The Family and the Workforce: Demography, Productivity and the Rise in Wage Inequality

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Abstract

Data from the CPS is used to estimate US State and year fixed effects in Mincer log wage equations for 50-59 year-old white males over the period 1970-2001. Using these fixed effects as data in a second-stage regression suggests that the family composition of the workforce is a significant determinant of inter-State and inter-year differences in log wages. Two working parent families with a young child (under 6 years of age) have a negative effect on log wages for childless 50-59 year old white males. The results also imply that the labor market effect of the rise of the two working parent family on log wages is distinct from the increasing labor force participation of women. Finally, much of the increase in residual wage inequality for 50-59 year old white males can be explained by the changing demographic structure of the workforce.

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1 Introduction

The proportion of families in which both parents work has increased in the US, almost without pause, during the post-WWII period. Yet there appears no evidence that the increase in two working parents has resulted in a fall in time spent by parents with children. Sandberg and Hofferth (2001) find that the proportion of time spent by children with either or both parent(s) is virtually unchanged between 1981 and 1997 and that both fathers and mothers spend more time now with their children than they did in the 1960s. Since parents that work are part of the national workforce, it seems natural to suppose that having children has labor market consequences. Indeed, Harkness and Waldfogel (1999), Waldfogel (1998) and Simonsen and Skipper (2003) find a 'family gap' in the wage profiles between women with children and women without. Such evidence suggests that children have an impact on the labor market success of a parent.

The effect of children on the joint labor productivity of their parents is not well understood. Gay *et al.* (2004) find that both fathers and mothers report higher than usual levels of sleep disturbance and fatigue in the first month post childbirth. They also find that levels of fatigue for the father are lower if he continues to work than if he does not which suggests that spousal differences in labor force status affect the intra-familia distribution of childcare demands (the mothers are all absent from work during this period). The effect of fatigue on productivity more generally is examined by Papp *et al.* (2004), who, in a study of medical residents, find that fatigue affects learning and cognition and task performance – all of which are, inarguably, productivity-related factors.

Much of macroeconomic theory suggests that labor is complementary to some degree – one interpretation is that workers work loosely as teams – so it is also natural to suspect that working parents may affect all wages through a productivity spillover. In this paper, I examine the effect of the changing family composition of the workforce (and demography more generally) for wages of non-parents. I find that the changing composition of the US workforce helps to explain changes in the both the level and variance of wages of non-parents.

In a recent paper, Dunbar and Easton (2009) suggest that the change in the workforce share of working parents helps to explain the slowdown in total factor productivity (TFP) during the 1970s and the subsequent rise during the 1990s. In addition, they argue that inter-state differences in average labor productivity can also be partly explained by inter-state differences in the workforce compositions of working parent families and that firms substitute away from labor as parental workforce shares rise.

If working parents are different labor inputs than non-parents and labor is complementary, then by extension, one should expect to see the workforce composition of working parents affect wages of all workers through the productivity channel. In this paper, I use wage equations for white males between 50-59 years of age with no children present in the household to identify State-level fixed effects in log wages. These State-level fixed effects include the aggregate productivity of the labor force (and TFP). To isolate the effects of workforce composition, I examine only the wages of white males, 50-59, without children to avoid selecting households that are directly affected by the presence of children. Although biologically, 50-59 year-old men are capable of becoming new parents, the proportion that do so is relatively small and thus their wages are unlikely to embed an indirect effect from an employer's expectation that they will become parents. I focus on the wages of white males because of the large and systematic changes in female labour force participation and gender and racial wage inequality over the period of the sample. As a second step, I investigate the correlation between the family composition of the State workforce and the state-level fixed effects in wages. The results suggest that working parent shares are highly significant determinants of both inter-State and intra-State wage variation for 50-59 year old white males without children.

Since the State-effect in wages is driven in part by the composition of the workforce operating through a State fixed-effect, a natural extension is to ask whether the variance of wages across States and years is affected by the same mechanism. Put differently, if the composition of the workforce affects the State fixed effect, it seems reasonable to suspect that within-State, firm-level, fixed effects may be affected as well since the State-effect is the (weighted) average of the firm-level fixed effects within the State. Although the firm-level effects are not identified (they are part of the wage equation residual), the variance of the residuals from the wage equations should be affected by the workforce composition. Several authors, *e.g.* Moffit and Gottschalk (1994), Acemoglu (2002) and Lemieux (2006), have noted increasing (residual) wage inequality in the US. I document that this phenomenon is apparent at the State-level for the wages of 50-59 year-old white males and show that the increase can be explained, in large part, by the increasing labor force participation of two-working-parent families with a child under 6.

It is possible that the significance of the workforce composition of working parents for log wages reflects a different demographic mechanism. In particular, the workforce share of working parents is correlated with the age structure of the workforce. Feyrer (2006), Jaimovich and Siu (2009) and Jaimovich, Pruitt and Siu (2009) suggest that the changing demographic composition of the workforce can explain changes in productivity and the time series behaviour of output and wages. Although it is natural to include the workforce age and gender structures as covariates, they themselves do not account for the significance of the working parent shares. The inclusion of female age cohort shares does not mitigate the effect or significance of the working parent shares – *the labor market effect of the rise of the two working parent family on log wages is distinct from the increasing labor force participation of women.* However, the results in this paper suggest that the increasing labor force participation of 50-59 year old women and increasing shares of 40-49 year old men may lower the wages of 50-59 year old men.

That the effect of working parent shares are distinct from increasing female labor force participation or changes in the age structure of the workforce should not be entirely surprising. Gender and age are observable characteristics of an employee which permit, legally or not, statistical discrimination in employment contracts. Family structure, in particular the employment status of a spouse or the age of a child, are not, in general, observable to an employer. While some inference regarding an employee's family structure, and associated transitory productivity shocks, may be possible, such statistical inferences on the part of employers are likely noisy. It is also unclear that the productivity costs to an employer or an employee of such transitory shocks are sufficient to justify job separation and job vacancy costs.

That families are different labor units than non-families is, in many respects, unsurprising. Much research in microeconomics has focussed on the effect of the family since the seminal work of Becker (1981), for instance on labor supply (*e.g.* Angrist and Evans (1998) and Apps and Rees (2001)) and collective consumption decisions (*e.g.* Browning, Chiappori and Lewbel (2007)). In light of the magnitude of the change in the family composition of the workforce, it would be surprising to find that families have no macroeconomic spill-over effects. Upon reflection, it seems unsurprising that changes in the family structure of the workforce are a component of changes in wages.

2 Decomposing Inter-State Wage Variation

To illustrate the importance of workforce attributes for log wages, assume that output in State j at time t is a function of State-specific technology, $Z_{j,t}$, State-specific capital, $K_{j,t}$, and State-specific effective labor, $A_{j,t}L_{j,t}$:

$$\begin{aligned} Y_{j,t} &= F\big(Z_{j,t}, K_{j,t}, (A_{j,t}L_{j,t})\big) \\ A_{j,t}L_{j,t} &= \sum_i a_{i,j,t} l_{i,j,t} \end{aligned} \tag{1}$$

where i indexes an individual in state j at time t and $a_{i,j,t}$ is an individual specific productivity factor and $l_{i,j,t}$ is an individual's labor time (hours). Thus, the total labor input, $A_{j,t}L_{j,t}$, is the sum of the labor inputs of the individual workers. I note that this formulation does not impose any restrictions on the correlations between State-specific technologies and nor is it necessary to impose any restrictions beyond differentiability on the aggregate production function, F.[1]

I assume that the labor market is competitive and that labor is paid its marginal product such that:

$$w_{i,j,t} = \frac{dY_{j,t}}{dl_{i,j,t}} = F_l(Z_{j,t}, K_{j,t}, (A_{j,t}L_{j,t}))a_{i,j,t} \tag{2}$$

where $w_{i,j,t}$ is the wage of worker i in State j at time t. Log-linearizing the wage equation implies that

$$\ln w_{i,j,t} = \ln F_l(Z_{j,t}, K_{j,t}, (A_{j,t}L_{j,t})) + \ln a_{i,j,t}. \tag{3}$$

which decomposes log wages for individual i into a State-specific component, $\ln F_l(Z_{j,t}, K_{j,t}, (A_{j,t}L_{j,t})$, and an idiosyncratic component, $\ln a_{i,j,t}$. The State-specific component in log wages is identical across all workers

[1] Indeed, the production could be generalized further to $F\big(Z_{j,t}, K_{j,t}, g(A_{j,t}L_{j,t})\big)$, where $g()$ is some concave function, without affecting the arguments in this paper.

in State j at time t. Thus any factors that inform the State-specific component inform the log wages of all individuals in that State, at that time. Hence, if the demographic characterisitics of the workforce are significant determinants of the total labor input, $A_{j,t}L_{j,t}$, then by extension those characteristics should be significant determinants of the State-specific component in log wages.

One approach to estimating the log-wage equation for individuals, Equation (3), is to posit a Mincer wage equation in which it is assumed that:

$$\ln w_{i,j,t} = \kappa_{j,t} + X_{i,j,t}\beta + u_{i,j,t}. \tag{4}$$

where $X_{i,j,t}$ is a $K \times H$ matrix of observed individual characteristics; such as polynomials of age, experience and education and $\kappa_{j,t}$ is a State-level fixed effect. Thus, the observable individual characteristics $X_{i,j,t}\beta$ control for the idiosyncratic component in log wages, $\ln a_{i,j,t}$.

One potential concern with estimation of the log wage equation is the apparent endogeneity of $\kappa_{j,t}$ and $\ln a_{i,j,t}$ operating through $a_{i,j,t}$. This apparent endogeneity is misleading. To see this, suppose that there are m individuals in a State and note that the individual wage equations, (3), can be rearranged as:

$$\ln w_{i,j,t} = \ln F_l(Z_{j,t}, K_{j,t}, (\sum_{m \neq i} a_{m,j,t} l_{m,j,t} + a_{i,j,t} l_{i,j,t})) + \ln a_{i,j,t} \tag{5}$$

which, as long as i is sufficiently small – *i.e.* an individual in a large group, implies:

$$\sum_{m \neq i} a_{m,j,t} l_{m,j,t} + a_{i,j,t} l_{i,j,t} \approx \sum_{m \neq i} a_{m,j,t} l_{m,j,t}. \tag{6}$$

Phrased another way, for individual wages the individual's effect on all other wages is an externality and may be ignored for m sufficiently large, as is the case with State-level data.

The object of interest, as noted in the introduction, is the effect of workforce family structure and demography on the wages of all workers. The working assumption is that parents, particularly of young children, have different labor productivity profiles than non-parents, *ceteris paribus*. If this conjecture is true, then the workforce share of two-working parent families affects individual wages through the State fixed-effect in log wages, $\kappa_{j,t}$. Thus, the share of two working-parent families should be negatively related to wages.

I consider estimating individual wage equations using data from the March CPS Annual Supplement. The March Supplement is the only period during the year in which the CPS collects sufficient data to identify families and their composition. To avoid the difficulty with measured hourly wages in the March data identified by Bound *et al.* (1990), Autor, Katz and Kearney (2005) and Lemieux (2006b), I use the annual wage income reported by households in the Supplement and also the years of completed education, number of weeks worked, age, gender, two-digit industry code and hours per week worked by household members.[2] I deflate all wages by the US GDP Deflator series in Table 1.1.9 from the US BEA. Some States have small samples and so I aggregate States into 21 geographic regions (see Appendix for a description).

To avoid selecting households that are directly affected by the presence of children, I select only white males that are 50-59 years of age and have no children under the age of 18 present in the household from the CPS March Supplements from 1970-2006. The working sample size is $153,420$. Although biologically, 50-59 year-old men are capable of becoming new parents, the proportion that do so is relatively small. Their wages are unlikely to embed an indirect effect from an employer's expectation that they will become parents. I focus on the wages of white males because of the large and systematic changes in female labour force participation and gender and racial wage inequality over the period of the sample. The working sample in each year is roughly $3,200$.

The workforce fraction of group J in state M is computed as the total number of full-time (greater than 32 hours worked in the reference week or on paid absence) workers in group J divided by the total number of full-time workers in State M. Since the wages are collected for the previous year, I lagged the workforce fractions in all models to match fractions with the wage year.[3] The fraction of two working-parent families, *e.g.* with a child under six years of age by State and year ($\lambda_{6,t}^{2p}$), is calculated using the data from the March Supplement. Table 6.3 in the Appendix presents summary statistics showing the levels and the variation in working parent shares across the 21 State-regions. The average share over the period 1970-2001 varies from 0.053 in New York State to 0.097 in the Carolinas and Georgia, with a single-year minimum of 0.011 (Connecticut) and a single-year maximum of 0.134 (Indiana). Generally, the shares are increasing from East to West although the Midwest has typically had the highest proportion of working parent families as a proportion of the workforce.

At its heart, the conjecture is that there is a (common) factor in individual wage equations that can be traced to productivity. In the context of the Mincer wage equation above, the common component is a common factor at the level of disaggregation of the data (here the State). For each year, t, in the data, T, I estimate a log-wage equation for 50-59 year-old white males:

$$\ln w_{i,j,t} = \kappa_{j,t} + X_{i,j,t}\beta + u_{i,j,t} \forall t \in T \tag{7}$$

and save the resulting coefficient estimates $\hat{\kappa}_{j,t}$. $X_{i,j,t}$ contains: two-digit industry dummies, a cubic in age, a cubic in education, weeks worked, marital status, class of firm (private or government), urban/rural

[2] The hours per week variable is actually measured during the period of the supplement and thus does not line-up exactly with the reported period of income. I argue that this is unlikely to bias the findings and in fact may be helpful as it provides a measured of variability in future income to which households may respond.

[3] I note that the results are nearly identical if one constructs the workforce shares using hours worked in the reference week. Indeed, the correlations in shares between the two methods is almost always equal or greater than 0.99. The only difference appears for the fractions of two working parents with children between 6-18 which is more volatile when measured in hours. I conjecture that this may reflect the effect of the March school break in our sample as the relative fraction of two working parents with a child between 6 and 18 falls by 1.5 percentage points and the standard deviation rises by 60 per cent when measured in hours. I therefore choose to use the fractions based on full-time work.

dummy and the current class of the worker.[4] Because the definition of the education variable changes across survey years, I define education as years of completed education compiled into 9 compatible categories from the CPS supplemental questions. Most of the estimates, $\hat{\kappa}_{i,j,t}$, are statistically different from zero at the one per cent level of significance using clustered standard errors (by State).

If two-working parent families affect TFP then $\hat{\kappa}_{j,t}$ is affected by the shares of two-working parent families in each State in each year. I consider the estimates $\hat{\kappa}_{j,t}$ as a panel dataset with $J = 20$ and $T = 37$ and investigate the effect of the shares of working parents on the State effect in wages. Thus, in the second stage of analysis, the demographic structure of the State, $D_{j,t}$, is regressed on the estimated State effect, $\hat{\kappa}_{j,t}$:

$$\hat{\kappa}_{j,t} = \Gamma_j + \gamma D_{j,t} + \zeta Control_{j,t} + e_{j,t} \tag{8}$$

where Γ_j is the average (unexplained) State effect, γ is the coefficient that measures the effect on wages of demographic and family composition variables contained in D (*e.g.* workforce shares of two-working parents) and $Control_{j,t}$ are a vector of dummy variables described below. The conjecture is that wages of non-parents are negatively affected by the share of two-working parent families. The measurement error in $\hat{\kappa}_{j,t}$ is assumed to be orthogonal to the remaining explanatory variables.[5]

As control variables, I include several dummy variables to capture economy-wide changes in labor conditions. I include an indicator variable, $FMLA$, that takes the value 1 for any State whose family leave policy in that year is at least as generous in terms of weeks leave as the Family and Medical Leave Act of 1993.[6] Obviously, $FMLA = 1$ for all States as of 1993. The inclusion of $FMLA$ is intended to capture the effect of policies that may encourage (or not discourage) two-working-parent families. I include also a dummy variable, PDA, for the introduction of the Pregancy Discrimination Act of 1978, and a dummy variable, $Recession$, for years in which the US economy was judged by the NBER to be in a recession. As possible additional controls, I consider the average government transfer to average income as a gauge of the social spending in the State (using data from the BEA). A large part of such transfers are payments to families. A second control is to use the birthrate by State (available from the CDC for 1990 onwards. Neither control changes the estimates (or the standard errors) of the parental effects in the models examined in this paper and are, in general, insignificant as regressors. The exceptions are the dynamic panel regressions reported in Section 2.1 but even here the estimates of the parental and cohort shares are qualitatively unaffected. For brevity, I present the results from the regressions without the transfer and birthrate data.[7]

[4] I normalize κ to the fixed effect for Washington, DC, although this normalization does not appear to affect the results.

[5] I note that including a constant, Γ_j, in the second stage regression is subject to debate. There is nothing in the model discussed thusfar that suggests its inclusion is merited. However, a more nuanced view may be that it capture a cultural norm which is unobserved or a production factor difference across States. Thus, for robustness, I estimate the models both with and without a constant where possible. The results suggest that the inclusion of a constant is immaterial qualitatively (and in many case quantitatively).

[6] I used the data presented in Waldfogel *et al.* (2007) to construct this dummy variable.

[7] These results are available from the author upon request.

A summary of the data is presented in Table 6.2 in the Appendix. I note that the estimated State-effect in log points is small as these effects are net of the nationwide constant included in the wage regressions. Thus, the estimates presented here identify the effect of two working parents shares on general wages through State variation in log wages.

I estimate Equation 8 using GMM with fixed effects under three different specifications of the included workforce composition shares:

1. only two-working parent families,
2. two-working parent families and age cohorts,
3. two-working parent families, age cohorts and female age cohorts.

I also estimate the static model under the assumption that the error process is heteroskedastic and is auto-correlated using Prais-Winsten (column 5) and FGLS (column 6).[8] Beck and Katz (1995) argue that Prais-Winsten produces test statistics that are more reliable in small panel samples. However, FGLS is theoretically more efficient if the structure of the covariance matrix is correctly specified. There appears to be some evidence of autocorrelated errors in the static model using the test proposed by Wooldridge (2002) but no evidence of autocorrelation using the Bharagava *et al.* (1982) test. I test the stationarity of each series using the Im-Pesaran-Shin (2003) panel unit root test and in almost all cases am able to reject that the data have a unit root. The two exceptions are the 30-39 year age cohort share and the female, 30-39 year age cohort shares and I address the possible concern regarding non-stationarity in the following subsection using dynamic panel estimation. However, theoretically it is not clear that the unit root finding is not simply a sampling problem as all shares are bounded above by 1 by construction. Encouragingly , the coefficient estimates and levels of significance are remarkably consistent across all specifications. The workforce shares of two working parent families with a child under six years of age and the demographic structure of the workforce are significant determinants of the cross-state variation in log wages.

The results, presented in Table 2.1, indicate that the share of two working parent families with a child under 6 years of age is a significant determinant of the inter-State variation in log wages for white males 50-59 years of age without children in the home. A one standard deviation increase in the fraction of two working parent families lowers the log wage fixed effect by roughly -0.07, or approximately 30 percent of the average log wage fixed effect. The estimates also suggest that the workforce shares of the 20-29 and 30-39 cohorts have a positive impact on log wages – confirming the findings of Feyrer (2006) and Jaimovich and Siu (2009) that demographic variation is a significant determinant of macro-economy aggregates. However, the inclusion of the female age-cohort shares in specification (3) demonstrates that the working-parent effect

[8] I estimate all three specifications but only report specification (2) for brevity. The remaining results are consistent and available upon request.

Table 2.1: **Working Parents And State Wage Effects, 50-59 White Male – Static Model**

	Fixed Effects - GMM			PW-PC	FGLS
	Only Parents	Inc. Age	Inc. Age and Female		
Working Parents < 6	-1.986**	-3.691***	-3.625***	-4.174***	-4.22***
	(0.734)	(0.758)	(0.756)	(0.584)	(0.578)
Working Parents < 18	-0.512	-0.440	-0.399	-0.433	-0.422
	(0.594)	(0.611)	(0.593)	(0.566)	(0.560)
FMLA	-0.0139	0.117***	0.0966**	0.077**	0.078**
	(0.0188)	(0.0339)	(0.0351)	(0.029)	(0.0289))
PDA	-0.0876**	-0.252***	-0.298***	-0.229***	-0.238***
	(0.0314)	(0.0339)	(0.0386)	(0.0331)	(0.0328)
Recession	-0.0436	-0.00824	-0.00417	-0.0139	-0.0124
	(0.0231)	(0.0219)	(0.0219)	(0.0210)	(0.0208)
20-29 Cohort		3.135***	4.011***	2.313***	2.426***
		(0.705)	(1.084)	(0.558)	(0.552)
30-39 Cohort		2.989***	2.716*	2.620***	2.787***
		(0.501)	(1.068)	(0.447)	(0.442)
40-49 Cohort		0.853	-0.493	0.685	0.670
		(0.825)	(1.313)	(0.679)	(0.670)
20-29 Female Cohort			-1.096		
			(1.449)		
30-39 Female Cohort			0.849		
			(1.472)		
40-49 Female Cohort			3.050		
			(1.594)		
50-59 Female Cohort			1.657		
			(1.649)		
N	640	640	640	640	640
R-sq	0.050	0.129	0.138	0.151	

PW-PC refers to Prais-Winsten with panel corrected standard errors as suggested in Beck and Katz (1995). Standard errors are reported in parentheses. Robust standard errors are used. * refers to p<0.05, ** refers to p<0.01 and *** refers to p<0.001. The Bharagava *et. al.* and Baltagi-Wu tests for autocorrelated disturbance suggest no autocorrelation in the residuals.

is *distinct* from a gender rebalancing of the workforce. The female age-cohort shares are not significant determinants of the inter-State log wage fixed effect.[9]

The included dummy variables suggest that the introduction of the Pregnancy Discrimination Act lowered wages for 50-59 year old white males without children present. This finding appears consistent with theory if one supposes that the act levered costs onto employers who then, in turn, responded by lowering wages of non-treated employees to compensate. Perhaps more surprising is that the introduction of the Family and Medical Level Act (FMLA) appears to have increased average wages for all employees.

There appears to be scant reason to believe that workforce shares of working parents are endogenously linked to the state fixed-effect of wages for 50-59 year old white males. Nevertheless, I test, using as instruments 1 to 3 year lags of the workforce shares, and cannot reject at the ten per cent level the exogeneity

[9] I also consider a specification in which I drop the 30-39 year old shares, both total and female, given the possible unit root in the series. The estimated effect of the working parent share falls (to 2.58 in the OLS case) but the significance remains unchanged. However, the 20-29 age cohort is no longer, in general, significant.

of the workforce shares using differences in Sargan-Hansen statistics. Hansen's J statistic and the Kleibergen-Paap LR statistic suggest that these instruments are valid. The coefficient estimates are similar to those reported in Table 2.1.[10]

To assess the role of demographic factors in explaining the variation in state fixed effects, I compare the predicted values of $\hat{\kappa}_{j,t}$ from the Prais-Winsten regression to the data. The results, presented in Table 2.2, show that the demographic shares explain roughly three quarters of the inter-State variation in fixed effects and approximately one third of the within-State variation.

Table 2.2: Decomposing the Effect of Working Parents

		Mean	Std. Dev.	Min	Max
$\hat{\kappa}_{j,t}$	overall	-0.231	0.248	-0.973	0.449
	between		0.065	-0.319	-0.118
	within		0.239	-0.911	0.459
OLS Predicted $\hat{\kappa}_{j,t}$	overall	-0.229	0.095	-0.592	0.133
	between		0.048	-0.309	-0.120
	within		0.082	-0.570	0.082

Standard errors are reported in parentheses. Robust standard errors are used. * refers to p<0.05, ** refers to p<0.01 and *** refers to p<0.001. The Bharagava *et. al.* and Baltagi-Wu tests for autocorrelated disturbance suggest no autocorrelation in the residuals.

2.1 Fixed Effects and Persistence

In theory, most macroeconomic models assert that capital stocks are auto-correlated and the Real Business Cycle literature asserts additionally that technology shocks are auto-correlated. The estimated State-effect $\hat{\kappa}_{j,t}$ includes, by construction, the State-specific technology shock and also the State-specific capital stock (but not the economy-wide analogs). If technology shocks and capital stocks are persistent then this suggests that the regression equations with $\hat{\kappa}_{j,t}$ as the dependent variable may require the inclusion of a lagged term, $\rho_\kappa \hat{\kappa}_{j,t-1}$, where ρ_κ is the auto-regressive coefficient for $\hat{\kappa}_{j,t}$. In addition, the possible concern regarding the non-stationarity of the 30-39 year old shares suggests more careful attention to the regression specification.

I estimate the model with the lagged state effect using both fixed effects and the dynamic panel approach of Arellano and Bond (1991). The results, presented in Table 2.3, indicate that effect of the workforce share of two working-parent families on the difference in State-level log wages is significant. Moreover, the consistency of the estimated effect is striking. Both fixed-effect estimators – the **FE** and the **Dynamic Panel** – return statistically indistinguishable estimates for the effect of two working parent families on State wages (see columns three and five). Indeed, the estimated effect of two-working parent families with a

[10]Since any endogeneity is likely to be most pronounced for the parents of newborns who face, in some sense, a discrete choice after maternity leave, I also test and do not reject the exogeneity of the workforce shares of two working parents with newborns which I separate from the shares of working parents with children under six. Thus, there appears no evidence that the workforce shares of working parents are endogenously linked to the state fixed-effect of wages.

child under 6 in the dynamic model is statistically indistinguishable to those reported for the static model in Table 2.1 for identical model specifications. This provides stronger support for the conclusion that the parental composition of the workforce is an important determinant of macro-economic outcomes. As well, the estimates and significance of the 30-39 age and female-age cohort shares are qualitatively identical to those obtained with the static model which suggests that the possible non-stationarity of the 30-39 year old shares is not important to the empirical results thusfar.

The Arellano and Bond (1991) test of the auto-covariance of the residuals implies that the residuals are autocorrelated of order 1 only at the ten per cent level of significance which suggests, when combined with the relatively small, albeit significant, point estimates, only weak evidence of a dynamic effect on state wages. The fixed-effect dynamic model yields insignificant estimates of the lagged state effect with the exception of the model including age cohorts which is listed in Table 2.3.[11]

In a number of respects, the apparent lack of auto-correlation in the State effect is unsurprising. The State effect is estimated across years in the data and thus it measures the cross-sectional variation in wage fixed effects. Put another way, if the State effect across years is auto-correlated then there is some underlying persistence in the inter-State differences in conditional wages. Such an outcome would cast doubt on the validity of using a wage equation derived from competitive markets with assumed labor mobility. It would be difficult to argue that persistent inter-State differences in conditional wages did not represent some type of labor market friction. As it stands, the results in Table 2.3 indicate that competitive markets may not be such a bad assumption. Indeed the estimates suggest that State-level fixed effect wages changes are reasonably uncorrelated across time and thus are similar to stochastic shocks. These shocks are correlated with family shares and demographic variables.

The evidence thusfar suggests two facts about the US labor market. (1) Demographic shares, particularly those of working parents, inform wages and this evidence is consistent with the complementarities in production assumed by most macroeconomic models. (2) There is little evidence of persistent inter-State differences in wages and so the assumption of Walrasian labor markets may be appropriate.

3 Year Effects: State by State

The State effect examined thusfar has measured inter-State variation in wages for 50-59 year old males and investigated the effect of workforce demography and family compostion on the State-effect in wages. This approach, while informative about the impact of demographic variables for wage variation *in a given year*, ignores the effect of such variables on wage variation across years. Any common changes in wages due to common changes in demography would not be identified by $\kappa_{j,t}$.

[11] Indeed, it may be the case that static fixed effect estimates are the most appropriate – the dynamic fixed-effect estimator is consistent in pseudo-panels when $N \to \infty$ as opposed to T or M. See Mackenzie (2004).

Table 2.3: Working Parents And State Wage Effects, 50-59 White Male

		Dynamic Panel		Fixed Effects
	Only Parents	**Inc. Age**	**Inc. Age and Gender**	**Inc. Age**
Working Parents <6	-1.696***	-3.937***	-3.853***	-2.755**
	(0.505)	(0.587)	(0.623)	(0.855)
Working Parents <18	0.0627	0.230	0.165	-0.215
	(0.501)	(0.570)	(0.518)	(0.559)
Recession	-0.0152***	0.0224***	0.0195**	0.0119
	(0.00414)	(0.00659)	(0.00743)	(0.0212)
PDA	-0.172***	-0.378***	-0.376***	-0.304***
	(0.0157)	(0.0288)	(0.0282)	(0.0363)
FMLA	-0.0260	0.116***	0.108**	0.0801*
	(0.0165)	(0.0339)	(0.0345)	(0.0315)
Lagged 50 wage	-0.0552***	-0.140***	-0.141***	-0.100*
	(0.0146)	(0.0189)	(0.0194)	(0.0504)
20-29 Cohort		3.140***	3.625**	2.237***
		(0.804)	(1.173)	(0.664)
30-39 Cohort		3.052***	3.623**	2.534***
		(0.623)	(1.051)	(0.491)
40-49 Cohort		0.281	-0.200	0.463
		(0.625)	(1.135)	(0.739)
20-29 Female Cohort			-1.031	
			(1.559)	
30-39 Female Cohort			-1.111	
			(1.685)	
40-49 Female Cohort			1.238	
			(1.901)	
50-59 Female Cohort			0.213	
			(1.873)	
Constant	0.000918	0.00409*	0.0042	
	(0.00155)	(0.00201)	(0.0031)	
N Obs	600	600	600	620

Standard errors are reported in parentheses. Robust standard errors are used. * refers to p<0.05, ** refers to p<0.01 and *** refers to p<0.001. The Dynamic Panel estimation restricts the maximum lags of the dependent and exogenous variables to 8 although the results are qualitatively identical with 5 and 10 maximal lags.

The same approach for estimating log wage equations to construct pseudo-panels at the State level can be used to examined intra-State yearly variation in these wages. By estimating the log wage equation for a given State with fixed-year intercepts one can determine the extent to which intra-State changes in wages reflect changes in workforce demography and family composition *within* that State. Thus, rather than estimating State-effects for each year in the data, I estimate year-effects for each State in the data. I re-estimate log wage equations for 50-59 year-old white males for each State in the data using the same control variables as described above:

$$\ln w_{i,j,t} = \tilde{\kappa}_{j,t} + X_{i,j,t}\beta + u_{i,j,t} \forall j \in J. \tag{9}$$

In this specification, the $\tilde{\kappa}_{j,t}$ capture the year-effect in log wages for State j at time t. Again, these year-effects are estimates of State-specific aggregate factors, *e.g.* $\ln F_l(Z_{j,t}, K_{j,t}, (A_{j,t}L_{j,t})$. Using the estimated year-effects, $\hat{\tilde{\kappa}}_{j,t}$, from the log wage, the regression:

$$\hat{\tilde{\kappa}}_{j,t} = \Gamma_j + \gamma D_{j,t} + \zeta Control_{j,t} + v_{j,t} \tag{10}$$

examines the effect of intra-State variation in demographic factors, $D_{j,t}$, on the year-effect in State log wages, where $v_{j,t}$ is an error term. As noted above, business cycle theory suggests that $v_{j,t}$ has both cross-sectional correlation and auto-correlation. It is plausible that $v_{j,t}$ features cross-sectional correlation because economy-wide macro-economic shocks are likely to impact all states and thus wages in all States. Auto-correlation follows from the apparent persistence of productivity shocks in many macro-economic settings. Equation 10 thus examines the extent to which the variation in log wages within State j is correlated with changes in workforce composition within State j.

I test and cannot reject the presence of cross-sectional heteroskedasticity (via a likelihood-ratio test) and auto-correlation (the latter either by Bharagava et al. (1982) or Wooldridge (2002)) and so I estimate Equation 10 using three approaches: (1) Prais-Winsten with panel-corrected standard errors (2) FGLS and (3) GMM with a lagged dependent variable. I also test the stationary of the estimated year effect using the Im-Pesaran-Shin (2003) test and reject the presence of a unit root. In all models, I weight panels according to the population underlying the estimates of $\tilde{\kappa}_{j,t}$ to account for the different sample size underlying the year-effect estimates.[12] Beck and Katz (1995) argue that (1) produces test statistics that are more reliable in small panel samples. However, (2) is theoretically more efficient if the structure of the covariance matrix is correctly specified.[13] In all three specifications, I examine both contemperaneous and 10 year-differenced data. I include as explanatory variables the workforce composition shares of working parents and also age-cohorts, the producer price index (PPI) and the real interest rate. I note that because the PPI and the real interest rate are economy-wide variables, they were unnecessary in the models in Section 2. For control variables, I include dummy variables for the FMLA, PDA Act and recessions. I also include a time trend to account for trend changes in the underlying technology.

The results, presented in Table 3.1, suggest that the workforce shares of parents are significant determinants of the year effects in State wages. The only model in which workforce shares are not significant at the five per cent level of significance is the ten-year differenced model, and even here the workforce shares of two working parents with a child under six are significant at the ten per cent level. The reported coefficient estimates are statistically identical across all specifications. The estimated effects of the age cohort variables are mixed – in general only the FGLS estimates suggest that age structure is a significant explanatory factor

[12]In contrast, the state fixed effect was estimated for each year and there is less sample size variation across years than there is across states. Thus, weighting the fixed effect estimates produced only negligible differences in the estimates.

[13]For robustness, I also test the same models under the assumption of heteroskedastic errors and no cross-sectional correlation and find similar results.

of the year-effect in log wages. Beck and Katz (1995) suggest that the FGLS covariance estimates may be biased lower in small panel samples, and so with the possible exception of the 40-49 age cohort shares, there appears little evidence that the age structure of the workforce (as distinct from the family structure) is important for the wages of 50-59 year white males. The included control variables suggest that recessions have a positive impact on wages for those who remain employed and that the input prices (as measured by the PPI) are significant determinants of wages. As well, the regressions all suggest that there has been an underlying trend change in the year effect of log wages of roughly 1.5-1.8 per cent per year.[14] The R-squared of the contemporaneous regressions implies that roughly forty per cent of the variation in the year effect for log wages can be explained by the model. This is consistent with the findings of Dunbar and Easton (2009) that changes in the family composition of the workforce can explain approximately forty per cent of total factor productivity growth in the US.

I also test, using the model specification (3), that the workforce shares of working parents are endogenously determined with the year-effect in log wages. I use as instruments lagged workforce shares of working parents and lagged workforce shares of women in the 20-29, 30-39 and 40-49 age cohorts. Hansen's J statistic and the Kleibergen-Paap LR statistic suggest that these instruments are valid. The difference in the Sargan-Hansen test statistic fails to reject that workforces shares of working parents are exogenously determined. In addition, tests of the exogeneity of the cohort shares fail to reject that the cohort shares are exogenously determined. The coefficient estimates from the IV estimates of model (3) are largely similar to those reported for model (3) itself (*e.g.* the coefficient estimate for the share of working parents with a child under six is -1.35 with a p-value of 0.056).

The Real Business Cycle literature argues persuasively that trend changes in technology may not be linear. As a final robustness exercise, I Hodrick-Prescott filter the year effect in log wages (with smoothing parameter 6.25) and use the detrended data as the regressand in models (1) - (3). The detrended data may control for business-cycle fluctuations in log-wages that are unrelated but contemporaneous to demographic variation. While the point estimates are lower when using detrended data, the qualitative results and the signficance of working parent shares are virtually identical to those reported in Table 3.1.[15] Thus, there appears no reason to suspect that the apparent importance of working parents shares for log wages results from a spurious correlation or an improper specification of trend technology growth.

The finding that the workforce shares of working parents affect the wages of 50-59 year-old, childless, white males is further evidence of the impact of families on the macro-economy. Just as the results from the state fixed effect regressions suggest that cross-State differences in the workforce composition of working families helps to explain cross-State variation in wages, the evidence from within-State yearly variation

[14]The remaining coefficient estimates are qualitatively identical if year dummies are included in the regression model as opposed to a trend term.
[15]Results available from the author upon request.

suggest that working parents help to explain cross-year variation in wages.

Table 3.1: Working Parents And Inter-Year Wage Effects, 50-59 White Male

	Contemporaneous			10-Year Differenced		
	PW-PC	**FGLS**	**GMM**	**PW-PC**	**FGLS**	**GMM**
Working Parents, <6	-0.565*	-0.475***	-0.595**	-0.622*	-0.556***	-0.488
	(0.220)	(0.110)	(0.222)	(0.250)	(0.0718)	(0.297)
Working Parents, <18	0.381*	0.418***	0.150	0.0931	0.127***	0.157
	(0.190)	(0.0840)	(0.187)	(0.207)	(0.0380)	(0.234)
20-29 Cohort	0.373	0.300**	0.333*	0.200	0.140**	0.249
	(0.193)	(0.0971)	(0.169)	(0.226)	(0.0494)	(0.207)
30-39 Cohort	-0.136	-0.127	0.0974	-0.208	-0.216***	-0.277
	(0.179)	(0.0849)	(0.169)	(0.180)	(0.0315)	(0.201)
40-49 Cohort	-0.458	-0.490***	-0.321	-0.670**	-0.725***	-0.583*
	(0.247)	(0.116)	(0.215)	(0.253)	(0.0576)	(0.242)
FMLA	-0.0172	-0.0143**	0.00532	0.00330	0.00428**	-0.00809
	(0.00988)	(0.00443)	(0.00950)	(0.0117)	(0.00140)	(0.0119)
PDA	0.00142	0.00998	0.0144	-0.000232	-0.00120	0.0112
	(0.0141)	(0.00542)	(0.0108)	(0.0192)	(0.00216)	(0.0178)
Recession	0.0179**	0.0203***	0.0160**	0.0272**	0.0280***	0.0280**
	(0.00580)	(0.00229)	(0.00504)	(0.00993)	(0.00101)	(0.00945)
Real Interest	0.0988	0.0631	0.123	-0.0360	-0.0550***	-0.00639
	(0.109)	(0.0427)	(0.0997)	(0.118)	(0.0142)	(0.114)
PPI	-0.00295***	-0.00271***	-0.00284***	-0.00192*	-0.00179***	-0.00228**
	(0.000687)	(0.000290)	(0.000623)	(0.000784)	(0.0000897)	(0.000812)
Trend	0.0182***	0.0169***	0.0146***			
	(0.00227)	(0.000970)	(0.00210)			
Lagged Yr Effect			0.190***			0.141**
			(0.0402)			(0.0487)
Constant	-0.112	-0.101		0.149***	0.145***	
	(0.116)	(0.0573)		(0.0264)	(0.00343)	
Obs	640	640	620	440	440	420

PW-PC refers to Prais-Winsten with panel-corrected standard errors as suggested by Beck and Katz (1995). Standard errors are reported in parentheses. Robust standard errors are used. * refers to p<0.05, ** refers to p<0.01 and *** refers to p<0.001.

3.1 Increasing Female Labor Force Participation

As noted above, one concern regarding the results presented in Table 3.1 is the extent to which the estimated impact of working-parent shares on log wages simply reflects the increasing labor force participation of women. Although the inclusion of female labor force shares in the fixed effect models of Section 2 did not alter the significance of working parent families to log wages, it is possible that the intra-State variation in female workforce shares might do so. For example, any institutional or cultural differences in female labor force participation across States could be confounded with the *inter*-State fixed effect. However, such a concern is less apparent for *intra*-State variation in female labor force participation. Thus, I augment the regressions presented in Table 3.1 by including the age cohort shares of women.

In Table 3.2 I report the estimates of the age and working parent share effects for overall log wages in columns 2-4, with each column reporting the estimates from Prais-Winsten, FGLS and GMM respectively (as in Table 3.1) under the assumption of heteroskedastic errors and auto-correlated disturbances.[16]

Strikingly, the inclusion of age cohort shares of women does not alter the conclusion that the workforce shares of two-working parent families are significant determinants of the year effect in log wages. Indeed, the coefficient estimates reported in Table 3.1 are essentially unchanged by the inclusion of the age cohort shares of women.[17] The results suggest that the increased labor force participation of women is distinct from the increasing labor force composition of two-working parent families. As well, the results confirm the finding above that the FMLA is a redistributive transfer.

However, while distinct, the results in Table 3.2 suggest that the increased labor force participation of women in the same age cohort, 50-59, lowered log wages for men in the same age cohort. The same cannot be said for other age and gender cohorts and, in general, the demographic structure of the workforce is not a significant determinant of log wages for white males 50-59.

4 Demography and The Residual Variance of Log Wages

There is much evidence in the literature (see for instance Lemieux (2006), Acemoglu (2002) and references therein) that residual wage inequality has risen in the US over the period 1970-2002. While there are several candidate explanations, such as skill-biased technical change or compositional effects, a straightforward extension of the wage equation, Equation (8), illustrates that the changing demographic composition of the labor force also may help to explain residual wage inequality via fixed effects.

To see this, assume that there exists a number P of firms (or plants) within each State. P need not be fixed across States and time but, for the sake of exposition, I assume that it is. Let each firm, $p \in P$, produce output, $y_{p,j,t}$, according to the production function:

$$\begin{aligned} y_{p,j,t} &= f\big(Z_{j,t}, K_{p,j,t}, (A_{p,j,t}L_{p,j,t})\big)\big) \\ A_{p,j,t}L_{p,j,t} &= \sum_i a_{i,p,j,t} l_{i,p,j,t} \end{aligned} \tag{11}$$

where the subscript p indexes firms (but otherwise the subscript definitions are unchanged) and total State output is the sum of firm output within the State, $Y_{j,t} = \sum_p y_{p,j,t}$. If the labor market is competitive and

[16] The assumption of heteroskedastic errors instead of heteroskedastic and cross-sectionally correlated errors is immaterial for the results from the OLS specification (and by construction, the GMM). However, FGLS estimates under the assumption of heteroskedastic and correlated errors were not robust for the FGLS specification here (unlike the results presented in Table 3.1). Since the conclusions from FGLS estimates under the assumption of heteroskedastic and correlated errors were, in some sense, stronger than those under the assumption of heteroskedastic errors and yet those results appear suspect, I choose to report the heteroskedastic results.

[17] I also consider a dynamic panel regression to account for the possible non-stationarity of the 30-39 year old shares and find stronger results for the working parent shares.

Table 3.2: Working Parents And Female Age Cohort Shares, 50-59 White Male

	PW-PC	**FGLS**	**GMM**
Working Parents, <6	-0.497*	-0.537**	-0.551*
	(0.218)	(0.202)	(0.225)
Working Parents, <18	0.467*	0.480**	0.198
	(0.195)	(0.178)	(0.188)
20-29 Cohort	-0.184	-0.317	0.0111
	(0.294)	(0.272)	(0.274)
30-39 Cohort	-0.473	-0.531	-0.273
	(0.310)	(0.290)	(0.318)
40-49 Cohort	-0.725*	-0.584	-0.487
	(0.354)	(0.323)	(0.346)
Women 20-29	0.394	0.618	0.0538
	(0.409)	(0.376)	(0.382)
Women 30-39	-0.200	0.0229	0.129
	(0.432)	(0.400)	(0.428)
Women 40-49	-0.321	-0.499	-0.353
	(0.482)	(0.440)	(0.482)
Women 50-59	-1.357*	-1.332**	-0.992*
	(0.553)	(0.505)	(0.483)
FMLA	-0.0148	-0.0131	0.00630
	(0.0109)	(0.0101)	(0.00953)
PDA	0.00184	0.00531	0.0149
	(0.0153)	(0.0141)	(0.0107)
Recession	0.0183**	0.0175**	0.0163**
	(0.00647)	(0.00595)	(0.00502)
Trend	0.0199***	0.0192***	0.0158***
	(0.00254)	(0.00232)	(0.00220)
Real Interest	0.101	0.125	0.117
	(0.121)	(0.111)	(0.0990)
PPI	-0.00296***	-0.00297***	-0.00284***
	(0.000783)	(0.000715)	(0.000626)
Lag Year Effect			0.184***
			(0.0403)
Constant	0.234	0.229	
	(0.193)	(0.177)	
Observations	640	640	620
R-squared	0.426		0.462

PW-PC refers to Prais-Winsten with panel-corrected standard errors as suggested by Beck and Katz (1995). Standard errors are reported in parentheses. Robust standard errors are used. * refers to p<0.05, ** refers to p<0.01 and *** refers to p<0.001.

labor is paid its marginal product then:

$$w_{i,p,j,t} = \frac{dy_{p,j,t}}{dl_{i,p,j,t}} = f_l(Z_{j,t}, K_{p,j,t}, (A_{p,j,t}L_{p,j,t}))a_{i,p,j,t}. \tag{12}$$

Log wages are therefore:

$$\ln w_{i,p,j,t} = \frac{dy_{p,j,t}}{dl_{i,p,j,t}} = \ln f_l(Z_{j,t}, K_{p,j,t}, (A_{p,j,t}L_{p,j,t})) + \ln a_{i,p,j,t}, \tag{13}$$

where $\ln a_{i,p,j,t}$ captures the idiosyncratic and observable component of wages, such as education, experience, etc., and $\ln f_l(Z_{j,t}, K_{p,j,t}, (A_{p,j,t}L_{p,j,t}))$ is a firm-State-year fixed-effect. I assume, given competitive labor markets, that the market price of experience and education is constant within a State.[18] Log wages are then:

$$\ln w_{i,p,j,t} = \frac{dy_{p,j,t}}{dl_{i,p,j,t}} = \ln f_l(Z_{j,t}, K_{p,j,t}, (A_{p,j,t}L_{p,j,t})) + \ln a_{i,j,t}, \tag{14}$$

which aggregate to the State-level wage regression as:

$$\begin{aligned} \ln w_{i,j,t} &= \frac{dy_{p,j,t}}{dl_{i,p,j,t}} = \ln F_l(Z_{j,t}, K_{j,t}, (A_{j,t}L_{j,t})) + \ln a_{i,j,t} + u_{i,j,t}, \\ u_{i,j,t} &= [\ln f_l(Z_{j,t}, K_{p,j,t}, (A_{p,j,t}L_{p,j,t})) - \ln F_l(Z_{j,t}, K_{j,t}, (A_{j,t}L_{j,t}))] + v_{i,j,t} \end{aligned} \tag{15}$$

where $u_{i,j,t}$ are the residuals from the State-level wage regression, 8. This specification shows that differences between firm-State fixed-effect and the (average) State fixed-effect are part of the wage regression error.

Focusing on the decomposition of the residuals, one can easily obtain:

$$var(u_{i,j,t}) = var\Bigg([\ln f_l(Z_{j,t}, K_{p,j,t}, (A_{p,j,t}L_{p,j,t})) - \ln F_l(Z_{j,t}, K_{j,t}, (A_{j,t}L_{j,t}))]\Bigg) + var(v_{i,j,t}) + cov \tag{16}$$

where $var(u_{i,j,t})$ is the variance of residual wages for individuals in State j at time t and cov is a covariance term.[19] Certainly, it is clear to see in Equation (16) how explanations surrounding the capital stock or technology (such as SBTC) can help to explain changes in residual wage inequality (operating through Z or K). There is, however, a second channel – differences in $A_{p,j,t}L_{p,j,t}$ from the State-level average $A_{j,t}L_{j,t}$). The evidence thusfar has shown that working parent shares are informative for *average* fixed effects in wages, either across States or across time. If parents are equally distributed across firms in P, then average firm labor productivity is equivalent to the State-level average and there is no contribution via fixed effects to residual wage variance. However, if, for example, working parents are unequally distributed across firms then the variance in fixed effects is directly, and positively, related to residual wage variance. Thus, demographic composition may also help to explain changes in residual wage variance over time. Table 6.4 in the Appendix details that the residual variance of wages has been, in most cases, rising over the period 1970-2001 at the level of the State.

For the working parent shares to affect the variance of fixed effects within a State, it must be that the firm-size weighted distribution of parents across firms is unequal. If the shares of parents across firms are

[18] Albouy (2009) and Moretti (2008) show that wages may have a geographic component and so it is unlikely that the market price of education is identical in New York and Nebraska. It seems less heroic to assume that the market price of education is roughly constant within each State. Nevertheless, this assumption, although not strictly necessary, does simplify the exposition to follow.

[19] It is not clear that modelling the covariance term is material for the results. If all firms are identical then the covariance term is zero because, by construction, the fixed-effect is orthogonal to the regression error. If there are systematic differences across firm fixed-effects that are correlated with differences in the market price of the idiosyncratic components then the covariance term will be positive. If, for example, poorly managed firms had a low marginal labor productivity f_l and paid less than the going rate for obervable skills then the covariance term will be positive and unlikely to drive $var\Bigg([\ln f_l(Z_{j,t}, K_{p,j,t}, (A_{p,j,t}L_{p,j,t})) - \ln F_l(Z_{j,t}, K_{j,t}, (A_{j,t}L_{j,t}))]\Bigg) + cov \to 0$. In contrast, a firm which had low marginal labor productivity and paid more than the going rate would have a negative covariance – however such firms would seem unlikely to remain an ongoing concern for very long. Hence, it is unlikely that the approach here will fail to find a relationship between demography and residual wage variance when in fact such a relationship exists.

equal then by construction the variance of wages explained by parental shares must be zero. Two extreme cases where this must be satisfied are when the workforce share of parents is zero or one. Between zero and one, and when parents are unequally distributed across firms, the variance of the fixed effects will be positive if parents affect firm productivity.[20] For example, when firms are of equal size, the peak of the fixed effects variance is obtained when the share of parents is 0.5 and those parents are employed at half of the firms. Thus, a simple, continuous and differentiable representation of the effect of parents (or any other shares) on residual wage inequality must be concave over the region $[0, 1]$. Two simple function form specifications fit this requirement: a logarithm and a quadratic function. After some experimentation, I choose to estimate and report the results of the logarithmic specification. Since working parent shares in the data range from 0.02 to 0.15, it seems reasonable to suspect that the effects of working parent shares are on the upward sloping segment of the concave function. In addition, using the coefficient estimates on the quadratic term (which are jointly significant at the 5 per cent level) from the quadratic specification and plotting the function over the range of working parent shares yields the upward segment.

The regression equation is therefore:

$$var(u_{i,j,t}) = \tilde{\Gamma}_j + \tilde{\gamma} \ln D_{j,t} + \tilde{\beta} X_{j,t} + \tilde{e}_{j,t} \tag{17}$$

where $D_{j,t}$ are the demographic shares and $X_{j,t}$ are the control variables described above (including a time trend). I again test and cannot reject the presence of heteroskedasticity and auto-correlation (the latter either by Bharagava et al. (1982) or Wooldridge (2002)) and so I estimate Equation (17) using two approaches: (1) Prais-Winsten with panel-corrected standard errors and (2) FGLS. There is no evidence of a unit root in the residual wage variance. In all models, I weight panels according to the population underlying the estimates of $\tilde{\kappa}_{j,t}$ to account for the different sample size underlying the year-effect estimates.[21] As noted previously, Beck and Katz (1995) argue that (1) produces test statistics that are more reliable in small panel samples. However, (2) is theoretically more efficient if the structure of the covariance matrix is correctly specified.

In Table 4.1 I report the results for three specifications of the demographic composition of the workforce: working parent shares, working parent and age cohort shares, and working parent, age and female-age shares. I also include as controls for other productivity changes: the real interest rate, the PPI, the FMLA, PDA and recession dummies and a trend term. The results suggest that the shares of working parents with a child under 6 are a significant determinant of the inter-State variation in residual wage inequality for 50-59 year-old white males.[22] As well, the shares of the 30-39 age cohort and the 50-59 female-age cohort are

[20] There appears good reason to suspect that working parents are unequally distributed across firms. For example, Working Mother Magazine has published a list of the best employers for working mothers for at least 15 years. There would appear to be little reason for this list unless it is used by mothers to select employers or as a benchmark index for 'best practises.'

[21] In contrast, the state fixed effect was estimated for each year and there is less sample size variation across years than there is across states. Thus, weighting the fixed effect estimates produced only negligible differences in the estimates.

[22] I consider the same regressions using year dummies and find qualitatively (and in most cases quantitatively) identical results.

Table 4.1: **Working Parents And Residual Wage Inequality, 50-59 White Male**

	Working Parents		Inc. Age Cohorts		Inc. Age and Women Cohorts	
	FGLS	**PW-PC**	**FGLS**	**PW-PC**	**FGLS**	**PW-PC**
Working Parents <6	0.0588***	0.0567***	0.0425***	0.0391***	0.0366***	0.0330**
	(0.00547)	(0.00953)	(0.00628)	(0.0115)	(0.00635)	(0.0120)
Working Parents <18	-0.0170	-0.00286	-0.0223*	-0.00520	-0.0312***	-0.0144
	(0.00953)	(0.0178)	(0.00893)	(0.0170)	(0.00901)	(0.0172)
FMLA	0.00235	0.000107	-0.00514	-0.00212	-0.00853*	-0.00276
	(0.00462)	(0.00773)	(0.00418)	(0.00698)	(0.00420)	(0.00684)
PDA	0.00704	-0.00140	0.00671	-0.000846	0.00419	-0.00151
	(0.00926)	(0.0158)	(0.00837)	(0.0146)	(0.00849)	(0.0144)
Recession	0.000681	-0.00742	0.00317	-0.00397	0.00223	-0.00352
	(0.00391)	(0.00663)	(0.00358)	(0.00608)	(0.00360)	(0.00605)
Real Interest Rate	0.151*	0.0915	0.159*	0.141	0.157*	0.137
	(0.0737)	(0.126)	(0.0680)	(0.116)	(0.0681)	(0.114)
PPI	-0.000318	0.000146	-0.00100*	-0.000611	-0.00124**	-0.000715
	(0.000372)	(0.000641)	(0.000397)	(0.000695)	(0.000399)	(0.000692)
Trend	0.00253*	0.00138	0.00315**	0.00235	0.00229	0.00104
	(0.00104)	(0.00182)	(0.00122)	(0.00219)	(0.00123)	(0.00221)
20-29 Cohort			0.0238	0.0285	0.132***	0.159*
			(0.0225)	(0.0437)	(0.0341)	(0.0652)
30-39 Cohort			0.145***	0.132***	0.200***	0.202**
			(0.0223)	(0.0397)	(0.0383)	(0.0686)
40-49 Cohort			0.0665**	0.0507	0.122***	0.0841
			(0.0250)	(0.0478)	(0.0363)	(0.0676)
Women 20-29					-0.00973	-0.0361
					(0.0207)	(0.0376)
Women 30-39					0.0326	0.0198
					(0.0176)	(0.0317)
Women 40-49					0.0310	0.0459
					(0.0202)	(0.0347)
Women 50-59					0.0872***	0.0857**
					(0.0165)	(0.0316)
Constant	-4.803*	-2.463	-5.722*	-4.143	-3.334	-0.924
	(2.079)	(3.622)	(2.422)	(4.350)	(2.460)	(4.413)
Observations	620	620	620	620	620	620
R-squared		0.777		0.758		0.763

PW-PC refers to Prais-Winsten with panel-corrected standard errors as suggested by Beck and Katz (1995). Standard errors are reported in parentheses. Robust standard errors are used. * refers to p<0.05, ** refers to p<0.01 and *** refers to p<0.001.

significant. Despite the differences in magnitudes between the estimated coefficients of working parent and age-cohort shares, the impact of each on residual wage inequality appears roughly equal because of the logarithmic transformation and the fact that the working parent shares are roughly half of the size of the age cohort shares. Finally, the results for residual wage inequality are generally consistent with the results for the State fixed effects: in nearly all cases, the share of two-working parents with a child under 6 years of age, the share of 30-39 year-old workers are significant determinant of inter-State differences. Indeed, the estimates suggest that roughly 75 per cent of the variation in residual wage inequality can be traced to changes in the demographic composition of the workforce.

The high R^2 may indicate endogeneity between residual wage variance and the demographic shares and so I consider GMM IV estimation with a robust covariance matrix. I instrument working parent shares using one and two year lags of those shares. Hansen's J statistic and the Kleibergen-Paap LR statistic suggest that these instruments are valid. The difference in the Sargan-Hansen test statistic does not fail to reject that workforces shares of working parents are exogenously determined at the five per cent level for the model including only parent shares and at the ten per cent level when parent and age cohort shares are included. However, the conclusions from the endogeneity tests are not robust to the inclusion of year dummies which suggests that serial correlation in the residuals may influence the endogeneity test results. The Sargan C statistic suggests that age and female cohort shares are exogenous. The estimates from the IV specification are an order of magnitude larger than either the Prais-Winsten or FGLS specifications (and significant at the 5 per cent level for the parent shares). However, non-IV GMM estimates are lower and less significant (usually only at the ten per cent level of significance). Nevertheless, the relative patterns of significance are unchanged in comparison with the Prais-Winsten or FGLS results and, in general, there appears to be only mixed evidence of endogeneity.[23] Thus, both the FGLS and Prais-Winsten estimates are preferred as they explicitly model the serial correlation in the error term.

To determine how much of the rise in residual wage inequality between the 1970s and the period from the early 1990's to 2001 can be explained by changes in two-working-parent shares, I compare the actual residual wage inequality with the predicted residual wage inequality over these two periods.[24] I consider two periods for illustrative purposes: 1973-1975 and 1999-2001. Table 4.2 presents the actual and predicted residual wage inequality over these two periods. As is evident in the table, the changing demographic composition of the workforce, including the impact of the rise in working parents, helps to explain the rise in residual wage inequality over the period 1973-1975 and 1999-2001.[25] In fact, the model appears to over-fit slightly the increase in residual wage inequality – it predicts a mean increase of 0.059 points rather than the 0.048 points observed. This could reflect any of several unobserved factors, for example: a change in the dispersion of working parents across firms, or; the effects of skill-biased technological change (although here, the unobserved technological change would necessarily diminish rather than increase the dispersion of residual wages). A more structural model would aid in identifying such underlying influences but, as noted earlier, I am unaware of any dataset for the US that includes both family and firm level data for an employee. Finally, I note that the model is able to capture roughly half of the standard deviation across States in residual wage inequality between 1973-1975 but only one quarter during the period 1999-2001. Thus, while demographic changes in the labor force appear capable of explaining the mean increase in residual wage inequality, they

[23] These results available upon request from the author.

[24] Recall that 2001 is the last year of data in which the CPS does not scramble the ages of children within the household.

[25] Other, longer, periods return essentially identical results, as does a more parsimonious regression model with only working parent shares, 20-29 and 30-39 age cohort shares and the share of women 50-59. As well, the models using year dummies rather than a trend term return qualitatively identical, and in most cases quantitively stronger, results. The results are available on request to the author.

do not explain the full story.

Table 4.2: Working Parents and the Trend in Residual Wage Inequality

	Mean	Std. Dev.	Min	Max
1973-1975				
residual wage variance	0.187	0.049	0.095	0.303
predicted variance, OLS Inc. Age and Women	0.191	0.024	0.135	0.233
1999-2001				
residual wage variance	0.235	0.058	0.136	0.392
predicted variance, OLS Inc. Age and Women	0.250	0.014	0.212	0.273

5 Conclusion

Using data from the CPS to estimate State-level fixed effects in wages and year-effects in wages for 50-59 year-old white males suggests that the workforce composition of working parents is a significant determinant of inter-state and inter-year differences in log wages. By focussing on 50-59 year-old white males without children present, I isolate the spill-over from workforce composition to wages.

The results in this paper suggest that the workforce share of working parents is a signficant determinant of both the inter-State and intra-State variation in log wages. In contrast, the age-structure of the workforce is, in general, not. Two working parent families with a child under 6 have a negative effect on log wages. Interestingly, the results also suggest that the Family and Medical Leave Act of 1993 redistributes income from workers unaffected by working parents to those that are affected negatively. The results are suggestive of important, but as yet poorly understood, labor complementarities in production.

The results in this paper also shed new light on the possible causes of rising residual wage inequality in the US. Demographic changes, in particular the rise of the two working parent family, help to explain inter-State differences in residual wage inequality. The results indicate the need for further research on the interactions between family structure and firm productivity.

That the household composition of the workforce has an effect on the general wage level is, in some respects unsurprising. Labor is typically utilized in teams and so the composition of team members is likely to influence labor productivity and hence wages. In many respects, the findings in this paper are indicative of the need for further study of the effects of the family composition of the workforce.

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6 Appendix

I aggregate the individual States into 21 geographic areas, largely on the basis of the Census Bureau Regions and Divisions schema (see Table 6.1). Some states (e.g. New York, New Jersey, California, etc.) have sufficient individual observations to identify year and state fixed effects and so I do not aggregate these states.

Table 6.1: State Aggregation

State Group	**States**
1	Maine, New Hampshire, Vermont, Massachuesetts, Rhode Island
2	Connecticut
3	New York
4	New Jersey
5	Pennsylvania
6	Ohio
7	Indiana
8	Illinois
9	Michigan, Wisconsin
10	Minnesota, Iowa, Missouri, North Dakota, South Dakota, Nebraska, Kansas
11	Delaware, Virginia, West Virginia, Maryland
12	Washington, DC [the Rererence State]
13	North Carolina, South Carolina, Georgia
14	Florida
15	Kentucky, Tennessee
16	Alabama, Mississippi
17	Arkansas, Oklahoma, Louisiana
18	Texas
19	Montana, Idaho, Wyoming, Utah, Nevada, Colorado, New Mexico, Arizona
20	Washington, Oregon, Alaska, Hawaii
21	California

Table 6.2: State Fixed Effect

State Abbreviation	κ	SE	Min	Max
ME, NH, VT, MA, RI	-0.240	0.234	-0.794	0.311
CT	-0.200	0.252	-0.696	0.245
NY	-0.196	0.231	-0.653	0.377
NJ	-0.118	0.242	-0.678	0.444
PA	-0.243	0.239	-0.74	0.329
OH	-0.205	0.241	-0.668	0.324
IN	-0.257	0.234	-0.771	0.276
IL	-0.149	0.252	-0.692	0.444
MI,WI	-0.162	0.238	-0.674	0.414
MN, IA,MO,ND, SD,NE,KS	-0.289	0.249	-0.806	0.27
DE, VA, WV, MD	-0.180	0.241	-0.726	0.406
NC, SC, GA	-0.299	0.239	-0.844	0.215
FL	-0.319	0.244	-0.85	0.206
KY, TN	-0.293	0.263	-0.973	0.25
AL, MS	-0.311	0.260	-0.867	0.379
AR, OK, LA	-0.319	0.235	-0.905	0.31
TX	-0.290	0.245	-0.798	0.255
MT, ID, WY, UT, NV, CO, NM, AZ	-0.248	0.247	-0.811	0.282
WA, OR, AK, HI	-0.147	0.227	-0.487	0.406
CA	-0.152	0.243	-0.649	0.449

Table 6.3: Workforce Share of 2 Working Parents with a Child <6

State Abbreviation	Share	SE	Min	Max
ME, NH, VT, MA, RI	0.055	0.0165	0.0313	0.1063
CT	0.059	0.0262	0.0109	0.1504
NY	0.053	0.0130	0.0326	0.0862
NJ	0.056	0.0178	0.0270	0.0977
PA	0.055	0.0139	0.0353	0.0904
OH	0.065	0.0156	0.0397	0.1143
IN	0.077	0.0194	0.0322	0.1341
IL	0.067	0.0126	0.0443	0.0966
MI,WI	0.067	0.0141	0.0449	0.1121
MN, IA,MO,ND, SD,NE,KS	0.087	0.0177	0.0584	0.1252
DE, VA, WV, MD	0.076	0.0117	0.0583	0.1074
NC, SC, GA	0.097	0.0112	0.0777	0.1209
FL	0.077	0.0128	0.0501	0.1175
KY, TN	0.083	0.0150	0.0481	0.1281
AL, MS	0.095	0.0135	0.0598	0.1310
AR, OK, LA	0.091	0.0115	0.0728	0.1122
TX	0.088	0.0112	0.0600	0.1159
MT, ID, WY, UT, NV, CO, NM, AZ	0.075	0.0125	0.0536	0.1054
WA, OR, AK, HI	0.064	0.0086	0.0476	0.0851
CA	0.073	0.0136	0.0486	0.1155

Table 6.4: Trend Change in Residual Variance of Log Wages

State Abbreviation	Trend
ME, NH, VT, MA, RI	0.392
CT	0.313
NY	0.389
NJ	0.355
PA	0.381
OH	0.302
IN	0.247
IL	0.234
MI,WI	0.186
MN, IA,MO,ND, SD,NE,KS	0.299
DE, VA, WV, MD	0.151
NC, SC, GA	0.0466
FL	0.19
KY, TN	-0.0256
AL, MS	0.141
AR, OK, LA	0.329
TX	0.285
MT, ID, WY, UT, NV, CO, NM, AZ	0.325
WA, OR, AK, HI	0.529
CA	0.346