the π matrix approach, which in addition includes leads and requires an equidistribution assumption. Our estimate is thus less asymptotically efficient but in return less liable to suffer from robustness problems in small samples caused by instruments that are too weakly correlated with the endogenous regressors.

The residuals from the differenced regression show no sign of the second-order serial correlation that would indicate misspecification. The basic hump shape of the age effects pattern not only survives but it is actually reinforced, although the negative retiree effect is more or less substituted by a negative young adult effect. The Sargan test for overidentifying restrictions do not reject the validity of the instruments. Still we suspect that the estimates in column 4 are not very reliable. Apart from the potential small sample bias of GMM estimates there are also other problems. Measurement errors may be substantially magnified by differencing, see Biørn (1996). Another problem is that the multicollinearity problem for the differenced age variables becomes considerably more serious, further undermining confidence in the individual age coefficients. Although overall precision decreases as expected it is not dramatically worse. The coefficient of initial GDP is not significantly different from the LSDV estimate so there is no indication of any really serious bias that could reverse the conclusions. The coefficient estimates may, however, be more imprecise than the estimated standard errors indicate.

Asymptotic consistency of the GMM estimator of a fixed effects model with valid instruments as the number of countries goes to infinity, after all, does not exclude poor performance in our small sample, see e.g. Kiviet (1995) who presents evidence that an approximate correction of the LSDV estimates for the dynamic bias may be preferrable in many small sample applications. Although GMM estimation seems to be the most popular method to deal with the problems of dynamic panels, these estimators have been found to perform very badly in small samples, while maximum likelihood (ML) estimators seem to have better small sample properties. Sevestre and Trognon (1996) gives an overview of the problems and points out that if, in fact, the random effects model is the most appropriate there are no feasible non-ML estimators that are consistent. The likelihood estimators are, however, still biased in many cases if reasonable specifications of the error distributions are not available. Judging from estimated residuals normality and homoskedasticity of errors will not be appropriate assumptions in our sample. Without any clear indications of alternative assumptions we refrain from experimentation at this stage.

We also need to consider the extensive heterogeneity due to differences in technology that is implied by our model with respect to all coefficients. Pesaran et al. (1996) provide Monte Carlo evidence that pooled estimators behave very badly in the presence of unaccounted slope heterogeneity and recommend using mean group estimators instead. Our time series is too short for that but to get some check on this issue we estimated a simplified model with only age variables on each time series and averaged the age coefficients. The estimates are much too imprecise to be worth reporting but the pattern of averaged coefficients was still similar to the pooled estimates.

Concerns about dynamic bias and heterogeneity bias still remain. But the evidence we have points in the direction that our allowance for heterogeneity through technology adjustment, however rough, seems to alleviate these problems.

As a final check we computed an approximation to a minimum absolute deviation (MAD) estimator along the lines of Huber (1973) for the fixed effects model in levels. This estimator is generally more robust than least squares in the presence of error distributions with fat tails. Since residual tests indicate significant kurtosis in the fixed effects model it is reassuring that this estimator yields coefficients with an age pattern that is similar although with less span than the GMM estimates.

To sum up we find little evidence that the potential sources for biased estimates considered here have any important impact on the qualitative nature of our results. However, there are sources for bias in the estimated coefficients that we cannot dismiss. The basic hump shape of the age effects pattern is remarkably stable, though.

Additional concerns

Most cross-country growth regressions have used average growth taken over longer periods (10–30 years) as dependent variable, often under the assumption that this variable represents steady-state growth rates. According to our model, however, the steady-state level of income shifts whenever there is a change in age structure. The availability of age structure data for every fifth year, thus, leads us to adopt five-year growth rates as our dependent variable. One could be concerned that this particular averaging generates aliasing effects from high frequency business cycles – see Priestley (1981) – causing spurious correlation with the low-frequency movements of age group data. To check the sensitivity of our results to this phenomenon we have also re-run our regressions using time-windows of different size.

When we decrease the size of the time-window, business cycle noise does start to become a problem, although mainly with respect to the non-age variables. Increasing the window only strengthens the results as long as there remains sufficient independent variation in the age variables. Still, when 20-year windows are used, estimates are similar to the original base estimates. As we go to a pure cross-section with 40-year averages the age shares approach near-exact linearity, so no precise estimates can be obtained (Judge et al. 1988).

Legitimate concerns could be raised regarding the different roles of females and males in the economy. However, substituting female age groups for population age groups only strengthens our results. That could be interpreted in favor of a broader human capital concept that takes account of the resource use in the informal sector.

The robustness of the derived growth model is also indicated by the low sensitivity of the estimation results on the exact specification. Somewhat surprisingly – although consistent with the experiment in column 1 of Table 4 – it does not matter much whether we use our preferred specification or interact

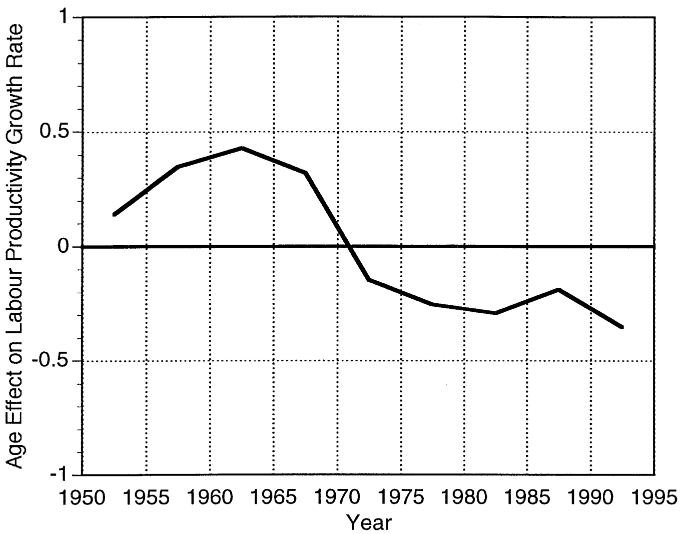

Fig. 1. Age effects on labour productivity growth rates in 23 OECD countries according to the estimates presented in Table 1, unrestricted base regression. Mean values.

the independent variables with Γ. Alternative approximations to the one in equation (7) also worked fine (see e.g. column 5 in Table 1).

The impact of our proxy for the technological convergence rate may still be a matter of concern. To check whether this feature is crucial we have added the age variables to a pure MRW specification. Essentially, the age pattern remains stable except for a higher point estimate for the prime age group. This specification as a whole is, however, less robust. The investment rate coefficient, for example, is not statistically significant.

Do age structure shifts influence the business cycle?

In Fig. 1 parameter estimates for the base model have been used to calculate the mean effect of age structure change on labor productivity growth. In the estimated model age structure variation is an important factor behind time trends in productivity growth. From the model's point of view, a deterioration of the age structure starts in 1965 when there is a widespread decline in the middle-age group combined with an increase in the old-age group. This shift in age structure is caused by a baby boom in the years prior to the First World War and by the baby bust that followed. According to the estimated model, age structure deterioration continues until the 1980s and is accompanied by age structure-induced declines in productivity.

It could, of course, be argued that the coincidence between declining productivity growth and shifting demographic trends is purely accidental. It would seem highly unlikely, though, that a purely accidental correlation survives the many sensitivity tests we have undertaken. This argument is further weakened by post-1985 patterns of growth. We know that many OECD countries experienced a period of renewed growth in the second half of the 1980s followed by a setback in the early 1990s, and the age model is able to reproduce this pattern. An out-of-sample projection indicates that age effects alone may have reduced mean productivity growth rates from 1990 to 1995 by 0.2 percentage points. Although this decline cannot by itself explain the more sizable downturn experienced by most OECD countries in the early 1990s, the age model projection has the right direction.

4. Conclusions

The objective of this paper has been to report evidence of age structure effects on OECD economic growth. Our conclusion is that age effects on growth are present in the data and should be taken into account when the macroeconomic performance of the OECD countries is discussed. We have also shown how these age effects could be accounted for within the framework of a human-capital-augmented Solow-model.

However, a number of issues remain unsettled. First, we are not able to conclusively demonstrate that the experience-based human-capital mechanism we propose is the only explanation for the age effects. Other mechanisms, e.g., savings behavior and demand effects in general equilibrium may also be important. Second, with the data available and the empirical approach chosen, only age effects that are proportional to the productivity gap to the United States in the OECD sample are captured. We have some indications that national patterns may differ also in other ways. Further studies based on more detailed data will be necessary to explain those differences. Third, further investigation of the consistency of the estimates using more sophisticated econometric methods is important in order to obtain more reliable estimates of the magnitudes of the effects. This is especially important since successful prediction is the ultimate test of a scientific hypothesis. Fourth, it thus also remains to be explored to what extent demographic forecasts can contribute to improvements in medium-term forecasts of growth rates.

Appendix

Transitional growth approximation

Equation (6) is derived here. First rewrite $\ln y^* - \ln y$ using equation (5) for k^* and h^*

$$\ln y^* - \ln y = \frac{1}{1-\alpha-\beta} \ln A^* + \frac{\alpha+\beta}{1-\alpha-\beta} \ln \frac{s}{\delta+w} + \frac{\beta}{1-\alpha-\beta} \ln N - \ln y.$$

By using the dynamic equations for the technique factor (4) and the two types of capital accumulation in (3) and decomposing $\ln y$ we arrive with some manipulation at

$$\ln y^* - \ln y = \frac{1}{1-\alpha-\beta} \times \left[\frac{\ln\left(A + \frac{\dot{A}}{\gamma}\right) - \ln A}{\frac{\dot{A}}{\gamma}} \cdot \frac{\dot{A}}{\gamma} + (\alpha+\beta) \frac{\ln\left(k + \frac{\dot{k}}{\delta+w}\right) - \ln k}{\frac{\dot{k}}{\delta+w}} \cdot \frac{\dot{k}}{\delta+w} \right].$$

Using the definition of the logarithmic derivative we have

$$\ln y^* - \ln y \approx \frac{1}{(1-\alpha-\beta)\gamma(\delta+w)}\left[(\delta+w)\frac{d\ln A}{dt} + \gamma(\alpha+\beta)\frac{d\ln k}{dt}\right],$$

where the expression in brackets is the directional time derivative of y in the direction $(\delta + w, \gamma)$ in the A-k plane. Strictly speaking we should add the dimension of h and decompose human and physical capital. But that only adds algebra and no content. Since we assume N to be constant over the period in question this is exactly $\gamma \frac{d\ln y}{dt}$ when $\delta + w$ and γ are equal numbers. It seems a rather innocuous assumption, when lacking information to the contrary, that these two terms are of approximately the same order of magnitude. Let $\lambda = (1-\alpha-\beta)\gamma(\delta+w)$ then

$$\frac{d\ln y}{dt} \approx \lambda(\ln y^* - \ln y)\cdot D$$

where D is a factor of the same order as the reciprocals of $\delta + w$ and γ. In the text D is ignored since we anyway measure γ by the GDP per capita gap, assumed to be proportional to γ.

When calculating the estimates of annual rates of convergence from parameter estimates, we need to take into account that our proxy for γ implicitly assumes the U.S. growth rate, g_{US}, to be zero. The error term in (7), thus, is really $u = \tilde{u} + g_{US}$, where $\tilde{u}$ is the approximation error above. Assuming this to be distributed with mean zero over our observations $E(u) = E(g_{US})$, which tallies well with our estimates. Consequently the estimate of $\lambda = (1-\alpha-\beta)$ $[E(g_{US}) + E(\Gamma\pi)] = (1-\alpha-\beta)b_5 - b_3\Gamma$.

Endnotes

[1] The assumption of separability of educational capital and the experience capital embodied in the age index implies that an increase in education has a proportional effect on the whole index. It would be preferable to let education have an age-specific impact but that would seriously complicate the specification of educational capital accumulation and also require much more detailed data.

[2] The assumption of an exogenous saving rate in this context need some justification. In spite of the life-cycle theory of saving demographic variables in empirical studies of saving and consumption are generally viewed with considerable skepticism, see for example Gersovitz (1988), Bosworth et al. (1991), Deaton (1992), and Muellbauer (1994). There are other researchers who attribute quite a lot of explanatory power to variations in the age structure, for example Leff (1969), Fry and Mason (1982), Mason (1987), Horioka (1991), Attanasio and Browning (1995), and Kelley and Schmidt (1996). In the OECD data age variables indeed have a significant impact on savings but explain only a tiny 4% of the variation, see Lindh (1998).

[3] This may well be an unwarranted assumption in a world-wide context, where we easily can find examples contradicting it. Within the convergence club of the OECD it is more appropriate, since these countries already are socially and politically committed to an industrial development process.

[4] The idea is an old one, see Fagerberg (1994) for literature on this topic. A recent contribution, Parente and Prescott (1994), incorporate such country-specific technology barriers in a general equilibrium model in a much more elaborate way directly related to embodiment in the firm's organizational (and unaccounted) investments (cf. ibid. p. 303).

[5] Statistics Sweden (1991), e.g., estimates that investment in intangible capital by Swedish firms is of the same order of magnitude as physical capital investment. With the same rate of depreciation, stocks should therefore be of comparable size. Barro, Mankiw and Sala-i-Martin (1994, pp. 108–109) conclude that human capital accumulation predicted from their model is roughly comparable in size to physical capital accumulation in the United States. Using schooling rates as a proxy for human capital accumulation, which is often done, works poorly in panel estimations, but we have estimated such models, too, see below.

[6] Age group data were obtained from the United Nations' population division (United Nations 1990). These are end-of-the-year estimates and are to some extent interpolated between census estimates. Data on real GDP, investment, and workforce were taken from Penn World Table 5.5 (cf. Summers and Heston 1991). The real indexes for early years are extrapolated data and may be less reliable. Initial GDP, age groups and GDP gap – see below – refer to the initial year in a period, while all other data have been computed as annual averages over the period. Although the data may have some deficiencies, the sources are constructed to yield comparable and consistent data definitions. They are also widely used and easily available for replication.

[7] The openness of the economies is ignored so the savings ratio equals the investment share. Experiments subtracting the trade surplus showed this simplification has a negligible impact.

[8] This leaves 21 countries in the sample: Australia, Austria, Belgium, Canada, Denmark, Finland, France, Greece, Iceland, Ireland, Italy, Japan, Luxembourg, the Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, the United Kingdom, and West Germany. With 8 time periods we thus have 168 observations.

[9] Among others, Quah (1993) has questioned that interpretation. We stick to the conventional interpretation, since our focus is not on convergence but on the age effects.

[10] White's (1980) heteroskedasticity test may also indicate general misspecification. Multiplying all variables with $\sqrt{T}$ the White test becomes insignificant without much change in either coefficients or standard errors, although the restriction on the coefficients of investment and work-force growth is rejected. See column 5 in Table 1.

[11] Average schooling years is, of course, a rough proxy. Apart from being easily available, it is consistent with our model, see footnote 1. The human capital accumulation is defined by enrollment rates in secondary school times the population share 15–19 years old plus higher education enrollment times the share 20–24 years old, to proxy forgone earnings in education, which would constitute the bulk of the cost invested in education. Experiments with introducing educational costs in government expenditures were not encouraging. If the relative cost of human capital and physical capital has varied substantially over time or over countries our proxy may be inappropriate but we know of no claim to that effect. MRW use only secondary school enrollment multiplied with the corresponding age group, but in the OECD this hardly yields enough variation to identify any effects. Enrollment and schooling data are taken from Barro and Lee (1993). Also note that the logarithm of the flow measure is averaged over periods.

[12] We feel, however, that adding on variables without a theoretical model specification is a rather dangerous practice. All the variables found significant here are obviously potential sources for simultaneity bias, and the functional form in which they should enter the regression is very uncertain.

[13] Although conventional wisdom is that the fixed effects model is more appropriate on country data, see e.g. Sevestre and Trognon (1996, p. 122), we have also estimated random effects models. When we use only time effects, the results are near identical to column 3 in Table 1, with both fixed and random effects. In all cases are the random effects estimates similar to the fixed effects results.

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