EFFECTS OF COHORT SIZE ON EARNINGS: THE BABY BOOM BABIES' FINANCIAL BUST*

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ABSTRACT

The job market arrival of the post-WWII baby boom cohorts raises many questions of effects associated with a rapid youthening of the labor force. This paper first summarizes 1967-1975 wage behavior showing that relative wages between schooling groups have not changed for prime aged workers, but there is some evidence for new job market entrants that wages of more schooled workers have fallen relative to those with less. However, changes among schooling groups are small in comparison to those between new entrants and peak earners within schooling group. The evidence is very direct: as work-experience distributions shifted toward increased proportions of young workers, their relative wages fell.

After examining a career phase model in which workers at different phases are imperfect substitutes, estimates of empirical relationships between cohort size and wages are presented. The main result is that income depressant effects of (own) cohort size decline over the career but do not vanish altogether. Initial effects include reductions in wage rates and in hours and weeks worked while persistent effects extend only to wages.

Effects of Cohort Size on Earnings: The Baby Boom Babies' Financial Bust

If everyone left school and started work on his twentieth birthday, the U.S. labor market entering class of 1977 would have been 44 percent larger than the class of 1962. In fact the class of 1977 would be this century's largest, and fifteen years after it, that is, in 1992 the entering class would be only slightly larger than the one of 1962. According to the entry at age 20 assumption, the number of new entrants has increased each of the last 20 to 25 years following the 1930s depression-related trough in numbers of births. But the number of births peaked in 1957 and has continued downward so that the number of new labor market entrants will fall for at least the next 18 years (for which data on numbers of births are now available) with no clear indication of an ensuing rise.[1] Instead of labor markets adjusting to increasingly large numbers of entrants, adjustment will be to a declining pool.

It is, of course, true that not everyone begins work at the same age and the higher the level of school attainment the later the age of job market entry so that cohort peaks for college graduates may not have passed. But it is true that in the last decade we witnessed an historically unique event--the market responses to large entering cohorts.

Not only have labor markets been pressed by entry of those just out of school but particpation rates of married women have increased and veterans of Vietnam have joined the labor force as well. In the eight years from March 1967 to March 1975, the (18-64 year-old) civilian labor force grew 21 percent. More importantly new entrants during this period had more schooling than earlier cohorts. For example, even though the labor force grew by one-fifth, numbers of participants with 5-8 years of schooling fell 32 percent and numbers with 1-3 years of high school fell by 5 percent. In contrast, during these eight years numbers of high school graduates grew 35 percent, and both for those with 1-3 years of college and for college graduates the number of persons in the civilian labor force jumped an astonishing 64 percent.[2]

The unprecedented change in the age composition of the work force has undoubtedly affected earnings patterns and this paper attempts to measure these effects. The main conclusion is that pressure of a rapidly youthening work force reduces wages of new entrants. As such, it supports Richard Easterlin's and Michael Wachter's views of imperfect substitution among workers of different age or work experience.[3] While there is evidence that large cohorts receive reduced wages throughout their careers, most of the loss seems to be concentrated in the early years. Evidently the baby boom cohorts are taxed, but their future seems brighter than their experience to date.

The empirical analysis concentrates on earnings within school completion level. The partition by schooling is used, first because I think new entrants and experienced workers are more sharply distinguished at higher levels of education and if they are, wage effects of expanded cohorts will be larger for them. Second, there is some evidence that during the early 1970's earnings of young college graduates fell relative to earnings of high school graduates. It obviously matters for assessments of the future of education as an investment and as an avenue of socioeconomic mobility whether this change signals a longer run depression in markets for the products of higher education or is only an aberration as markets digest large entering cohorts.

The next section describes data sources and summarizes changes occurring between 1967 and 1975. Following that, a fairly simple model of career phases is examined for implications of effects of cohort size on earnings. Finally regression results are presented with estimates of these effects.

The Data Base and the Empirical Setting

The data refer to white males described in the 1968-1976 March *Current Population Surveys* (CPS). Each of these nine surveys includes from 130,000 to 152,000 people. Of these, from 25 to 27,000 are included in the analytical population. They are civilian, white male, age 14 to 65, not now in school (as major activity last week), who either worked 50-52 weeks in the previous year or report the reason for working fewer weeks as something other than being in school or retired. Those self-employed or working without pay are also excluded.

The data are organized on the basis of years of school completed (8-11 years, 12 years, 1-3 years of college, or 4 or more years of college) and estimated years of work experience. Table 10 (appended) reports number of observations by schooling class for each survey year.

One feature of the CPS public use surveys that is especially noteworthy for earnings or wage comparisons is that non-trivial proportions of observations who apparently worked during the year in question, do not report their earnings. When an earnings observation is missing, the omitted value has been filled with an imputation. Unfortunately the imputation procedure used in the first eight surveys differs from that of the ninth so that summary statistics for the 1976 survey (i.e., for 1975 earnings) are not comparable to other years.[4] To further confound analyses, individual records for the first eight surveys contain no flag to identify cases when earnings are imputed. Family records do however identify imputation of total family earnings which presumably means that earnings for at least one family member are imputed. In contrast the 1976 survey contains flags for individual imputations but none for families.

I exclude all individuals in the first eight surveys who are in families for which family earnings are imputed. For the final survey, a flag is constructed for families in which any member has imputed earnings, and all persons in these families are excluded. Table 11 (appended) reports proportions by schooling level and survey year of otherwise valid observations excluded on this basis. These proportions range from 7 to 22 percent. They are generally higher for groups with more schooling and are trended, showing higher exclusion rates in more recent years. Unfortunately, exclusions are more frequent for the 1976 survey suggesting that the constructed family imputation flag may not correspond perfectly to those for earlier surveys.

Summary statistics reported in this section contain no correction for earnings not reported. There are, however, corrections included in the subsequent regressions to suggest that those not reporting are atypical and, on average, have higher wages than those reporting earnings.

Another feature of the CPS surveys is that work experience is not reported. While age, year of birth, and years of schooling are given, we do not know dates when the individuals observed entered the work force. This is troublesome because the purpose here is to examine effects of changing age or experience distributions on earnings patterns. Of course, missing information on dates of job market entry and accumulated work experience is common to most "census styled" data and researchers concerned with income-experience relationships have resorted to various schemes for inferring experience. Here, too, experience is imputed. In fact the assumption of continuous participation following school completion and initial entry is explicit, so the imputation problem is that of estimating ages of job market entry. This imputation is described in detail by William Gould and Finis Welch (1976). Briefly, the procedure estimates a year-of-job-market entry distribution for each birth-school completion cohort. Account is taken of the fact that more recent birth cohorts finished school at earlier ages. A person observed in one of the CPS surveys who worked in the preceding year is assumed to be representative of his cohort subject to the extra information that he began work last year or sometime earlier. Instead of treating an individual as being in a single year on the job, the full work experience is carried so that the number of people in their first year is the sum across individual observations of first-year probabilities. Accordingly when examining statistics like wages by single experience years, averages are computed using individual probability weights. An example of these year-on-the-job probabilities is given for high school graduates in Table 12 (appended). Throughout the summary discussion and subsequent regressions, the data refer to national averages within schooling group for single years of job market experience. For earnings comparisons, individuals who did not work last year and those who reported an average weekly wage of less than $10 are excluded, yet these people are included in cohort size calculations as are those excluded for failure to report earnings. Since earnings data refer to the year prior to the March survey, the dates used refer to earning rather than survey year. That is, the reference period included 1967 through 1975.

To summarize changes over this period, I first examine earnings differentials between high school graduates and those with different amounts of schooling. Next, I turn to evidence, within schooling levels, of the general youthening of the work force and then compare earnings of recent job market entrants to earnings of more experienced workers.

Table 1 reports average weekly earnings ratios, 1967-1975, for two work experience intervals, recent entrants (1-5 years) and peak earners (23-27 years). Each ratio is the (geometric) average weekly wage for the indicated schooling group relative to the wage of high school graduates. Table 2 gives corresponding ratios for annual earnings. Although these calculations are based on what by traditional standards are large numbers of observations, the adjacent year comparisons suggest an uncomfortably high degree of variation relative to overall trend. Regardless of whether this variation refers to ordinary sampling error or to year-to-year variance in labor markets, it should be kept in mind in making trend comparisons.

Relative to high school graduates, those new entrants with 8-11 years of schooling pretty much came full cycle over the period. Both weekly and annual earnings declined to 1971 and rose to 1967 levels by 1975. For peak earners, very little happened between 1967 and 1974.

Relative earnings fell in 1975 which was by far the worst year of the survey *vis a vis* aggregate unemployment.

For those who attended but did not complete college, weekly wages of new entrants followed a similar pattern of relative decay to 1971 which at least returned to initial levels by 1974-75. For peak earners, weekly wages fell in 1975 relative to high school graduates, but remained roughly constant until then. Annual earnings show no pattern. Only the 1975 jump for new entrants seems important.

Finally for college graduates, the new entrant weekly wage pattern is one of rising relative wages to 1971 and falling relative wages thereafter. Relative wages in 1975 are lower than in 1967. This, in a nutshell, is what has been called "the new depression in higher education." But, even for new entrants, annual earnings pattern contrasts with weekly wages. The rising profile to 1971 is consistent but the subsequent decay is less pronounced. By 1974, relative earnings exceeded 1967-68 levels and the 1975 recession seems to have dramatically expanded weeks worked for college relative to high-school graduates. For peak earners, there is little systematic change over the period in earnings of college relative to high school graduates.

Noting trends in earnings of new entrant college relative to high school graduates between 1969 and 1973 or 1974, Richard Freeman expressed concern that erosion in relative annual incomes of young college graduates signaled a longer run depression in the earnings power of higher education.[5] Freeman conjectured this trend occurred despite and not because of the deteriorating general level of economic activity that occurred during this period because the preponderance of the evidence suggests countercyclic motion in relative income of college graduates. Had Freeman awaited the 1975 data or had his analysis extended backwards to 1967 (the first annual survey available), his conclusions might have been less pessimistic.

Nonetheless, it is clear that comparisons like those of Tables 1 and 2 are confounded by cyclic instability. It is true that college graduates appear to generally be less vulnerable to recessions than are those with less schooling. Most of the literature on this subject explains this phenomenon by reference to one of two closely related observations.

One is that capital goods are less substitutable for college trained manpower than for high school graduates.[6] Since in the short run capital is largely fixed, reduced recessionary product demand is accompanied by disproportionate reliance on capital substitutes for reduced output. The other idea is that more educated workers carry larger firm-specific investments in them and like other forms of capital, their employment is protected during recessions.[7] While both of these views have similar predictions for experienced workers, the firm-specific view sharply distinguishes new entrants from experienced workers. If experienced workers carry sunk investments, new entrants imply extra (training) costs and the analogy to capital is precise: experienced workers are to fixed capital as new entrants are to investments in capital. In investments models, it is the fixity of capital that destabilizes demand for investment and for firm-specific skills it is the fixity of experienced workers that destabilizes demand for new entrants.

In a recent paper James Smith and I (1977) examined earnings of new entrant high school and college graduates using data from the March 1968-1975 CPS's. Our finding is that within industries, business cycles are neutral between high school and college graduates. Any aggregate nonneutrality for them seems to be an artifact of differences in the industrial composition of employment. But, college/high school graduate differences in employment patterns are large and there is much room for compositional effects. For example 43 percent of all new entrant college graduates work in service industries (largely health, education and professional services) while only eleven percent of high school graduates work in these industries. In contrast, 39 percent of high school graduates and only 24 percent of college graduates work in manufacturing. High school graduates work in industries that are disproportionately vulnerable to business cycles and, for "normal recessions," they are more affected than college graduates.

In fact, 1971 appears to have been a normal recession and as Tables 1 and 2 show, relative earnings of college graduates were unusually high that year. But 1973 and 1974 were atypical. In both years industrial employment patterns were mixed with some industries lying above long run trend and others below trend and in the aggregate employment was relatively depressed in industries disproportionately employing college graduates.[8] Even so, Smith and I could attribute little of the 1969-1974 trend in relative earnings of college graduates to business cycles. Either our approach was faulty, or the explanation of this change lies elsewhere.

I consider the bulge of entering cohorts a likely alternative. Table 3 shows for each schooling group, the percent of all workers in their first five years on the job. If experience distributions within schooling group are regular, the fraction of new entrants in the total signals the rate of growth of the total. Higher entry rates for college imply that average levels of schooling are rising. In this vein, convergence between 1967 and

and 1975 in differences among schooling groups suggests that the rate of growth in average schooling levels is itself falling. This points to what is well known, that rates of progression into college fell after 1969. Although cohorts entering since then have more-than-national-average levels of schooling so that their arrival increases the average, they are not as highly schooled as their recent predecessors. In any case, the changes in entering cohort shares are large for all levels of schooling, and unless new entrants and experienced workers are perfect substitutes, the entry bulge must have depressed new entrant wages. Table 4 addresses this point. It compares weekly earnings of those with less than five years experience to those with 23 to 27 years. In every schooling group new entrant wages fell and 1967-1975 changes swamp anything shown in Tables 1 or 2. For example, relative to peak earners, weekly wages of new entrant college graduates dropped 14 percent. Either for new entrants or peak earners, college/high school graduate wage ratios changed by less than 3 percent. Clearly, the most dramatic changes of this period were not in relative wages among schooling groups but in wage structures within schooling group and, I think, increased new entrant cohort size is the most likely explanation of this change.

How Does Cohort Size Affect Earnings?

Only if all workers, regardless of experience, substitute perfectly for each other is the structure of earnings independent of cohort size, of the number of workers at a particular experience level. If perfect substitution were to hold, then the only feasible interpretation of life-cycle profiles would be one of purely physical aging. The investment views, that age profiles are products of learning and depreciation, all suggest that people at different stages of the career do different things. If so, then the value of each thing would reasonably depend on the number of people doing it and cohort size matters.

But as quickly as we admit this possibility, we are left with the thorny problem of specifying such an effect. At issue is the question of the way the number of workers in one particular class affects their own productivity and that of all other classes.[9] This is a question of substitutability between groups and we know too little about the nature of these relationships for easy inference.

With workers classified by schooling and experience or date of entering the work force, the number of specifications of substitution patterns is too large to expect unstructured data to be able to sort through them. For example, there may be asymmetries. Young high school and college graduates may be better substitutes in jobs ordinarily performed by high school graduates than in jobs ordinarily performed by college graduates. If so, large entering classes of college graduates may partly ratchet into typical high school jobs when options for entering cohorts of high school jobs do not include switching into jobs ordinarily performed by college graduates.

If job experience congeals initially maleable skills, then substitutability across activities may erode as experience accrues. At the other extreme if what distinguishes workers is the skills acquired in school, then as these skills erode with work experience, substitutability among schooling groups may increase.

The approach I follow here is to ignore substitution between schooling classes in favor of a sharpened focus within classes. The structure is as though each school completion group forms a separable branch of an aggregate production process and substitution among schooling classes is independent of levels of experience. Within each schooling group relative wages (productivities) are determined by numbers of workers at each experience level. I assume that these productivity effects depend only on ratios of number of workers so that the structure of relative wages across experience levels (the wage profile) depends only on the experience distribution within the group.

The empirical specification I use is to construct a measure of own cohort size and to estimate own wage elasticities for this measure. Estimated elasticities are interacted with work experience in an attempt to see if effects dilute as time since entry increases. I think it likely that if larger cohorts have a more difficult start, these effects will erode as the career unfolds.

Career Phases

Perhaps the simplest view of the way cohort size affects earnings follows from the notion that work careers consist of a series of more or less distinct phases. A new job market entrant arrives fresh from school and enters his profession as a trainee, apprentice, or learner. Only rarely, if his school training is narrowly applied or if the tasks of his profession are very simple, will he transit immediately into being a full-fledged member of the profession. More generally, he enters as a raw recruit or learner, transits first to junior membership and somewhat later to senior membership in the profession.

Just how many phases there are does not really matter. What does matter is that at each phase members of the profession do different things and *vis a vis* aggregate product, these things are not perfect substitutes. Each activity is productive and marginal productivities are determined, as for any factor, by numbers of workers engaged in all activities. In this view, a profession is an ordered series of worker types. At any moment in the career, a member of the profession is in transit between two of these types and can be viewed as a (convex) combination of them.

This view is essentially identical to the optimal life-cycle configuration suggested by Sherwin Rosen (1972). In his statement of the problem, a career consisted of a continuum of occupations and a worker solves for an optimal occupational sequence by recognizing that each occupation corresponds to learning options that affect performance in subsequent occupations. Rosen allowed productivity in each occupation to depend on number of workers in that and other occupations and it is clear that had he considered cohort size, the theory would have predicted adverse effects on earnings.

To highlight effects of cohort size, I abstract from questions of optimal rates of progression, of transit between career phases, and take them as exogeneous. I also abstract from depreciation or skill obsolescence. Presumably the skills possessed on job market entry are more conducive to learning (think of schooling as learning to learn) and are depleted as the career progresses so that comparative advantage switches from learning to more directly productive activities. As such, progression is toward higher realized wage activities.

Consider an aggregate production function of the form

$$y = f(N,Z) \tag{1}$$

where N refers to the productive effort of persons in a given profession (characterized by a given level of schooling). Those things contained in Z include other schooling composites and, like N, each is assumed to form a (weakly) separable branch of the aggregate process, $f(\cdot)$, so that N is only illustrative.

The total effort, N is itself a function of numbers of workers in each of several worker types,

$$N = g(N_1, N_2, \ldots) \tag{2}$$

where the number of workers of each type is the number of members of the profession devoting their effort to that type of activity. A career phase involves transition between two types such that members of the profession in their first x_1 years on the job are in transition between the first two activities or worker types, in their next $x_2 - x_1$ years transition is between activities two and three, and so on. The i^{th} career phase is passed in experience year, x_i.

If n(x) refers to the number of members of the profession with x-years of work experience, then the number of workers of type j is given by,

$$N_j = \int_{x_{j-1}}^{x_j} (1-p_{j-1}(x))n(x)dx + \int_{x_j}^{x_{j+1}} p_j(x)n(x)dx \tag{3}$$

where $P_j(x)$ refers to the part of a worker's time spent in activity j and

$$p_j'(x) > 0;\ x_j \leq x \leq x_{j+1};\ \text{with } p_j(x_j) = 1 \text{ and } p_j(x_{j+1}) = 0.$$[10]

That is, as a worker enters the i^{th} career phase he initially devotes full time to the i^{th} activity and at that moment begins transition into the $i+1^{st}$ activity. As the i^{th} phase progresses the proportion of time spent in the i^{th} activity decreases until at the end of the phase all of his time is devoted to the next activity and a new phase begins.

The wage of those with x years of experience, their marginal product, is

$$w(x) = \frac{\partial f}{\partial n(x)} = f_1(p_i g_i + (1-p_i)g_{i+1}) \tag{4}$$

where x falls in the i^{th} career phase. Notice in this phase,

$$\frac{\partial N_i}{\partial n(x)} = p_i \text{ and } \frac{\partial N_{i+1}}{\partial n(x)} = 1 - p_i. \tag{5}$$

Since $w_i = f_1 g_i$ is the wage of activity i workers, the wage of persons in the i^{th} career phase is the combination,

$$w(x) = p_i(x)\ w_i + (1-p_i(x))w_{i+1} \tag{6}$$

and wage growth during this phase, $\frac{\partial w(x)}{\partial x}$, follows from the presumption that the fraction of time allocated to activity i is falling and that realized wages rise with activity level, $w_{i+1} > w_i$.

An individual's wage consists of two parts, the price or marginal product of the profession, f_1, and the individual's own contribution to the level of effort of his profession,

(7) $$\frac{\partial N}{\partial n(x)} = w(x)/f_1 = p_i g_i + (1-p_i) g_{i+1}.$$

Changes in f_1 are neutral across experience groups and f_1 determines levels but not shapes of wage profiles. In analyzing cohort size effects I abstract from f_1 which is determined, among other things, by aggregate factor ratios and in empirical analysis over fairly short periods is unlikely to be distinguished from pure trend.

Effects of cohort size on (own) wages are given by the quadratic form,

(8) $$\frac{\partial w(x)/f_i}{\partial n(x)} = \frac{\partial^2 N}{\partial n(x)^2} = (p_i, 1-p_i)\{G_{jk}\}(p_i, 1-p_i)' < 0$$

where $G_{jk} = \begin{pmatrix} g_{ii} & g_{i,i+1} \\ g_{i,i+1} & g_{i+1,i+1} \end{pmatrix}$ and, not surprising, if $g(\cdot)$ is (quasi -) concave, the effect of larger cohort size is to reduce wages if there are more than two factors in $g(\cdot)$.

For simplicity consider the two factor constant elasticity of substitution case,

(9) $$g = \left(\delta_1 N_1^{-\beta} + \delta_2 N_2^{-\beta}\right)^{-1/\beta}$$

where $\sigma = 1/(1+\beta)$ is the elasticity of substitution between N_1 and N_2. In this case there are only two activities, learner (N_1) and worker (N_2) and the life cycle can be viewed first as one of transition from learner to worker followed by a period as a fully vested worker.

In this example,

$$(10) \qquad \frac{\partial w(x)/f_1}{\partial n(x)} = -\frac{1}{\sigma}\,\theta N_1 N_2 \left(\frac{p}{N_1} - \frac{(1-p)}{N_2}\right)^2$$

where $\theta = g_1 g_2/N$ and $p = p(x)$ is the fraction of time, at x, spent as a learner.

The profile implied by equation (10) is illustrated in Figure 1. The difference between the normal wage profile and that for an illustratively large cohort is never positive. There is, however, a neutrality point where the cohort's division between worker and learner is the same as for all members of the group and since at that point, the example cohort has no effect on factor ratios, it has no effect on wages.[11]

In this example, new entrants are exclusively learners and at point of entry they not only draw all their wages as learners, but have the greatest depressing effect on learner wages. As experience accrues the cohort transits to the worker phase, drawing an increasingly larger share of a wage being depressed by their arrival. In the early career as the cohort is disproportionately involved in the learning phase, transition implies faster-than-normal wage growth to the neutrality point. Afterward, the depressant effect on worker productivity dominates and wage growth is slower than normal. Finally at the point indicated as p=0 in Figure 1, when the cohort is fully vested in worker status the process is completed. Thereafter, wages are depressed and the extent of the depression remains constant.

Several points deserve note. Effects of increased cohort size are inversely proportionate to the elasticity of substitution. The substitution elasticity indexes worker-learner differences in the nature of jobs performed. Greater similarity of activities implies greater substitutability. It is likely that the substitution elasticity is related to the transition function, p(x). Rapid transition from learner to worker status implies that learners can easily adapt to worker tasks. I expect that when transition occurs easily, worker-learner tasks are more similar, i.e., workers and learners are better substitutes.

This leads immediately to predictions across schooling groups of differences in worker-learner substitution elasticities. It is a near tautology that those who acquire more schooling ultimately perform tasks requiring more training. As total training increases a balance is reached between learning in the isolation and abstraction of schools and learning on-the-job. If so, then those having more schooling transit less rapidly from learner to worker status after beginning work and it is likely that worker-learner substitution elasticities are smaller than for those with less schooling. I will not pretend that I have not seen the data; I do contend, however, that the story the data tell of cohort size effects increasing with schooling is highly plausible.

Worker/learner wage ratios are determined by three factors in this model. Two of these are treated as parameters in the intermediate process forming the schooling composite. The first refers to the distribution parameter,

δ_2/δ_1, and the second refers to the elasticity of substitution. The third is the worker/learner ratio, N_2/N_1.

The normal wage path depicted in Figure 1 holds these three factors constant and only describes transition from learner to worker status. If the experience distribution were uniform, i.e., if n(x) = n for all x, then the worker/learner ratio is the ratio of the fraction spent in learner status. But a uniform experience distribution implies no growth. Each year the number of new entrants equals the number of retirees. If steady growth occurs the experience distribution is a negative exponential of the form

$$q(x) \propto e^{-\rho x},$$

such that the ratio, $\ln(n(x-1)/n(x)) = \rho$, is constant. If growth remains steady, the ratio N_2/N_1 is constant and the more rapid the growth, the lower the ratio. Thus, in a regime of steady growth, for other things equal, the wage ratio W_2/W_1 is a positive function of the growth rate.

This is an obvious point which has been ignored in the literature of income returns to schooling. A common finding of such estimates is that life cycle earnings profiles are more concave for higher levels of schooling, which has been interpreted as implying that early career investments in training (investments in human capital) expressed as a fraction of income potential, increase with the level of schooling. Such a view has obvious intuitive appeal, but greater concavity is just another way of saying that new entrant wages are reduced relative to those of peak earners. And, the reported positive association between concavity of the wage profile and schooling may partly be an artifact of rising average education levels: this, because growing school completion levels imply positive association between schooling and the learner/worker employment ratio within schooling level which, in turn, implies lower learner/worker wage ratios.

As a crude approximation to average growth rates among schooling groups, I calculated regressions of the form:

$$\ell n\ n(x)_{it} = a_i + \rho_i(t-x) + u_{xit} \tag{11}$$

where t indicates survey year, x is experience and i refers to schooling group.[12]

Implied growth rates are summarized in Table 5 and tell an unsurprising story of rising average educational levels. What may be surprising is the number of persons with 8-11 years of schooling is actually falling while the number of college graduates is averaging an annual growth of over six percent.[13]

Before examining the evidence of cohort size effects on earnings, a few other features of the career phase model should be considered. First, recall the difference between the normal wage page and that of a large cohort illustrated in Figure 1. Relative to the normal path, earnings for the large cohort grow more rapidly before the neutrality point is reached and less rapidly thereafter. Although this model ignores individual optimization concerning on-the-job learning, its predictions are similar to what we might expect from optimizing behavior. If large entering cohorts depress wages, then the opportunity cost of on-the-job training is depressed on entry and cost incentives are to speed learning.

In this model, the depressant effect on entry wages diminishes as the N_1/N_2 ratio rises.[14] This follows from the observation that the higher is N_1/N_2, the smaller is the effect of a given sized cohort, n(x) on factor ratios at point of entry, x=o.

The level of experience when wage neutrality is reached occurs earlier, the higher is N_1/N_2 and later in the career as the transition process is itself delayed. The existence of a fully neutral point is an artifact of the simplification that the career consists of only two phases, a transitional one and a worker phase. If the career structure is fully hierarchical such that individuals are continuously transiting between adjacent stages, then there would be a series of neutrality points or points of minimum depressant effect, but at no point would wage depression vanish altogether.

Regardless of whether a point of full neutrality exists, any career phase model will predict that wage depressant effects on job market entry erode in the early career as transition spreads the cohort bulge among more than one activity. This kind of result is intuitively appealing and (largely) supported by the data.

The Regression Analysis

Before turning to regressions it may be useful to review the underlying data. There are nine surveys referring to earnings years 1967 to 1975 and although there is some overlap between adjacent years resulting from the CPS's in 4- out-8- in-4 month rotation, overlapping observations are not identified. Thus the surveys are treated as though they are statistically independent. All observations refer to white males who presumably are out of school and not retired and are between 14 and 65 years old. Individuals are placed in four school completion groups.

For each individual, j, work experience is estimated in the form of a full density, p_{ij}:i=1,45, where p_{ij} corresponds to the individual's estimated probability of being in the i^{th} year on the job and p_{45} refers to the open-end class, more than 44 years. Experience probabilities are conditional on age, schooling and year of birth as described in Welch-Gould (1976). These data are aggregated (within schooling group) based on single years of job market experience and the total number of persons with i-years of experience in a particular survey year is simply the aggregate of the individual probabilities, i.e., $n_i = \sum_j p_{ij}$.

Within each experience interval two proportions are calculated. One refers to the fraction of observations lost due to failure to report earnings for those who worked (i.e., those individuals in families for which CPS imputes earnings) and the other refers to the fraction of observations who did not work and therefore have no reported earnings. Conditional on observations of earnings, (geometric) means of annual and average weekly earnings are then computed within experience interval.

The regression observations refer to means computed within survey year for single-year experience intervals. The open-end interval (more than 44 years of experience) is omitted and observations are pooled across surveys. There are therefore 396 (44 experience levels x 9 surveys) observations for each schooling group.

The objective is to estimate effects of own cohort size on earnings. The first step in computing cohort size involves normalization by the size of the work force. It seems likely that wages of a particular cohort are affected both by its own size and by the size of surrounding cohorts as well. Furthermore calculated proportions include sampling errors so for estimation the proportion of group members at each experience level is smoothed by computing a moving average with inverted V weights. Cohort size is then defined as

$$c(x) = \sum_{i=-2}^{2} \alpha_i n_{x+i} \tag{12}$$

where n_x is the fraction of those in the group who are in their x^{th} year of work experience. The α weights are: $\underline{\alpha} = 1/3\ (1/3,\ 2/3,\ 1,\ 2/3,\ 1/3)$, except for recent entrants where succeeding cohort fractions are not defined. In this case, the α distribution is truncated and remaining weights are scaled accordingly to sum to one.

The objective is to estimate wage elasticities with respect to cohort size and to allow these elasticities to vary over the career. In fact only one form of experience interaction is examined. It is a spline of the form.

$$\frac{Ew(x)}{Ec(x)} = \gamma_o S + \gamma_1 \tag{13}$$

where $EZ = \partial Z/Z$ and where $S = \max\ \{0,\ 1-x/\bar{x}\}$. The wage elasticity begins at an entry ($x=0$) value of $\gamma_0 + \gamma_1$ and then declines linearly (at a rate $-\gamma_0/\bar{x}$) to a permanent level, γ_1 in $\bar{x}$ years and remains at γ_1 thereafter. Experimentation with alternative values of $\bar{x}$ (the duration of the unique initial effect) over the four schooling groups suggests that it increases with schooling. The estimates reported impose, $\bar{x}$ = (6, 7, 8, and 9 years) respectively for those with 8-11, 12, 13-15, and 16 or more years of schooling.

One characteristic which Jim Smith and I noted in our earlier paper (1977) is that for the CPS data, wage profiles are not adequately described by the commonly used quadratic, experience and experience-squared. When estimated profiles are restricted to this form, they consistently overestimate early career earnings. Although we suggested an alternative specification, in this case I have added S (as defined on the preceding page) to experience and experience-squared to allow more flexibility to the wage path. This (splined) experience profile is itself continuous but its derivative is discontinuous at $\bar{x}$.

In addition to the cohort and experience variables, a linear trend term is included along with the national aggregate unemployment rate for white males. The two variables for proportions of observations excluded (described on the preceding page) also appear as explanatory variables. One is called the exclusion rate due to income imputation. It is the fraction of observations lost in computing mean earnings because of the imputation problem. Inclusion of this variable represents a straightforward attempt to control for selectivity bias in reporting income. If those not reporting have above average earnings then the higher the proportion of those not reporting, the lower will be the mean wage of those who do.

The other is called the exclusion rate due to non-work. It refers to those without valid income observations. Most of those excluded on this basis are either not in the labor force or did not work. Others who appear to have worked were coded as having no earnings. Still others show reported earnings that are so low that I presume they are coded incorrectly. Therefore I exclude from earnings comparisons all who either did not work or who had calculated weekly earnings of less than $10. The variable describing proportions of observations within each experience and survey year lost on this basis is viewed as a composite of selectivity and cyclical effects. Presumably those who are excluded on this basis have below average earnings so that as this proportion increases, the mean wage of those included should also increase. But, to the extent that the proxy variable used for business cycles (the national average unemployment rate for white males which varies between but not within surveys) controls imperfectly for them, it seems likely that cyclical effects which drive this measure of exclusion may imply that as conditions worsen to increase proportions not working, they also reduce mean earnings of those who do work.

Tables 6, 7 and 8 summarize regression results for mean earnings within schooling class. The regressions reported are weighted by numbers of earners in each experience cell.

Table 6 contains regressions for (ln) annual earnings, Table 7 refers to weekly wages and Table 8 refers to weekly wage with the list of explanatory variables being augmented by a part-time variable indicating the proportion of persons in an observational cell who "usually" worked less than 35 hours per week. The part-time variable is viewed as partial control for hours worked. It is not perfect (and if hourly wages were observed, I would have used them), but it is more than "just a dummy" variable. With individual observations, a dummy for less than 35 hours only controls for variation between those working less and those working more than 35 hours leaving within group variance uncontrolled. But with grouped data, this variable is the proportion of people working less than 35 hours, and with it, there is more effective control for average hours. Consider, for example, an hours worked distribution over a limited range of say 0 to 70 hours. As work experience changes or as cyclic conditions change the hours density shifts and as it does the frequency of hours less than 35 changes. In general, for well behaved functions we would presume that average hours and the frequency of hours less than 35 move closely together and if they do, the part-time variable is a "good" control for average hours.[15]

In examining the estimates of Tables 6, 7 and 8 notice that calculated standard errors for coefficients understate estimation error. With the data arrayed across experience levels and across years, there is probably non-trivial positive serial correlation which I ignored in the calculations reported. Furthermore, residuals are probably (positively) correlated across equations (schooling groups) so that estimates should not be viewed as independent. A preferred estimation procedure to gain efficiency would have recognized both contemporaneous and serial correlations, but software for this kind of procedure is not available to me. As is always true in cases like this, estimates may be unbiased, but are inefficient and computed standard errors are biased.

Notice that the three sets of regressions in Tables 6, 7, and 8 can be contrasted for implications on hourly wages, hours per week and weeks per year. The dependent variable in Table 6 is the logarithm of annual earnings and is therefore the sum of the logarithms the weekly wage and of weeks worked per year. Coefficient differences, Table 6 less 7, are then coefficients for weeks worked. Similarly, if the part-time variable effectively controls for hours, differences between Tables 7 and 8 refer to hours worked while Table 8 coefficients refer to the hourly wage.

These distinctions are useful in contrasting cohort effects since if larger entering cohorts imply increased job competition, effects should emerge not only for wages but also for labor utilization rates (weeks and hours worked). Before examining cohort elasticities, consider other implications of these estimates. As is common for this kind of analysis, there is evidence that early career wage growth is more rapid for those with more schooling. The splined experience coefficients generally imply more rapid early career earnings growth than can be captured in the experience quadratic alone.

Evidently, earnings are very sensitive to aggregate unemployment. This perhaps is not surprising for annual earnings where weeks worked are free to vary but may be for weekly earnings holding part-time status constant (Table 8). There is at least some support for notions of wage flexibility. In general, wage sensitivity to aggregate unemployment falls with increased schooling so that these data continue to support the idea that more schooled workers are less affected by swings in aggregate labor demand.

Coefficients on the exclusion rate due to income imputation refer to adjustments in mean earnings of those reporting income associated with variations in proportions reporting. These coefficients are consistently negative to suggest that those with the highest wages are the most likely to not report earnings. In fact, mean rates for income not reported together with these coefficient estimates imply average annual earnings for the combined total of those who do and not report that exceed the average of those reporting by 3 to 5 percent in the first and fourth schooling groups and by 6 percent for high school and 14 percent for college graduates. This is important especially for income comparisons across schooling groups, as for example in rate of return calculations. The implication of Table 6 is that annual earnings of college relative to high school graduates is eight percent higher in the total population than in the reporting population. This could result in underestimates of rates of return that as an order of magnitude would approach two percentage points, about 20 percent of most current estimates.

Notice also that magnitudes of coefficients on this variable increase as control for labor utilization increases (in Tables 7 and 8) so that the problem of understatement due to non-reporting seems more severe in weekly wage comparisons.

There is an often stated belief that the annual Current Population Surveys are more reliable than the decennial Census data mainly because of greater effort to ensure reporting. Evidently, non-reporting remains a serious problem even for CPS. Furthermore, analysts using CPS data should be aware that when missing observations are filled with imputations, the assumption of the procedure used is that non-reporters are representative of reporters. Unless my estimates are completely wrong, they are not.[16]

Coefficients on exclusions due to non-work do not have consistent sign patterns across schooling groups but are consistent across income definition. As such unambiguous interpretation is not easy. In any case the overall average exclusion rate due to non-work is less than three percent so that this correction has little effect on estimated mean earnings.

In these data, dependent variables are measured in constant (CPI) dollars and the evidence is of very low rates of growth in real earnings or wages. Since the unemployment rate is held constant, it is tempting to argue that growth implied by the year variable reflects cyclic corrected longer run trends but in my opinion it would be a mistake to do so. Recall that the observation period is itself strongly trended *vis a vis* aggregate economic activity. The first three years, 1967-1969, were more robust than any to follow and 1975 was by far the most depressed. The question relevant to trend comparisons is whether the unemployment rate alongside the nonwork exclusion rate adequately controls for cycles so that the residual trend can be taken as a secular one.

In these data, there are a variety of indexes of labor utilization rates which are themselves indexes of labor demand. In addition to weeks worked and the part-time variable, the most obvious is weeks unemployed or looking for work and the exclusion rate itself. In auxiliary calculations (not reported here), I regressed each of these variables on the same set of independent variables as those of Tables 6 and 7.[17] The result in every schooling group is that even though the aggregate unemployment rate is taken into account, weeks worked are negatively and weeks looking for work are positively trended through the period. Except for college graduates, the exclusion rate due to not working is also positively trended. Part-time status alone seems devoid of trend. Evidently, the unemployment rate is too imperfect an instrumental predictor of labor demand for the implied trend rates of earnings to be taken as estimates of long-run patterns. In any case, estimated trend growth rates show little evidence of differences among schooling groups.

Estimated Cohort Effects

Table 9 summarizes cohort elasticities from regressions reported in Tables 6-8. The entry elasticity is the respective wage or earning elasticity evaluated for those in their first year on the job (x=1) and the persistent elasticity is the main effect, copied from Tables 6-8.

Only those with 8-11 years of schooling show persistent elasticities exceeding entry elasticities (in panels A and C). This result is anomalous and I have no explanation for it. Each of the other schooling groups shows that entry effects exceed persistent ones.

Aside from high school dropouts, wage and earning elasticities rise with level of schooling and most of the discrepancy occurs in the early career. Recall predictions of the career phase model that effects are inversely proportionate to worker-learner substitution elasticities. Thus the finding of effects increasing with schooling level suggests that worker-learner substitution elasticities fall with increased schooling.

One of the most interesting patterns exhibited in Table 9 is that entry elasticities fall between panel A where weeks worked are free to vary and panel B where they are not. These elasticities also fall between panel B where hours worked are variable and panel C where they are partially controlled. The evidence then is that large entry cohorts not only depress wages but they also depress hours and weeks worked. In contrast, persistent elasticities are much less sensitive to control for weeks and hours. Evidently, although new entrant labor utilization rates are depressed, this effect is transient.

It may be surprising that wage effects have as large a persistent component as they do. At least, I for one, would not have been surprised to find no persistent elasticity, an indication that cohort size effects are restricted to congestion at point of entry. Nonetheless, career phase models suggest that although entry effects may exceed persistent ones, it is imperfect substitution between work activities performed at different phases of the career that generates both entry and persistent effects. From this perspective, the existence of an entry effect implies a persistent effect and vice versa. It is important, however, in assessing the size of persistent effects to keep in mind the operational definition of cohort size as the share of the current total work force. Through time as the working population expands, the share accounted for by a particular cohort declines.

How Large are the Estimated Effects?

Are the estimated effects presented in Table 9 large enough for those interested in earning patterns to view fluctuations in cohort size as a legitimate concern? We know from Table 3 that the age or experience composition of the work force changed significantly in an eight-year interval. And Table 4 shows falling relative earnings of new entrants that are coincident with increasing new entrant shares of the total work force.

There are two obvious ways to try to get an intuitive feel for orders of magnitude of earnings changes implied by the parameter estimates in Table 9. The first is to examine predicted changes in the range of the data used here, that is for the 1967-1975 interval. The second is to examine the historical record for longer periods to see what patterns of shifting cohort size have been and to combine them with parameter estimates for implications of effects on lifetime earnings.

For the CPS data, among new entrants (those in their first five years on the job) 1967-1975 growth is cohort size yields predicted reductions in weekly wages ranging from a low of six percent for those with 1-3 years of college to a high of 13 percent for college graduates. Implied reductions are eight percent for high school graduates and 12 percent for those with 8-11 years of schooling.

For all but college graduates, relative cohort size of peak earners (23-27 years of work experience) fell over the period and resultant predictions of wage growth range between 3 and 5 percent. For college graduates, relative cohort size of peak earners did not change. Thus, new entrant weekly earnings are predicted to have fallen relative to wages of peak earners by 15 percent for those with 8-11 years of schooling, by 10 percent both for high school graduates and for those who attended but did not finish college, and by 13 percent for college graduates. These predictions which ignore changes other than cohort size are reasonably close to the observations reported in Table 4, especially for those with 8-11 years of grade school and for college graduates. Predicted changes understate observations for high school graduates and for those with 1-3 years of college.

In comparing wages across schooling groups, between 1967 and 1975 the observed college/high school graduate new entrant weekly wage ratio fell something less than 3 percent. The change predicted by cohort growth with larger wage responses for college graduates is five percent. If these estimates are correct then what has passed for a new depression in higher education may be unique to the entrants of the early 1970's. For them, as effects erode over the career, the future is brighter and for subsequent arrivals who themselves will be members of smaller cohorts, the future is also brighter.

The story of large swings in total numbers of births and of secular drift in average schooling levels is well known. To get a rough idea of how these changes affect cohort shares of the work force and, therefore, lifetime earnings, I have merged the historical record of numbers of births with what evidence I could find for school completion distributions to construct cohort shares similar to those used in the regressions described above.

I have proceeded under the assumption that after 1975, the number of births in the U.S. will stabilize at 2.5 million per year (roughly the level maintained from 1971-1975) and that average levels of school completion will continue their upward drift for a few years and then stabilize. For each level of school completion, I assume that all members of a birth cohort who attain that level begin work at the same age and retire after 45 years. With these assumptions, I construct a guesstimated work experience x schooling distribution for each calendar year, 1940 to 2035. The final year corresponds to retirement for new entrants of 1990 so that the constructed distributions permit estimates of lifetime earnings for cohorts joining the work force over the half-century 1940 to 1990.

For each market entering cohort, work force shares are calculated for every year from entry to retirement and these shares are the raw input for calculations of cohort size effects on life earnings. Aggregation of a particular entering cohort's shares over its career gives a crude basis for measuring total cohort size.

These aggregates show extreme swings in cohort size as is expected. For example, the group with 8-11 years of schooling has a minimum average cohort share for 1953 entrants and a maximum for 1977 entrants and the average share of the largest cohort exceeds that of the smallest by 62 percent. For high school graduates, the smallest cohort entered in 1954 and the largest in 1975 and the largest accounts for a 55 percent greater lifetime average work force share than the smallest. Those with 1-3 years of college have even greater variation in lifetime cohort shares. The minimum for 1956 entrants is only 45 percent of the maximum (for 1967 entrants). For college graduates, the smallest cohort entered in 1962 and the largest in 1970 and the largest has a lifetime average share 51 percent bigger than the smallest. In general, these patterns reflect the depression related trough in number of births and the late 1950's culmination of the post-war baby boom. Timing differentials reflect delayed entry for those with more schooling and swings about trend in school completion.

It is of course obvious that cohort size effects on life earnings do not correspond perfectly to the average lifetime shares just described. Cohorts that begin their career as large and end it as small are penalized relative to those with similar average shares but with more uniform career representation. In a sense, something analogous to double-discounting is involved. First there is time preference and I assume a five percent discount rate to represent it. Second, there is the fact that aside from high school dropouts, estimated cohort size effects erode over the career.

In view of the extraordinarily large swings in cohort size, the estimates for effects on life earnings are not massive but, to the cohorts most affected, they may not be trivial. Using coefficient estimates for panel C of Table 9, which refer to my best estimates of hourly wage effects and presumably, therefore, correspond more closely to full incomes, variations for high school graduates are restricted to a four percent range. Not surprisingly, there is more action for college graduates where a ten percent range is implied.

Calculations for college graduates suggest that in terms of pure cohort size alone, the most favored generation entered the work force in 1962 and the least favored entered only eight years later. As noted, life earnings for 1970 entrants (ignoring secular real wage growth) is estimated as about 90 percent of the level for 1962 entrants. This focus on extremes omits much that is of interest in comparisons across entering cohorts.

For example, if the presumptions of future birth rates and school attainment levels are in the ballpark, then the wage depression for college graduates implied by the arrival of baby boom cohorts that starts in the mid-1960's will begin to erode around 1980 and the estimates for 1990 entrants suggest career earnings equal to those of the "most favored" classes who enter between the mid-1950's and early 1960's. Although the baby boom cohorts seem to have the poorest career income prospects, my simulations suggest that their lifetime incomes are not significantly below the life earnings of cohorts entering in the mid-1940's. These earlier entrants joined the work force during a period of rapid acceleration in numbers of college graduates. As such, they began their careers with very large cohort shares and although these shares fell rapidly under continuing growth in numbers of entrants the early career effects swamped the (double-discounted) late career effects. In comparison to the baby boom groups, the mid-1940 entrants have smaller average work force shares but the shape of the career profile nullifies what would otherwise be an advantage.

It is not necessary to enumerate the heroic nature of these simulations. They are only an exercise to derive a "feel" for orders of magnitude of the estimated effects involved and to examine the historical record of swings in the experience composition of the work force. But, if these machinations reasonably describe cohort patterns in life earnings, then one final observation is in order: the depression in early career earnings of college graduates witnessed in the first part of this decade is not unique and just as earlier depressions eroded into higher incomes, the recent depression will be followed by higher college graduate earnings.[18]

Summary and Conclusions

Anyone working with wage and earnings data for the period 1967 to 1975 must be impressed by variability. Real annual earnings fell in 1971, in 1974 and again in 1975 for experienced workers. For new entrants, real earnings fell in 1970 and in 1972 and again in 1974 and 1975. College graduates are, however, somewhat atypical. Although they shared in the 1970-1972 recession, effects were less severe. However, the foundering economy of late 1973 and 1974 affected college graduates more than others and in 1975 when real income fell sharply for most schooling groups, income of college graduates, especially those with considerable work experience, actually rose.

This kind of variability challenges those concerned with wage behavior and gives much room for statistical exploration, yet so much happened during the period that any set of relatively simple explanations is likely to be confounded by competing explanations. Clearly business cycles should be taken into account, but the evidence is that the composition of the cycles that occurred was different enough to raise questions of "how."

Relative incomes changed among schooling groups, especially for new entrants. Why? Is it cycles or a longer run depression in the market for college graduates? The hypothesis forwarded here is that whatever it is, it is not long run. The 1975 data challenge any notions of persistence.

The most dramatic change in earnings structures witnessed during these nine years was the drop in earnings of new entrants relative to more experienced workers and this change coincided with the arrival of the peak sized cohorts spawned by the post WWII baby boom. How does a market digest an increasingly large number of new arrivals? As a device for describing effects of cohort size on earnings, I explore a career phase model in which activities performed at various phases are not perfect substitutes for activities performed at other phases. I then attempt to fit a (loosely specified) version of this model to earnings data taken from the March *Current Population Surveys* for 1968 to 1976.

By conventional statistical standards, there is fairly strong support that large cohorts do depress earnings and that most of the effect comes early in the career. The evidence is also that cohort size effects increase with level of schooling. Whether these effects are real or just statistical illusions depends on what the future brings. Population age structures suggest that entering cohort sizes are probably already falling for lower levels of schooling and will do so soon for higher levels. The prediction of the estimates presented is that when this happens, wages of new entrants will rise relative to experienced workers and that given similar reductions in cohort size, wages of new entrant college graduates will rise relative to those with less schooling.

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FOOTNOTES

[1]The peak-sized baby boom cohorts may pull birth totals upward as they reach peak fertility ages, but the early evidence for these cohorts is of very low birth rates. They are so low in fact, that numbers of birth could conceivably continue the downward trend. See the paper by W. Butz and M. Ward (1979).

[2]These data refer to Table A of "Educational Attainment of Workers," *Special Labor Force Report No. 186*, March 1975.

[3]Easterlin's cohort effect fertility models are well known and in them imperfect substitution plays a central role. More recently, Wachter and/or Wachter and Easterlin have pursued ideas of imperfect substitution for implications of intermediate cycles both in labor force participation and economic stability. See for example, Easterlin (1978), Wachter (1977) and Easterlin, Wachter, and Wachter (1978).

Michael Wachter (1977) in particular pointed to recent changes in earnings of young relative to older males and interpreted the decline as a result of the increased proportion of the population that is "young". While he did not analyze the relationship between wages and factor ratios, his presumption of this relation served as the justification for examination of relationships between age-specific labor force participation rates and the age distribution of the population. My approach takes participation as given and concentrates of wage determination.

[4]I am grateful to Richard Freeman for pointing this out to me.

[5]For examples of his work on this subject, See Freeman (1975, 1976, and 1977).

[6]See Rosen (1968), Griliches (1969), Berndt and Christensen (1974) and Welch (1970).

[7]See Oi (1962), Becker (1964) and Rosen (1968).

[8]See Smith-Welch (1977), Table 4.

[9]There have, of course, been several attempts to estimate substitution relationships for worker groups segregated on the basis of age and other things. Recent papers by Freeman (1978) and Joseph Anderson (1977) are examples. I avoid this kind of specification, first because I do not know how many age groups to specify, but I do know that many inputs are generally not empirically manageable. Second, the abrupt now-you're-in now-you're-out implications of rigid age demarcations that are usually imposed are unappealing.

[10]In expression (3), the first (RHS) term is omitted for the first career phase since negative experience has no meaning.

[11]This is, of course, an artifact of the two-factor assumption. If there were three or more factors and if $g(\cdot)$ were linear homogeneous with a two-factor branch like that of Eq. (9), Eq. (10) would partition into two effects, one always negative and the other never positive. Equation (10) captures this second effect only and it vanishes when the cohort being considered divides its time such that it has no effect on the N_1/N_2 ratio. With more than two factors, the cohort being considered

would necessarily increase both N_1 and N_2 relative to other factors (regardless of whether it changed the N_1/N_2 ratio) and would therefore reduce wages for the N_1-N_2 composite.

[12]There are 9 surveys and 44 experience levels for a total of 396 observations for each schooling group.

[13]Recall that Table 3 shows a rising new entrant share of the work force with 8-11 years of schooling. The observation in Table 5 of negative average growth for this group only demonstrates that in the cross-section number of workers increases with experience *on average* and this effect dominates shifts between years. Actually, by 1975 the experience distribution in this group is bi-modal. Contrary to earlier trend, the baby boom entry bulge has its impact on high school dropouts as well as for those with more schooling.

[14]Actually, there is some ambiguity on this score. There is none if the worker share of the composite product exceeds one-half or if the worker-learner substitution elasticity exceeds two. Otherwise if $\delta_2 < 1/2 - \frac{\sigma}{1-\sigma/2}$ (where δ refers to worker share) the entry effect increases, in absolute value, as N_1/N_2 increases. There is no ambiguity about the size of the entry effect (p=1) relative to the persistent effect once transition is complete, p=0. That ratio is $(N_2/N_1)^2$.

[15]The type of variation that this measure misses stems from compensated shifts in the density that hold the part-time frequency constant and changes the mean. Such motion seems unlikely. More importantly, by forcing the part-time measure to have only one parameter it captures only

average covariation between part-time frequency and mean hours. Since covariance from changes in levels of economic activity is probably different than covariance resulting from changes, say, in potential experience, the forced average effect is incorrectly specified. In any case, in these data the part-time variable is the best available adjustment for hours worked short of resorting to an instrument between it and the hours worked last week for the March survey.

[16]To impute missing income observations, the Census Bureau uses what they call a "hot deck" procedure. Briefly, this procedure fills a matrix stratified on various characteristics with the "last" valid observation and uses that observation to fill in the blanks. Aside from convenience there is little to recommend this approach especially in light of the emerging literature on selectivity biases. The number of strata on which imputations are based was expanded for the 1976 survey to include schooling, labor force status of spouse, marital status, region and type of residence and more detail was introduced for age, family relationship, occupation, class of worker, weeks worked, race, etc. For a description of this procedure and for contrasts with earlier imputations, see U.S. Department of Commerce, Bureau of the Census (1976).

[17]Of course when the exclusion rate is the dependent variable, it is not included as an independent variable. It is included, however, for other dependent variables.

[18]In a recent paper, T. W. Schultz (1977) exhibits a variety of relative wage calculations for different times in the twentieth century. Working with four groups: unskilled, manufacturing production workers,

teachers, and associate professors, Schultz describes four periods of rapidly declining relative wages of skilled workers (1915-19, 1932-35, 1940-42 and 1969-75). Since only the last two of these periods correspond to rapid cohort expansion, there would seem to be a real advantage to further exploration of the historical record. In particular, Schultz stresses that both inflation and levels of economic activity are candidates for further investigation.

Table 1

Weekly Wages of Those With 8-11 Years of Grade School,
1-3 Years of College and College Graduates, Relative
to Wages of High School Graduates, 1967-1975
(Ratios of geometric means)

Years of School Completed	1967	1968	1969	1970	1971	1972	1973	1974	1975
A. New Entrants, persons with less than five years of work experience									
Grade School, 8-11 years	0.75	0.71	0.69	0.69	0.65	0.71	0.71	0.74	0.76
College, 1-3 years	1.09	1.16	1.10	1.10	1.05	1.09	1.07	1.13	1.13
4 or more yrs	1.49	1.50	1.53	1.54	1.55	1.50	1.48	1.46	1.45
B. Peak Earners, persons with 23-27 years of work experience									
Grade School, 8-11 years	0.86	0.86	0.86	0.86	0.89	0.88	0.86	0.87	0.86
College, 1-3 years	1.19	1.18	1.19	1.19	1.18	1.17	1.18	1.18	1.12
4 or more yrs	1.54	1.49	1.51	1.50	1.51	1.52	1.49	1.54	1.47

Table 2

Annual Earnings of Those with 8-11 Years of Grade School, 1-3 Years of College and College Graduates, Relative to Earnings of High School Graduates, 1967-1975
(Ratios of geometric means)

Years of School Completed	1967	1968	1969	1970	1971	1972	1973	1974	1975
A. New Entrants, persons with less than five years of work experience									
Grade School, 8-11 years	0.66	0.62	0.59	0.61	0.57	0.63	0.63	0.63	0.64
College, 1-3 years	1.12	1.20	1.13	1.16	1.14	1.12	1.14	1.18	1.26
4 or more yrs	1.55	1.57	1.66	1.72	1.72	1.66	1.57	1.59	1.68
B. Peak Earners, persons with 23-27 years of work experience									
Grade School, 8-11 years	0.84	0.83	0.83	0.83	0.87	0.85	0.83	0.85	0.83
College, 1-3 years	1.18	1.15	1.21	1.18	1.19	1.17	1.18	1.20	1.17
4 or more yrs	1.56	1.50	1.57	1.52	1.56	1.55	1.53	1.59	1.56

Table 3

Percent of Work Force with Less Than Five Years of Work Experience by Years of School Completed, 1967-1975

Years of School Completed	1967	1968	1969	1970	Year 1971	1972	1973	1974	1975
Grade School									
8-11 years	8.1	8.5	10.0	12.0	12.3	13.7	15.0	15.1	16.0
12 years	14.4	15.3	15.2	16.5	18.0	19.1	20.7	20.8	20.9
College									
1-3 years	19.2	19.1	18.7	22.0	23.9	25.3	26.7	25.3	23.5
4 or more yrs	17.8	19.4	18.9	19.7	21.1	21.1	22.6	22.6	23.5

Table 4

Weekly Wages of New Entrants Relative to Peak Earners by Years of Schooling, 1967-1975

Years of School Completed	1967	1968	1969	1970	Year 1971	1972	1973	1974	1975
Grade School									
8-11 years	0.56	0.52	0.50	0.46	0.41	0.45	0.44	0.47	0.48
12 years	0.65	0.63	0.62	0.58	0.56	0.55	0.54	0.55	0.55
College									
1-3 years	0.59	0.62	0.58	0.53	0.50	0.51	0.49	0.53	0.55
4 or more yrs	0.63	0.63	0.63	0.59	0.58	0.55	0.54	0.54	0.54

Table 5

Estimated Average Annual Growth Rates in Numbers of White Male Workers by Schooling Class, 1967-1975

	Years of School Completed			
	8-11	12	13-15	16 or More
Estimated Growth Rate as a Percentage	-0.31	2.89	4.59	6.25
(Standard Error)	(0.07)	(0.09)	(0.11)	(0.17)

Note: Numbers of people observed are scaled to take account of the declining size of Current Population Surveys relative to the total U.S. population.

Table 6

Regression Estimates of Determinants of Annual Earnings of White Males, 1967-1975

(absolute values of t-statistics are in parentheses)

Independent Variables	Grade School 8-11	Grade School 12	College 1-3	College 4 or more
1. Cohort Size				
a. Main Effect	-.252	-.120	-.194	-.204
	(12.3)	(6.7)	(13.5)	(11.3)
b. Interaction with Early Career Spline	.0140	-.302	-.365	-.791
	(.33)	(6.7)	(7.3)	(12.0)
2. Experience				
a. Early Career Spline	-.690	-1.46	-1.52	-2.71
	(4.2)	(9.7)	(9.9)	(12.7)
b. Experience	.0428	.0393	.0374	.0600
	(43.2)	(32.7)	(21.7)	(36.9)
c. Experience Squared	-.00073	-.00083	-.00086	-.00146
	(31.8)	(28.3)	(21.6)	(28.0)
3. Exclusion Rate Due to Non-Work	-.647	.472	-.745	.622
	(4.7)	(2.2)	(2.2)	(2.1)
4. Exclusion Rate Due to Income Imputation	-.231	-.430	-.249	-.855
	(3.1)	(5.6)	(2.8)	(12.7)
5. Unemployment Rate	-.0270	-.0360	-.0287	-.0176
	(10.7)	(16.9)	(13.4)	(8.4)
6. Trend	.0137	.0137	.0108	.0131
	(10.5)	(11.1)	(7.8)	(12.1)
Intercept	7.3	8.25	8.14	8.23
R^2	.989	.983	.984	.985
Standard Error of Estimate	.034	.033	.035	.030

Note: Number of observations = 396. The dependent and cohort size variables are in (natural) logarithms. The exclusion and unemployment rates are expressed as fractions.

Table 7

Regression Estimates of Determinants of Weekly Earnings of White Males, 1967-1975

(absolute values of t-statistics are in parentheses)

Independent Variables	Years of School Completed: Grade School 8-11	Grade School 12	College 1-3	College 4 or more
1. Cohort Size				
a. Main Effect	-.161	-.080	-.163	-.194
	(9.4)	(6.5)	(12.4)	(11.6)
b. Interaction with Early Career Spline	-.072	-.237	-.278	-.591
	(2.0)	(7.0)	(6.1)	(9.6)
2. Experience				
a. Early Career Spline	-.849	-1.156	-1.180	2.010
	(6.2)	(10.2)	(8.4)	(10.1)
b. Experience	.033	.033	.037	.050
	(40.2)	(36.9)	(23.5)	(39.4)
c. Experience Squared	-.00055	-.00067	-.00080	-.00143
	(28.8)	(30.3)	(22.1)	(29.4)
3. Exclusion Rate Due to Non-Work	-.615	+.103	-.751	+.744
	(5.3)	(.64)	(2.5)	(2.7)
4. Exclusion Rate Due to Income Imputation	-.338	-.441	-.366	-.924
	(5.4)	(7.7)	(4.5)	(14.7)
5. Unemployment Rate	-.0117	-.0184	-.0197	-.0121
	(5.6)	(11.5)	(10.2)	(6.2)
6. Trend	.0172	.0147	.0129	.0130
	(15.8)	(15.9)	(10.2)	(12.9)
Intercept	3.90	4.47	4.33	4.33
R^2	.988	.986	.984	.985
Standard Error of Estimate	.028	.025	.032	.028

Note: Number of observations = 396. The dependent and cohort size variables are in (natural) logarithms. The exclusion and unemployment rates are expressed as fractions.

Table 8

Regression Estimates of Determinants (Including Part-Time Status) of Weekly Earnings of White Males, 1967-1975

(absolute values of t-statistics are in parentheses)

Independent Variables	Years of School Completed: Grade School 8-11	Grade School 12	College 1-3	College 4 or more
1. Cohort Size				
a. Main Effect	-.181 (12.6)	-.096 (8.7)	-.168 (14.0)	-.218 (13.7)
b. Interaction with Early Career Spline	.118 (3.5)	-.193 (6.4)	-.141 (3.2)	-.503 (8.6)
2. Experience				
a. Early Career Spline	.184 (1.3)	-.860 (8.3)	-.596 (4.1)	-1.620 (8.5)
b. Experience	.035 (49.4)	.033 (41.9)	.0367 (25.6)	.0581 (40.8)
c. Experience Squared	-.00058 (35.9)	-.00067 (34.5)	-.00080 (24.1)	-.00141 (31.2)
3. Exclusion Rate Due to Non-Work	-.274 (2.7)	+.266 (1.9)	-.517 (1.9)	.608 (2.4)
4. Exclusion Rate Due to Income Imputation	-.516 (9.5)	-.498 (9.8)	-.457 (6.1)	-.915 (15.7)
5. Unemployment Rate	-.0139 (7.9)	-.0163 (11.5)	-.0170 (9.4)	-.0095 (5.1)
6. Part-Time	-.905 (12.8)	-1.113 (10.7)	-.869 (8.8)	-1.400 (7.8)
7. Trend	.0172 (18.9)	.0149 (18.3)	.0124 (10.8)	.0136 (14.4)
Intercept	3.84	4.41	4.32	4.27
R^2	.99	.989	.987	.987
Standard Error of Estimate	.024	.022	.029	.026

Note: Number of observations = 396. The dependent and cohort size variables are in (natural) logarithms. The exclusion and unemployment rates are expressed as fractions.

Table 9

Estimated Elasticities of Annual and Weekly Earnings With Respect to Cohort Size

(absolute values of t-statistics in parentheses)

Estimated Elasticity	Years of School Completed: Grade School 8-11	Grade School 12	College 1-3	College 4 or more
	A. Annual Earnings			
Entry	-.240 (8.8)	-.369 (10.9)	-.514 (12.7)	-.907 (12.1)
Persistent	-.252 (112.3)	-.080 (6.6)	-.194 (13.5)	-.204 (11.3)
	B. Weekly Wages, Part-time Status Excluded			
Entry	-.221 (9.8)	-.283 (11.2)	-.406 (11.1)	-.720 (14.8)
Persistent	-.161 (9.4)	-.080 (6.6)	-.163 (12.4)	-.194 (11.6)
	C. Weekly Wages, Part-time Status Included			
Entry	-.082 (3.8)	-.261 (11.6)	-.291 (8.1)	-.665 (14.5)
Persistent	-.181 (12.6)	-.096 (8.7)	-.168 (14.0)	-.218 (13.7)

APPENDIX

Table 10

Numbers of Individual Records from March *Current Population Surveys* included for Estimation of Earnings Profiles and Cohort Sizes by Years of School Completed, 1968-1976

Years of School Completed	1968	1969	1970	1971	1972	1973	1974	1975	1976
Grade School									
8-11 years	6,823	6,466	6,344	6,159	5,892	5,453	4,997	4,578	4,406
12 years	7,817	7,934	8,123	8,487	8,352	8,280	7,994	7,763	7,462
College									
1-3 years	2,373	2,489	2,584	2,699	2,651	2,712	2,731	2,729	2,772
4 or more years	3,065	3,061	3,097	3,312	3,550	3,383	3,546	3,518	3,477

Table 11

Percentages of Otherwise Valid Observations Lost Due to Income Imputations by Years of School Completed and Survey Years, 1968-1976

Years of School Completed	1968	1969	1970	1971	Year 1972	1973	1974	1975	1976
<u>Grade School</u>									
8-11 years	10.6	13.7	10.0	10.9	10.1	11.6	14.2	15.8	18.8
12 years	10.9	13.8	10.8	9.9	10.2	11.9	14.0	15.0	20.0
<u>College</u>									
1-3 years	12.0	15.0	11.4	11.7	11.6	12.7	14.3	16.4	19.6
4 or more years	12.4	15.3	12.4	12.6	7.3	13.3	14.4	16.2	22.4

Table 12

Estimated Average Experience Profiles by Age (17-30) for High School Graduates, 1967-75

Age	Probability that individual is in his 1st	2nd	3rd	4th	5th	6th	7th	8th year at work
17	.592	.276	.098	.029	.005	--	--	--
18	.535	.275	.128	.046	.014	.002	--	--
19	.458	.287	.151	.071	.025	.007	.001	--
20	.359	.290	.186	.097	.046	.016	.005	.001
21	.244	.259	.220	.146	.077	.036	.013	.004
22	.153	.201	.217	.189	.127	.067	.031	.011
23	.086	.136	.181	.198	.174	.119	.062	.029
24	.045	.083	.129	.173	.189	.166	.113	.060
25	.017	.044	.082	.127	.169	.186	.164	.112
26	.005	.017	.043	.080	.125	.168	.185	.164
27	.003	.007	.108	.043	.080	.124	.166	.183
28	.002	.004	.009	.020	.004	.079	.123	.165
29	.001	.002	.004	.010	.021	.044	.079	.122
30	.001	.002	.003	.005	.011	.021	.043	.077

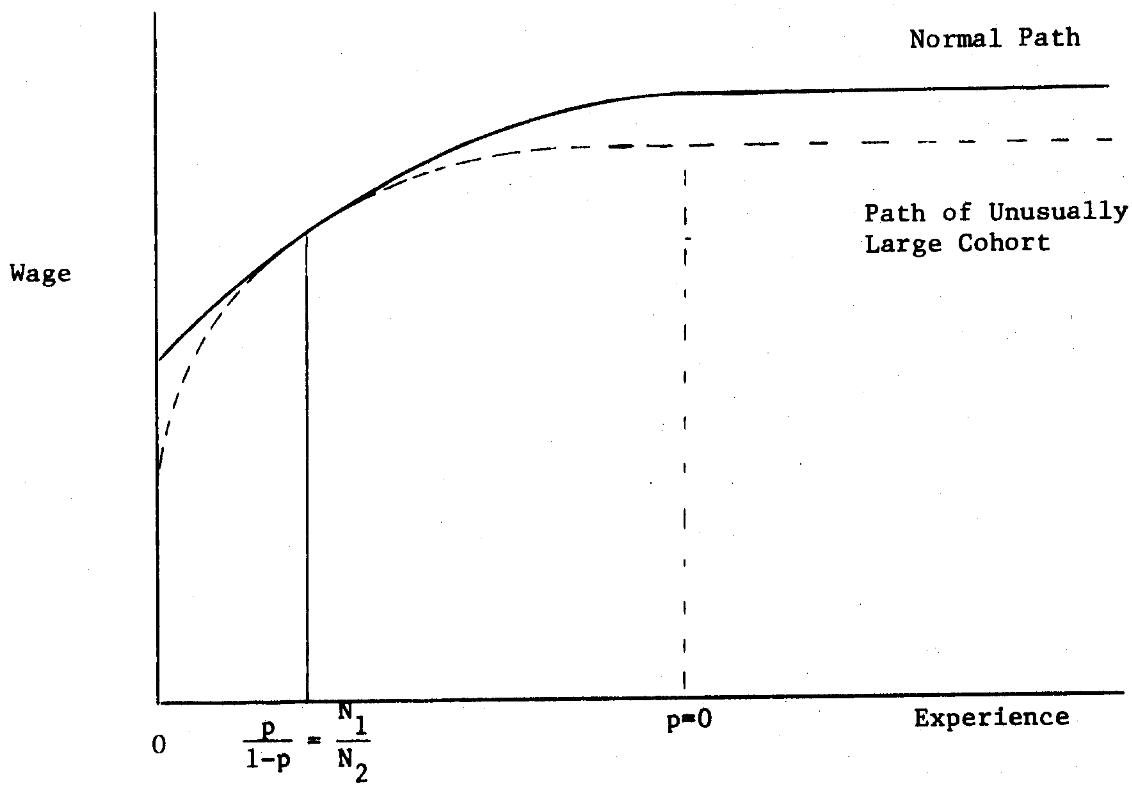

Fig. 1 -- Hypothetical Contrast Between Career Wage Paths of Normal and Unusually Large Cohort