Primary and Secondary School Class Size and Intergenerational Earnings Mobility

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I am indebted to Bhash Mazumder for giving me batch files used in Aaronson and Mazumder (2007) to organize data from the Integrated Public Use Microsample Series. I am responsible for all remaining errors.

Abstract
While theory suggests that public expenditures on education may affect intergenerational earnings mobility, the direction of the effect hinges on whether such outlays substitute for or complement private human capital investments. This paper empirically evaluates the question using census data in the Integrated Public Use Micro Sample from 1940-2000. The results show that state-cohorts with smaller class sizes generally enjoy less intergenerational mobility, indicating that class size reductions benefit children from high-income families more than those from low-income families. The size of the effect is substantial: the effect of moving from one standard deviation above to one standard deviation below the mean class size increases earnings persistence by more than 40%. These results are robust to controls for the average class size in the state in years the individual was *not* in school, a finding which rules out many endogeneity explanations.

I. Introduction

The economic theory of intergenerational educational investment presented by Becker and Tomes (1986) suggests that improvements to public education will increase intergenerational mobility. Because rich families can provide their children with the efficient level of education on their own, expansions of public schooling do not affect the educational attainment of their children: public funding merely substitutes for private investment. But similar improvements in public schooling lead to greater schooling among children from poor families who are otherwise unable to afford the efficient level of education investment. So, we expect children's earnings to be less dependent on parent income in economies with better quality schools.

As Goldberger (1989) points out, this interpretation assumes that public and private education investments are substitutes and not complements. If instead enhancements to public school quality raise the return to private investments, then we would expect to see greater investment in children from all families. Indeed, Card and Krueger (1992) find that reduced class size, increased term length, and higher relative teacher pay lead to greater returns to education. When public and private expenditures are complements, the effect on the intergenerational earnings correlation would depend on whether it is the children of the poor or the children of the rich whose returns are most positively impacted. Thus, whether increased school quality leads to more or less mobility is an empirical question.

Unfortunately, data limitations have hampered our ability to study the issue. Despite econometric advances which have improved the reliability of mobility estimates (see Solon 1989 and 1992, Mazumder 2005, Grawe 2006, and Haider and Solon 2006), demanding data requirements have limited the number of economies studied. The limited nature of the available evidence can be seen in Corak (2006) who plots available national estimates of the intergenerational earnings elasticity (IGE) against the OECD's report of total per pupil expenditures (both public and private).[1] His findings, repeated in Figure 1 below, show very little correlation (-0.062). Because 7 of the 9 observations fall on a clear, negative trend line, it may be tempting to remove the outlying observations from the United States and Finland by citing un-named "institutional heterogeneity". But this only serves to emphasize the fundamental problem: with only 9 observations we do not have enough degrees of freedom to account for all of the obvious policy and social differences between these countries. Even if we were to estimate the IGE in 30 countries, we would likely have too few observations to credibly assign mobility differences to one or several of the many, large differences in policy (income taxes, inheritance taxes, tax consequences of fertility, K-12 education finance, post-secondary education support, bureaucratic barriers to entrepreneurship, labor laws, unemployment insurance, retirement benefits,...) and social norms (perceptions of the obligation to support a child's education, attitudes toward risk, views on what constitutes fair treatment of heterogeneous siblings, ...).

[Figure 1 goes here]

This paper extends the method used by Aaronson and Mazumder (2007) to study the connection between K-12 class size and intergenerational mobility across the 50 states and District of Columbia among men born between 1921 and 1975. Unlike the available international data, this approach creates many estimates of the IGE from a

[1] The intergenerational earnings elasticity is the standard measure of intergenerational earnings mobility. It measures the slope coefficient from a regression of son's log earnings on father's log earnings, controlling for the ages of both the father and the son.

sample of institutionally-similar economies. The next section surveys the relevant theory. Section III presents methods and data. The results reported in Section IV document a negative relationship between class size and earnings persistence: the father-son earnings association in state-birth cohorts with class sizes one standard deviation below the mean is more than 40% stronger than in state-birth cohorts with class sizes one standard deviation above the mean. Taken at face value, these results suggest that children from high-income families are more benefited in states with small class sizes. Section V explores alternative interpretations of the results. Adding controls for the average class size within a state in years the individual was *not* in school does not substantially alter these results. While endogeneity can never be ruled out, this finding suggests the results do not simply reflect differences in state attitudes toward education which are correlated with mobility. The final section concludes and presents future lines of inquiry.

II. Theory of Class Size and Intergenerational Earnings Mobility

The dominant model of intergenerational earnings mobility, presented in Becker and Tomes (1986), emphasizes the potential for credit constraints to limit educational investments in children and increase earnings persistence across generations. The intuition for this prediction is quite simple. In the absence of credit market failure, parents invest in child education so long as the benefits (higher wages for the child in adulthood) outweigh the costs. Assuming the returns to education are greater among more able children, those with greater abilities acquire more human capital and earn more than their less-able peers. In this environment, intergenerational persistence in ability leads to a positive parent-child earnings association.

Becker and Tomes go on to consider an economy in which parents are unable to borrow to pay for educational investments in their children. Now, a child's human capital depends both on her own ability (which determines the rate of return on investment) and the parent's income (which determines the parent's ability to pay). This second, direct connection between the earnings of the parent and the education of the child leads to an even greater parent-child earnings correlation.

Becker and Tomes (1986) and Becker (1989) model public education expenditures as substitutes for private investments. In families not facing a credit constraint, the child is already receiving the level of education at which the marginal return just equals the marginal cost. So long as the government investment falls short of this efficient level of education, parents reduce their private contributions dollar for dollar as state expenditures grow. In credit constrained families, however, the policy can lead to greater human capital acquisition. Public investment relaxes the constraint and allows parents to come closer to the efficient level of education investment in their child. Viewing expenditures on school quality through this lens, smaller class sizes lead to greater intergenerational earnings mobility by relaxing credit constraints faced by poor families.

Goldberger (1989) argues that public education investments may actually complement parent investments, raising the returns to private education expenditures. Empirically, Card and Krueger (1992) find higher returns to years of education in states with smaller class sizes, longer term length, and higher relative teacher pay. If the model in Becker and Tomes (1986) is amended to allow for public-private complementarities, then public education expenditures like class size reductions may aid students from all families, rich and poor. The greater the public expenditure, the higher the returns to private investment, and the more human capital will be acquired. It is not clear *a priori* that such complementarities are largest for children from low-income families. And if credit constraints limit the ability of such families to capitalize fully on the higher returns, we may even have reason to suspect that children from high-income families are likely to benefit the most. Without knowing which group is most positively impacted, it is impossible to predict the effect of public school quality on intergenerational mobility.

Of course, it is possible that public expenditures have both substitutable and complementary characteristics. Becker and Tomes (1986) and Becker (1989) acknowledge this, but argue that the substitution effects are clear while the complementary characteristics are less so—in fact, some public investments may even reduce returns to private investment. (For instance, see Peltzman 1973). In the context of theoretical ambiguity, the question can only be answered empirically.

III. Methods and Data

One of the greatest challenges in the study of intergenerational mobility is finding a dataset containing earnings observations for both fathers and sons. Early efforts sought extensive panels like those in the Panel Study of Income Dynamics, the German Socioeconomic Panel, and the Canadian Intergenerational Income Data. Of course, developing such long, intergenerational panels is expensive and slow.

Björklund and Jäntti (1997) circumvented this problem by applying two-sample instrumental variables (TSIV), an estimation technique developed in Angrist and Krueger (1992) and Arellano and Meghir (1992). The intuition of this approach is a simple extension of the instrumental variables (IV) estimator. IV identifies the effect of x on y

by comparing cov(y,z) and cov(x,z) where z is a valid instrument. While we may find x, y, and z in the same data set, we could estimate these covariances using two data sets—one including y and z and the other containing x and z. Björklund and Jäntti's approach to estimating the IGE has been followed in Dunn (2003), Ferreira and Veloso (2004), Grawe (2004), Lefranc and Trannoy (2004), Mocetti (2007), and Piraino (2006).

Most recently, Aaronson and Mazumder (2007) (hereafter AM) apply TSIV to estimating the IGE in US census data, 1940-2000. The key insight in their work is that state of residence can instrument father's earnings. This choice of instrument is important in two ways. First, even as Solon (1992) suggested father's education as an instrument, he noted it is likely endogenous and so produces an upward bias in IV estimates of the IGE. Using data from the PSID and National Longitudinal Survey of Youth, AM use true intergenerational panels to estimate the IGE with and without controls for father's education and state of residence. The results confirm the endogeneity of father's education: conditional on father's earnings, education has an independent, positive effect on son's earnings. By contrast, they find no independent effect of father's state of residence on son's earnings. (They also show state of residence to be a strong instrument, thus addressing concerns of weak instrument bias.) At least among sons born between 1950 and 1970, state of residence appears to be a valid instrument.

More importantly, since state of birth is routinely collected in the US census, this instrument allows them to estimate the IGE using census data across six decades. When AM estimate earnings persistence across birth cohorts from 1921-1975, they find a u-shaped pattern. Earnings persistence is high in early cohorts, diminished at the middle of the century, and then rises again. While the peak IGE (0.694) was reached in the 1961-1965 birth cohort, the earnings persistence in the 1971-1975 cohort (0.476) was roughly 50% higher than that found in the 1921-1925 birth group.

A. Regression specification

This paper extends the work of AM, exploiting variation in average class size across the 50 states and District of Columbia to estimate the relationship between school quality and intergenerational mobility. The base specification in AM regresses log earnings on the log of average total family income (not including children's earnings) at the time of the individual's childhood among families with children in the individual's state of birth. Controls for the individual's age (differenced from age 40), birth cohort, and year of income observations are included to allow for age, cohort, and time effects. Age is interacted with year to allow the age earnings profile to vary across time.[2] In addition, AM follow the method of Lee and Solon (2005), interacting age (differenced from age 40) and log of average total family income to correct for the lifecycle bias in IGE estimation identified by Grawe (2006) and Haider and Solon (2006). The regression equation is

$$y_{ibst} = \alpha + \gamma_{1t}(age\text{-}40) + \gamma_{2t}(age\text{-}40)^2 + \gamma_{3t}(age\text{-}40)^3 + \gamma_{4t}(age\text{-}40)^4 + u_t + v_b + \delta_{1t}(age\text{-}40)\, X_{ibs} + \delta_{2t}(age\text{-}40)^2 X_{ibs} + \delta_{3t}(age\text{-}40)^3 X_{ibs} + \delta_{4t}(age\text{-}40)^4 X_{ibs} + \theta X_{ibs} + \varepsilon_{ibst} \tag{1}$$

where i indexes the individual, b notes birth cohort, s is individual's state of birth, t represents the census year in which the individual's earnings are measured, and X_{ibs} is the log of average total family income in state s during the childhood of individual i. u_t and

[2] However, based on the data, AM find the age earnings profile to be constant in 1950, 1960, and 1970 and so impose this restriction.

v_b capture time and birth cohort effects respectively while ε_{ibst} is a random error term. The model is then extended to include interactions between the log of average family income and birth cohort or time dummy variables to estimate trends in the IGE over time.

By replacing actual total family income with the average in the state of the child's youth, AM implement something very close to TSIV with state of residence as an instrument for total family income. Their method differs from TSIV as presented in Angrist and Krueger (1992) and Arellano and Meghir (1992) in two ways. First, the method of AM is a two-stage process—2STSIV—in which the log of average total family income is first regressed on state of residence and then the predicted log of average total family income is used in regression (1) above. Inoue and Solon (2005) show that 2STSIV is preferable to TSIV in terms of both efficiency and robustness. Second, AM use the log of average total family income rather than the average of the log. This allows them to include families with not family income. Because log income is not defined when income is zero, this choice differs from strict 2STSIV (and from common practice in estimating the IGE). In keeping with other studies, I deviate from AM in this choice and instead use the average of log total family income.[3]

To introduce the effects of class size, the model of AM is modified to include both class size and its interaction with log family income. In effect, just as family income is instrumented with state, the interaction between family income and class size is instrumented by the product of state and class size. Given the extensive literature documenting increasing returns to skill, the effect of class size may vary across cohort

[3] In results available on request, I show that using the standard method of excluding zero-income families does not substantially alter the u-shaped trend in IGE reported in AM. However, with the standard "average of the log" definition, the IGE is lower in the cohorts born after 1941 and so the recent increase in the IGE reported by AM is a bit muted.

and/or year. Regressions not reported here show effects do vary across cohort, but not across year (after accounting for cross-cohort variation). These effects are captured by an interaction between class size and cohort. Including these additional terms, my base regression equation is

$$y_{ibst} = \alpha + \gamma_{1t}(age\text{-}40) + \gamma_{2t}(age\text{-}40)^2 + \gamma_{3t}(age\text{-}40)^3 + \gamma_{4t}(age\text{-}40)^4 + u_t + v_b +$$
$$\delta_{1t}(age\text{-}40)\, X_{ibs} + \delta_{2t}(age\text{-}40)^2 X_{ibs} + \delta_{3t}(age\text{-}40)^3 X_{ibs} + \delta_{4t}(age\text{-}40)^4 X_{ibs} +$$
$$\theta X_{ibs} + \varphi Class\ size + \varphi_b Class\ size{*}Cohort + \eta Class\ size{*}X_{ibs} + \varepsilon_{ibst} \qquad (2)$$

where η measures the effect of class size on the IGE. This base regression model is expanded to allow the IGE and the effects of class size on the IGE to vary across birth cohorts.

B. Data

The demographic data used in this work are drawn from the Integrated Public Use Microsample Series (IPUMS), 1940-2000. My sample differs from that in AM in two ways. First, while AM use the 15% census sample in years 1980, 1990, and 2000 and the 5% sample in 1940, 1950, 1960, and 1970, I use the 5% sample for all census years. Second due to computational constraints, I randomly select one half of the observations, resulting in a final sample size of 1,136,007.

From 1950-2000 censuses, I collect log real earnings and year and state of birth for US-born men ages 25-54. Year of birth is used to divide the men into 11 birth cohorts: 1921-1925, 1926-1930, 1931-1935, 1936-1940, 1941-1945, 1946-1950, 1951-1955, 1956-1960, 1961-1965, 1966-1970, and 1971-1975. I will refer to this sample as the “sons sample.”

From the 1940-1990 censuses I collect total family income for all boys ages 0-4, 5-9, 10-14, and 15-19 who are living with their father. In the case of boys ages 15-19, family income excludes the earnings of the boy. After adjusting for inflation, I calculate average log family income experienced by boys in the age groups 0-4, 5-9, 10-14, and 15-19 years by state. These averages are matched by state and birth cohort to the sons sample. The birth cohorts 1921-1925 and 1926-1930 can only be matched to one prior census (the 1940 census). The other nine cohorts can be matched to two prior censuses. Following AM, I average these two measures of log of average total family income. This is X_{ibs} in the regression equations above.

The IPUMS data is then merged with class size data. The National Center for Education Statistics' *Digest of Education Statistics* reports class size in each state from 1988-1993 and public primary and secondary school enrollment from 1980-1987. The Statistical Abstract of the United States includes biennial reports of enrollment and number of teachers in public primary and secondary schools for each state from 1926-1943 and 1950-1955. Annual reports are included from 1944-1949 and 1956-1987. During the period of biennial reporting, I average the observed enrollment and number of teachers in the years before and after a given year.[4] In each state-year I calculate average class size as the number of pupils per teacher. For each birth cohort in each state, I calculate the mean of average class size within that state for the years in which individuals of that cohort would have been ages 6, 8, 10, …18. By only using every

[4] In several cases, enrollment data were missing for states in a given year. These include: CA, IL, and MT in 1971, all states in 1973, MA in 1977. These missing observations were also replaced with the average of enrollment and number of teachers in the year preceding and following the missing date.

other year I treat individuals in early cohorts (when education data is largely biennial) the same as those in later cohorts (when annual data are available).[5]

As documented in Hanushek (1998, 1999) and elsewhere, average class size in the US fell dramatically during the 20th century. What is more, the variance across states also fell. Figure 1 plots the trends in mean and variance of class size among states over the time period studied here. While the controls for birth cohort in regression (2) largely eliminate the trend in the mean. But the fact that cross-state variation in class size has dramatically diminished over time means that, once cohort affects are accounted for, the highest and lowest class size observations are likely to be found in the earliest cohorts. Given the substantial trend in the IGE across cohorts found by AM, it is possible this could lead to spurious conclusions. To address this concern, I standardize the class size variable within each year by subtracting the mean across states and dividing by the cross-state standard deviation. Using standardized class size, eliminates the change in standard deviation across time.[6] Table 1 presents summary statistics for all variables.

[Figure 1 goes here.]

[Table 1 goes here.]

IV. Results

Table 2 presents estimates on variations of regression (2). The simplest specification is in column 1 which assumes that both the IGE and the effect of school quality are constant across cohorts. The estimated intergenerational earnings elasticity, 0.397, is slightly lower than that reported in AM, but very close to the IGE when amending AM by using the average of log total family income—the measure used in this

[5] The results do not change noticeably if I average class size within the state for all 13 years in which the birth cohorts were between ages 6 and 18.

[6] When the analysis is repeated using raw class size, the results are consistent with those presented here.

study.[7] Decreases in class size substantially diminish intergenerational mobility. The effect of moving from one standard deviation above to one standard deviation below the mean class size increases earnings persistence by more than 40%. It should be noted that for the range of total family income observed across the states, child earnings at all family income levels are increased by smaller class sizes. The rise in the IGE simply indicates that states with small class sizes benefit children from high-income state-cohorts than for children from low-income state-cohorts.

[Table 2 goes here.]

Column 2 relaxes the assumption that the effects of class size on the IGE are constant across birth cohorts. In all but the 1921-1925 cohort, larger class size leads to more mobility (and in the 1921-1925 cohort, the effect of class size is not statistically significant). Moreover, the mobility-reducing effects class size reductions have strengthened over the cohorts studied. Figure 3 plots the estimated effect of class size on the IGE. In the beginning of the 20th century, improvements to school quality may have increased mobility slightly. But by the 1931-1935 birth cohort, this relationship was reversed. And but for a brief turnaround among men born in the 1960s, larger class sizes reduce the IGE more and more over time.

[Figure 3 goes here.]

Of course, one of the key findings in AM is that the IGE is not constant. Columns 3 and 4 of Table 2 repeat the analysis allowing the IGE to change across cohorts. The cross-cohort pattern found in the IGE is nearly identical to that reported by AM. The estimated effects of class size on the IGE are qualitatively the same as in columns 1 and

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[7] Applying the more standard definition of family income—the average of log total family income –to AM yields an estimate of 0.377. Full regression results are available from the author.

2. If anything, allowing for cohort variation in the IGE amplifies trend in class size effects on intergenerational mobility (see Figure 2). We also see that the aberrant estimates for the cohorts born in the 1960s under specification (2) stem from the failure to account for the substantial increase in the IGE among these same men.

This cross-cohort pattern might be explained by the theories discussed in Section II above. Theory suggests that school quality improvements increase intergenerational mobility when public schooling substitutes for private expenditures and credit constraints limit the educational investments of low-income families. This description might well describe early-20th century America. Most men born prior to the 1930s would not have attended college. And so primary and secondary school quality may effectively proxied for "quantity of public schooling provided." As the century progressed, returns to skill increased and quality high school training became important to college entrance. In that context, greater school quality may have been more beneficial to children from high-income families than those from low-income families.

V. Alternative Interpretations: Omitted Variables, Endogenous Class Size, and Unequal Policy Implementation

The four regression specifications in Table 2 demonstrate a robust correlation between class size and intergenerational mobility. States with large class sizes show greater intergenerational mobility. And this pattern strengthened between the 1921 and 1975 birth cohorts. Previous research on the functional form of the intergenerational earnings regression suggests these patterns may be explained by omitted variables bias. A large number of studies document a positive relationship between family income and the IGE in the US (Behrman and Taubman 1990, Solon 1992, Mulligan 1997 and 1999, Grawe 2001b, and Mazumder 2005). Indeed, when the work of AM is replicated with a quadratic relationship between child earnings and total family income, the estimated regression line is highly convex.[8] It also seems likely that states with higher family incomes have smaller class sizes. Together, these raise the possibility that the class size-family income interaction is picking up the effects of an omitted quadratic term in family income.

Table 3 repeats the regressions in Table 2, but allows a quadratic intergenerational earnings relationship. The reported IGE and effect of class size on the IGE are calculated at the mean level of total family income. Qualitatively, the results are the same as reported in Table 2, though the size of the effect may be somewhat smaller. When the IGE varies across cohorts but the effect of class size constant (column 3), the estimated effect of class size is about 20% smaller. The results are less precise when class size effects vary across birth cohort: large standard errors on interactions between class size and squared family income often lead to large standard errors on the cumulative effect reported in the table. This is especially true when the quadratic IGE varies across cohort (introducing considerable multicollinearity). On the whole, the results of Table 3 continue to suggest a negative relationship between class size and the IGE.

[Table 3 goes here.]

Endogeneity in the class size variable may also explain the effects on mobility. Perhaps some unknown variable causes both greater earnings persistence and a political penchant for funding smaller class sizes. If this were the case, we would expect some states to generally have lower (higher) class sizes and a larger (smaller) IGE. To check for this, for each individual I compute the average class size in that person's state of birth

[8] Regression results available from the author.

in all years the individual was *not* between the ages of 6 and 18. I then add this average class size variable to the regression analysis along with its interaction with average log family income.

The results in Table 4 are generally inconsistent with this endogeneity story. The effects on IGE of class size in years in which the individual was not in K-12 schooling are very close to zero. And the estimated effect of experienced class size on the IGE is nearly identical to that reported in Table 2. These findings are exactly what we would expect if the results in Table 2 were *not* driven by policy endogeneity. Of course, the test in Table 4 cannot rule out all possible forms of endogeneity. But the strong robustness of the results to the inclusion of class size outside the individual's schooling years suggests the results in Table 2 warrant careful consideration.

[Table 4 goes here.]

Even if the mobility-reducing effects of class size reductions in Table 2 are real, this does not imply that reducing the pupil-to-teacher ratio in all classes will negatively impact mobility. The data used here are aggregated to the state level. The results above indicate that lower class size within a state correlates with less mobility. It may not be the class size reduction so much as the implementation of the reduction that drives the results. In particular, if states' class size reduction policies give schools in high-income districts preferential treatment, this would explain why state-cohorts with lower pupil-to-teacher ratios show less mobility. For example, the recent class size reduction in California required a more than 30% increase in the number teachers in just two years. Stetcher et al. (2005) find that rapid implementation of the policy led to a reduction in average teacher qualifications that was disproportionately experienced in minority communities. Thus, it may not be class size reductions *per se*, but rather the way in which US states have implemented reductions, that reduce mobility.

This interpretation would reconcile the mobility results reported here with the broader literature on class size impacts which often finds greater benefits of school quality among children from disadvantaged groups. For example, Krueger (1999) studies the Tennessee STAR program and finds that children randomly assigned to a class with five fewer pupils performed better on exams. The impact, however, was stronger for blacks, free-lunch recipients, and inner-city residents; suggesting that class size reductions are stronger for low-income families than for high-income families. As Summers (2003) points out, the key question is whether it is even possible to scale-up classroom-level programs like STAR while maintaining the size and distribution of their effects.

VI. Conclusion

Using US census data from 1940-2000, this paper estimates intergenerational mobility in the 50 states and District of Columbia among 12 birth cohorts of men born between 1921 and 1975. When combined with information on K-12 school quality in each state at the time each cohort was between the ages of 6 and 18, the data show a positive relationship between class size and mobility: the father-son earnings association in state-birth cohorts with class sizes one standard deviation below the mean is more than 40% stronger than in state-birth cohorts with class sizes one standard deviation above the mean. Moreover, the reduction in mobility associated with class size reductions has strengthened over time.

One possible explanation for this result is that class size is endogenous. It may simply be that states with lower class sizes are relatively immobile for other reasons entirely. To explore this possibility, the analysis was repeated including controls for the average class size within the state in years the individual was *not* of primary or secondary school age. The results are essentially unchanged. Taken at face value, the results indicate that children from high-income families are more benefited by statewide class size reductions than those from low-income families.

Care must be taken when generalizing these results. It may be that improvements to public school quality result in greater earnings persistence while increases in public school quantity lead to greater mobility. This may be especially relevant when considering developing nations with relatively little educational support. Second, the effects of school quality on mobility may depend on the level of schooling. In particular, this study can say nothing concerning improvements in pre-K or higher education. Finally, the US may be different from even other developed nations. Recalling Corak's (2006) comparisons shown in Figure 1 above, it appears that the US is an outlier. In particular, the implementation of class size reductions in US states may be less progressive than in other countries. With the large administrative data sets available in several of these countries, future research may look to see whether the US experience is unique.

Figure 1: Intergenerational earnings persistence and per pupil education expenditure

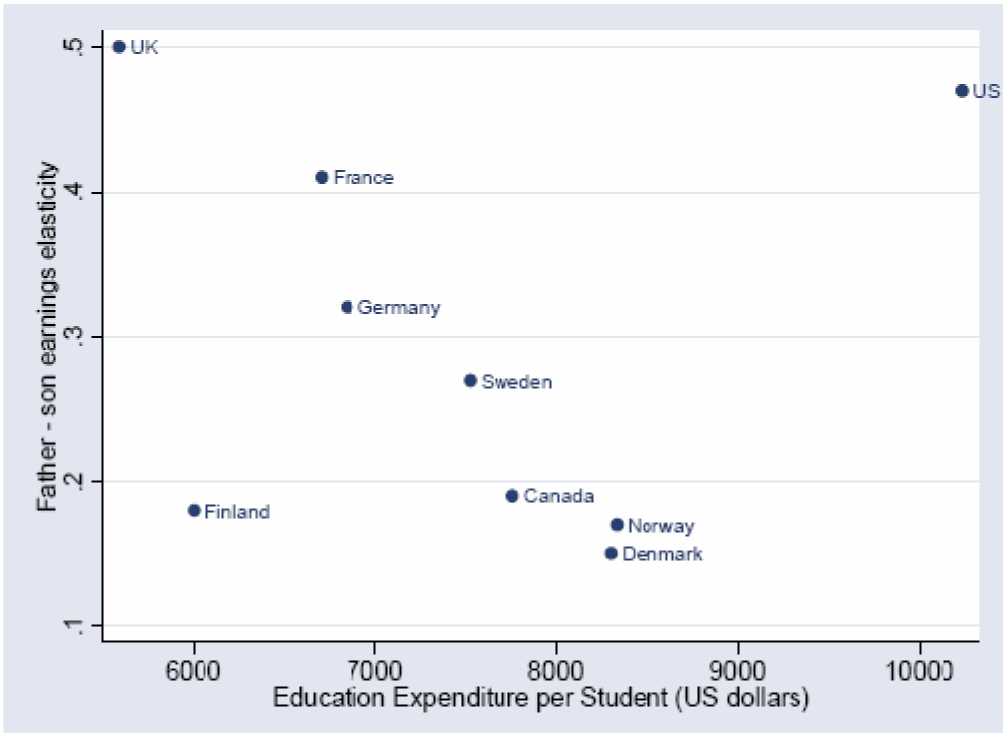

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Figure 2: Mean and standard deviation of class size across US States: 1927-1993

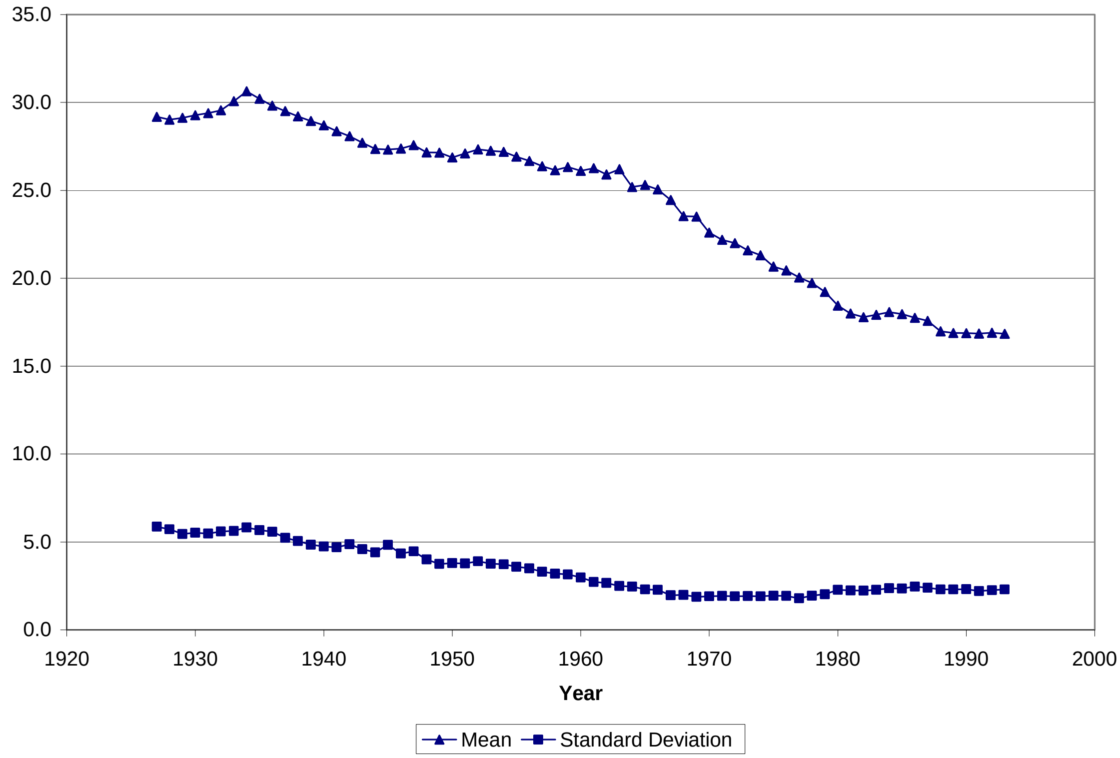

Figure 3: Estimated effect of class size on intergenerational earnings elasticity (IGE) by birth cohort: 1921-1975

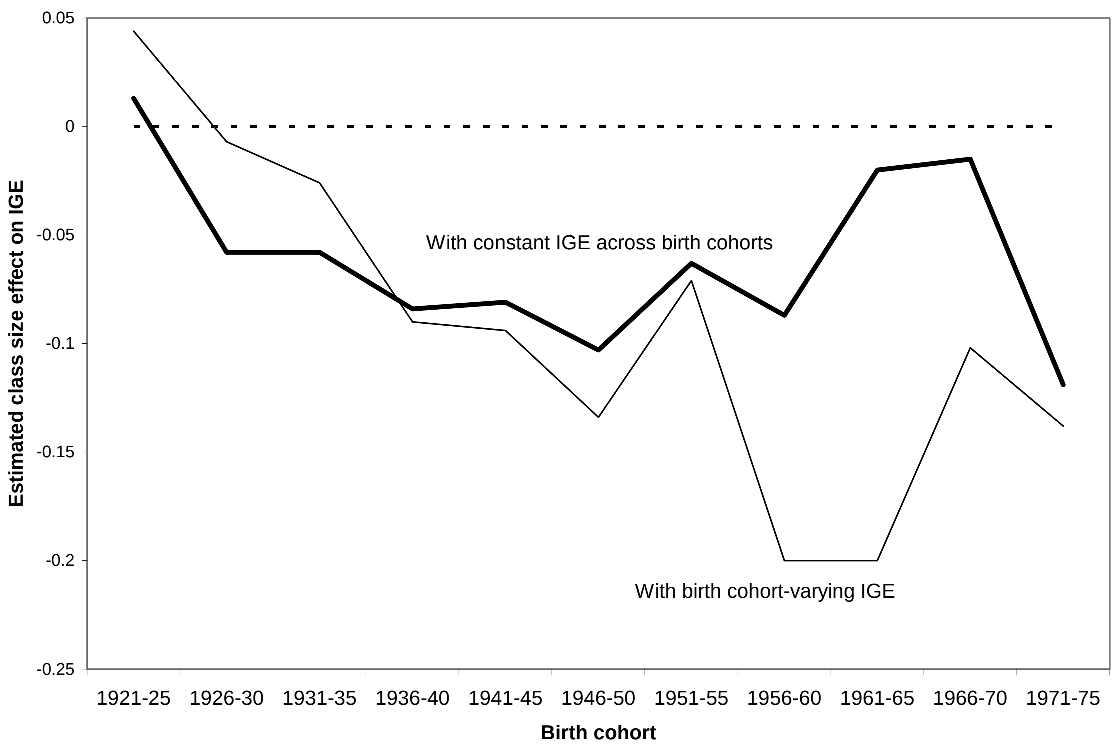

Table 1: Summary statistics

	Mean	Standard Deviation	Minimum	Maximum
Log earnings	10.32	0.85	0.28	12.861
Age	37.57	8.26	25.00	54.00
Log total family income	10.09	0.53	8.31	10.81
Class size (level)	25.29	4.22	13.85	40.07
Class size (standardized)	0.17	0.82	-2.66	3.19
N	1,136,007			

Table 2: Effect of class size on intergenerational earnings elasticity (IGE)

	Model Specification			
	(1)	(2)	(3)	(4)
IGE	0.397	0.398	-	-
	(0.015)	(0.016)		
Birth cohort 1921-25	-	-	0.354	0.323
			(0.029)	(0.022)
1926-30	-	-	0.307	0.285
			(0.026)	(0.025)
1931-35	-	-	0.373	0.346
			(0.028)	(0.030)
1936-40	-	-	0.387	0.397
			(0.023)	(0.024)
1941-45	-	-	0.404	0.418
			(0.024)	(0.027)
1946-50	-	-	0.393	0.443
			(0.025)	(0.032)
1951-55	-	-	0.417	0.414
			(0.023)	(0.023)
1956-60	-	-	0.488	0.562
			(0.026)	(0.046)
1961-65	-	-	0.586	0.641
			(0.048)	(0.059)
1966-70	-	-	0.502	0.513
			(0.048)	(0.069)
1971-75	-	-	0.437	0.452
			(0.047)	(0.049)
Class size effect on IGE	-0.070	-	-0.075	-
	(0.013)		(0.014)	
Birth cohort 1921-25	-	0.013	-	0.044
		(0.033)		(0.026)
1926-30	-	-0.058	-	-0.007
		(0.029)		(0.028)
1931-35	-	-0.058	-	-0.026
		(0.026)		(0.031)
1936-40	-	-0.084	-	-0.090
		(0.025)		(0.027)
1941-45	-	-0.081	-	-0.094
		(0.017)		(0.023)
1946-50	-	-0.103	-	-0.134
		(0.019)		(0.029)
1951-55	-	-0.063	-	-0.071
		(0.016)		(0.017)
1956-60	-	-0.087	-	-0.200
		(0.043)		(0.045)

1961-65	-	-0.020	-	-0.200
		(0.053)		(0.059)
1966-70	-	-0.015	-	-0.102
		(0.067)		(0.100)
1971-75	-	-0.119	-	-0.138
		(0.054)		(0.061)
N	1,136,007	1,136,007	1,136,007	1,136,007

Note: All regressions include a quartic in age-40, age-40 interacted with log family income, age-40 interacted with year of earnings observation (with a common age-earnings profile in 1950, 1960, and 1970), class size, class size interacted with cohort, and dummy variables for year of earnings observation and birth cohort. Clustered (by state) standard errors in parentheses.

Table 3: Effect of class size on intergenerational earnings elasticity (IGE) allowing for non-linear IGE

	Model Specification			
	(1)	(2)	(3)	(4)
IGE (at mean family income)	0.445 (0.19)	0.450 (0.022)	-	-
Birth cohort 1921-25	-	-	0.395 (0.168)	0.359 (0.124)
1926-30	-	-	0.566 (0.151)	0.372 (0.147)
1931-35	-	-	0.593 (0.108)	0.525 (0.129)
1936-40	-	-	0.702 (0.085)	0.657 (0.118)
1941-45	-	-	0.430 (0.028)	0.440 (0.027)
1946-50	-	-	0.434 (0.034)	0.455 (0.030)
1951-55	-	-	0.379 (0.049)	0.189 (0.073)
1956-60	-	-	0.296 (0.072)	0.188 (0.100)
1961-65	-	-	0.235 (0.174)	0.362 (0.292)
1966-70	-	-	0.383 (0.182)	0.600 (0.317)
1971-75	-	-	0.605 (0.218)	0.713 (0.259)
Class size effect on IGE (at mean family income)	-0.092 (0.015)	-	-0.058 (0.015)	-
Birth cohort 1921-25	-	0.370 (0.159)	-	0.400 (0.157)
1926-30	-	0.355 (0.104)	-	0.360 (0.109)
1931-35	-	0.197 (0.089)	-	0.180 (0.082)
1936-40	-	0.099 (0.087)	-	0.051 (0.063)
1941-45	-	-0.132 (0.036)	-	-0.092 (0.036)
1946-50	-	-0.175 (0.036)	-	-0.163 (0.041)
1951-55	-	-0.073 (0.020)	-	0.084 (0.043)
1956-60	-	-0.081 (0.039)	-	0.100 (0.090)

1961-65	-	0.064	-	-0.210
		(0.295)		(0.489)
1966-70	-	-0.546	-	-0.815
		(0.319)		(0.677)
1971-75	-	-0.379	-	-0.227
		(0.388)		(0.457)
N	1,136,007	1,136,007	1,136,007	1,136,007

Note: All regressions include a quartic in age-40, age-40 interacted with log family income, age-40 interacted with year of earnings observation (with a common age-earnings profile in 1950, 1960, and 1970), class size, class size interacted with cohort, and dummy variables for year of earnings observation and birth cohort. Clustered (by state) standard errors in parentheses.

Table 4: Effect of class size on intergenerational earnings elasticity (IGE) conditional on class size in state during years the individual is *not* of school age

	Model Specification			
	(1)	(2)	(3)	(4)
IGE	0.400	0.401	-	-
	(0.016)	(0.016)		
Birth cohort 1921-25	-	-	0.358	0.325
			(0.029)	(0.021)
1926-30	-	-	0.311	0.290
			(0.026)	(0.025)
1931-35	-	-	0.378	0.353
			(0.030)	(0.021)
1936-40	-	-	0.391	0.404
			(0.024)	(0.026)
1941-45	-	-	0.403	0.420
			(0.026)	(0.029)
1946-50	-	-	0.388	0.438
			(0.027)	(0.032)
1951-55	-	-	0.418	0.414
			(0.023)	(0.021)
1956-60	-	-	0.500	0.569
			(0.035)	(0.043)
1961-65	-	-	0.615	0.659
			(0.046)	(0.057)
1966-70	-	-	0.549	0.545
			(0.051)	(0.068)
1971-75	-	-	0.482	0.484
			(0.049)	(0.051)
Class size effect on IGE	-0.075	-	-0.082	-
	(0.013)		(0.014)	
Birth cohort 1921-25	-	0.010	-	0.041
		(0.032)		(0.026)
1926-30	-	-0.067	-	-0.018
		(0.027)		(0.027)
1931-35	-	-0.067	-	-0.037
		(0.025)		(0.031)
1936-40	-	-0.092	-	-0.101
		(0.024)		(0.027)
1941-45	-	-0.091	-	-0.105
		(0.019)		(0.025)
1946-50	-	-0.112	-	-0.140
		(0.021)		(0.029)
1951-55	-	-0.068	-	-0.075
		(0.017)		(0.017)
1956-60	-	-0.085	-	-0.199
		(0.042)		(0.042)

1961-65	-	-0.004	-	-0.188
		(0.054)		(0.059)
1966-70	-	0.018	-	-0.082
		(0.075)		(0.104)
1971-75	-	-0.088	-	-0.110
		(0.059)		(0.065)
Effect on IGE of class size in years individual was *not* of school age	0.001	0.002		0.002
	(0.001)	(0.001)		(0.001)
N	1,136,007	1,136,007	1,136,007	1,136,007

Note: All regressions include a quartic in age-40, age-40 interacted with log family income, age-40 interacted with year of earnings observation (with a common age-earnings profile in 1950, 1960, and 1970), class size, class size interacted with cohort, and dummy variables for year of earnings observation and birth cohort. Clustered (by state) standard errors in parentheses.

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