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The Effects of School Quality on the Youth Labor Market

Kevin Murphy and Sam Peltzman¹

I. Introduction

How does the quality of education received by children affect their performance when they enter the labor market? This paper is an attempt to answer this question for new entrants to the labor market over a period from 1970 to the mid 1990s. In so doing we try to pull together some strands in the literature on both education and the labor market. We also hope to shed light on some of the policy concerns lurking in the background of the relevant literature.

Most previous studies of the effect of school quality on the labor market, beginning with Card and Krueger (1992), measure quality with inputs (school expenditures, teacher-pupil ratios). We focus instead on an output measure - test scores. Thus our work is also related to the literature on 'education production functions,' which tries to estimate a link between education inputs and outputs.

Both the school inputs-labor market and the education production literatures are unsettled.² But they also stand in uncomfortable juxtaposition. According to Eric Hanushek (1996) the central tendency of hundreds of education production function studies is that there is no reliable connection between school inputs and outputs. According to Card and Krueger (1996) there is usually a positive relation between school inputs and earnings. While one or both of these results may be wrong, ³they raise an obvious question:

"How is it that principals and teachers can effectively use school resources to produce improvement in labor market outcomes but fail to use extra resources to produce gains in academic achievement?" (Burtless, 1996)

Our results do not directly resolve this conundrum. But they provide an important perspective: academic achievement is also valued in the labor market. And, in a sense to be described, this link is the more important empirically.

These academic debates mirror larger concerns about the quality of public education and stagnating incomes of high school graduates. In the case of public education, the broad trend since the late 1960s has been first declining and then stagnant test scores in the face of a substantial increase in per pupil expenditures. The worrisome labor market trends begin about a decade later and entail a significant fall in real earnings of those not going beyond high school. The timing raises another obvious question: did the decline in school performance contribute to the decline in returns to completing high school? While the reduction in returns to high school completion is too pervasive for school performance to be a major cause, our results suggest that there is some link between the two.

The first contribution of this paper is to describe the relation between changes in school system performance and changes in earnings for those who enter the labor force without going on to college⁴. By focusing on this group we hope to capture a 'pure' effect of school quality that is undistorted by subsequent college education or on the job training and experience. Accordingly, we analyze

labor market outcomes for these workers only in the first few years after their entry into the labor force.

We have a ‘view’ about what forces other than school quality affect the earnings of these new entrants, and our empirical analysis incorporates it. While this aspect of the empirical work is not a major focus of the paper some of the results we obtain seem interesting enough to warrant further scrutiny.

The decision to enter the labor force or go on to college can itself be affected by school quality. Accordingly, we examine that possibility empirically.

Finally, we analyze some aggregate outcomes: specifically, is the growth in employment related to school quality? More specifically, do employers tend to migrate to areas with improving school systems?

Our unit-of-analysis is the state, because this is the political entity constitutionally responsible for public education in the U. S. (and public schools enroll around 90 per cent of all students). Consequently, the questions we try to answer are about state cross-sections: how are changes in the relative performance of a state’s schools related to the relative changes in outcomes for the schools’ graduates?

The next section describes the data we use and how we use them. This is followed by our results. In a nutshell, these are that declining school quality is associated with lower wages for new labor market entrants, reduced ‘job quality’ and a lower probability of college entrance. There is weak evidence that on-the-job training or experience dilutes some of the wage effect. We also find that total

employment – not just the employment of young workers – is reduced when school quality declines.

II. Data

A. State School System Performance

The raw material for our school performance measure is around 10 million records of 17-20 year olds who were administered the Armed Forces Qualifying Test (AFQT) by the U. S. Department of Defense from 1971 to 1991 as part of the military recruitment process. A detailed description of how this raw material was processed into state school performance measures is in Peltzman (1996). In brief summary, each year's vector of AFQT scores is regressed on individual characteristics, characteristics of the individual's household and neighborhood and state dummies. This yields a panel of 20 years of 48 state fixed effects. These fixed effects are interpreted as measures of the relative level of school performance in a state in a given year. (The mean level is constrained to equal zero each year).

This interpretation raises several problems that are discussed at some length in Peltzman (1996). One that will probably occur to most readers is that the underlying sample is non-random. So it is perhaps worth mentioning that our measure is an essentially unbiased estimate of state fixed effects for similar tests given to two random samples around 10 years apart⁵. Also, much of our focus is on the change in these fixed effects over time. This eliminates the

influence of persisting unmeasured state level differences in student backgrounds. Finally, the changes in our fixed effects essentially mirror the geographic patterns of changes in available random samples.⁶ Therefore, at least as far as we can tell, the non-random character of the sample is not introducing any bias into our estimates.

Our measure also has a compelling virtue. It reflects the performance of the group we are specifically interested in – youngsters going directly into the labor force from high school.⁷ By contrast, the most widely available alternative state level panel data – SAT scores – measures the performance of college entrants. The available evidence suggests that there are important differences in recent school performance trends of these two groups.⁸ Further, our measure is based on large samples, and it is predominately male, as are the labor market entrants we choose to focus on.

Another issue is whether any effects we uncover are due to school quality or some unmeasured worker characteristics. For example, at the individual level, we know that someone who does well on a test like the AFQT also tends to get paid well.⁹ Accordingly, high scoring groups would be expected to be well paid on average whether or not their schooling contributed to their high scores. How can we be sure any relation between an average test score and earnings measures an impact of schooling?

The short answer of course is that we can never be sure. However, we believe we have gone about as far as the data permit in controlling for

measurable characteristics that affect both test scores and labor market outcomes. For the AFQT scores, for example, we have controlled not only for the standard personal characteristics but also characteristics of the household and neighborhood – incomes, poverty rates, incidence of single heads of household, worker occupations – where the individual lives. Then, effectively, we average what remains over large samples (around 10,000 per state per year). Finally, we focus on the change over time. Thus, to absolve schools from affecting our measure, a difference in the average score for two¹⁰ groups of 10,000 individuals, identical in every respect we can measure, but educated 20 years apart, has to be due to some systematic difference in personal characteristics that we cannot measure. We proceed as if this is unlikely.

Also, our measure of earnings for the young labor market entrants is net of (time-varying) state fixed effects. So our measure will be unaffected by changes in the character of a state's older workers (including the parents of the young entrants) that are reflected in earnings of both young and old.¹¹

Most of our work includes, in addition to the test-score based measures of school performance, the growth of school inputs (teacher/pupil ratios) in each state.

B. Earnings of Young Workers

The earnings data comes from the 1970 and 1990 Census Public Use (IPUMS) files. The group we are specifically interested in is: male, 19-23 years old

and has 12 or less years of education, hereinafter YHS. (Thus, there is a slight mismatch between this group and the 17-20 year olds from whom we draw our school performance measure.)

We extract from the IPUMS files a sample consisting of males over 18 who are not students, who have up to 40 years' experience and who have wage and salary income. Then, we regress the log of hourly earnings on a standard list of characteristics¹² plus state fixed effects plus fixed effects for the YHS cohort in each state. The latter – the deviation of YHS average log hourly earnings in each state from its predicted earnings – is the focus of our analysis. Specifically, we analyze the change from 1970 to 1990 in the YHS fixed effects.

We want to see if this change is related to the change in performance of the state's schools. Accordingly, 'state' is defined as the state of residence 5 years ago for the purpose of estimating the YHS fixed effects. In this way we are more or less certain that the 19-23 year old workers actually obtained at least some of their schooling in the state to which they are assigned.

III. Changes in School Quality and Labor Market Outcomes

To implement our goal of relating changes in measured school performance to changes in earnings of young workers we pay attention to some of the broader labor market trends of the 1970-90 period. This period is characterized by an increased demand for and return to skill, particularly technical skills (Juhn, Murphy and Pierce (1993), Murphy and Welch (2000)). One

implication of this is that the initial level of school performance, as well as the change, should affect changes in labor market outcomes. Graduates of school systems with a high initial quality level would benefit from the increased demand for skill even if there were no change in the school system's performance.

To illustrate this point assume for simplicity that there are only two kinds of workers, high quality and low quality, who are paid nationally determined wages of w_H or w_L respectively. Thus local school quality affects wages only via effects on the share, s , of high quality workers. Specifically, the average wage in a state, w , will be:

$$w = s w_H + (1 - s) w_L = s p + 1 \quad (1.1)$$

where, for convenience, we set $w_L=1$ and $w_H= 1 + \text{a skill premium, } p$. We are going to analyze the change in w over time, and this is approximately

$$\frac{dw}{dt} = s \frac{dp}{dt} + p \frac{ds}{dt} \quad (1.2)$$

So an increase in the national skill premium ($\frac{dp}{dt} > 0$) implies that wages grow with the level, as well as the change, of school quality.

We are less sure about the impact of the growing demand for technical skills on the YHS group. They have few such skills themselves. So the crucial question is whether young untrained workers are complements or substitutes for more skilled workers. Since we do not know the answer, we simply add a crude

control for the initial level of technology-intensity of the state's industry (per-capita research and development spending) to the subsequent analysis.

Growth in demand for labor generally – or, even more generally, of output – should have a more predictable effect. The YHS group has few skills, little experience and low firm-specific attachments (seniority, specific skills). Thus demand for them should be unusually responsive to growth, especially to the growth of new businesses where their lack of seniority and firm specific skills is no handicap. In the empirical work, we attempt to control for some 'background' conditions that could foster or retard business formation. (Actual business entry/exit is, of course, in principle endogenous to school quality, and we address that linkage later.) These are state and local tax rates and a simple count of tax and regulatory incentives offered by the state for new businesses.

In addition, we use national employment and wage trends and the state's 1970 industrial mix to control for some exogenous sources of growth in demand for the state's work force. For example, if the state had a small steel industry and a large computer industry in 1970 it should have experienced above average growth in demand for labor in the next two decades. To get at this is we use 1970 industry employment shares in each state as weights, multiply these by the subsequent growth in US total employment or wages for each industry and sum. This yields two demand-growth proxies, both independent of actual growth in the state. The proxy would equal actual growth in a state if each of the state's industries experienced the national growth rate of employment or wages.

Table 1 provides a summary of both the school quality measures and the controls used in subsequent analysis. The table also summarizes the YHS wage change variable as well as other dependent variables analyzed in the paper. An appendix provides more detail and sources on all of these.

A. Wages and School Performance

Table 2 summarizes the link between school quality and wages. The first two columns show that there is no straightforward connection that leaps from the data. Neither the test score variables nor the change in school resources are significantly related to changes in wages over time for young labor market entrants standing by themselves.¹³ But this pattern changes markedly when some non-schooling labor market controls are added. The thrust of these results, in regressions (3) and (4), is that the performance of schools affects the wages of its graduates. And, to add fuel to an ongoing debate, school resources also matter.

These schooling effects are significant in magnitude as well as statistically, as indicated in the last column of the table. For example, consider two young workers entering the labor market out of high school. One was educated in a state with unchanging (relative) test scores, the other in a state with test scores that rose by a standard deviation. The beta of .51 for the change in school performance means that the second worker would receive, on average, 3 per cent higher pay than the first. To put this in context, note that from 1970-90 average pay for the YHS cohort declined by 3 per cent nationally relative to similarly

educated older workers (see Table 1). Thus, our results suggest that comparatively modest improvements in school performance could have entirely offset this.

The results also show that the level of school performance was at least as important as the change. If our interpretation of this result – that it reflects an increased demand for quality – is correct, there is an implicit warning for future graduates of high performing school systems. Geographic differences in school performance have been narrowing over time.¹⁴ If this continues, our results imply that graduates of school systems with high but falling performance will begin to pay a price that has so far been masked by the increased quality premium in the labor market.

The other results in Table 2 suggest that young workers' wages are sensitive to the overall state of the local labor market. The ideal locality for a young worker, according to these results, would be one with a lot of high tech industry and industries with growing employment and with an increasingly 'business friendly' government. (So, while the labor market rewards young workers educated in smaller classes, it also penalizes them for the associated taxes.¹⁵)

B. Job Quality

We also analyzed the 'quality' of jobs obtained by young workers to try to gain insight into the sources of the wage effects of school performance.

Graduates of improving schools could earn more because they are paid more for the same job and/or because they obtain higher paying jobs. To sort between these possibilities, we estimated regressions like those in Table 2, except that the worth of an individual's job, not the individual's earnings, is the dependent variable.

Specifically, we first assigned every individual in the sample the 'value' of his job. This is just the national average of hourly earnings for the individual's reported industry and occupation. (See appendix for details.) We then regressed individuals' job values in 1970 and 1990 on the same set of variables as in the log hourly earnings regression – i.e., characteristics plus state fixed effects plus YHS cohort-state fixed effects. Finally, we used the 1970-90 change in the YHS state fixed effects as the dependent variable in regressions with the same right-hand side variables as in Table 2. (See Table 1 for summary statistics for this dependent variable.)

The results of interest are the school performance coefficients in counterparts of regressions [3] or [4] in Table 2. For the latter, these coefficients ($|t|$ in parentheses) were:

dAFQT: .082 (1.9)

AFQT71: .042 (2.6)

dTchr/Pupil: -.223 (0.8).

And dropping the school resources variable yields:

dAFQT: .093 (2.3)

AFQT71: .048 (3.4).

These results imply that school performance - but not resources - favorably affects the quality of jobs obtained by young labor market entrants.¹⁶ Moreover, the effects are quantitatively meaningful: the beta coefficients for the two school performance variables here are roughly the same as those in the wage regression. Given the magnitudes involved, the wage and value results imply that the effect of school performance is about evenly split between higher pay for the same job and higher job quality.¹⁷

We show later that some of the job quality effects may stem from employer location decisions.

IV. Who Goes on to College?

So far we have looked at outcomes for those going from high school into the work force, taking that transition as a given. However, around half of this age cohort continues their education beyond high school. And school quality could, in principle, affect the decision on the margin between work and college. Table 3 explores whether this margin is affected in practice.

The dependent variable here is the change in a state fixed effect from 1970 and 1990 logistic regressions describing the college/work choice of 19-23 year old males. The logistic regressions (not shown) include everyone in this age group, and the dependent variable = +1 for those high school graduates who report that they are currently a student or have completed at least one year of

education beyond high school. The independent variables are age and race dummies plus state fixed effects. Accordingly the dependent variable in Table 3 is approximately the average change in the state from 1970 to 1990 of the log-odds that a 19-23 year old male of given characteristics will go on to college.¹⁸

The first regression in Table 3 includes all of the independent variables used to explain wage changes in Table 2. None of the school quality variables are significant here.

However, it is unclear that the initial level of school quality (AFQT71) belongs in this model. It is a proxy for the fraction of the YHS cohort that is eligible for college. This fraction should be systematically related to the change in the odds of going to college only if the share of college-eligible who matriculated (m) also changed systematically.¹⁹

But m was pulled in opposing directions during this period. Table 2 tells us that, relative to other young workers, wages of college-eligible youth rose (i.e., the coefficient of AFQT71 is positive in the wage regression). This would pull m down. However, we also know that the rate of return to college rose (Juhn, Murphy and Pierce, 1993), and this would pull m up. Accordingly, there is no reason for expecting either a positive or negative effect from the level of school quality.

When the quality level variable is dropped (regression [2]), the coefficient of the change in school quality – but not that of the change in school resources –

becomes significantly positive. The magnitude of the effects of the two changes follows the same ranking as their significance.

By contrast to the school quality change effects, the coefficients of the other controls in Table 3 tend to have signs opposite to those in Table 2. This is the expected pattern: increases in the demand for this cohort's labor would tend to pull those on the margin toward work and away from college.

The results in Table 3 also suggest that Table 2 understates the effect of improved schools on wages. Presumably high school students on the margin of going on to college have more unobserved ability than those who will not go on. Accordingly, as more students from improved schools go on to college the average ability (and wages) of labor force entrants – the group whose wage we measure in Table 2 – would decline. That decline would reduce the estimated wage effect of improved schools in Table 2. So, the effect for a worker of *given* quality will be larger than shown in Table 2.

V. Job Creation and School Quality

So far we have emphasized the effects of changed school performance on the affected students. Now we ask whether employers and possibly other workers share in those effects. Specifically, we ask whether overall employment growth is sensitive to changes in school quality.

Such an effect could arise if the benefits of school performance are not fully capitalized into some combination of wages and property values. In that

case, investment decisions, like where to locate a business, will be sensitive to the expected benefits and costs associated with the quality of schools. These include more than the net productivity of today's high school graduates. For example, the expected productivity of future new hires, the cost of on-the-job training (including remedial efforts) and the cost of attracting mature workers with school-age children could all be affected by school quality.

Table 4 explores how important such school quality effects on the state macro economy are in practice. The dependent variable in the first two regressions is the change in the log of total employment in the state. The first regression includes as controls only exogenous predictors of state labor market growth: the previously used share weighted national growth rates of employment and wages plus a projection of total state employment in 1990 made around 1970 by the Census' Bureau of Economic Analysis. The second regression adds some more controls that sometimes helped explain the demand for young workers, but which add little here. The only notable feature of the controls is that the BEA projections are unbiased over two decades.²⁰

Both regressions reveal a statistically and economically meaningful relation between school performance and state economic growth. For example, a state whose schools' relative performance declined by a standard deviation lost 8 percent of its jobs over the sample period. This is nearly half the sample standard deviation of job growth.

The third regression in Table 4 tries to see how much of the effect on growth is plausibly coming from migration of employers to states with improving schools. Here we focus on employment growth in a relatively footloose ('mobile') set of industries, net of employment growth in less mobile industries. We define industry mobility according to the cross-state variability in industry employment growth that is unexplained by population growth.²¹

While there is some room for doubt, the coefficients of regression [3] imply that a disproportionate share of the employment effects of school performance is coming from migration of industry to areas with improving school performance. A one standard deviation improvement in relative school performance is associated with 7 per cent more employment growth in the mobile than the non-mobile industries. But the employment growth effect is significantly positive in both industry groups.²²

There is a direct link between this last result and the earlier results on job quality. The mobile industries, as we define them, are dominated by manufacturing, and wages in this sector are relatively high for the kind of work done by youngsters entering the labor market from high school. Thus, when an improved school system attracts footloose employers, more 'good jobs' tend to be available for the young labor market entrants in the area.

Finally, note that there is no – or perhaps a slightly negative - effect of school resources on employment growth in all of the regressions.

VI. How Long do School Quality Effects on Wages Last?

Post-schooling efforts, such as on-the-job or in-house training, can substitute for skills that may not have been acquired in school. If the labor market values school quality and a school system does not deliver it, workers (and employers) will have an incentive to seek these substitutes. Accordingly, we would expect some narrowing of the effects of school quality we have observed for entry-level workers as those workers age.

This possibility is examined in Table 5. Here we compare wages for the YHS cohort in 1990 to their wages in the mid-1990s when they were 4 to 8 years older. The mid-1990s data comes from the Current Population Surveys (CPS). For example, a 19 year old in 1990 would be 23 in 1994 and 28 in 1998. So we include 23 year-olds in the 1994 CPS and 28 year-olds in 1998 in our comparison sample. More generally, the comparison sample includes workers in the mid-1990s who could have been in the 1990 YHS cohort.²³

Even with five CPS' worth of data, the relevant sample sizes are much smaller than for the 1990 YHS cohorts. The comparison cohort totals around 6000, or less than 100 for most states. This lends some fragility to any conclusions.²⁴ In addition, we lose information on the state where the individual was educated and have to classify by state of residence instead. This is a more serious shortcoming for individuals in their late 20s than for a recent high school graduate.²⁵

These caveats in mind, Table 5 hints that some of the school quality effects might wear off over time. However, the magnitude of any ‘mean-reversion’ is imprecisely estimated and sensitive to which other controls are included.

The first panel looks at what happened to (state fixed effects for) wages of 19-23 year olds as they got older and more experienced. Panel B compares these older workers in the mid 1990s to their age counterparts in 1990. The main independent variable is the premium (relative to 1970) these older, mid 1990s workers received in 1990 for the quality of their education. It is simply the sum of the three school quality coefficients in the last regression of Table 3 times the values of those variables in the state.²⁶

The first regression in each panel regresses the change in fixed effects on just the quality premium. The other regressions add updated versions of the controls that affected wages for these workers when they entered the labor force. The only control that seems close to mattering is the ‘high-tech’ proxy, per capita R & D spending in the state. Its own coefficient is *negative*, and its inclusion substantially increases the point estimate of the degree of mean reversion in school quality.

While these two results are somewhat weak statistically, they suggest a general reversal of the impact of the demand for skills on the youth labor market in the 1990s. The growth of skill differentials in the broader labor market has slowed or even ceased in the 1990s (Murphy and Welch, 2000). So, the results in Table 5 are consistent with the view that wages of young workers are highly

sensitive to changes in the broad labor market. On this interpretation, a good part of the school quality premium acquired in the 1970s and 1980s came from the growth in demand for skills generally. When that growth abated those premia receded.

The safest conclusion from Table 5 is probably that there is some convergence of the school quality premia over time. But it is hard to say to what extent this is driven by labor market conditions in the 1990s as opposed to a more general process of adaptation by workers and employers over time to the quality of the education they received. Moreover, our data say nothing about the costs of any such adaptation.

VII. Summary and Conclusions

The performance of schools has important effects on the labor market. No single result in this paper would support so unqualified a conclusion, but we think the overall pattern of results does. We can convey that pattern with a parable.

Consider the State of Midland, an average state in every respect 20 years ago. Of its 5 million people, 2 ½ million worked and ½ million had recently graduated or left high school. Half of these products of Midland's schools went on to college and half went into the work force and earned around \$6 per hour in their entry-level jobs. Over the next 20 years Midland remained average except for one thing: its schools slipped behind those elsewhere. As measured by scores

on standard achievement tests, Midland declined from 25th in the nation to 37th - i.e. from the middle to the middle of the bottom. This was a notable, but hardly unprecedented decline.²⁷

What were the consequences of this decline in the performance of Midland's schools? Putting complications (from gender and normal population and income growth) aside, here is where our results suggest Midland is today: First, Midland is smaller. There are 150,000 fewer jobs (and presumably 300,000 fewer people), and the job loss is disproportionately in the relatively well-paid manufacturing sector. Of the now 470,000 recent high school graduates, the group going on to college is now less than half the total (231,000). The 239,000 who work make 13 cents less per hour than their counterparts elsewhere, i.e. they lose about a week's wages in a year. (One reason for the lower pay is the aforementioned exodus of high quality blue-collar jobs.)

This is hardly a disaster on the order of what, say, a halving of the price of oil would portend for Texas. But it is a meaningful impact, one that we venture Midland's policy makers would not want to ignore in formulating education policy.

As far as we know, it is an impact of school quality that has not heretofore been documented. Previous related work has almost exclusively focused on the effect of inputs – spending on schools – rather than outputs as we have. We have also tried to gauge the effect of inputs and found one. If Midland's teacher/pupil ratio had also slipped from 25th to 37th in the nation (and taxes had

not been reduced), the 13-cent per hour loss to young workers would have been 7 cents greater. However, there would, as far as we can tell, be no further loss of jobs or job quality and an uncertain effect on college matriculation.²⁸

An important question raised, but not answered, by our results is how they are affected by national labor market trends. Our results come from a period when the relative demand for skills, particularly technical skills, was increasing. Our results also suggest that young workers' wages are sensitive to overall labor market conditions. For example, if there had been a substantial, instead of merely average, high tech component in Midland's industrial base, some of the adverse wage effects on its recent high school graduates would have been avoided.

The plausible reason for this is that these young workers and skilled workers are complements in production. If that is so, we could expect less severe effects of a decline in school performance in an era of declining relative demand for skills.

Appendix

Dependent Variables (and related data)

Tables 2 and 3.

General. All of the outcome measures (log hourly wages, job value, college attendance) used as dependent variables in these tables as well as those mentioned in note__ are 1970-90 changes in state fixed effects for the YHS cohort in regressions as described in the text. The samples used to estimate these regressions are drawn from Census public use files (IPUMS) for the 1970 and 1990 censuses. (www.ipums.umn.edu). Our sample consists of males over 18 with 40 or less years of work experience (defined as age-education-6.)

Our 1990 sample has over 2 million observations, while the 1970 sample only has a little over 700,000. There are 6 public use samples for 1970, but we could use only 2 of them. These were the 'state' sample (ip19702) and a 'county group' sample (ip19704). The relevant difference is that, when a county group crosses state lines (about 9 per cent of the ip19704 group), we have no information about state of residence now or 5 years ago. Therefore we allocated these individuals first according to state of birth. Those born in none of the states in a county group (around 2 per cent of this sample) were then allocated randomly among them so that the states' shares of the county group sample would equal the corresponding population shares.

Outcomes. Except as noted below, outcome measures pertain to year prior to the census.

Log of hourly wage. (Table 2) The sample here is limited to individuals reporting wage and salary income; the regression includes a dummy for those also reporting their main occupation as self-employment. The hourly wage is total wage and salary income divided by total hours as defined below.

Value per hour. (Text) Based on individual's reported industry and occupation. Each individual is assigned the national average of wage and salary per hour for his industry/occupation pair. The national average is computed over all workers aged 25-44 who worked at least 30 hours. There are 441 non-empty industry/occupation pairs from 48 industry and 11 occupation groups. To mitigate measurement problems, we exclude anyone who is self-employed (or has substantial self-employment income) or is in agriculture or the military from the sample.

Economically Active? (note 17) A dummy = +1 for everyone reporting they worked (including self employment), were a student or are currently in the military (but likely

draftees in 1970 – under 26 and in the military – are excluded from the sample), 0 otherwise. The regression is a linear probability model.

Weeks worked/52. As reported by the individual, including zero. For 1970, this variable is reported in 6 intervals. We estimated each individual's location within an interval by using coefficients of a regression of 1980 hours data on interval dummies plus personal characteristics. (The 1970 estimates were adjusted by a constant to yield the 1970 sample mean of weeks worked.)

Total hours. 'Usual hours' per week times weeks worked, including zero hours. For 1970, only 'hours worked last week' is available, and only in 8 intervals. We estimated 1970 usual hours with coefficients from a regression of 1980 usual hours on hours-last-week-interval dummies plus individual characteristics (plus a mean shift adjustment).

Table 4. Employment and Employment Projections

Sources. From U.S. Department of Census, Bureau of Economic Analysis and U.S. Department of Agriculture, Economic Research Service. *1972 OBERS Projections: Economic Activity in the U. S.* This gives 1990 projections for employment and earnings for each state. It also breaks down the state earnings projections (and gives 1970 actual earnings and employment) by 14 industry groups. We estimated implied industry employment projections by assuming that earnings per employee in each state grew at the projected national average. This procedure yielded state/industry employment projections that nearly always summed to within 1 per cent of the projected total state employment. The 1990 actual employment for the same 14-industry breakdown by state is from www.bea.doc.gov/bea/regional/project/projlist.htm. The correlation of actual and projected employment growth across states is around .5.

Mobile and non-mobile industries. For each of the 14 industry groups defined by the BEA we regressed the 1970-90 change in log state employment on the change in log population. Then we ranked the standard errors of estimate from this regression and called the highest 5 of the 14 industries 'mobile.' Thus, we define mobility according to how much the geographic distributions of employment and population changes diverge. The mobile industry groups, in rank order, were mining, durables manufacturing, agriculture, amusement/recreation services, and non-durables manufacturing. The three least mobile groups, on our criterion, were construction, retail trade and professional services. Thus our procedure, while somewhat arbitrary, produces a sensible ordering whereby industries relatively untied to local demand are categorized as mobile.

The mobile industry groups account for about ¼ of total employment, and around ¾ of that is manufacturing (with both figures smaller in 1990 than in 1970). As might be expected, the correlation between actual and projected state employment growth is higher for the non-mobile (.6) than the mobile (.3) category.

Table 5. Wages of Young Workers in the mid 1990s

The dependent variables in this table use data from the Current Population Surveys (CPS) of 1994-1998. We include in the sample all non-overlapping observations on males over 18 and with 40 or fewer years' experience. We use this sample to estimate state fixed effects for a cohort of workers who are no older than 28, have 12 or fewer years of education and who would have been 19-23 years old in 1990. So we include 23-27 year olds from the '94 CPS, 24-28 year olds from the '95 CPS, 25-28 year olds from the '96 CPS, and 27-28 year olds from the '97 CPS and only 28 year olds from the '98 CPS.

The fixed effects are defined by state of residence, the only state locator available in the CPS. They come from log wage regressions of the same form used for Table 2 (see text.) Then we estimate state fixed effects from the 1990 census for 19-23 year olds and 23-28 year olds based on state of residence.

The dependent variables in the regressions are differences between the 1994-98 fixed effects and the 1990 fixed effects. In panel A. the dependent variable answers "what was the change in wages for 19-23 year olds in a state as they got older?" In panel B. the question answered is "what is the difference between the wages of workers in their late 20s who graduated high school around 1990 and workers who were in their late 20s in 1990?"

Independent Variables (Tables 1-4)

A. School Performance and Quality

AFQT, *dAFQT*: see text and Peltzman (1996). State fixed effects are estimated each year 1971-1991 for scores on the Armed Forces Qualifying Test for 17-20 year old individuals who are potential military recruits. The score is a percentile (1 to 99). Each years' sample mean of fixed effects is set = 0. Then the 1971 intercept and trend is estimated for each state's 1971-91 time series of fixed effects from three time series models: 1) simple OLS; 2) with correction for first-order autocorrelation of residuals; 3) OLS trend plus the lagged dependent variable. The three intercepts and trends are then averaged to yield *AFQT* and *dAFQT* respectively. (However, the correlation among the three estimates always exceeds .9).

dTP: U. S. Office of Education, *Digest of Education Statistics*. Change in the log of teachers per pupil in a state's public schools from the 1960-1970 average to the 1980-1990 average. These terminal dates are chosen to approximate the average exposure to schooling inputs for the 1970 YHS cohort (they entered grade 1 in 1958) and the corresponding average for the 1990 cohort.

B. Industry Mix

E (dWages): IPUMS 1970 and 1990 databases. This is

$$\sum_i s_{ij} \bar{W}_i,$$

where s_{ij} = the share of state j's labor force in industry i in 1970, and

\overline{W}_i = the change in log hourly earnings (in 1990 dollars) in the industry
for the whole US from 1970-90.

Earnings are for full time workers, excluding those in agriculture, self-employed or in the military. This variable is a measure of the degree to which the state's 1970 labor force was concentrated in high or low wage growth industries: a high value implies that the state's 1970 work force was concentrated in industries that experienced above average wage growth nation-wide.

E (dEmp): same source and method as *E (dWages)*, except \overline{W} is replaced by the change in log of employment in industry *i* for the whole US from 1970 to 1990 (less the change in log total US employment). A high value implies that the state's 1970 work force was concentrated in industries with above average employment growth for the nation as a whole.

rd/cap: US National Science Foundation, *National Patterns of R & D Resources* (various years). Estimate of per capita total R & D spending in the state in 1971. The source gives spending by industry for that year. Geographical detail on spending by government and non-industry private sector (universities, foundations, etc.) is first available in 1985. We multiply 1971 industry R&D by the 1985 ratio of non-industry to industry R&D to estimate the 1971 value of non-industry R&D in each state. Then we constrain these state estimates to add to published figures for the national total of non-industry R&D. (For 1971, non-industry R&D spending is 31.5 percent of the total).

C. Business Climate

dTaxRates: Advisory Commission on Intergovernmental Relations, *State Fiscal Capacity and Effort* (various years). The change in a state's 'tax effort' from 1967 to 1988 (the last available year). Tax effort is defined as the ratio of actual state and local tax collections to the state's 'capacity' to collect taxes. Capacity is, in turn, defined as the amount that would be collected if US average rates were applied to the state's tax base (assuming no response of the base to the tax). Rates are computed for a 'Representative Tax System' (RTS) in which every state's particular taxes are classified into 26 categories. Then a base and a rate are computed for each category. Thus, tax effort is a weighted average of a state's tax rates relative to US average rates, where the tax base for any tax is the weight. By construction the US average = 1.0 in any year.

dIncentives: Conway Data, Inc. *Site Selection Handbook*, June, 1970 and *Site Selection*, October, 1990. The source is a trade journal for industrial location practitioners. Each year it tabulates the number of location incentives available in each state. These incentives are either tax breaks (e.g. exemptions from some taxes for new businesses) or aspects of state regulation deemed favorable to new business (e.g., right-to-work laws). The list of available incentives has grown over time – e.g., it now includes various direct subsidies. We find the common elements on the 1970 and 1990 list; there are 17 such items. Then we simply count how many of the 17 are available in each state in 1970 and 1990 and take the difference between the two counts.

Table 5. Independent Variables

The main independent variable is the ‘school quality premium.’ It is based on the school quality variables in a state and their coefficients in regression [4] of Table 2. Specifically, it is: $.032*dAFQT+.016*AFQT71+.184 *dTchr/Pupil$ – i.e. the sum of coefficients times the associated variables.

The other independent variables have the same definitions as their counterparts in Table 3, but they are updated to the 1990 to '94-98 time period:

For $E(dWage)$ and $E(dEmp)$ we calculate the national change in the log of wages or employment for each industry from 1990 to the average of 1994-98 from Census and CPS data respectively. Then we multiply by the 1990 employment shares for each industry in each state and sum.

$dIncentives$ comes from the October, 1990 and 1996 issues of *Site Selection*, and it includes all of the 33 subsidies and tax breaks common to those two years.

$dTaxRates$ is unavailable for 1996. So we estimated it from a regression of the 1970 to 1990 change in this variable on three observables: the changes in the log of 1) per capita state and local taxes, 2) per capita income and 3) population. This regression explained about half the variance in the 1970-90 change of $dTaxRates$. We used coefficients from this regression and 1990-96 changes in the observables to estimate the 1990-96 changes in $dTaxRates$.

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Endnotes

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² The main issues in both literatures are summarized in Burtless (1996).

³ For the main counter-arguments to Hanushek see Hedges and Greenwald (1996). See Heckman et al (1996) and Betts (1996) for criticism of Card and Krueger.

⁴ As described more fully later, we also analyzed other labor market outcomes for these young workers, such as employment probabilities and hours, with mainly negative results.

⁵ One of these is the National Longitudinal Survey of Youth (NLSY), which administered the AFQT to a random sample of young people in 1980. Peltzman (1996) shows that, for a sub sample of states, the 1980 fixed effects we use here are unbiased estimates of the NLSY fixed effects.

The National Assessment of Educational Progress (NAEP) sponsored by the U. S. Department of Education provides another cross-section that can be compared to our measure of school quality. This program has administered standard tests to random samples of students since 1969. However state level data has been made available sporadically and only since 1990. A 1990 cross-section of state average scores on a mathematics test given to 8th graders is the most germane of these for us. It has many drawbacks, including; 1) a much smaller sample size (under 10,000) than our fixed effects (around 500,000), 2) students around four years younger than those we are interested in, 3) test scores that are unadjusted for student background characteristics, and 4) narrower scope than the AFQT. We can partly overcome one of these by regressing the NAEP math scores on state averages of student racial composition and the fraction of their age group in poverty households. The correlation of the residual from this regression with our contemporaneous fixed effects is approximately .7. When we put the two on the same scale we just barely reject (at $p=.05$) the hypothesis that our fixed effects are unbiased estimators of the NAEP.

⁶ Geographic data for the NAEP are available for four regions (northeast, southeast, central and west) beginning 1969 for an 8th grade and a 12th grade reading test. The changes in these two NAEP scores are correlated .86 and .95 respectively with the corresponding average changes in our fixed effects. But each correlation is based on only four observations.

⁷ Only 2 percent of the applicants to the military have any college education in our sample.

⁸ See Peltzman (1996, p.74)

⁹ See, for example, O'Neill (1990).

¹⁰ We actually use 20 such groups, since we estimate a trend from 20 successive cohorts of test takers in each state.

¹¹ For example, suppose 'better quality' workers move to a state, and both they and their children earn 10 per cent more than other workers in that state. The average wage for labor market entrants in this state would rise, but our measure would show no change.

¹² The characteristics are: a quartic in experience (defined as Age - 6 - years of education); years of education plus dummies for 12, 16 and 17+ years of education; dummies for race, marital status, military and self employment. Draft eligible military (26 years and younger) are excluded from the 1970 sample.

¹³ The level of the school resources variable was added to the regressions in this and subsequent tables, but it was nearly always insignificant.

¹⁴ In both our data and the NAEP data (see n. 5 above), the southeast had the lowest initial level and the highest change while the northeast and west had the best 1970 levels and worst subsequent performance.

¹⁵ Our results imply that the tax offset is over half the benefit. This follows from the coefficients plus the fact that public school spending is about 40 per cent of state and local taxes. Thus, increasing teacher employment (and presumably ancillary inputs) by 10 per cent and raising tax rates (and, let us assume, revenues) by 4 per cent would, according to regression [4], raise YHS wages by 0.7 per cent (v. the 1.8 implied by the teacher/pupil coefficient alone).

¹⁶ Indeed these are the only variables that seem to matter. The coefficients of the control variables had mainly the same signs as in Table 2, but none were significant.

¹⁷ We explored, without success, the effect of school performance on some other labor market outcomes for the YHS cohort. These were: 1) the probability of being 'economically active' (employed, self-employed, in the military or in school); 2) weeks worked/52, including zero; hour; 3) hours worked, including zero. (Students were excluded from the latter two samples). None of these outcomes was significantly related to either school performance or school resources.

¹⁸ The group going on to college after age 23 is so small it can be ignored. However, we will misclassify some of the 19 year olds, etc. who have only deferred college entrance. The misclassification is, however, potentially informative, since the deferral decision can also be affected by school quality.

¹⁹ The underlying logic is the same as in equation (1.2). The fraction of the cohort attending college (f) is the product of the fraction eligible (e) and the fraction of those eligible that matriculate (m). Table 3 is concerned with the change in f over time. The level of e enters this time derivative with the sign of the time derivative of m . So if m rose (fell) then the odds that a member of the cohort goes on to college also would rise (fall).

²⁰ This follows from the near unity coefficient of the projection. In the aggregate, however, the BEA underestimated the growth of employment from 1970-90 (the relevant log changes are .43 for employment and .27 for projected employment), mainly because the Census underestimated population growth. See appendix for source of the BEA projections.

²¹ The mobile industries are manufacturing, agriculture, mining, and amusement/recreation services. See appendix for details.

²² Specifically, +15 per cent ($t=2.8$) for the mobile industries and +8 per cent ($t=2.6$) for the non-mobile industries – when regression [3] is estimated separately on each industry group.

²³ Specifically, we used 1994-98 CPS data, and we included those 28 and under who were 19-23 in 1990. Thus, we include 23-27 year olds from the 1994 CPS, 24-28 year olds from the 1995 CPS, 25-28 year olds from the 1996 CPS, 26-28 year olds from the 1997 CPS and 27 and 28 year olds from the 1998 CPS. The appendix has more details.

²⁴ We overcome some of this by using weighted least squares (with the inverse of the standard error of the relevant difference in fixed effects as the weight). Preliminary analysis revealed a substantial positive correlation between the variance of OLS residuals and the estimated variance of the dependent variable.

²⁵ For example, if employers gradually substitute higher quality workers from other states for poor quality products of the local system this will show up as a wearing off of the quality premium in our data.

²⁶ Thus it is, more precisely, the change in the premium for school quality from 1970-90 that got built into 1990 wages.

²⁷ It is roughly a .7 standard deviation decline.

²⁸ The point estimate would imply 2,000 fewer matriculants.

Table 1. Variable Descriptions and Summary Statistics

Name	Short Description	units	Mean	St. Dev.
I. Independent Variables				
A. School Quality Variables				
dAFQT	Change in state fixed effect for AFQT, 1971-91	standardized	0.000	1.000
AFQT71	1971 level of state fixed effect	percentile	0.000	2.920
dTchr/Pupil	change in log of teacher-pupil ratio in state, 1980-90 v. 1960-70 averages	log	0.278	0.096
B. Industry Mix and Growth Variables				
E(dWage)	sum over 50 industries (1970 employment share in state x 1970-90 change in log national wage)	log	0.022	0.008
E(dEmp)	sum over 50 industries (1970 employment share in state x 1970-90 change log national employment)	log	-0.051	0.047
rd/cap	per capita research & development, 1971	\$thousand	0.107	0.111
C. Business Climate Variables				
dTaxRates	change in index of state & local tax rates, 1967-88	Index, % of US Avg=1.00	-0.007	0.101
dIncentives	change in number of tax/regulatory incentives offered by state for new businesses, 1970-90	Number (maximum=17)	3.500	2.540
II. Dependent Variables				
d log wage	change in state fixed effect for 19-23 year old workers, 12 or less years of education, 1970-90 log hourly earnings regressions	log	-0.033	0.063
d value	change in state fixed effect for 19-23 year old workers, 12 or less years of education, 1970-90 'value'* regressions.	\$ per hour	0.011	0.178
d college	change in state fixed effect, 1970-90. logistic regressions, 19-23 year olds. Years of education >12 = +1	log (odds ratio)	-0.239	0.196
employment growth	change in log of total state employment, 1970-90	log	0.432	0.184
mobile-non growth	difference between change in log of total state employment in mobile and non-mobile industries**, 1970-90	log	-0.359	0.208
d log wage in 1990s	state fixed effect for 23-28 year olds, log hourly earnings, 1994-1998 less fixed effect for: 19-23 year olds in 1990	log	-0.030	0.073
	23-28 year olds in 1990	log	-0.018	0.062

Note: See Appendix for detailed description and sources

* Value is the national average hourly earnings for the individual's industry/occupation

** mobile industries are: durable and non-durable manufacturing, mining, agriculture and entertainment services

Table 2. Wage Changes and School Performance. Young Workers, 1970-90

Independent Variables	[1] Coefficient	t	[2] Coefficient	t	[3] Coefficient	t	[4] Coefficient	t	Beta*
A. School Quality									
dAFQT	0.008	0.6	0.009	0.6	0.022	2.2	0.032	3.1	0.51
AFQT71	0.008	1.8	0.009	1.6	0.011	3.1	0.016	4.1	0.74
dTchr/Pupil			0.026	0.2			0.184	2.6	0.28
B. Industry Mix/Growth									
E(dWage)					0.770	0.9	0.800	1.0	0.10
E(dEmp)					0.403	2.4	0.432	2.8	0.32
1971 rd/cap					0.226	3.6	0.246	4.1	0.43
C. Business Climate									
dTaxRates					-0.257	4.1	-0.281	4.7	-0.45
dIncentives					0.006	2.4	0.006	2.7	0.25
adj R ² /SEE	0.047	0.062	0.027	0.062	0.561	0.042	0.617	0.039	

Note: Dependent Variable is difference in state fixed effect for 19-23 year old workers in wage regressions for 1970 and 1990. Dependent variable in those regressions is the log of wage and salary income per hour worked.

* The number of standard deviations change in the dependent variable for a 1 standard deviation increase in the independent variable, based on regression [4].

**Table 3. College Attendance and School Performance.
Males 19-23, 1970-90 Changes**

Independent Variables	[1]		[2]		Beta*
	Coefficient	t	Coefficient	t	
A. School Quality					
dAFQT	0.025	0.6	0.065	2.9	0.33
AFQT71	-0.018	1.3			
dTchr/Pupil	0.173	0.6	0.331	1.4	0.16
B. Industry Mix/Growth					
E(dWage)	-7.33	2.4	-7.450	2.4	-0.30
E(dEmp)	-2.258	3.8	-2.480	4.4	-0.59
1971 rd/cap	0.158	0.7	0.272	1.3	0.15
C. Business Climate					
dTaxRates	0.284	1.2	0.224	1.0	0.12
dIncentives	-0.01	1.1	-0.009	1.0	-0.12
adj R ² /SEE	0.422	0.149	0.414	0.15	

Note: Dependent Variable is difference in state fixed effect for 19-23 year olds .
in 1990 and 1970 logit regressions for college choice.

Dependent variable in the logit regressions is dummy=+1 if the individual is
a college student or has completed at least a year of college.

* The number of standard deviations change in the dependent variable for a
1 standard deviation increase in the independent variable, based on regression [2].

Table 4. School Performance and Employment Growth, 1970-90

Type of Employment Growth							
Independent Variables	[1] Total		[2] Total			[3] Mobile -Non	
	Coefficien	t	Coefficien	t	Beta*	Coefficien	t
A. School Quality							
dAFQT	0.086	2.8	0.083	2.4	0.45	0.073	1.7
AFQT71	0.038	3.1	0.036	2.7	0.57	0.040	2.5
dTchr/Pupil	-0.140	0.7	-0.122	0.6	-0.06	-0.315	1.0
B. Projected Employment Growth**							
Total Employment	1.050	4.7	0.998	4.4	0.57	1.193	3.6
Mobile - non-Mobile							
C. National Trends							
E(dWage)	3.440	1.3	3.560	1.3	0.16		
E(dEmp)	1.650	3.7	1.705	3.6	0.44		
D. Other Variables							
1971 rd/cap			-0.149	0.8	-0.09		
dTaxRates			-0.311	1.8	-0.17		
dIncentives			0.000	0.0	0.00		
adj R ² /SEE	0.592	0.118	0.602	0.116		0.252	0.180

Note: Dependent Variable is change in log of total employment, 1970-90 for [1] and [2] and the difference in the log changes of employment in 'mobile' and 'non-mobile' industries for [3].

See text and appendix for definition of mobile and non-mobile industries

* The number of standard deviations change in the dependent variable for a 1 standard deviation increase in the independent variable, based on regression [2].

**log of 1990 projections by Census Bureau of Economic Analysis made in 1967-log of 1970 employment. See appendix for details.

Table 5. What Happens to School Quality Premiums as Young Workers Acquire Experience? Wages in 1994-98 v Wages in 1990

A. 24-28 year olds in 1994-98 v 19-23 year olds in 1990

Independent Variable	[1] Coefficient	t	[2] Coefficient	t	[3] Coefficient	t
I. School Quality Premium in 1990, 19-23 yr olds	-0.237	0.8	-0.297	1.0	-0.635	1.6
II. Industry Mix/Growth						
E(dWage) '90-94/98			3.585	1.6	4.98	2.1
E(dEmp) '90-94/98			0.142	0.2	0.617	0.6
1990 rd/cap					-0.046	1.5
III. Business Climate						
dTaxRates (est 90-96)					-0.123	0.7
dIncentives (90-96)					-0.003	0.8
adj R ² /SEE	0.01	0.057	0.006	0.061	0.01	0.061

B. 24-28 year olds in 1994-98 v 24-28 year olds in 1990

Independent Variable	[1] Coefficient	t	[2] Coefficient	t	[3] Coefficient	t
I. School Quality Premium in 1990, 19-23 yr olds	-0.091	0.4	-0.122	0.5	-0.433	1.3
II. Industry Mix/Growth						
E(dWage) '90-94/98			1.965	1.1	3.193	1.6
E(dEmp) '90-94/98			0.132	0.2	0.536	0.7
1990 rd/cap					-0.045	2.4
III. Business Climate						
dTaxRates (est 90-96)					0.038	0.2
dIncentives (90-96)					0	0.0
adj R ² /SEE	0	0.051	0	0.051	0	0.051

All regressions are weighted by reciprocal of the estimated standard error of the dependent variable.

Dependent variable is the difference between state fixed effects for the indicated cohorts. The 1990 regression is from census data while the 1994-98 regression is from the Current Population Survey. Each cohort includes only workers with 12 or less years of education

The 1990 school quality premium is the sum of the coefficients in panel A., regression [4] Table 3 times the value of the variables for the state in that regression. The other controls are appropriately updated versions of those in Table 3. (dTaxRates is unavailable, value is estimated.)

More detail on construction of the variables is in the appendix

